R&D Management and the Use of Dynamic Metrics

by

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Submitted to the Sloan School of Management
in Partial Fulfillment of the Requirements for the Degree of
Master of Science in Management of Technology

at the

Massachusetts Institute of Technology

June 1997

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15 May 1997

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Abstract

R&D is an expensive investment that is treated as a cost or a burden in many organizations. We adopt the view that R&D not only increases the level of knowledge within a firm at a time when more firms are competing on the basis of knowledge, but also increases the capacity to acquire new knowledge, thereby permitting the firm to adapt to unforeseen changes. As an investment, R&D must be cultivated, actively managed, and directed. In this thesis we propose a set of metrics that reinforces these notions. Additionally, a scoring function is devised such that different metrics are emphasized as the degree of maturity of an R&D project evolves, permitting the scoring function to capture the dynamic nature of R&D projects. Using a number of high-tech R&D projects within a single firm as case studies, detailed scores are computed and are shown to correlate highly with management's perception of success for each project. This metric framework is then extended in order to provide insights into the management of a portfolio of projects. Lastly, the ability to generalize this metric framework is considered in the context of the pharmaceutical industry. It is the conclusion of this thesis that the multitude of factors examined by the proposed dynamic metric framework provides considerable insights into the health of R&D projects, as well as the project portfolio as a whole.

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Acknowledgment

The MIT Sloan School, as well as the Management of Technology Program, is an enormously wonderful environment within which a student can be immersed in the learning experience. I owe a great debt of gratitude to my fellow students and the program directors (Rochelle Weichman, Jennifer Mapes, Jim Utterback, Ed Roberts) for this wonderful learning environment. The faculty at Sloan was outstanding; I have learned a great deal from Shlomo Maital, Ralph Katz, Arnie Barnett, Russ Olive, Jim Utterback, Tim Luehrman, John Sterman, Barbara Bund, Michael Cusumano, Michael Scott Morton, Drazen Prelec, and Ed Roberts. Tim Luehrman, in particular, was instrumental in conveying to me the importance of viewing R&D projects as call-options – the subject of Chapter 5 of this thesis.

This wonderful educational opportunity would not have been possible if not for the tremendous support from Draper Laboratory. I am indebted to my CEO Ralph Jacobson for taking one more chance with the Draper Staff Associates Program. I am also extremely grateful to the other members of the executive council – Bill MacPherson, Dave Burke, and Dave Driscoll – for their support. Everyone at Draper has been very helpful and tolerant of my incessant surveys and interviews; I owe each one a heart-felt "thank you."

A number of people deserves special thanks: Jim Negro for his constant feedback and support; Jon Higginson – my fellow sloanie – for his many insights, and Mukund Desai and Anthony Sacramone for picking up the slack at Draper. To the special person at "Worldwide Pharmaceuticals" that provided so much raw materials and rich insights – you know who you are, and thank you. I have learned a tremendous amount from Jim Harrison: from his deep understanding of organizational issues, his thought processes, and his commitment to the people of Draper Laboratory – Jim is a wonderful role model for me. And to John Hauser – the best advisor anyone can ask for – my deepest appreciation to him for sharing his depth and breadth of knowledge, for always being available despite his busy schedule, and for his constant words of wisdom and encouragement.

Lastly, a word of thanks to my parents and brothers for putting up with me. To Irene (this time you'll have to share the dedication with someone else) and Lauren – this year of school would not be possible without your love and understanding. Thank you for all your sacrifices; 'tis time we smelled the roses – together. This thesis is dedicated to you, and to our future together.

This thesis was prepared with the generous support of the Charles Stark Draper Laboratory, Inc.

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1 Introduction

It is becoming a cliche to speak of the increasingly complex, competitive, and dynamic environment within which firms must operate today. Management gurus have for years proposed re-engineering, lean organizations, learning organizations, TQM, core competencies, empowerments, etc., all in an attempt to endow the firm with the ability to adapt to rapidly changing conditions. Often, however, management looks to these practices as quick fixes without ever focusing on their value creation aspects. Mindless pursuit of such fads has probably done more damage than good for many firms. Shapiro (1995), for example, defines the act of management fad surfing as "The practice of riding the crest of the latest management panacea and then paddling out again just in time to ride the next one; always absorbing for managers and lucrative for consultants; frequently disastrous for organizations" (p. 216).

Fads aside, an undeniable trend in today's business world is the increasing importance of knowledge. Every aspect of business – marketing, research, manufacturing, sales, forecasting, and management – is being redefined via increased reliance on the worker's ability to utilize knowledge. Thurow (1996), for example, considers the technological shift to an era dominated by man-made brain power one of the "economic tectonic plates" confronting the business world (pp. 8 - 10).

One of the surest ways of creating value for the firm is to invest in, and properly manage, research and development (R&D) activities. Such a course of action not only increases the level of knowledge within the firm, but also increases the *capacity* for knowledge. R&D activities not only correlate with innovativeness and adaptability, but also correlate with the firm's revenues, profits, market share, and shareholder value. If we believe knowledge and brain power to be among the sources of competitive advantage of firms in the foreseeable future, then it is imperative that R&D be properly managed. Of all the activities that can bring about an increased level of knowledge within a firm, R&D is perhaps the only common denominator that can bring about a sustained competitive advantage.

An example helps to illustrate our point. Perhaps the most famous downfall of an American empire is that of Xerox. Having launched its first product in 1959, by 1974 Xerox commanded 85% of the worldwide plain paper copier market. By 1981, Xerox's market share had fallen to 35%. Even during this enormous landslide, one of the most brilliant collections of scientists ever assembled was at Xerox's Palo Alto Research Center (PARC); it was remarked that some 58 out of the world's top 100 computer scientists were employed by PARC (Jacobson and Hillkirk, 1986, p. 257). PARC has been responsible for many significant innovations in the field of computer science, including the ethernet, the first personal computer (the Alto), graphical user interfaces, and the mouse. However, George Pake, president of Xerox PARC, noted that "In the early days, I can't remember any of the scientists wanting anything to do with copiers, even though they all knew that the copier business was paying everybody's salary" (Jacobson and Hillkirk, 1986, p. 257). During the 80's, however, when Xerox fought back and regained its status as a market leader, it focused a significant amount of PARC's research on copier activities. In addition to strategic focus, PARC's capacity to adapt was a significant factor in its turnaround success. It built a mini-ethernet for inter-module communications within the copier; it used artificial intelligence to decompose the electronics and diagnose problems; it used human-computer interface engineers to simulate copier behavior; and it used anthropologists from the University of California to help understand and convey user frustration when confronted with poorly-written error messages. In all, the value of PARC was fully realized only when management gained an appreciation for its proper role within the organization.

As the Xerox case demonstrates, corporate success cannot be achieved by simply throwing dollars at R&D; the systematic cultivation of the right talents, combined with management vision and know-how, are all necessary elements of building a useful R&D team. Yet today's firms are also confronted with a number of constraints that detracts from focused R&D efforts: conflicting demands for resources, increasingly vocal shareholders and stock analysts that demand immediate profits, and the high degree of uncertainty and long development times for some R&D projects. As James Tipping, Director of R&D at ICI Americas, Inc., noted, "R&D expenditure is often a convenient target when it comes to maintaining or increasing the company dividend. In fact, with R&D expenditure roughly the same amount as the dividend in many companies, it is a significant temptation" (Tipping, 1993, p. 13). Tipping has claimed further that:

> Despite the recognized importance of technology it would appear that many corporations are finding it easier to look elsewhere for competitive advantage – for example, from marketing, acquisition, capital investment, and so on. Even those world-class corporations that do look seriously to technology for competitive advantage are frustrated in their attempts to couple their R&D effectively into their business, in part because of the absence of an accepted methodology to measure effectiveness (value) and continuously improve their R&D. (Tipping, 1995, p. 22).

In an editorial in *Science*, Abelson (1994) noted that many U. S. firms have had to go through restructuring during the 1980's, and the resulting burden of debt has led many firms to curtail industrial research. Indeed, this focus on short-term returns led Hayes and Abernathy (1980) to write a paper in *Harvard Business Review* entitled "managing our way to economic decline."

One reason R&D has been an easy target for fiscal cutbacks is that its benefits are intangible, and its progress difficult to assess. For decades, researchers have struggled to devise a set of criteria, or metrics, for measuring the productivity and progress of R&D projects. Yet despite these efforts, there is no generally accepted set of metrics for gauging R&D, in part due to the wide range of metrics that are possible, from subjective, to bibliometric, to financial.

The purpose of this thesis is to examine in detail the efficacy associated with R&D metrics. While it is generally recognized that R&D projects evolve in focus over time, from basic research to prototypes to product/service introduction, the metrics used to evaluate R&D efforts have traditionally remained static. In this thesis we examine the hypothesis that *dynamic* metrics are better suited as indicators and predictors of project success than static ones.

1.1 Thesis Overview

This thesis adopts the view that R&D metrics form one important component in the toolbox of R&D managers. Metrics provide the ability to benchmark one effort against another, create historical data from which success can be determined, compel the manager to think thoroughly about the myriad of issues that confront every R&D project, and permit the compilation of data for project portfolio analysis. To accomplish these objectives, this thesis first reviews the literature to determine the factors of greatest importance to R&D team performance. Subsequently, detailed case studies are performed in order to assess specific projects against a set of proposed metrics.

This thesis is organized as follows. In Chapter 2 of this thesis, a review of the literature is provided. The review is focused on addressing two issues. First, what is the value of R&D? And second, what factors determine the success of R&D efforts.

The factors found in Chapter 2 are combined into a comprehensive set of metrics proposed in Chapter 3. These metrics lie along six dimensions: personnel, technical, strategic, project management, marketing, and financial. In addition to identifying this set of metrics, we also put forth the hypothesis that the relative importance of various metrics changes as the degree of maturity of a project evolves. A dynamic scoring function is thus proposed as part of Chapter 3.

To determine the efficacy of the proposed set of metrics, as well as the notion of dynamic metrics, we examine a research-intensive high-tech R&D organization – Draper Laboratory. Specifically, we demonstrate how scoring is performed using two projects taken from two different phases in their development process. The figure of merit used in assessing the predictive ability of the metric framework is that of management ratings of the successfulness of these projects. This subjective figure-of-merit is used for two reasons. First, since both technical and business managers are polled, it is believed that the collective opinions of these managers will reflect both the technical and the financial success of each project. Second, for a non-profit organization dedicated to advancing the state-of-the-art in science and technology, objective figures-of-merit are difficult to come by.

Having validated the concept of dynamic metrics in Chapter 4, Chapter 5 then builds upon the metrics in order to assist the R&D manager in assessing the health of the R&D pipeline. Specifically, by focusing on Draper Laboratory's internally-funded R&D projects, we show that a project portfolio comprised of these eight projects does not appear to possess the characteristics desired. That is, using an options-based argument, we show that Draper's R&D pipeline should contain more high-risk and high-payoff projects; this type of project was very much in the minority of the eight projects examined.

After examining issues pertaining to high-tech R&D, we then turn to another R&D-intensive industry – pharmaceuticals – to see the degree to which our framework generalizes. In particular, we look at the R&D process of an international pharmaceutical corporation, and examine in detail the evolution of two drug development processes – one successful and one unsuccessful. The determining factors are again cast into the metric framework posed earlier, and a number of interesting observations are made.

Finally, in Chapter 7, we provide our concluding remarks – summary, conclusions, and future work.

1.2 Contributions

While numerous R&D metrics have been proposed by a number of authors, most of these have been heuristic in nature and were created top-down, anecdotally. The primary contribution of this thesis is the bottom-up process of reviewing the literature to determine which metrics are important (and why) before inclusion in the set of metrics. The degree with which these metrics (or at least a subset of them) reflect management's perceptions – as shown in Chapter 4 – provides additional confidence that these are indeed the right measures to examine.

Of secondary importance is the use of dynamic metrics that evolve as the R&D project itself evolves, as well as the thought-process that is forced upon managers as they visualize and assess the portfolio of R&D projects. Although the dynamic metrics resulted in strong predictive capabilities, the added accuracy is somewhat marginal. Thus, aside from imposing an explicit recognition of the evolutionary nature of R&D projects, the real value may not justify the increased complexity. One value of the metric framework, however, is that it permits one to construct a visual rendition of the portfolio of projects. Although not ground-breaking, this exercise has provided valuable insights into the need for balance in Draper's internally-funded R&D efforts.

In all, there are many benefits to using a metric framework such as the one proposed herein. Although no single benefit is sufficiently compelling to justify the level of labor involved in creating a score for every project, the collective set of benefits appear to support the usefulness of such a framework. That is, by focusing attention on a wide-ranging set of issues, the metrics serve as a valuable reminder of the competing considerations that must be weighed by R&D managers, both on a per-project basis and on a portfolio-wide basis.

2 Background

There is compelling evidence that R&D creates value through a variety of mechanisms that generate new knowledge and information for the firm. Additionally, the more judicious the choice of R&D projects, and the better managed they are, the greater is the utility of this new information. The fundamental objective of R&D is to learn from, to exploit, and to bring into fruition new information. In today's technology-dominated, highly dynamic, and highly competitive business environment, a firm's ability to engage in meaningful R&D has a significant impact on its future success. Indeed, it has been suggested that an increase in R&D activity increases a firm's earnings, its market value, its level of innovations, its ability to exploit the innovations of others, its capacity to learn from industry research spillovers and university research, and its ability to adapt to unforeseen changes. Therefore, during a time when the global economy is becoming more knowledge driven, and as major innovations become more multi-disciplinary, R&D is increasingly critical to future success. In short, firms that manage R&D well have a major strategic weapon that provides considerable competitive advantage.

A number of studies have linked R&D to the fiscal soundness of a firm. Pakes and Griliches (1984) found strong correlations between R&D expenditures and the number of patents (also see Griliches (1990) for a survey of the uses of patent statistics as an economic indicator). Pakes (1985) also found a strong relationship between patents and stock value: based on regression analysis, an increase of one patent is associated with an increase in the firm's market value of $810,000, while an increase of $100 of R&D expenditure is associated (on average) with a $1,870 increase in the value of the firm. Ben-Zion (1984) and Bound (1984) noted the positive correlation between a firm's market value and R&D intensity. Similarly, Bachman (1972) argued that R&D increases the firm's profits, but with a 5 year time lag. Cohen and Levinthal (1989) argued that

> ... while R&D obviously generates innovations, it also develops the firm's ability to identify, assimilate, and exploit knowledge from the environment - in what we call a firm's 'learning' or 'absorptive' capacity. While encompassing a firm's ability to imitate new process or product innovations, absorptive capacity also includes the firm's ability to exploit outside knowledge of a more intermediate sort, such as basic research findings that provide the basis for subsequent applied research and development.

Cohen and Levinthal further argued that since a significant amount of innovation is derived from extramural knowledge, absorptive capacity "represents an important part of a firm's ability to create new knowledge."

A firm's absorptive capacity for knowledge can be extremely beneficial for exploiting spillover effects from both academic and industrial research. Jaffe (1989), and later Acs, et al., (1991), have found that significant amount of university research spills over to the industrial world, especially in the areas of pharmaceutical, medical, electronics, optics, and nuclear engineering. Both studies also indicated that such spillover effects are geographically dependent, however. Griliches (1992) has examined a number of earlier studies, and while some weaknesses were found in some of these studies, he concluded that there is a preponderance of evidence supporting the contention that "R&D spillovers are present, [and] their magnitudes may be quite large" (p. S43). An example is in the pharmaceutical field, where one study found that 54% of all major innovations were based on discoveries made outside the firm, during 1935-1962 (Mansfield, et al., 1971, p 178). Most interestingly, however, is the study by Jaffe (1986) which concluded that, in industries where R&D spillover is high, the number of patents per dollar of R&D expenditure correlates with the profit margin and market value of the firm. Thus not only is there a significant amount of external knowledge to be exploited, but the firms which are most capable of exploiting such knowledge are precisely those with the most significant R&D programs.

R&D spending – by itself – cannot assure success, however. In her study of the pharmaceutical industry, Henderson (1994) found that the most successful companies are as much as 40% more productive than their rivals, obtained more than twice the number of patents per research dollar, advanced to clinical trials more often, and were twice as likely to bring new drugs to market. Henderson found that it was the organizational structure – one that promote external awareness, contentious resource allocation, and attending to the tensions created by the functional structure – that accounted for the differences between successful and not-so-successful companies. As another example of R&D expenditures not telling the whole story, *Fortune Magazine*'s 1997 survey of America's most admired corporations showed an analysis of 143 companies comparing each company's R&D expenditure (as a percentage of revenues) to its reputation for innovativeness; the study showed little correlation between the two (O'Reilly, 1997). Nonetheless, "Although the evidence is limited, there appears to be a significant relationship between an industry or company's expenditures on basic research and its rate of productivity increase" (Mansfield, 1982, p. 25).

2.1 The Role of R&D within the Organization

In the book *Third Generation R&D*, Roussel, et al., (1991) argued that contemporary philosophy on R&D have evolved in the last few decades through three distinct phases. In the first generation, R&D is managed according to intuition, where R&D is seen largely as an independent entity with budgets computed in relation to corporate performance (e.g., sales), and is usually treated as an overhead expense. Roussel, et al., in fact referred to something far worse than overhead expense:

they mentioned the view of some CEOs that R&D expenditures are much like taxes: a necessary evil to be tolerated (p. 45).

In the second generation, according to Roussel et al., the management process has become more systematic: individual projects are reviewed by managers both inside and outside the R&D organization to ensure business focus and strategic fit. The authors, however, are strong proponents of a third generation approach to R&D, in which the portfolio of projects are constantly evaluated in relations to business and strategic focus. In particular, the authors believe that at this level, cross functional teams comprised of interests spanning all functional areas of the organization must work together to transcend functional lines in order to ensure the success of the effort. Although we will argue later against this view (for long term R&D projects intended to produce radical innovations, we will argue in favor of having a different blend of personnel as the project evolves, instead of a static cross-functional view), the authors' point is an important one – R&D projects are most successful when they are performed within the context (and interests) of the entire organization, not just of the R&D area.

In the same vein, Roberts (1983) observed that strategic planning has evolved through three distinct stages. In the first stage, the focus was entirely financial. The second stage, characterized by corporate behaviors during the 70's, is "less on financial measures and more on markets and the analysis of market participation of organizations" (p. 1). By the third stage, "the thinking is based on the recognition that technology has become of primary importance in domestic and international competition, and that technology must therefore increasingly be reflected in the bases for corporate plans and strategies" (p. 1). Indeed, in a study that summarizes the findings of four consulting companies,[1] "All four sources argue for strong alignment of the technology strategy with the corporate and business strategy. This requires clearly articulated statements of mission and vision, starting at the corporate level. The role of technology can then be incorporated into the specific business goals and communicated clearly to all functions" (Ransley and Rogers, 1994, p. 24).

Indeed, there is a preponderance of evidence supporting the need to integrate R&D activities with other functions in the organization. For example, Hise, et al., looked at 252 large manufacturing companies and concluded that collaboration between marketing and R&D during the product design phase is a key factor in product success (1990). Souder and Chakrabarti (1978) examined 114 R&D projects spanning basic consumer products, bulk industrial products, and industrial components fields, and found that "a high degree of R&D/marketing interaction, participation in problem definition, and integration all relate to both commercial and technical success," while low

[1]The four studies were performed by Meritus Consulting, Pugh-Roberts, SRI International, and Arthur D. Little. The ADL study was based upon the Roussel, et al (1991) work cited earlier. While not a direct author, Roberts – whose work (Roberts, 1983) was cited earlier – is a principal member of Pugh-Roberts.

degrees of interaction relate to commercial and technical failures (p. 90). Although other studies (cf. Cooper (1979); Maidique and Zirger (1984)) have reached similar conclusions, Gupta, et al., argued that one major contributing factor to the difficulties of integrating marketing and R&D is the sociocultural differences between them in their orientation toward time, project preference, tolerance for ambiguity, and professional orientation (1986). As an example, in an empirical study of more than 200 R&D and marketing managers, Gupta et al (1985) noted a difference in the perceived value of R&D/marketing interface by R&D and marketing managers. That is, "firms with successful new product programs achieve significantly greater R&D/marketing integration in each area requiring integration than the firms with unsuccessful new product programs. This conclusion is based on responses from the marketing managers. Responses from R&D managers do not support this conclusion so overwhelmingly" (p. 298). Although most studies agree on the economic benefits of early and consistent communications between R&D and marketing, such a coupling is not easy to accomplish. For a more extensive summary of the literature concerning R&D/marketing integration, see Griffin and Hauser (1995) and Urban and Hauser (1993, chapter 2).

The need for a similarly strong relationship between R&D and corporate strategy can also be articulated. For example, if the corporate strategy of a firm is an offensive one, then there is a greater need to develop more radically new products, thus leading to a greater need to allocate larger portions of the R&D budget toward basic/long-term research efforts (Link, 1985). Similarly, a firm needs to decide from where its future sources of revenues will likely come – from product innovations, from process innovations, or from new technological breakthroughs (Baker, et al, 1986). Indeed, in a study of 211 R&D projects, Baker, et al (1986) found that projects with an exclusively process focus were about 20% more likely to succeed than those with an exclusively product focus. In terms of the bottom line, the same study showed that product-oriented projects had the effect of increasing sales volume, whereas process-oriented projects had the effect of lowering production cost. Philip Smith, the CEO of General Foods, believes that "The linkage between research, business strategy, and marketing must begin with an overall corporate vision," where vision is a fully articulated picture of what the company is to become (Smith, 1988, p. 6). In this sense, the technology strategy of a firm not only relates to its present business strategy, it also relates to the strategic intent of the firm.

Consider the case of NEC. NEC top management determined that semiconductors would be the company's "core product." As a result of this focus, NEC entered a myriad of strategic alliances with the collective objective of building a number of competencies at relatively low costs (Prahalad and Hamel, 1990). NEC systematically identified its needs and its core competencies, and sought alliances to fill its voids. Cannon, similarly, strategically leveraged its core competencies in optics, microelectronics, and precision mechanics, and became a powerful player in the copier market

(Chrysler, 1990). In fact, in a tabulation of Canon 23 product lines, all but one (the calculator) is the result of at least two core competencies (Prahalad and Hamel, 1990). Thus, the "strategic intent" of the company must combine the vision of a winning position, as well as the articulation of how to get there (Hamel and Prahalad, 1989).

Another perspective on the coupling between R&D and strategy is related to the firm's desired role or position within the industry. Foster of McKinsey and Co., observed that technological advancements of an industry, as a function of time, occurs in the shape of an S-curve (Foster, 1982; Gluck and Foster, 1975). Thus to maximize revenues, a firm always wants to position itself at the beginning of the upward trend. Although somewhat simplistic in hindsight, this notion of S-curves had an enormous impact on management practices of the 70's and 80's. More recently, Utterback (1994) examined a number of industries, and identified the role that a dominant design has in altering the landscape of the industry. Utterback showed that the number of firms competing within an industry typically rises as the new industry matures, and undergoes a reduction once an "industry standard" – what Utterback calls a dominant design – emerges. Once again, the firm's ability to absorb new innovations, capture market share, influence industry standards, and develop new innovations all play a major role in the livelihood of the firm.

Indeed, Anderson and Tushman (1991) have observed that technology progresses through a succession of cycles, influenced by discontinuities and the emergence of dominant designs. That is:

> ... industries go through long periods of incremental technological change, punctuated by occasional technological discontinuities, major breakthroughs that push forward the state of the art in an industry's core technologies by an order of magnitude. Each discontinuity inaugurates an era of ferment, a period of rapid technological change in which different designs often clash as a new technology supplants its predecessor. This culminates in a dominant design that evolves into the standard architecture expressing the original, crude, breakthrough idea. With the appearance of a standard, incremental change reigns again until the next discontinuity. (Tushman and Anderson, 1997, p. v-vi).

In another study, Cooper (1984) examined 122 companies and 66 strategy variables in 19 categories, and grouped these companies into one of five strategies: technologically driven, balanced (between technology and business strategies), defensive but technologically deficient, low budget conservative, and high budget diverse. The study concluded that the balanced companies were overwhelmingly more successful in their new product development efforts. Thus, a firm's business strategy and innovation strategy are intimately related. Goodman and Lawless (1994) identified nine technology strategies. For example, is the firm's objective to be a pioneer, leader, or follower in new product development? Does the firm aim to capture market share through production efficiencies or does it aim to charge a premium for its highly customized products/services? These strategic decisions depend upon market considerations and the likely reaction of competitors, but they also have considerable impact on the R&D process (should R&D focus on process or product innovations? incremental or radical improvements?) as well as sources of revenues.

In short, Kantrow (1980) summed it up nicely when he wrote "Technological decisions are of fundamental importance to business, and therefore, must be made in the fullest context of each company's strategic thinking. This is plain common sense" (p. 21).

2.2 Management of R&D

R&D is difficult to manage for a number of reasons; perhaps foremost among these is the fact that R&D, by its very nature, is occurring for the first time, every time. In contrast to manufacturing, where continuous improvements have had an enormous impact, R&D does not pose the repeatedness and regularity of a production line; one cannot tweak a knob and immediately know the impact of such a change in R&D. Although R&D processes may repeat, the outcome does not. Fortunately, the cumulative wealth of knowledge from research on factors of success can yield considerable insights. In this section we review some of these factors of success.

Conventionally, R&D is comprised of five stages: basic research, exploratory research, applied research, development, and product improvement (or what some others have called technical services) (Pappas and Remer, 1985). Some companies have tailored the spectrum of R&D activities to their own internal functionalities. Allied Signal, for example, decomposes R&D into eight areas, including exploratory research, new products for existing markets, product extension, process improvement, raw materials substitution, regulatory response, energy savings, and diversification (Allio and Sheehan, 1984). We take the position that the granularity of decomposition doesn't matter much to our goal of understanding the R&D process, as long as the collective set of responsibilities remains consistent.

There is remarkable agreement on the best practices in corporate R&D management. Ransley and Rogers (1994) found seven such practices; these are summarized in Table 1. Note in particular that the first two best practices are highly consistent with the wealth of literature showing the need to better integrate R&D, marketing, and strategy that was reviewed in the previous section. The principles underlying Ranley and Rogers' best practices were also observed by Szakonyi (1990a, 1990b). Krause and Liu (1993), while agreeing with these best practices, added a few more, including the use of analytic tools for project assessment and portfolio balancing, the need

Table 1: Consensus of best practices in corporate R&D management.

1	Corporate, business, and technology strategies and plans are clear, well-integrated, communicated, and understood across all functions.
2.	Marketing, manufacturing, and planning functions join with R&D in assessing and selecting technology programs.
3.	Core technologies are defined and integrated into long-term technology and business plans.
4.	Results of R&D are measured against technology and business objectives.
5.	External threats and opportunities are systematically monitored.
6.	Cross functional teams and job rotation across functions are consistently used to foster technology transfer.
7.	Recruiting, training, and career development are integrated into the long-term R&D strategy.

Source: Ransley and Rogers (1994).

for a global R&D investment perspective, and the need for formal interaction mechanisms across functional lines. In the same study, Krause and Liu also observed as a best practice that "funding for basic research comes from corporate sources to ensure a long-term focus, whereas funding for development comes from business units to ensure accountability" (p. 18). At ARCO Chemical, Braunstein and Salsamendi stressed the need for balance among the portfolio of R&D projects, the skill base, the required resources, and the supporting facilities (1994). Lastly Szakonyi (1994a, 1994b), in identifying 10 indicators of effectiveness of R&D departments, also considered the degree to which technical personnel were motivated, and the degree to which new product ideas are generated. To evaluate effectiveness, Szakonyi used a 6-level scoring system (with values from 0 to 5), where 0 = issue is not recognized; 1 = initial efforts are being made; 2 = right skills are in place; 3 = appropriate methods are being used; 4 = responsibilities and priorities are clearly identified and articulated, and 5 = continuous improvements are taking place.

One major objective of corporate management is to ensure the relevance of the R&D department. To this end, many measures have been proposed. Steele (1988), for example, reviewed four such techniques. The first technique, a relatively conventional one, utilizes the relationship $V = R \cdot P/C$ where V denotes the R&D value index, R = net return, P = probability of attaining commercial success, and C = cost of research to achieve success. In the second approach, one that was used at GE Corporate R&D, two figures of merit are computed:

$$Impact = M \cdot S \cdot G \cdot T, \qquad Prob(success) = D \cdot C \cdot F \cdot O \tag{1}$$

where impact denotes the likely impact of the R&D program if it succeeds, and is defined as the product of market size (M), likely GE share (S), anticipated rate of growth (G), and sensitivity

Table 2: 33 metrics that comprise the technology value pyramid.

1.	financial return	18.	quality of personnel
2.	value of R&D pipeline	19.	development cycle time
3.	comparative manufacturing cost	20.	customer rating of technical capability
4.	product quality/reliability	21.	number and quality of patents
5.	gross profit margin	22.	protection from proprietary position
6.	market share	23.	evaluation of technology position
7.	strategic alignment	24.	% development milestones achieved
8.	distribution of tech. investments	25.	customer satisfaction
9.	no. ways technology is exploited	26.	direct-contact time with customer
10.	no. projects w/ business unit approval	27.	employee morale
11.	use of project milestones	28.	efficiency of technical processes
12.	% funding from business units	29.	preservation of technical output
13.	degree of tech. transfer to manufacturing	30.	goal clarity
14.	use of cross-functional teams	31.	project ownership and empowerment
15.	rating of product technology benefits	32.	management support
16.	response time to competitive moves	33.	project championship
17.	current technology investment		

Source: Tipping, et al., (1995).

to technological advance (T). The probability of success is defined as the product of technical difficulty (D), competitiveness of the effort (C), fit within the laboratory's resources (F), and the ease of transition to operations (O). Additionally, Steele discussed cost/benefits analysis that rely on financial assessments, and linear and dynamic programming techniques that seeks to maximize the benefits of a portfolio-based objective function subject to resource constraints.

Tipping, et al. (1995) adopt the view that "the true value from R&D becomes apparent only when one looks closely at the role that R&D plays throughout the creation and development of the innovations necessary to both defend and grow the corporation's businesses" (p. 22). To capture the "value creation" aspect of R&D, they looked at a set of 33 metrics that defined the "technology value pyramid." These metrics are shown in Table 2. Although these metrics highlight many of the important aspects of R&D, they rest upon an assumption that we cannot agree with: when allocating increases in revenues to R&D only, it presupposes that innovations in other parts of the corporation – marketing, sales, etc. – played no part in the revenue increase. This simplification is presumably necessary, however, when computing financial returns; for example, intermediate quantities such as new sales due to R&D, as well as R&D returns, become computable.

Minnesota Mining and Manufacturing (3M), one of the most innovative companies in the world, also stresses the importance of a process for evaluating R&D, as well as the importance of having a balanced view of R&D projects (Krogh, et al., 1988). 3M uses a technology audit

Table 3: 3M's R&D audit factors.

Program Technical Factors
• Technology strength (breadth, patentability, competitiveness) • Personnel (numbers, skills) • Competitive factors (knowledge of competition, 3M product performance) • Remaining R&D investment (vs. time to complete the project) • Manufacturing implementation (feasibility, cost, protectability) • Probability of technical success
Program Business Factors
• Financial potential (sales, profits) • 3M competitive position (market channels, product value) • Probability of marketing success
Laboratory Rating Factors
• Organization/planning (strategy, focus, clarity of goals) • Staffing (numbers, skills) • Program balance • Coordination/interaction (with marketing, manufacturing, other 3M labs)

Source: Krogh, et al., (1988).

process conducted by the Corporate Technical Planning and Coordination group, from which recommendations are made (not mandated). The audit is viewed as a tool to assist the operating unit in planning and allocating resources. To shelter embryonic efforts, projects in their infancy are not audited. Instead, this group generally audits major new product programs requiring significant investments, technology building programs, process development and cost savings programs, and incremental product improvement efforts. The 3M audit looks at both R&D program issues as well as R&D lab-wide issues. The specific factors upon which both program and the overall laboratory are evaluated are summarized in Table 3. As a whole, there appears to be considerable utility in 3M's audit system. With decades of data, "it has been found that the overall probability of program success, obtained by multiplying the probability of technical success by the probability of marketing success, correctly predicts a program's outcome a high percentage of the time" (Krogh, et al., 1988, p. 12).

Numerous studies have also identified reasons for the success and failure of new products. A typical set of new product development success factors, originally due to Montoya-Weiss and Calantone (1994), is shown in Table 4. New product development, however, is facing tremendous pressures from the shortened product life cycles and the faster rate of product obsolescence. As such, speed-to-market, or product development cycle time, is becoming increasingly important (Griffin, 1993). So important, in fact, is development cycle time that Clark and Fujimoto have used it to explain the Japanese auto industry's advantage over their American counterparts (1989).

Table 4: Typical product development success factors.

Strategic Factors
• Product advantage
• Technological synergy
• Marketing synergy
• Company resources
• Strategy of product
Development Process Factors
• Proficiency of technical activities
• Proficiency of up-front (homework) activities
• Proficiency of marketing activities
• Protocol (product definition)
• Top management support
• Speed to market
• Financial/business analysis
Market Environment Factors
• Market potential/size
• Market competitiveness
• External environment
Organizational Factors
• Top management support
• Internal/external relations
• Organizational factors

Source: Cooper and Kleinschmidt (1995, p. 375).

More recently, another success factor that has received significant attention is that of *product families*, that is, the design of products not in isolation but as part of a common architecture that can accommodate incremental modifications quickly and cost effectively (cf. Lehnerd (1987); Meyer, et al., (1995); Sanderson and Uzumeri (1995)).

The success factors for new product development have also been extended to new service development (cf. de Brentani (1989)). Table 5 lists 11 factors found to be important to the financial services industry in an analysis of the success factors of 56 successful and 50 failed efforts (Cooper and de Brentani (1991)).

2.2.1 Organizational and Human Resource Issues

One issue of great importance to successful R&D that is frequently overlooked by practicing managers is that of *human resources*. In particular, the management of the human skill base, the communication among the staff, and the incentives for these staff, are vitally important but often

Table 5: New service development success factors from the financial services industry.

• synergy of products
• product/market fit
• quality of execution of the launch
• unique/superior product
• quality of execution of marketing activities
• market growth and size
• service expertise
• quality of execution of technical activities
• quality of service delivery
• quality of execution of pre-development activities
• presence of intangible elements of the service offering

Source: Cooper and de Brentani (1991).

seen as intangibles not worth worrying about.

In his landmark studies on the role of gatekeepers within technical organizations, Allen identified technical gatekeepers as a critical link in the communication process (cf. Allen (1984)). In some organizations, these gatekeepers form the conduit through which information from the outside and from the organization at large are communicated to (and from) the technical subspecialty group. There is a strong tendency for gatekeepers to be promoted to management (Katz and Tushman, 1981), and working for a gatekeeper can significantly and positively influence an individual's career (Katz and Tushman, 1983). Gatekeepers are much more prevalent in organizational units that emphasis development than research (Allen, et al., 1979), presumably because scientists at research facilities are generally less encumbered by organizational cultures. Allen has identified three characteristics of gatekeepers (Allen, 1984, p. 163):

1. The gatekeeper is a high technical performer.
2. A high proportion of gatekeepers are first line supervisors.
3. With a little thought technical management can generally guess accurately who gatekeepers are.

The notion that there are individuals crucial to communications within a technical organization has been generalized to those that act as liaisons for the laboratory or the organization (Tushman, 1977). Ancona and Caldwell have also examined the role of communications within the organization as performed by ambassadors, task coordinators, and scouts, as well as the impact such communicators have on the success of the project within the firm (Ancona and Caldwell, 1997).

In all, the patterns of communications and the individuals responsible for them play a critical role in enhancing teamwork and promoting information diffusion within the organization.

To properly staff R&D projects, management must realize that not all technical professionals are the same. It was recognized that the career orientation between engineering and science undergraduates are different (Krulee and Nadler, 1960), and such differences continued well after graduation (Ritti, 1971). Engineers, as a group, tend to value achieving corporate objectives (e.g., meeting schedules, developing successful products, assisting in corporate growth), whereas scientists tend to seek reward and recognition not from within the corporation but instead from his/her peer group of scientists at large. And whereas scientists value journal publications as a means of communicating with the outside world, engineers have ranked publications as one of their least valued activities. The educational background of scientists almost always includes a PhD, whereas that of engineers frequently does not. Thus, "To treat both professions (i.e., scientists and engineers) as one and then to search for consistencies in behavior and outlook is almost certain to produce error and confusion of result" (Allen, 1988).

Incentives also need to play a major role in motivating R&D staff. In an attempt to encourage Hughes Research Laboratories (Malibu, CA) to be "relevant," director Arthur Chester devised a system that goes beyond the traditional salary increases, bonuses, and promotions. He devised an incentive system around five principles: (i) the customer of the R&D organization is the business units; (ii) an integrated technical-business plan revolves around knowing the customer; (iii) the R&D organization must bridge the gap between basic research occurring outside of Hughes and the short-term needs of the business units; (iv) the R&D organization acts not only as the inventor of new and improved products/processes, it must also act as internal consultants, and (v) wherever possible, incentives are based on quantifiable accomplishments and assessed by parties outside the laboratory (Chester, 1995).

Improvements in R&D, innovativeness, and overall quality must not come from strict control, but instead must incorporate changes in organizational culture. In the early 1980s when both Roger Smith (CEO of GM) and Don Peterson (CEO of Ford) initiated efforts to improve quality, Ford had the worst quality of the U. S. Big 3 auto makers. Whereas GM focused on technological and financial changes, over 80% of the improvements at Ford were from the cultural and human side. And while Ford has become the highest quality of the Big 3, GM's efforts (with the exception of NUMMI) have failed (O'Reilly and Tushman, 1997). Waterman, in his account of Federal Express, credits the combination of formal quality metrics with cultural changes surrounding empowerment as the basis of their success (Waterman, 1994). Indeed, O'Reilly and Tushman have found that company norms that promote creativity – by providing incentives for risk taking and change instead of penalties for failures – as well as fast implementations appear to create more innovative companies (O'Reilly and Tushman, 1997).

2.3 Summary

The purpose of this literature review is two fold. First, to understand the economic and financial justifications for investing in R&D; second, to extract a set factors that are likely to indicate successful R&D outcomes.

The literature appears to have overwhelming justification for R&D investments: it is linked to innovativeness, revenues, profits, patents, stock value, total market capitalization, creation of new knowledge, spillover effects, and the ability to exploit outside knowledge. It is also shown, however, that R&D spending by itself is insufficient, it must be accompanied by judicious management of R&D resources.

Judicious management of R&D resources comes in many flavors. The success factors we found in our literature review can be broadly grouped into six categories:

1. Personnel: a project must have the right mixture of capabilities, motivations, qualifications, functional expertise, and advocacy;
2. Technical: a project must have sufficient infrastructure and technical expertise to utilize innovative and competitive technologies;
3. Strategic: individual projects must fit within the overall scope of the firm's strategic and business directions, and thus the individual project must contribute to the overall R&D and technology portfolio, and produce opportunities for future leveraging;
4. Project management: each effort must have a communication network – be it formal or informal – that coordinates the activities. Project management must also stay on track with respect to the plan, promote the development of cohesion within the team, and minimize development time.
5. Marketing: each R&D effort must be able to deploy sufficient marketing resources to understand the market trends, listen to the voice of the customer, gauge the reaction of competitors, and be able to successfully market the fruits of the R&D effort.
6. Financial: a project that is focused on producing revenues for the firm must assess the potential financial returns versus the investment expenditures.

The literature review also implicitly noted the differences in focus between the new, embryonic, and more basic research efforts versus the more focused, mature, and advanced development efforts. In the next chapter we will define a set of 44 metrics based upon the observations we've made about factors of successful R&D efforts; furthermore, we will devise a scoring system that is capable of capturing the changes in focus of an R&D effort as it progresses from the more embryonic phase to the more focused and mature phase.

3 A Dynamic Framework for R&D Metrics

The background discussions of the previous section provide considerable insight into the set of issues that are important for an R&D effort. In this chapter we put forth a proposal for a set of metrics that we believe, when taken collectively, provides a good indicator of the strengths and weaknesses of an R&D project. We proceed in two parts. First, we outline the set of metrics we believe to be important; second, we propose a framework for capturing the *dynamic* aspect of R&D metrics.

3.1 A Proposed Set of Metrics

The set of metrics we propose to examine are divided into six categories:

1. *Personnel*: the set of benchmarks that indicates the backgrounds, skills, and mix of the R&D team members.
2. *Technical*: the set of benchmarks that conveys the technical capabilities and awareness of the team.
3. *Strategic*: the set of metrics that indicates the strategic fit of the project with respect to issues of major long-term importance to the organization, such as investment and business strategies.
4. *Project Management*: the set of metrics that represents the degree to which management has control over the course of the project.
5. *Marketing*: the set of metrics which relates to competitor and marketplace responses.
6. *Financial*: the set of metrics that indicates the financial soundness of the project.

In total, we propose a set of 44 metrics; they are listed in Table 6. The exact nature of these metrics are described next.

3.1.1 Personnel Metrics

Proper mixture of scientists/engineers. As discussed earlier, there is a difference in attitudes, motivations, rewards, and methodologies between engineers and scientists. The proper staffing of a project, therefore, depends upon the objectives of the project *at any particular moment.* Although

Table 6: A proposed set of metrics.

Personnel	proper mixture of scientists/engineers appropriate level of involvement from marketing, manufacturing, etc. technical reputation of team members technical qualification of team members motivation level of team members prior technical output of team members, incl. patents, publications, etc. advocacy from a strong project champion mutual respect and adequate "chemistry" among team members
Technical	proper technical depth and breadth probability of technical success continued monitoring of outside innovations and emerging technologies sufficient technical infrastructure (instruments, equipment, etc.) innovativeness of approach competitiveness of technology quality of work to date clarity of technical objectives
Strategic	compatibility and consistency among project, corporate, and business objectives consistency between project demands and investment strategy degree of project contribution toward a balanced R&D or technology portfolio degree of coordination with other parts of the firm extent to which project outcomes are useful to other parts of the firm extent to which the project leverages or extends core competencies availability of teaming/alliance arrangements to remedy weaknesses opportunity for future efforts to leverage project ability to secure proprietary protection
Project Mgmt	spending rate vs. budget progress against milestones sufficiency of remaining resources level of technical coordination achieved effectiveness of internal communications development cycle time level of cohesion within team
Marketing	favorability of projected market trends and growth rates suitability of project objectives to meeting customer needs ability of firm to market the product/service probable reaction of competitors competitiveness of product/service
Financial	sales/profit potential ensuing market share potential liability total capital expenditure expected return on investment expected time to breakeven protection afforded by proprietary means

it is recognized that different individuals may exhibit differences, the prototypical scientist may be more interested in understanding the fundamental knowledge than in making a deliverable. Similarly, engineers trained to focus on delivering the stated task may be less inclined to pursue unexpected results that frequently lead to serendipitous discoveries.

Appropriate level of involvement from marketing, manufacturing, etc. Also discussed earlier were the myriad of studies showing the increase in likelihood of project success when multi-functional teams are at work. Although such teams may very well be important at the later phases of a project, we would argue that organizational and marketing considerations – even at the earliest stages – will help to focus the project. Indeed, there is some evidence supporting this view, as shown by Katz in his study of the DEC Alpha team (1997). One must be careful, however, not to be overly constrained by such concerns at the exploratory research stage.

Technical reputation of team members, and *Technical qualification of team members.* We have chosen to represent technical personnel quality by two metrics: reputation and qualification. Collectively, a high score on these two metrics should indicate that the technical capabilities (not necessarily the appropriateness) of the team is high.

Motivation level of team members. The motivation level of a team impacts its ability to withstand setbacks, overcome obstacles, and be persistent. Motivation levels are also infectious -- a highly motivated team can raise the performance level of a down-in-the-dumps individual.

Prior technical output of team members, including patents, publications, etc. One indicator of the innovativeness of an individual is his/her past contributions. For some industries, patents can be used as an indicator for innovativeness. For scientists or corporate cultures that value publications, bibliometric measures can also be used. Indeed, there is a long history of using such measures in the study of R&D metrics. Although different firms may assign different degrees of importance to various types of technical output, the converse is presumably always true – those that, given the opportunity, have provided little or no technical output in the past are unlikely to do so now.

Advocacy from a strong project champion. Survival of an idea within the firm is in general a prerequisite to market success. A strong voice of advocacy from within the organization can significantly raise the level of organizational commitment to the effort. This may be especially important for large projects, radically innovative projects, or projects in a highly resource-constrained environment.

Mutual respect and adequate "chemistry" among team members. Cross functional and multi-disciplined teams are of value only when differing viewpoints are listened to. A prerequisite to taking someone's ideas seriously is that of respect. Thus the appropriate chemistry is necessary for ensuring that ideas are heard and alternatives are considered.

3.1.2 Technical Metrics

Proper technical depth, and *Proper technical breadth.* These two metrics begin to define the appropriateness of the technical talents at hand. Specifically, the team must have sufficient depth to go beyond superficial solutions. Furthermore, since innovations are frequently derived from applying conventional concepts to new situations, the appropriate breadth within a team may also foster innovative solutions to problems.

Probability of technical success. The value of this metric depends upon the technology strategy of the firm. Although most firms tend to minimize risks, the recognition that R&D expenditures are investments makes it clear that high-risk and high-payoff projects which do not involve large up-front investments are precisely the projects that an organization may want to pursue (see Section 5). Note that a high numerical score here implies low risk, and a low value implies high risk. The interpretation is as follows. At the end, a combined score will be computed: the higher the score, the "better" the project according to this set of metrics. A high value for probability of technical success thus implies that, all else being equal (including financial returns), the lower the technical risk the better.

Continued monitoring of outside innovations and emerging technologies. It is sometimes the case that a particular need provides the impetus for an organization to initiate a research program. This is the case in the defense industry, for example, when the Gulf War precipitated a multitude of activities in mine hunting. Under such circumstances, many organizations may initiate similar R&D programs at the same time. It is therefore critically important for the project team to continue to monitor outside innovations and emerging technologies so that it does not get "blind sided" by others. Such monitoring activities are also important when an organization is the market leader in a particular field, for it has been documented numerous times that it is precisely these market leaders that fail to see the next radical innovation which threatens its very existence (cf. Utterback (1994)). If it is also the charter of the R&D group to utilize academic research or to exploit industrial research from other organizations, then the monitoring of journal publications and trade magazines can also provide valuable information.

Sufficient technical infrastructure (instruments, equipment, etc.). R&D, by its nature, is an inherently knowledge-intensive activity requiring technical infrastructures. Such infrastructures include scientific instruments, computational equipment, and even access to technical publications. Insufficient infrastructures can significantly impair the team's performance.

Innovativeness of approach. As discussed earlier, there is widespread consumer preference for products that are more innovative, and there is no reason to expect this preference to be different for services or industrial (i.e., non-consumer) products. Note that we do not consider product/service improvements - efforts focused on incremental improvements and routine engineering - as part of the purview of the R&D organization; these are typically performed within the business units.

Competitiveness of technology. In general, given the risky nature of R&D, the cost associated with R&D, and finally, for consumer products, the cost of new product launch, it is hardly worth the cost if the introduced product is not competitive. This is especially true today when there is tremendous industrial capacity to "clone" or reverse engineer a product or service. The technology involved in the R&D effort must be competitive for the end-product to be viable.

Quality of work to date. Intuitively, it appears that the best indicator of the quality of future work is the quality of work to date. Although we know of no systematic study that validates this intuition, anecdotal evidence seems to support this observation.

Clarity of technical objectives. Disagreements are inevitable in any complex R&D effort. When this occurs, it is frequently necessary to return to the objectives of the effort in order to determine the best course of action. This is only possible if the objectives are clearly defined by, communicated, and agreed upon by all parties involved. In other words, it is important to have a shared vision of the technical objectives of the effort.

3.1.3 Strategic Metrics

Compatibility and consistency among project, corporate, and business objectives. As discussed earlier, there is considerable evidence that projects not aligned with the corporate and business objectives do not receive sufficient resources and are almost always unsuccessful. Projects should only be pursued if they further the corporate and business objectives of the firm.

Consistency between project demands and investment strategy. The corporate strategy defines the future direction of the firm. Any R&D project that requires investment commitments that differ substantially from the corporate strategy should raise a flag. This does not imply that the project is not worth pursuing - corporate strategy should be sufficiently flexible to exploit new opportunities that arise. A discrepancy between project demands and investment strategy simply indicates that there's a consistency problem that needs to be resolved.

Degree of project contribution toward a balanced R&D or technical portfolio. A project should seldom be pursued on a stand-alone basis; its contribution toward the corporate portfolio of projects is one of the most important contributions. An excessively large set of risky projects, for example, may jeopardize the future livelihood of the firm.

Degree of coordination with other parts of the laboratory. We've mentioned the importance of cross-functional involvement. From an organizational point of view, it is not sufficient to simply have involvement from various functions, there must be coordination. The synergy that is desired from cross functional involvement can only occur if the coordinated-actions aspect of the involvement is present.

Extent to which project outcomes are useful to other parts of the firm. The spillover effect of R&D, as discussed earlier, forms a major synergistic effect among R&D projects. As such, the value of an R&D project contains more than just the value of its outcome, but also the degree to which it contributes to other efforts within the firm. For example, a R&D project that seeks new ways to improve the communication infrastructure may not directly benefit the external customer, but may still be worth pursuing due to the impact it will have on other efforts.

Extent to which the project leverages or extends core competencies. As noted in earlier discussions, projects are most likely to succeed when they either exploit existing core competencies or extend these competencies to achieve new ones. This is presumably because core competencies are, by definition, capabilities of the firm which it can do better than nearly everyone else, and serve as effective differentiators. All R&D projects, to some degree, should extend the capabilities of the firm. Projects that are routine should not be pursued under the heading of R&D.

Opportunity for future efforts to leverage project. R&D projects are important not only for the new things they discover, but also as a conduit to impart benefits to future efforts. That is, the organization as a whole derives the greatest benefit when every effort builds upon other efforts; the knowledge acquired from one project must be passed on to other efforts. Note that this knowledge does not only come from successful projects; it is frequently the case that a project does not achieve the objectives defined at the onset, yet the inability to achieve those results may be very important to future projects.

Availability of teaming/alliance arrangements to remedy weaknesses. R&D is expensive, and frequently risky. As new ventures become more complex and multi-disciplinary, it is increasingly difficult to expect all the relevant expertise to reside within the firm. There are three choices in this case: (i) abandon the venture; (ii) build up internal expertise, or (iii) use partnering to remedy weaknesses. As a risk-sharing mechanism, strategic partnering generally requires less (initial) cost than growing a new internal capability, but requires skills and patience to extract the rewards (cf. Ouchi and Bolton (1988); Hamel, et al., (1989)). Thus partnering provides the opportunity to mitigate risks, reduce investment expenditure, and rapidly acquire information and expertise.

Ability to secure proprietary protection. Because of the cost of R&D and the rapid pace with which firms can now reverse engineer or clone products and services, it is important to protect the firm's investment in the project. One way to achieve such protection is to consider methods of protection such as trade secrets, copyrights, and patents.

3.1.4 Project Management Metrics

Spending rate versus budget, and *Progress against milestones.* Project management requires the ability to plan. The ability to stay within budget, while executing the planned tasks, is thus an important indicator of the project leaders' management capabilities.

Sufficiency of remaining resources. A major role of project management is to ensure that the remaining resources are sufficient to achieve the remaining tasks. When the resources are insufficient and additional resources cannot be obtained, then the abandonment option must be considered in order to minimize losses.

Level of technical coordination achieved. In many of today's complex R&D tasks, there is necessarily multi-disciplinary involvement. Project managers must ensure that proper coordination is achieved among personnel from different disciplines, organizational sub-cultures, and geographical locations, all employing different modes of operation. We have discussed the differences between engineers and scientists, for example, and others have examined the differences between hardware and software engineers. Coordination among these different disciplines is frequently difficult, but absolutely necessary.

Effectiveness of internal communications. We reviewed earlier the crucial role internal communications play in determining the success of an effort. One vital aspect of project management, therefore, is the consistent communication of the project status, resource demands, and usefulness of the project to other stakeholders within the firm. Additionally, project managers must ensure that the communication among team members is clear, timely, and complete.

Development cycle time. We noted earlier the importance of development cycle time to the competitive positioning of the firm. Although the evidence generally comes from consumer products (automotive and pharmaceutical industries), there is no reason to believe this is different for industrial customers, since presumably their customers are facing similar fast cycle time demands.

Level of cohesion within team. Not only is it the responsibility of project managers to provide effective communications, it is also their responsibility to ensure the presence of a shared vision of the project objectives. Cohesion, or the ability to work as one unit, only occurs when every member of the team buys into both the objectives and the approach of the project.

3.1.5 Marketing Metrics

Favorability of projected market trends and growth rates. From marketing's perspective, a project should only be pursued if there are favorable market conditions for the product to be revenue-producing. Furthermore, depending on the technical risks and initial investments, a high internal hurdle rate may be applied – one that is sufficiently high that only favorable market trends may satisfy it.

Suitability of project objectives to meeting customer needs. Perhaps the most important input from marketing is the assurance that the R&D effort is focused on objectives that will meet the needs of the customer. Some companies, such as Hoechst Celanese (see Chapter 6) even require customer-verified competitive advantages of a product prior to proceeding.

Ability of firm to market the product/service. Marketing must also ensure that the R&D effort produces products/services that can be effectively marketed by the firm; if the firm does not have the know-how or resources to market the R&D output, then it must acquire the know-how, otherwise the R&D investment will never become revenue-producing.

Probable reaction of competitors. One job of marketing is to know the competitors. The likely reaction of competitors – from starting a price war, to introducing a new product that directly competes with the result of the R&D effort, to cloning – will have significant consequences for the profitability of the new product. Kodak, for example, failed to correctly assess its competitors' reactions when it released the Disc Camera aimed at the low-end, 110-film photography market, and its competitors countered with auto-focus cameras that utilized the higher-resolution 35-mm film. Kodak has decided to discontinue supporting the Disc Camera, less than 10 years after product introduction (Mizelle, 1997).

Competitiveness of product/service. In general, the purpose of R&D is not to produce commodity products; instead, it is to produce new and differentiable products. As a differentiated product or service, therefore, it must be competitive with respect to existing products/services in order to attract customers.

3.1.6 Financial Metrics

Sales/profit potential. Few organizations can afford to engage in R&D if it doesn't contribute to new revenues. By the same token, although we have argued that not every R&D project needs to be revenue producing, every project should have a positive impact on the final cash flow. For near-term R&D projects, the sales or profit potential is one of the most important metrics.

Ensuing market share. Many studies have identified the numerous advantages of having a commanding market share; such advantages include brand recognition, ability to influence buyers and sellers, and better position from which to attack competitors. If a new product/service can provide the firm with a significant market position, then the firm as a whole becomes financially stronger.

Potential liability. For many industries, potential liability, and the resources necessary to protect the firm against litigation, creates a significant drain on both cash flow and management's attention. This is true, for example, in the biomedical field, where the firm must be concerned about the long-term ill-effects of its products. The expenditures associated with protection from liability must be used to discount any positive cash flow expected from the outcomes of the R&D project.

Total capital expenditure. The total capital expenditures necessary for a project – as described earlier for the strategic metric of consistency between corporate and project investments – must also be used to discount any potentially positive cash flow.

Expected return on investment, and *Expected time to breakeven.* These are two metrics that provide indications of the expected amount of profits to be produced by the R&D venture; the first indicating the total amount of revenues over the life of the new product/service, the second indicating how quickly the R&D investment can be paid back. Another commonly used metric that achieves similar objectives is to compute the expected return on investment over a 3-year period starting from product launch (see Hauser and Zettelmeyer (1996)).

Protection afforded by proprietary means. The use of proprietary means to protect the R&D investment has been discussed earlier. From a strictly financial perspective, the important factor here is the degree to which the investment is exposed – i.e., not protected by any such means over the lifetime of the product/service. For example, if the development cycle is very long (say 10 years) and it was decided that a 20-year patent is the best means for protection, then the financial forecast must take into account the exposure to cloning after the first 10 years of the product's life.

Table 7: Definition of Tiers of R&D.

Tier 0	Tier 1	Tier 2	Tier 3	Tier 4
Basic Research	Exploratory Research	Applied Research	Development	Technical Services

3.2 Dynamic Metrics

R&D, by its very nature, is inherently evolutionary: it proceeds from the embryonic, exploratory stages through feasibility studies and prototypes, to actual end-products. Various terms are used to denote these different stages: Hauser and Zettelmeyer (1996) refer to these stages as *tiers of R&D*; Department of Defense research centers (e.g., Office of Naval Research, Air Force Office of Scientific Research, or Army Research Office) refer to the stages as 6.1, 6.2, 6.3, and 6.4[2]; we will use the terms basic research, exploratory research, applied research, development, and technical services to denote the various tiers (see Table 7).

Tier 0, or basic research, is research generally performed at the university level. The focus of this research is on the acquisition of new knowledge, typically for the purpose of educating students and new researchers. By its very nature, Tier 0 is focused on fundamental knowledge, rather than on practical applications of that knowledge. For this reason, corporate R&D seldom undertakes Tier 0 research.

Tier 1, or exploratory research, is more frequently performed by corporate R&D centers, with the focus of identifying promising new technologies that will eventually form the basis for new break-throughs and future revenues. Tier 1 research is typically embryonic in nature, and bridges the gap between the corporate and academic worlds. Frequently, Tier 1 researchers scan the outside world (including universities) as well as other technical disciplines for innovative ideas and emerging technologies.

By Tier 2, the research has become more focused. Typically, by this applied research stage, technical feasibility and alignment with the firm's strategies become the more major concerns. It is also at this stage that some considerations to future earning potentials are given. While the technical activities of Tier 2 generally continue to be performed by the R&D group under corporate funding, more of the work is performed by engineers instead of scientists, and cross-functional and multi-disciplinary involvement begin to enter the R&D picture.

By Tier 3, the development stage, the focus has shifted to deliverables, profits, and meeting

[2]More explicitly, 6.1 refers to basic research; 6.2 refers to exploratory development; 6.3 refers to advanced development, and 6.4 refers to engineering development.

customer needs. The integration among finance, marketing, design, manufacturing, and corporate strategy becomes critical. In general, this is the most expensive stage, and the business focus of this tier implies that R&D at this stage is frequently funded by the business units.

Tier 4, or technical services, is generally not performed by the R&D group, and typically not considered part of corporate R&D. The focus of this stage is on incremental product improvements to satisfy the needs of the customer, so that the life cycle of the product/service can be increased, market share increased, and production cost decreased.

Although exceptions to the above generalizations most certainly exist, there is fairly good agreement on the roles and functions of the various tiers, as shown by Hauser and Zettelmeyer's interview of 43 CTOs, CEOs, and researchers from 10 research-intensive organizations (Hauser and Zettelmeyer, 1996). We summarize in Table 8 our perception of the tiers to which each metric is of greatest importance.

Conceptually, we depict the corporate R&D process by Figure 1. Specifically, exploratory research is an ongoing process, from which promising efforts are selected to become applied research. During this applied research stage, the business unit begins to have some input into the direction of the R&D effort. The applied research eventually leads to development, which has considerable business unit involvement and is frequently funded by the business unit. These three tiers comprise the corporate R&D world. Tier 4, technical services, continues with even more business unit involvement, but is generally considered to be outside the purview of the R&D laboratory. This sequence of steps are executed continuously; that is, there is a constant flow of projects being transitioned from exploratory research to applied research, etc.

It is the hypothesis of this thesis that a metric scoring function which takes into account the transitional, evolutionary, and dynamic nature of a R&D project is a better indicator of the efficacy of the project than a static metric function.

One approach to constructing a scoring function is as follows. First, note that it is desirable to have a variable weighting of the different categories of metrics as a function of the maturity of the R&D project. Second, a scoring function needs to handle two cases: a symmetric case (e.g., for a metric that is of primary importance in Tier 2) and an asymmetric case (e.g., for a metric that is of primary importance in Tier 1 or Tier 3).

To satisfy the objective of having the greatest weight on the metric appropriate for a particular tier, we decided to use a normally-weighted function. That is, the weighting function is designed

Table 8: Metrics and the tiers to which they are of primary importance.

category	*metric*	*tier 1*	*tier 2*	*tier 3*
Personnel	proper mixture of scientists/engineers	√		
	cross functional involvement	√		
	technical reputation of team members	√		
	technical qualification of team members	√		
	motivation level of team members	√		
	prior technical output of team members	√	√	
	advocacy from a strong project champion	√	√	
	mutual respect among team members	√	√	
Technical	proper technical depth and breadth	√	√	
	probability of technical success	√	√	
	continued outside monitoring	√	√	
	quality of work to date	√	√	
	sufficient technical infrastructure		√	
	innovativeness of approach		√	
	competitiveness of technology		√	
	clarity of technical objectives		√	
Strategic	alignment of project/corporate/business objectives		√	
	consistency in project demands & investment strategy		√	
	degree of project contribution toward R&D portfolio		√	
	degree of coordination with other parts of the firm		√	√
	usefulness of project outcomes to other parts of the firm		√	√
	leveraging or extension of core competencies		√	√
	availability of teaming/alliance arrangements		√	√
	opportunity for future efforts to leverage project		√	√
	ability to secure proprietary protection		√	√
Project Mgmt.	spending rate vs. budget		√	√
	progress against milestones		√	√
	sufficiency of remaining resources		√	√
	level of technical coordination achieved		√	√
	effectiveness of internal communications		√	√
	development cycle time		√	√
	level of cohesion within team		√	√
Marketing	favorability of projected market trends			√
	suitability to meeting customer needs			√
	ability to market the product/service			√
	probable reaction of competitors			√
	competitiveness of product/service			√
Financial	sales/profit potential			√
	ensuing market share			√
	potential liability			√
	total capital expenditure			√
	expected return on investment			√
	expected time to breakeven			√
	protection afforded by proprietary means			√

Figure 1: The evolution of corporate R&D projects.

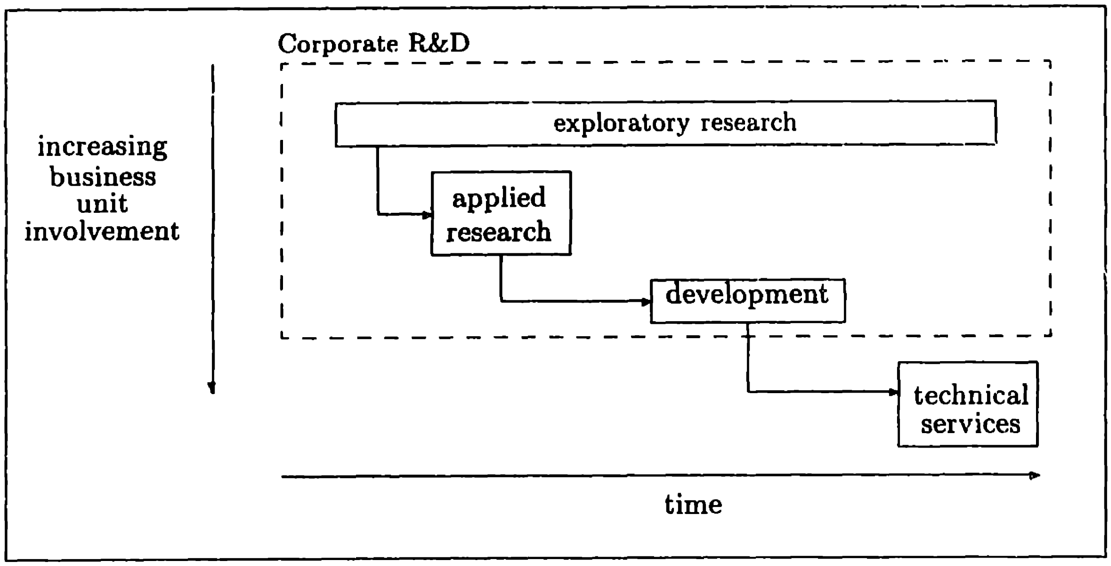


Table 9: Weighting terms for attributes in each tier.

Of primary importance to ...	*Tier 1 Weight*	*Tier 2 Weight*	*Tier 3 Weight*
... *Tier 1 project*	5	4	1
... *Tier 2 project*	2.5	5	2.5
... *Tier 3 project*	1	4	5

to be a discrete approximation to the normal distribution function:

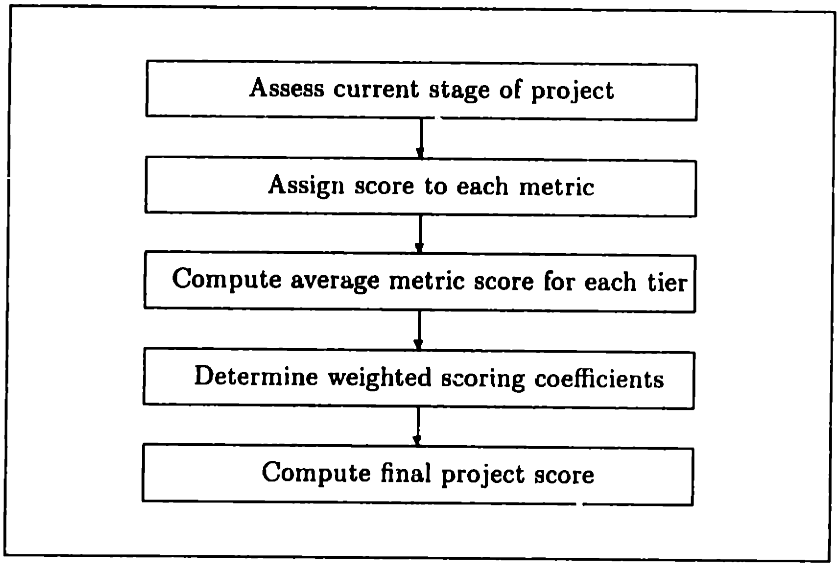
$$N_{\sigma,\mu}(x) = \frac{1}{\sigma\sqrt{2\pi}} e^{-\frac{(x-\mu)^2}{2\sigma^2}} \tag{2}$$

where μ,σ denote the mean and standard deviation of the distribution, respectively. To obtain the weights for the centrally-weighted case, the σ is chosen such that a cell greater than 1 cell away from the mean has a discretized value of 0. Similarly, for the asymmetric case, the σ is chosen such that a cell greater than 2 cells away from the mean has a discretized value of 0.[3] In both cases, the areas under the curve are subsequently normalized to 10 and discretized. The resulting weights are summarized in Table 9.

To construct the final score of a project, therefore, we go through the following steps (a summary is shown in Figure 2):

[3]The resulting σ's are 0.85 for the symmetric case, and 1.1 for the asymmetric case.

Figure 2: Steps in the construction of a project score.



1. Assess the current stage of the project; i.e., determine whether this project is a Tier 1, Tier 2, or Tier 3 project. Call the current stage i.

2. Assign a score to each metric for this project; each score is in the range of 0 - 5, with 5 being the highest value.

3. For each metric of primary importance to each tier (i.e., use Table 8), compute the average score. Thus for each project, there are three component scores (call them a, b, and c), where each component is a value between 0 - 5, and a corresponds to the average score of all metrics of primary importance to Tier 1, b for Tier 2, and c for Tier 3.

4. Determine the normally-weighted coefficients from Table 9 appropriate for Tier i; denote these coefficients by w_1, w_2, and w_3.

5. Compute final project score via $S = a \cdot w_1 + b \cdot w_2 + c \cdot w_3$.

The end result is a score ranging from 0 - 50. Note that if a project rightfully belongs, say, half way between Tier 1 and Tier 2, this scoring framework can accommodate it simply by computing separately the project scores as if it were Tier 1 and Tier 2, then interpolating in between.

To illustrate this procedure, we assume that we have a Tier-2 project A measured on five metrics: $\alpha, \beta, \gamma, \delta, \epsilon$. Furthermore, let α and β denote metrics of primary importance to Tier 1, γ and δ for Tier 2, and ϵ for Tier 3. The assessed scores are summarized in Table 10. Since this is a Tier 2 project, we know from Table 9 that the appropriate weights are: $w_1 = 2.5, w_2 = 5, w_3 = 2.5$.

Table 10: An illustration of how a project score is computed.

metric	*tier 1*	*tier 2*	*tier 3*
α	4		
β	3		
γ		5	
δ		3	
ϵ			4
average	3.5	4	4

Multiplying the weights with the component averages in Table 10 yields a final score of 38.75 (out of 50).

3.2.1 Weaknesses of the Dynamic Metric Concept

Perhaps the greatest weakness of our metric framework is the degree to which subjective judgments are made. The implication is that two users of these metrics may end up with very different scores. Furthermore, when judgments are subjective, they are more susceptible to personal biases. To be sure, these are valid criticisms. However, we note that (i) many firms have successfully adopted R&D processes based upon subjective metrics; (ii) any R&D review process will necessarily rely on subjective measurements, since the intangible nature of R&D precludes a purely objective evaluation, and (iii) despite the subjective nature of these metrics, considerable insights and utility can still be gained from applying them in a rigorous and consistent manner, as we will demonstrate in the subsequent chapter.

Another weakness of the proposed measurement system is that all metrics within the same tier are subject to the same weight. One can very well imagine, for example, that for a radically different project, one may want to emphasize the past, demonstrated innovativeness of the project team members more than, say technical depth. We have kept the uniform weights, however, in an attempt to make the metric simple and useful. An R&D manager who is fully cognizant of the underlying intangibles we're trying to gauge with this metric will, we are sure, modify the metric to reflect the attributes most important to him/her, using a weighting function that appears most reasonable to him/her.

Although we have attempted to capture some flavor of the "cultural" aspect of the team (e.g., cohesiveness, chemistry), these metrics do not do justice to the real impact of organizational culture. That is, these metrics do not capture in anyway the innovative and entrepreneurial spirit of organizations such as 3M or ThermalElectron. The implication is two fold. First, the user must appreciate the role of cultures and the limitations of this set of metrics. Second, one must be cautious applying these metrics across different companies.

4 Applying Dynamic Metrics to High-Tech R&D Projects

In the previous section we proposed a framework for assessing the efficacy of R&D projects along a number of dimensions; the framework captures the evolutionary nature of R&D projects by applying differential weights to individual metrics as a function of the maturity of the R&D project. To examine the utility of the proposed framework, we consider in this chapter a research-intensive organization that is engaged in high-tech contract R&D. Specifically, we will examine a number of projects at Draper Laboratory, and validate the proposed framework by analyzing two projects in detail. Six additional projects will also be examined, for the purpose of gaining a broader perspective on Draper projects; these six projects will not be discussed to the same level of detail, however.

This chapter is organized as follows. First, background information concerning Draper Laboratory is provided, followed by a short description of our research methodology. We then discuss in detail the micromechanical inertial instruments project and the intelligent sonobuoy project. These discussions are followed by a less detailed description of six other projects. Lastly, we ask the question of how our proposed metrics compare against the metrics used by Draper managers in evaluating R&D projects.

4.1 Case Study: Draper Laboratory

Draper Laboratory, or more formally, the Charles Stark Draper Laboratory, Inc., is an independent, Massachusetts-chartered not-for-profit corporation dedicated to applied research, engineering development, education, and technology transfer. Prior to 1973, Draper Lab was known as MIT's Instrumentation Laboratory; it spun off to become an independent corporation in 1973. Draper Lab's corporate headquarters are located in Cambridge, Massachusetts; it also operates 14 offsite locations across the U.S. During Draper Fiscal Year 1996, Draper Lab received $230M in gross revenues, approximately $98M of which was for subcontracts that flowed through Draper. The remainder – $132M – was for Draper's in-house operations. During 1996, Draper employed slightly over 1200 employees, approximately 2/3 of whom are technical staff or technicians (Draper, 1996a, 1996b).

As a not-for-profit R&D organization, Draper's objectives are more technically-pioneering focused than profit focused. More explicitly, Draper Laboratory's mission is (Draper, 1996a):

> To serve the national interest in applied research, engineering development, education, and technology transfer by ...

- Helping our customers clarify their requirements and conceptualize innovative solutions to their problems
- Demonstrating those solutions through the design and development of fieldable engineering prototypes
- Transitioning our products and processes to industry for production and providing follow-on support
- Promoting and supporting practical technical education

Historically, Draper has met these mission objectives by developing first-of-a-kind systems (examples include the guidance and navigation modules for the Apollo Program for NASA, and the Unmanned Underwater Vehicle for DARPA), transitioning these designs to industry, delivering high value added engineering services, and sponsoring numerous local university students (49 graduate fellowships and 64 undergraduate co-op opportunities during 1996).

Draper is organized as a matrix, with Engineering along one dimension and Programs along on the other dimension of the matrix. Thus each staff engineer is accountable to both Engineering line management as well as the managers of the programs in which the staff engineer is involved. Draper's Engineering organization is divided into (i) systems engineering; (ii) inertial instruments and mechanical design; (iii) electronic design and sensor development; (iv) guidance, navigation and control; (v) software engineering; (vi) hardware engineering and prototyping, and (vii) system integration, test evaluation, and quality management. Draper's Programs organization is divided into (i) Navy strategic programs; (ii) tactical systems; (iii) space and missile programs; (iv) ocean systems and special operations, and (v) applied information and automation systems.

Draper provides engineering services, in general, on a cost-plus-fee basis. As a not-for-profit, its fees are subsequently plowed back into the operations of the Laboratory. Draper engages in an aggressive internal independent R&D (IR&D) program, paid for in part by the fees that it receives. Draper's IR&D program provides the Laboratory with the opportunity to pursue technical activities that it believes will further its mission, without necessarily being constrained by the current funding climate. More explicitly (Ahn, 1995):

> The goals of the IR&D program are:
>
> (a) To develop the advanced technologies needed to support our primary mission areas.
>
> (b) To support innovative research.
>
> (c) To support educational activities.

For the remainder of this chapter we will examine a number of IR&D projects that were active during the 1992-1993 time period. These projects were chosen on the basis of several parameters: each one was a multi-year effort, each one had a research and/or development focus (as opposed to some projects that were infrastructure related), and each one had approximately $1M in total investment.[4] Specifically, we will examine the micromechanical inertial instruments and the intelligent sonobuoy projects in detail, followed by a less detailed examination of six other multi-year projects. The primary conclusion from our analysis is that the dynamic metrics framework appears to be valuable in two different ways. First, the set of proposed metrics serves as a checklist to ensure that various important considerations are taken into account. As will be shown by the two detailed examples, areas in which the intelligent sonobuoy project were less strong turned out to be major reasons for its less successful outcome. Second, by looking at the 8 projects in total, it is shown that the scoring system from the proposed framework appears to correlate well with the perception of the management as to the degree of success of each project. Thus the dynamic metric framework appears to be a reasonable proxy for predicting project success.

4.2 Research Methodology

For our study of Draper Laboratory a number of sources of information were utilized. For every Draper internal R&D project, there are technical plans, year-end reports, and final reports; these sources were used extensively. Additionally, status reports, presentations, meeting reports, and weekly/monthly progress reports were available for some projects and also used. For a number of projects, either outside publications or internal reports were also issued. Lastly, extensive interviews were conducted with the principle investigators, project participants, Engineering and Programs managers, and other stakeholders as appropriate. Through these mechanisms, we were able to obtain both realtime knowledge as they were available at the time of the projects, as well as retrospective information.

To assess management's retrospective opinion with respect to the degree of success of each project, a survey was conducted. In all, 67 managers were polled, and 29 (or 43.3%) responded. The surveys were sent to Engineering line managers, program managers and assistant program managers, heads of business departments and their respective business development managers, corporate officers, director of strategic planning, and director of IR&D. The respondents represent a fair cross section of functionalities: responses were received from every Programs directorate,

[4] As it turned out, one of the projects that was selected did *not* meet this last criterion – although the internal account number was the same, the effort underwent a sufficiently significant redirection that we excluded the technical activities and funding after redirection.

and all but one Engineering department responded. Additionally, both VP of Programs and VP of Engineering responded. Engineering represented 45% of the responses, and Programs the remaining 55%. Each survey asked each respondent to rate the degree of success of each project, on a 7-point scale. Space for free-form responses regarding these projects, as well as the metrics they used, were also provided. The survey was done anonymously, although respondents that wanted a copy of the analysis results or those who welcomed follow-up interviews were asked to include their names.

To assess the R&D metrics most valued by management, these same managers were also asked to provide, on a 7-point scale, their perceived level of importance of the metrics listed in Table 6.[5] This was done separately for IR&D and external R&D contracts. Once again, a free-form area was provided for respondents to indicate their opinions of these metrics.

It is worthwhile noting that the survey explicitly asked the respondents to identify their own R&D metrics before viewing the list of metrics that were provide on a separate page, so that the respondents were not biased by the metrics proposed within this thesis.

4.3 Micromechanical Sensors

The micromechanical inertial sensors project is an internally-funded R&D effort that started in 1987. It represents the single largest commitment of internal funding to a project in Draper's history. In total, it has consumed many millions of dollars, and continues to do so to this day.

The single most noteworthy aspect of the micromechanical sensors project is that it is fully aligned with the core competencies of the Lab – guidance, navigation and control. This initiative represents a new way of sensing accelerations and angular motion – it essentially replicates the conventional gyroscope and accelerometer in silicon, and promises to perform these operations using sensors that are millimeters in size. The number of applications of such sensors is immense -- from guided munitions, to robotics, to vehicles (cars, planes, underwater vehicles, etc.), to biomedical instrumentation. It is this strategic alignment that has sheltered it from budget cuts and attrition, and has sustained the number of scientists and engineers working it.

More specifically, micromechanical sensors are silicon-based structures that exploit various physical properties on a microscopic scale to measure acceleration and angular motion. One such recent approach is the micro-machined comb-drive tuning fork gyro (TFG). That is (Kourepenis,

[5] Only 42 out of the 44 metrics were used in the management survey; the financial metrics *ensuing market share* and *potential liability* were excluded from the survey due to the lack of applicability within the Draper Laboratory operating environment.

et al., 1996, p. 2):

> The TFG ... consists of a silicon structure suspended above a glass substrate containing metallization deposited for sensor interfacing. The silicon structure contains two masses suspended by a sequence of beams that are anchored to the substrate at specific points. By applying voltages to the outer motor drives, the two masses are electrostatically forced to generate lateral, in-plane oscillatory motion. An angular rate, Ω, applied about the input axis, perpendicular to the velocity vector of the masses, generate a Coriolis Force that acts to push the masses in and out of the plane of oscillation. ... The resultant motion is measured by capacitor plates under each of the two masses, providing a signal proportional to the rate input.

In the sections that follow we will examine the various R&D metrics as they pertain to the micromechanical sensors project at two different time periods: 1989 and 1993. Specifically, in 1989 the project was still in its infancy (i.e., Tier 1), struggling to achieve a design which would permit the construction of a high-performance sensor. By 1993, the project was somewhere between Tiers 2 and 3, in the sense that a design breakthrough has occurred, and spill-over effects into other research and contract projects were starting to occur. Note that a summary of the scores for each category of metrics is provided at the end of that section. We begin with personnel factors.

4.3.1 Micromechanical Sensors Personnel Factors

Right mixture of scientists/engineers; Technical reputation of team members, and *Technical qualification of team members.* The micromechanical project has consistently utilized top-notch technical talent. A number of scientists and engineers were involved in the effort both during 1989 and 1993; they were comprised of PhDs, MSs, BSs. By 1993, a new PhD from University of California at Berkeley added to the strength of the design team, and was partially responsible for transferring the Berkeley design to Draper, which turned out to be a critical factor in achieving technical success. Collectively, the reputation and qualifications of the team members were all of high caliber.

Cross functional involvement, and *Advocacy from champion.* From early in the program, five different functions were identified, and each function contributed greatly to the project. The five functions are:

1. Mechanical design and fabrication
2. Electronics

Table 11: Micromechanical sensors: personnel factors.

factor	*1989*	*1993*
Right mixture of scientists/engineers	4	5
Cross functional involvement	4	5
Reputation	4	5
Qualification	5	5
Prior outputs	3	5
Motivation	5	5
Advocacy from champion	5	5
Mutual respect and chemistry	4	4

3. Packaging
4. Integration, testing, and development
5. Program management and marketing

Although the boundaries of these different functions were not always clear, cross functional involvement was always maintained. Program management, in particular, played a strong role in championing the project. The program managers were all well-respected, long-time members of the organization. Combined with the quality of the technical staff, the collective voice of advocacy was quite strong. So entrenched in the IR&D funding cycle, in fact, was the micromechanical project that other projects competing for resources often felt that the micromechanical project received too many resources.

Prior outputs. Prior outputs, during 1989, were average with respect to other Draper project teams. Because of the breakthroughs that occurred along the way, by 1993, however, there was an impressive body of published works and patents credited to the project team.

Mutual respect and chemistry. The mutual respect of the team members became quite apparent during the interviews. On numerous occasions, members of different functions voiced the opinion that some particular breakthrough only occurred because of the cross functional involvement. In fact, there appears to be explicit recognition on the part of most project team members that the end-objectives can only be achieved through the talents and capabilities of *all* of the functional areas.

A summary of the scores for personnel factors are shown in Table 11.

4.3.2 Micromechanical Sensors Technical Factors

Technical depth/breadth, and *Clarity of technical objectives.* As was mentioned earlier, the micromechanical sensors project assembled a strong technical team with quality scientists and engineers. The work quality was high, and the technical objectives were well defined.

Continued outside monitoring. During 1989, the team was still focusing on designing a double-gimbaled monolithic gyro; the effort, however, was not a great success. There were several alternative design approaches, however, and through continued monitoring as well as collaborations with University of California's Berkeley Sensors and Actuators Center, an improved design was found by the 1991-92 timeframe. In particular, Draper continued to monitor outside designs, including those taking place at Berkeley, Michigan, and MIT. This monitoring activity and the Berkeley design would turn out to be major factors in the technical success of the tuning fork gyro design.

Quality of work to date, and *Probability of technical success.* During 1989, although positive results were obtained with the monolithic gimbal design, technical success was by no means assured. In fact, by ~ 1991, there was a push for an alternative design because of unresolved issues pertaining to the "manufacturability, cost of static and dynamic balancing, and readout noise" associated with the gimbaled design.[6] Thus technical risk was substantially mitigated with the tuning fork gyro design.

Technical infrastructure; *Innovativeness of approach*, *Competitiveness of technology.* The micromechanical project was (and still is) a significant investment. In part, the investment was necessary for infrastructures – for example, a micro-machine fabrication facility. In part as a result of improvements in infrastructure, and in part as a result of the infusion of new design approaches, the level of innovativeness and technological competitiveness increased from 1989 to 1993.

A summary of the scores for the technical factors are shown in Table 12.

4.3.3 Micromechanical Sensors Strategic Factors

Compatibility of project/corporate/business objectives, *Usefulness of project outcomes to other parts of the firm*, *Leveraging of core competencies*, and *Opportunity for future leveraging of project.* Draper's bread-and-butter is guidance, navigation, and control. The objectives of the micromechanical sensors project further these core competencies by providing low-cost, moderate-accuracy,

[6]Source: unpublished Draper internal memo, 1992.

Table 12: Micromechanical sensors: technical factors.

factor	*1989*	*1993*
Technical depth/breadth	4	5
Continued outside monitoring	3	5
Quality of work to date	4	5
Probability of technical success	2	4
Technical infrastructure	3	4
Innovativeness of approach	4	5
Competitiveness of technology	4	5
Clarity of technical objectives	4	4

and small sensors. Thus not only does this leverage capabilities in guidance/navigation algorithms and hardware, but also miniature electronics, fabrication, packaging, etc. The results of the advances from this project have been useful in a number of other applications, including a micromechanical hydrophone design (which itself produced a few patents as well as significant revenues). Furthermore, as Draper pursues newer applications in the civilian realm, such as biomedical applications, micromechanical sensors will undoubtedly play a role in these new arenas.

Consistency between project demands and investment strategy, and *Project contribution towards a balanced portfolio.* As mentioned before, the micromechanical venture is highly expensive, and as is typically the case with new product development, the expenditures increased as the project matured and efforts are directed at all the different aspects of the prototype instead of merely design. Although still consistent with Draper's investment strategy, such a high demand for investments has deflected resources away from other projects. This initiative, however, has the "high-risk, high-payoff" profile that is desired in new projects (see Chapter 5), and as such, certainly contributes positively toward the portfolio of projects.

Availability of teaming/alliances, and *Proprietary protection.* In part, because of the magnitude of Draper's investment, senior management devoted greater efforts to reaping the rewards of this venture. As an example, Draper formed an alliance with Rockwell (now Boeing) to transfer micromechanical inertial sensors technology to commercial and military products; Draper retained its position for non-inertial micromechanical devices, however. In addition to the Rockwell alliance, Draper pursued academic alliances from the very beginning, which also contributed to Draper's final design methodology. Another aspect of the size of the investment is the compelling need to protect such an investment. To that end, Draper has been aggressive in seeking patent protection; in all, a few tens of patents have been awarded to Draper, especially as the new design emerged.

Table 13: Micromechanical sensors: strategic factors.

factor	*1989*	*1993*
Compatibility of project/corporate/business objectives	5	5
Consistency between project demands and investment strategy	5	4
Project contribution towards a balanced portfolio	4	4
Coordination with other parts of firm	4	5
Usefulness of project outcomes to other parts of the firm	4	5
Leveraging of core competencies	5	5
Availability of teaming/alliances	5	5
Opportunity for future leveraging of project	5	5
Proprietary protection	4	5

A summary of the scores for the technical factors are shown in Table 13.

4.3.4 Micromechanical Sensors Project Management Factors

Project management factors, overall, were on par with or slightly better than other Draper projects, with a couple of exceptions.

Level of technical coordination, and *Effectiveness of internal communications.* The *de facto* technical leader of the effort is a gate-keeper in every sense of the term. He is a PhD who has contributed significantly to the design of the sensor. Additionally, he is also a highly responsible leader – he calls the weekly meetings, types up the minutes, tracks the progress of every task (complete and incomplete), and sets the tone for the internal communication. He is well respected witbin the organization, and is frequently called upon to present on behalf of the micromechanical effort to outside sponsors. Additionally, this gate-keeper writes papers and travels to conferences, and brings back information from the outside world as well as conveys internal capability outwards. To a large extent, this gate keeper has been the glue that kept the team together and communicating.

A summary of the project management factors scores is shown in Table 14.

4.3.5 Micromechanical Sensors Marketing Factors

Projected market growth. From the very beginning of the project, there was a realization that the potential applicability of micromechanical sensors would be enormous, provided that the technical difficulties can be overcome. As the project evolved, the number of potential applications have

Table 14: Micromechanical sensors: project management factors.

factor	*1989*	*1993*
Spending rate vs. budget	3	3
Progress against milestones	4	4
Sufficiency of remaining resources	3	3
Level of technical coordination	4	5
Effectiveness of internal communications	4	5
Development cycle time	3	3
Team cohesion	3	3

continued to grow. Indeed, although it was believed by some that the government would be the primary customer for these sensors, it became clear in the very early 1990's that the consumer market may be far greater. By 1993, Draper entered into the strategic alliance with Rockwell in part for applications in the automotive industry, and other applications such as precision guided munitions and biomedical instrumentation are on the horizon.

Meets customer needs. Overall, the "customer" needs sensors that are small, reliable, robust, reasonably accurate, and inexpensive. As such, the micromechanical effort is focusing on precisely these objectives. In 1989, however, the product was clearly not capable of delivering such capabilities because of the immaturity of the product. By 1993, the tuning fork gyro appeared to be well-positioned for meeting the needs of most customers. Efforts are still continuing to this day in improving the accuracy of the system.

One caveat to the above characterization is that of cost. In part, Draper is anticipating that the cost of these sensors would be low when produced in quantity. If, however, the Rockwell alliance was to dissipate and this high-volume application disappeared, it would be highly uncertain whether these sensors can be produced sufficiently inexpensively to satisfy Draper's customers.

Ability to market product. Draper, in general, is quite good at marketing guidance, navigation, and control capabilities to its traditional customers. Although some learning needed to take place early in the project as to how to best promote these sensors to government sponsors, the Draper marketing staff appeared to top that learning curve rather quickly. As in the previous bullet, there is one caveat – Draper is probably still not fully comfortable marketing such products to commercial or industrial clients.

Reaction of competitors, and *Competitiveness of product.* Draper's philosophy in the development of micromechanical sensors is to stay ahead of the competition. By one estimate, the gyros appear to be 3-years ahead of the capabilities of other firms, and the accelerometers appear to

Table 15: Micromechanical sensors: marketing factors.

factor	*1989*	*1993*
Projected market growth	5	5
Meets customer needs	2	4
Ability to market product	4	5
Reaction of competitors	3	3
Competitiveness of product	4	5

be at least one-year ahead. One would have to expect, however, that if the market is as large as is anticipated, Draper cannot be as far ahead of its competitors as it has in the past. That is, as more applications are developed that encompass these sensors, it is likely that the reactions of competitors will become stronger. Draper, however, has sought to protect its position via patents.

A summary of the scores for the marketing factors are shown in Table 15.

4.3.6 Micromechanical Sensors Financial Factors

Sales potential, and *Return on investment.* As mentioned previously, the potential market for this product is quite sizable. Although by 1993 there was still not a 100% return on investment, several large contracts were on the horizon. By one estimate, with recent increases in internal investments, the return on investment is just short of 100%.

Total capital expenditure, and *Time to breakeven.* The total capital expenditure on this project has been quite large, and is increasing. As such, this project received a low score for *total capital expenditure* for 1993. The total time to breakeven has been fairly long, and with increases in expenditures, it is difficult to anticipate when breakeven will occur.

Proprietary protection. As mentioned previously, Draper has been aggressive in seeking patent protections; in particular, the entire micromechanical effort is comprised of relatively identifiable designs and processes, making proprietary protection fairly simple.

A summary of the scores for the financial factors are shown in Table 16.

4.4 Intelligent Sonobuoy

The Intelligent Sonobuoy project is an internally funded R&D project that had as its focus a revolutionary way of performing underwater minehunting for the Department of Defense. The

Table 16: Micromechanical Sensors: Financial Factors

factor	*1989*	*1993*
Sales potential	4	5
Total capital expenditure	3	1
Return on investment	4	5
Time to breakeven	4	3
Proprietary protection	4	5

project evolved over a 3 year period, very much following the 3-Tier structure described previously: Year 1 was spent on conceiving of an innovative system concept and assessing what else was on the market; Year 2 was focused on feasibility studies and component demonstrations, and Year 3 was focused on establishing the market for the prototype. Year 3 was funded by the business unit (i.e., Programs).

The Intelligent Sonobuoy project was an example of a technology-push project: Draper believes its technical staff needs to be challenged to produce highly innovative solutions to important problems, and funds numerous technology-push projects to promote technical innovativeness. In the early, embryonic stage of these projects, marketing focus was not a requirement; in fact, marketing is somewhat discouraged as some members of management felt that marketing focus at such an early stage would hinder and unnecessarily constrain the technical approach.

Using the Persian Gulf War as the backdrop, the Intelligent Sonobuoy project's focus was to conceive of a cost-effective system for defeating underwater mines, since mines were highly effective as a threat against the coalition forces, and indeed one such mine crippled the USS Princeton. The Intelligent Sonobuoy concept was to deploy a set of grapefruit-sized floating sonars (buoys), each capable of transmitting sonar signals, receiving signals, processing the data, communicating the results, navigating (determining its position), and ultimately producing a highly accurate map of the terrain showing locations of mines. It was believed that the system would have higher mine designation capabilities than conventional systems because acoustic data is obtained from multiple perspectives.

After three years of internal funding totaling more than $1M, it was concluded that:

1. The initial technical vision had to be modified significantly;
2. A scaled-down demonstration system was used to show that, although the system concept worked, it did not show the degree of improvement that was hoped;
3. Although the probability of marketing success was initially believed to be reasonable, after

an initial foray into this market it was realized that marketing success probability was low.

And finally, no subsequent revenues were produced.

We now examine how this project rated with respect to the metrics defined earlier. We will once again focus on two different time periods: Year 1 and Year 3 of the project.

4.4.1 Intelligent Sonobuoy Personnel Factors

Right mixture of scientists/engineers, Technical reputation of team members, Technical qualification of team members, and *Prior outputs*. The sonobuoy project team was comprised of a number of talented technical contributors with a reasonable mixture of scientists and engineers, from a number of technical disciplines. The reputation and qualification of these contributors were all fairly high, and all the dominant contributors have a history of producing quality outputs. However, as the project evolved from Year 1 to Year 3, there was also a significant loss in technical personnel (see next section), resulting in a decrease in scores among these factors.

Cross functional involvement. The nature of Draper's technology-push projects is that no marketing involvement was necessary, and for this reason there was only minimal cross-functional involvement. Although the most relevant business department was involved in "red team" reviews of the project, that was essentially the extent of the marketing involvement for the first year. By Year 3, however, when the project transitioned to Programs, there was considerable marketing involvement.

Advocacy from champion. One other element that was missing from the sonobuoy project is strong advocacy within the organization, at either time period. There were several reasons for this. First, when selecting technology-push projects, essentially two years of funding are committed, and the projects are thus (for the most part) sheltered from attack. Thus, there is never the incentive to build internal support after funding is committed. In this sense, the effort had "sponsorship," but not "advocacy." Second, as will be discussed in greater depth later, the intelligent sonobuoy effort was not perceived to be central to the core interests of the Laboratory.

Motivation, and *Mutual respect and chemistry.* During Year 1 of the project, the motivational level was quite high, since it was considered an honor to be selected as one of the leading technology-push projects for the year. After the departure of some key team members (see the technical factors section), and realization of the difficulties surrounding the marketing of this concept, both internally and externally, the motivational level appeared to wean by Year 3. The sense of respect for fellow team members, however, remained high.

Table 17: Intelligent sonobuoy: personnel factors.

factor	*Year 1*	*Year 3*
Right mixture of scientists/engineers	4	3
Cross functional involvement	1	3
Reputation	5	3
Qualification	4	3
Prior outputs	4	3
Motivation	4	3
Advocacy from champion	1	1
Mutual respect and chemistry	4	4

A summary of the personnel factors is shown in Table 17.

4.4.2 Intelligent Sonobuoy Technical Factors

Technical depth/breadth, and *Quality of work to date*. The single greatest technical issue affecting the Intelligent Sonobuoy project was the loss of key personnel during the 3-year effort – the principle investigator of the project for the first year left during Year 2, and similarly, the new principle investigator left during Year 3. Although continuity of the project was somewhat preserved since the third principle investigator was on the project from the very start, the loss of technical depth, breadth, and marketing skills, and diversity of opinions from very capable individuals were nonetheless detrimental to the project. As discussed in the previous section, the personnel were all of high quality, and when the disruptions in personnel continuity is taken into consideration, the quality of the work was relatively high, especially as prototypes and proof-of-concept hardware were demonstrated toward Year-3.

Clarity of technical objectives. One crucial technical insight that was always missing from the team is knowledge of how minehunting is actually performed in the field by the armed services personnel. Specifically, although the engineering fundamentals of the approach was sound, the breadth of knowledge that would include system-level information was lacking (this issue will surface again as a marketing factor). The result of this lack of knowledge is that the technical objectives kept evolving. Although the project objective remained focused on minehunting, the system concept, and hence the figure-of-merit for the design, kept changing. For example, when it became apparent that ocean currents can quickly alter the locations of the buoys and subsequently create gaps in the acoustic coverage, the system scenario changed to incorporate a propulsion system. Thus the system resembled an autonomous vehicle instead of a sonobuoy, substantially raising the cost and complexity of the system, and changing the potential Government agency

Table 18: Intelligent sonobuoy: technical factors.

factor	*Year 1*	*Year 3*
Technical depth/breadth	4	3
Continued outside monitoring	2	2
Quality of work to date	3	4
Probability of technical success	2	3
Technical infrastructure	4	4
Innovativeness of approach	5	4
Competitiveness of technology	4	3
Clarity of technical objectives	3	3

that would have sponsored the effort.

Probability of technical success, Innovativeness of approach, and *Competitiveness of technology.* A difficult scoring issue is that of the probability of technical success. As the project evolved and more prototype demonstrations were performed, the probability of technical success increased. However, the technical objectives of the project had evolved during this period of time as well, so that more realistic and achievable goals were formulated as more was understood about the constraints of the system. Thus as the innovativeness of the approach decreased, the probability of success increased. In a similar vein, the competitiveness of the technology decreased over time, since the system concept became more conventional.

Technical infrastructures. The intelligent sonobuoy project was selected immediately after another initiative to spawn undersea sensing capabilities. Although that initiative was only marginally successful, it did acquire numerous pieces of data acquisition and electronics hardware that benefited the infrastructures necessary for undertaking the sonobuoy project.

Continued outside monitoring. In all, there was only a moderate amount of monitoring of outside activities, in part because the team members felt the sonobuoy concept was sufficiently different that few outside activities could impact this project.

A summary of the analysis of technical factors is shown in Table 18.

4.4.3 Intelligent Sonobuoy Strategic Factors

Compatibility of project/corporate/business objectives, Usefulness of project outcomes to other parts of the firm, Leveraging of core competencies, and *Opportunity for future leveraging of project.* The core of Draper's interests is guidance, navigation, and control. Although Draper decided that autonomous vehicles would play an important role in its future, vehicles were seen by some merely as platforms to carry Draper's guidance, navigation and control systems. Furthermore, although acoustic sensors were seen as an important capability to have within the Laboratory in support of undersea autonomous vehicles, the intelligent sonobuoy effort, with its focus on mine detection, was one step removed from the more central undersea acoustic activities. Thus despite the relatively minor involvement of navigation within the sonobouy work, the main thrust of the activities deviated from the core corporate objectives and core competencies of Draper. Being somewhat tangential to the mainstream Draper business also implies that the outcomes of this project would likely be of limited use to other parts of the firm.

Availability of teaming/alliances. One approach to overcoming the problem of not having tremendous expertise in the sonobuoy area is to outsource or team with others. Although discussions with commercial establishments took place, the consensus appeared to be that these commercial firms needed to see a demonstrated prototype before entering into a collaborative relationship; in short, the technology was viewed as unproven and risky.

Consistency between project demands and investment strategy, and *Project contribution towards a balanced portfolio.* As a leading technology-push project, the resources demanded by the sonobuoy project were reasonably consistent with senior management's investment strategy. The project, however, did not have the profile that fits a high-risk, high-payoff technical effort; and in fact, the project further deviated from that profile as time went on (see Chapter 5).

Coordination with other parts of firm. The sonobuoy project showed a definite trend of greater multi-disciplinary and multi-functional involvement as the project evolved. Specifically, during the initial design and planning stages, it was primarily the acoustic sensor engineers that were on the project. By Year-3, many more disciplines were involved – both with respect to the construction of the hardware and with respect to possible applications of the system.

A summary of the strategic factors are shown in Table 19.

4.4.4 Intelligent Sonobuoy Project Management Factors

Spending rate versus budget, Progress against milestones, Sufficiency of remaining resources, and *Level of technical coordination.* Considering the flux in personnel that occurred over the 3-year effort, the project as a whole was well managed, having met its major milestones (except those pertaining to revenue generation) and stayed within budget.

Effectiveness of internal communications. Although Programs became more involved in the sonobuoy project towards the end of Year-2 and continued into Year-3, the team seemed to

Table 19: Intelligent sonobuoy: strategic factors.

factor	*Year 1*	*Year 3*
Compatibility of project/corporate/business objectives	2	2
Consistency between project demands and investment strategy	3	3
Project contribution towards a balanced portfolio	2	2
Coordination with other parts of firm	3	5
Usefulness of project outcomes to other parts of the firm	2	2
Leveraging of core competencies	2	2
Availability of teaming/alliances	3	3
Opportunity for future leveraging of project	2	2
Proprietary protection	3	3

Table 20: Intelligent sonobuoy: project management factors

factor	*Year 1*	*Year 3*
Spending rate vs. budget	4	4
Progress against milestones	4	4
Sufficiency of remaining resources	4	4
Level of technical coordination	4	4
Effectiveness of internal communications	3	2
Development cycle time	3	3
Team cohesion	3	3

have lost a sufficient number of key personnel that no one was left to keep Draper management informed of its progress. In fact, in interviews with other managers, several thought the project had terminated at the end of Year-2 when funding was transitioned from Engineering to Programs.

A summary of the project management factors are shown in Table 20.

4.4.5 Intelligent Sonobuoy Marketing Factors

Projected market growth, Meets customer needs, and *Competitiveness of product.* The market for Intelligent Sonobuoy is limited to only the U.S. defense establishment. Thus, the growth potential is limited. Furthermore, the contractual awards during this time for minehunting were also generally fairly small (see next section on financial factors). Lastly, it became quite apparent by Year-3 that the sonobuoy concept would not meet either the coverage rate nor the designation accuracy requirements of the customer. Thus, although the design was felt to be competitive during Year-1, it became clear by Year-3 that it was not.

Table 21: Intelligent sonobuoy: marketing factors

factor	*Year 1*	*Year 3*
Projected market growth	3	2
Meets customer needs	3	1
Ability to market product	2	1
Reaction of competitors	3	3
Competitiveness of product	3	1

Ability to market product. To some extent, the difficulties associated with the sonobuoy concept stems from Draper's relative ignorance of the minehunting scenario. Although Draper has a history of minehunting contracts, these were for stand-along image processing applications, rather than system-design applications. Thus even if the technical design had been superior, it is unclear if Draper had enough knowledge of the customer or his needs to be able to market the product effectively.

A summary of the marketing factors are shown in Table 21.

4.4.6 Intelligent Sonobuoy Financial Factors

Sales potential. Given that the Intelligent Sonobuoy project was intended to pursue government funding, the sales potential can be assessed by examining government funding programs during the same time period. By searching through the Dialog database with keywords *underwater* and *mine*, we were able to uncover the following:

- In 1992, there was a Broad Agency Announcement (BAA) for proposals from the Naval Surface Weapons Center at Coastal Systems Station for Sea Mine Countermeasures for the shallow and very-shallow water regions; each award is for a maximum of $1M (No. 92-R-0044).
- In 1992, ONR awarded Metratek, Inc. a research grant for $49K for Rapid Area Search for Locating Underwater Mines.
- In 1993, NAVSEA solicited proposals in buried mine detection systems for an undisclosed contract award (No. 93-R-6326).
- In 1993, DARPA sought proposals under the Near Land Warfare program for detecting and neutralizing shallow and very-shallow underwater mines; each award was for a maximum of $500K (No. 93-04).

Table 22: Intelligent sonobuoy: financial factors.

factor	*Year 1*	*Year 3*
Sales potential	3	1
Total capital expenditure	3	3
Return on investment	3	1
Time to breakeven	3	1
Proprietary protection	3	3

- In 1994, DARPA again sought proposals in Near Land Warfare, at the same $500K funding level (No. 94-19).
- In 1994, ONR awarded 3-D Ultrasound, Inc., $100K for the development of a Real-Time Underwater Camera for Mine Detection Using Massively Parallel Sonar Signal Processing.

We note that these were the only publicly announced contract opportunities during this time period. Although additional contracts were awarded within the underwater mine hunting area, those sole-source contract awards require past relationships and established reputations with the customer – which was certainly not the case with the Intelligent Sonobuoy project. As such, the revenue potential for this project was relatively low, and went lower still as the project continued and the difficulties of securing a contract became more apparent.

Total capital expenditure, Return on investment, and *Time to breakeven.* The total expenditures for this project, although in excess of one million dollars, was not inconsistent with the type of lead technology-push project that Draper funds. Similarly, since it was believed initially that this project would evolve into a new minehunting technology, the return on investment was projected to be reasonable. However, as the project evolved toward Year-3, the funding outlook began to look less promising, and the projected return-on-investment also looked poor.

4.5 Other Projects

In addition to the micromechanical inertial sensors and intelligent sonobuoy projects, six other projects were examined in detail. All six of these projects were chosen from the same timeframe, and received multi-year funding. Terse descriptions of the project types, the years within which each project received funding, and the total funding amounts are shown in Table 23.

For each of the six projects shown in Table 23, an exercise of scoring each project using the methodology described for both micromechanical sensors and intelligent sonobuoy was performed.

Table 23: A summary of other IR&D projects examined.

proj. no.	*description*	*funding years*	*amount*
1	sensor hardware and software	1990-92	$1,352K
2	software	1992-94	$1,005K
3	sensor hardware	1992-94	$1,025K
4	new computing hardware architecture	1990-92	$978K
5	new computing hardware architecture	1990-93	$1,215K
6	sensor platform design	1991-94	$797K

Table 24: A comparison between dynamic metric scores and management perceived level of success for each of 8 projects.

proj.	*tier*	*mgmt score - raw*	*mgmt score - scaled*	*metric score*
MMS	-	6.3	45.0	42.3
IS	-	3.7	26.4	29.7
Proj 1	2	3.4	24.3	25.9
Proj 2	1	3.2	22.9	23.5
Proj 3	2	3.9	27.9	27.6
Proj 4	3	3.1	22.1	22.6
Proj 5	3	3.7	26.4	22.9
Proj 6	3	4.6	32.9	28.2
correlation coefficient				*0.934*

The resulting scores, along with the average scores of management's assessments, are shown in Table 24. For the micromechanical sensors and intelligent sonobuoy projects, the *average* score of the two times that were examined are used. For the other six projects, it is the score corresponding to the R&D tier in 1992 that was computed. Specifically, shown in Table 24 are (i) project tier; (ii) the raw average score from management based on rating values of 1 - 7; (iii) the average management score re-normalized to the range of 1 - 50 to make it comparable to metric scores, and (iv) the metric scores using the proposed framework. The resulting correlation coefficient, 0.935, is shown on the bottom of the table. A graphical display of the degree of correlation is shown in Figure 3.

It is worthwhile noting that despite the high degree of agreement between the metric scores and management's perceptions, the agreement is even greater if the scores applied to the micromechanical sensors and the intelligent sonobuoy projects are not the average of the two times examined, but instead the more recent scores. One explanation for this is that, since the management's scores were obtained in 1997, one would expect management's perceptions to correlate

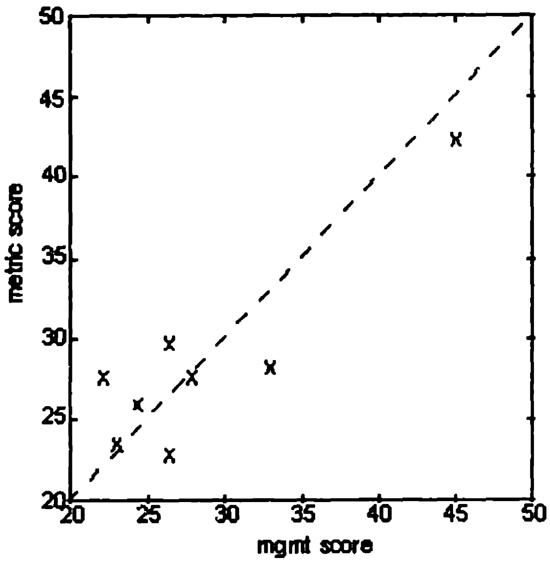


Figure 3: Correlation between metric scores and management ratings for eight projects.

more closely with the situation in 1993 rather than 1989. Clearly, perceptions may be skewed by events occurring with the passage of time.

To assess the significance of the correlation coefficient r, we tested the null hypothesis (i.e., $H_0 : \rho = 0$) for the population correlation coefficient ρ. Under the bi-variate normal assumption, we use the t-distribution:

$$t = \frac{r\sqrt{n-2}}{\sqrt{1-r^2}} \tag{3}$$

with $n - 2$ degrees-of-freedom. For $r = 0.934$ and $n = 8$, the resulting score of $t = 6.46$ is significant at the 99.9% confidence level (Hogg and Tanis, 1977). Thus the dynamic metrics framework appears to have significant predictive capability in terms of estimating the perceived successfulness of a project.

4.6 What's Important to Management?

We reviewed in Chapter 2 a number of factors important in the management and measurement of R&D. It is also of significant interest, however, to understand the difference between current management practice and academic research. As such, we undertook a survey to assess the metrics of primary importance to Draper Laboratory management personnel. The analysis of this survey data is presented in this section.

It's important to note that there are some inherent sources of bias and inaccuracies in the

Table 25: The ten most important factors of internally funded R&D projects according to Draper Lab management.

rank	*metric*	*avg. score*
1	innovativeness of approach	6.0
2	future opportunity to leverage off of the project	5.9
3	ability to market the product/service	5.9
4	motivation level of team members	5.7
5	competitiveness of technology	5.7
6	competitiveness of product/service	5.7
	expected return on investment	5.7
8	advocacy from a strong project champion	5.5
9	clarity of technical objectives	5.5
10	level of project contribution toward technology portfolio	5.5

survey responses. One such bias is the tendency for a manager to rate the project that he/she has championed somewhat higher. However, by having a cross section of respondents, we believe the effect of this bias has been minimized. Another source of inaccuracy comes from the wording of the survey. Specifically, to make the survey manageable, short, terse descriptions of each metric (i.e., those shown in Table 6) were used instead of prolonged descriptions, which necessarily lead to differences in interpretation. During a follow-up interview with one Programs Director, for example, the metric *proper mixture of scientists/engineers* – which was intended to denote whether staff with appropriate backgrounds and motivations were assigned to the task – was rated as unimportant because to him, it did not matter whether the person on the task had an engineering or scientific degree, just as long as the task got done on time.

The survey asked Draper managers to rate the metrics in Table 6 using a score of 1 - 7.[7] None of the metrics were found to be unimportant (i.e., with an average score of 3 or lower). The ten most important metrics are shown in Table 25; the ten least important metrics are shown in Table 26.

There is considerable consistency in the survey data. In particular, instead of using the 40+ metrics from Table 6, a new scoring function can be constructed from the 10 most important factors of Table 25. The resulting scores deviate slightly more with respect to management scores than the complete set of metrics, and yield a very slightly lower correlation coefficient value. This is reassuring for two reasons. First, the management's perceived successfulness of each IR&D project is indeed based upon the criteria judged most important by the managers. Second, the subjective assessments we applied to derive a score for every metric appears to represent, at least

[7]Note once again that two metrics were excluded from this study.

Table 26: The ten least important factors of internally funded R&D projects according to Draper Lab management.

rank	*metric*	*avg. score*
1	availability of teaming/alliance arrangements	3.1
2	probable reaction of competitors	3.3
3	proper mixture of engineers and scientists	3.3
4	prior technical output of team members (papers, patents, etc.)	3.5
5	potential liability	3.8
6	spending rate versus budget	3.9
7	cross functional involvement	4.0
8	total capital expenditure	4.0
9	expected time to breakeven	4.0
10	development cycle time	4.1

on a collective basis, the perceptions of management. Not only was the entire set of metrics capable of predicting management's assessments, but the subset of these scores corresponding to the metrics judged to be most important was also able to predict the outcomes.

When considering contract R&D instead of IR&D, a somewhat different story emerges. With respect to personnel factors, *advocacy from a strong project champion* was no longer on the list of top-10 factors; it was replaced by *motivation level of team members* and *technical reputation of team members*. Similarly, of the technical factors, *competitiveness of technology* was dropped from the list in favor of *probability of technical success*. Whereas none of the program management factors were in the top-10 list of IR&D factors, both *level of cohesion within team* and *sufficiency of remaining resources* were on the top-10 list of contract R&D factors. For marketing factors, only *suitability of project objectives to meeting customer needs* remained on the top-10 list; in fact, this was the number one factor for contract R&D projects. Lastly, *expected return on investment* from the original list was dropped in favor of *sales/profit potential*.

Perhaps the most surprising result to emerge from considering contract R&D projects, however, is that the *innovativeness of approach* became the 9th *least* important factor. Although to be fair, this factor had an average score of 4.1, so that it is not *un*important, merely not as important as most other factors. One other surprise is that the factors *extent to which project outcomes are useful to other parts of the firm* and *degree of project contribution toward a balanced R&D or technology portfolio* were both among the *least* important factors for contract R&D, perhaps implying that as far as the average manager is concerned, funded projects should be pursued regardless of the usefulness of the projects to the Laboratory at large. The least important factor for contract R&D is *probable reaction of competitors*, consistent with its lack of perceived importance as a factor for internally-funded R&D projects.

One way in which our metrics may not necessarily measure what we intended to measure was revealed by one of the free-form responses. The respondent, a Programs Director, gave a negative rating to one of the projects, and responded with "We knew it was a dead end but continued to put money into it. Remember, you cannot pee against the wind." Thus even though we considered *advocacy* an important metric, it's clear that excessively strong advocacy can produce the unintentional result of sustaining a hopeless effort.

We can examine the metrics that gave rise to the largest disagreements among managers by looking at the variance of the responses. Although we mentioned *probable reaction of competitors* to be among the least important metric on average, it is also the metric with the highest variance. The technical metric *competitiveness of technology* was rated among the 10 most important metrics, but was also the second highest in variance. Lastly, the strategic metric *ability to secure proprietary protection* also incurred the third highest variance.

To examine the potential differences in perception between Engineering and Programs managers, we looked at the data in two ways: by examining the differences in mean response, and by looking at a perception map constructed from factor analysis (see Urban and Hauser, 1993).

Looking at the difference in mean responses between Engineering and Programs managers, the three greatest differences are:

1. The project management metric *progress against milestones* was rated far more heavily by Engineering (average score of 6.0) than by Programs (average score of 4.2).
2. The strategic metric *consistency between project demands and investment strategy* was rated more heavily by Engineering (score = 5.9) than by Programs (score = 4.9).
3. The strategic metric *opportunity for future efforts to leverage project* was also rated more heavily by Engineering (score = 5.5) than by Programs (score = 4.5).

These were the only metrics for which the difference between Engineering and Programs scores exceeded 1.0.

This pattern of Engineering management placing greater emphasis on various metrics than Programs persisted throughout the set of metrics. Although in general the differences in mean responses were not large, the non-parametric statistical test Wilcoxon matched-pairs signed rank test (Hamburg, 1977) showed that there was a statistically significant difference among the two sets of responses, at the 99.9% confidence level, indicating a high likelihood that the set of Engi-

Table 27: Factor analysis eigenvalues.

factor	*eigenvalue*	*%variance*	*cumulative var.*
1	2.38	40.0%	40%
2	1.40	23.3%	63%
3	0.84	14.0%	77%
4	0.70	11.7%	89%
5	0.43	7.2%	96%
6	0.24	4.0%	100%

neering management responses and the set of Programs management responses were from different distributions.

Another way of assessing the difference in perception between Engineering and Programs management is to perform a factor analysis. We chose a subset of the metrics from Table 6 by selecting the highest scoring metric from each category. The purpose of factor analysis is to rotate observed data from a potentially non-orthogonal basis to one that is orthogonal (i.e., decorrelated), and determine the degree to which a smaller set of variables can explain the observed responses. The variables we examined from each category are:

1. Personnel: motivation level of team members;
2. Technical: innovativeness of approach;
3. Strategic: opportunity for future leveraging;
4. Project Management: level of cohesion within team;
5. Marketing: ability of firm to market the product/service;
6. Financial: expected return on investment.

The eigenvalues corresponding to the factors are shown in Table 27. Note that the first two factors combined accounted for 63% of the observables, and following the conventional rule of retaining factors with eigenvalues above 1, we construct the perception map using only the first two factors. The resulting perception map is shown in Figure 4. Note that in this two-dimensional space there is no real clustering of responses. Furthermore, using "x" and "o" to denote Engineering and Programs responses, respectively, the map shows that there is no systematic difference between their perceptions. The factor loadings, or the degree to which each of the original, observed, variables accounts for the two unobserved factors (in a correlation sense), are shown in Table 28.

Table 28: Factor loadings for the two derived factors.

	factor loadings	
attribute	*factor 1*	*factor 2*
motivation	0.317	0.825
innovativeness	0.430	0.393
leverage	0.827	-0.403
cohesion	0.697	-0.112
marketing	0.699	0.416
ROI	0.659	-0.470

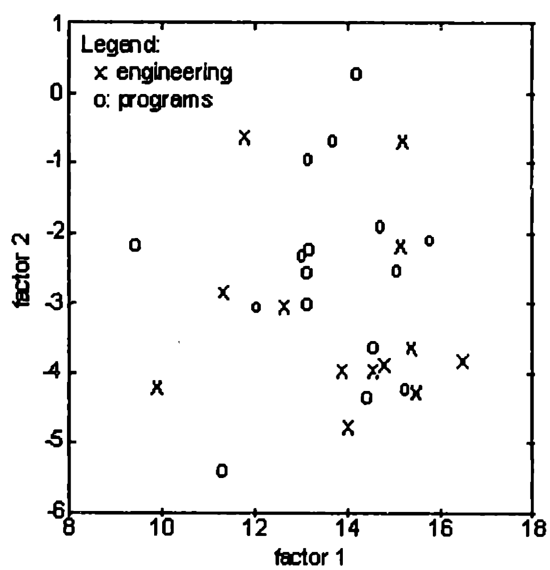


Figure 4: Perception map using the two most dominant factors.

The set of metrics described thus far is a fairly comprehensive set; considerable amount of time and energy must be expended to completely and accurately assess these metrics. The natural question that arises is whether there is a "minimal" set of metrics that can produce similar predictive behaviors. To determine this reduced set of metrics, we employ a stepwise multiple linear regression.

Specifically, we began with 11 metrics: the ten that were identified as being the most important to IR&D projects by Draper management (Table 25), and the project management metric *level of cohesion within team*. This last metric was added because none of the project management metric was included in the list of the ten most important metrics; the cohesion metric, being the highest scoring project management metric, was therefore included. The dependent variables $Y_i, (i = 1, \cdots, 8)$, are the management assessment scores for each of the eight IR&D projects examined. The independent variables $X_{i,j}, (i = 1, \cdots, 8; j = 1, \cdots, 11)$, are the assessed scores for each of the 11 metrics for each of the eight projects. The purpose of the multiple regression is to determine the coefficients α_i, y-intercept β, and residuals ϵ_i that satisfy

$$Y_i = \beta + \alpha_1 X_{1,i} + \alpha_2 X_{2,i} + \cdots + \alpha_{11} X_{11,i} + \epsilon_i, \; i = 1, 2, \cdots, 8 \tag{4}$$

in the least-squared sense. Since there are more independent variables than dependent variables, the full set over-fits the data and yields an R^2 value of 1.

To obtain the best reduced set of features, we employed a stepwise forward selection strategy (Dillon and Goldstein, 1984). We began by selecting the pair of variables that yields the highest R^2 value. Given this set of two features, we then selected (out of the remaining 9 variables) the one that, when combined with the first two, yielded the highest R^2. When this process was repeated once more and four variables were selected, we took a step backwards by eliminating one variable; this step eliminated the possibility of having two attributes that are negatively correlated within our reduced feature set. This overall procedure was repeated until a set of six variables were obtained. At this point ($R^2 = 0.97$, F-statistic $= 5.13$, F-significance $= 33\%$), the inclusion of any additional variable produced an $R^2 = 1.0$ and a F-significance in excess of 99.9%. As such, we elected to include the highest-ranking (by management's assessment) metric not already in the set. The resulting reduced set of seven metrics, along with their respective coefficients, are listed in the column *set 1* in Table 29. Since this regression is overfitting the data however, a second reduced set of only three variables is also shown in Table 29, under the column heading of *set 2*. This set of three variables represents the best 3-feature subset of the initial set of 11 variables.

Table 29: A reduced set of metrics.

		coefficients	
no.	*metric*	*set 1*	*set 2*
1.	future opportunity to leverage project	-0.81	-0.90
2.	competitiveness of product/service	2.06	0.94
3.	advocacy from a strong project champion	-1.16	-0.36
4.	expected return on investment	-2.33	
5.	motivation level of team members	3.07	
6.	innovativeness of approach	-0.4	
7.	ability to market the product/service	0.48	
y-intercept		-1.34	5.37

4.7 Summary

The primary purpose of this chapter was to apply the set of metrics described in Chapter 3, and determine their usefulness in providing insights into the perceived success of projects, as well as differences in perspectives among managers with different functional responsibilities.

In all, a tremendous amount of insight was gained. First, the dynamic metrics framework appears to accurately capture the subjective impressions of Draper's management. Second, a survey of management's assessment of the importance of each metric revealed which metrics are most important and which are least important, for both IR&D and contract R&D work. The differences between IR&D and contract appear to dictate a greater emphasis on team-related metrics (reputation, cohesion, resources) than on either technological superiority or organizational support. Furthermore, the "voice of the customer" clearly became the overriding concern in contract R&D work.

The survey also showed that although there are differences between Engineering and Programs managers, these differences are generally not large in magnitude. In all, Engineering management appears to sense the need to respond to a greater number of demands – ensuring both the technical quality and the usefulness to Programs, thereby causing them to rank most metrics higher than their Programs counterparts.

Lastly, recognizing the cost-benefit tradeoff associated with a large set of metrics such as the one we have proposed, we have attempted to distill the set of 44 metrics down to a smaller set. This was done in two ways: by seeing which metrics are most important to management, and by seeing which smaller set can provide sufficient predictive powers in a regression sense. Although the analysis showed convincingly that a smaller set of 7 - 10 metrics can account for the observed management responses, we caution against relying too heavily on the reduced set: it has been documented repeatedly in the literature that engineers can adapt to specific metrics without ever improving the quality of work. As such, it would be far more difficult to "cheat" on a large and comprehensive set of metrics than it would on a subset of 7 metrics. The reduced set of metrics, however, can still be a useful tool in generating agreement between a quantitative assessment and the manager's own "gut feeling."

5 Strategic Concerns and the Management of R&D Portfolios

In the previous chapters we defined a set of metrics with which one can benchmark different R&D projects. We furthered the notion of benchmarks by proposing the framework of dynamic metrics, and showed that it does a reasonable job of predicting the likely success of the project. Although managers must deal with the management of specific projects, managers must also deal with resource allocations for a *portfolio* of R&D projects. The considerations here become vastly different. Whereas in the former case a manager's primary concern is the success of a single project, in the latter case the manager must strike a balance between low-risk and low-payoff projects with high-risk, long-term, potentially high-payoff projects. More specifically, the manager concerned with the portfolio must understand that there will be inevitable project failures, and must construct a portfolio such that the livelihood of the firm is not jeopardized by these inevitable failures. It is this set of strategic issues that we consider in this chapter.

5.1 Financial Analysis of Risky Projects

R&D projects, like any other capital projects, need to be considered in the context of a financial investment. Traditionally, such investments are calculated by computing the net present value (NPV) of the investment (see Brealey and Myers (1996)). More recently, however, it's been realized that such investments can be more accurately valued by treating them as call-options (see Mitchell and Hamilton (1988); Luehrman (1994); Dixit and Pindyck (1995)). To illustrate this distinction, we will use a simple example adapted from Dixit and Pindyck (1995), and we will ignore for this example the time-value of money (i.e., we will ignore the discount rate).

Consider a contrived example involving a hypothetical manager: he/she is faced with the decision of whether to invest $15M in an R&D effort that may produce a revolutionary design for a new production plant. The outcome of the effort is a production facility that may cost, depending on the level of success of the R&D effort, $40M for the low-cost option, $80M for the medium-cost option, or $120M for the high-cost option. We'll assume these three outcomes are equally likely. Furthermore, the market analysis has produced two ensuing scenarios: either the product will be hugely successful, producing revenues of $130M, or mildly successful and producing revenues of only $50M, with equal probabilities. (The decision tree is shown in Figure 5.) Should the manager approve the R&D effort?

Consider first the NPV calculations. Using *Profits = Revenues - Expenditures*, we see that the expected revenue is $\frac{1}{2}$ of $130M + $50M. Similarly, the expected expenditure is $15M for stage 1

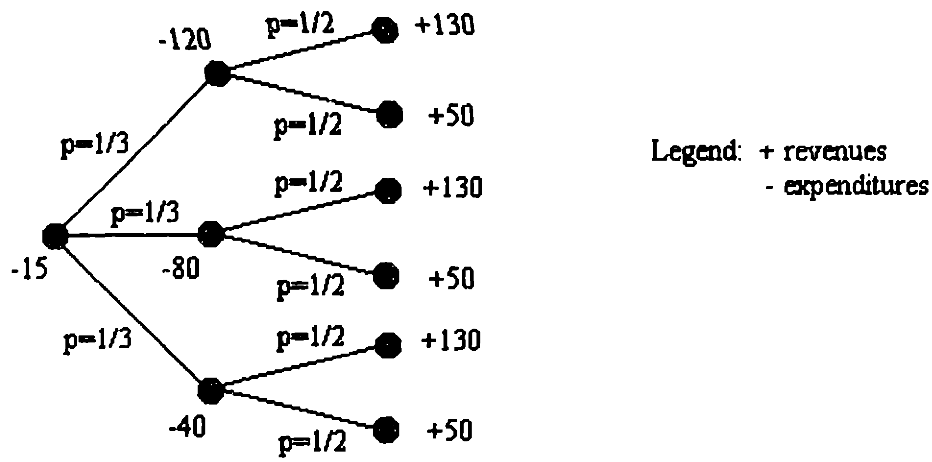


Figure 5: A sample decision tree for a R&D project.

and $\frac{1}{3}$ of \$120M + \$80M + \$40M for stage 2, for a total profit (denoted by π) of

$$\begin{aligned} \pi_{npv} &= \frac{1}{2}(130+50) - (15 + \frac{1}{3}(120+80+40)) && (5)\\ &= 90 - 95 && (6)\\ &= -5. && (7) \end{aligned}$$

Now consider the options-view of the same world: assume the manager commits the \$15M for R&D – the \$15M is used in this context to gather more information about the cost of the plant for stage 2. If the R&D is only marginally successful and the production plant will cost \$120M, then the manager abandons the option. However, if the R&D is successful and the plant will cost either \$40M or \$80M, then the manager exercises the option by committing the funding for the second phase. The value of this options-view is then weighted more towards the less expensive branches of the decision tree. That is, the (phase 2) profits associated with the high-, medium-, and low-cost branches are:

$$\begin{aligned} \pi_{high} &= 0 && (8)\\ \pi_{medium} &= \frac{1}{2}(130+50) - 80 = 10 && (9)\\ \pi_{low} &= \frac{1}{2}(130+50) - 40 = 50 && (10) \end{aligned}$$

for a total profit of

$$\pi_{options} = \frac{1}{3}(\pi_{high} + \pi_{medium} + \pi_{low}) - 15 \qquad (11)$$

$$= \frac{1}{3}(0 + 10 + 50) - 15 \quad (12)$$
$$= 5. \quad (13)$$

Thus we see that NPV and options-pricing yield different values, with the options-pricing model providing greater resemblance to how managers make decisions in real-life.

The purpose of the example is to illustrate how value is added when expenditures for an R&D project are not considered a commitment, but instead are viewed as incremental options to gain additional information concerning the potential payoff. That is, "Opportunities are *options* – rights but not obligations to take some action in the future" (Dixit and Pindyck, 1995, p. 105).

There is a very important generalization to be gained from this example: the *volatility*, or the uncertainties regarding the payoff, is what provides value for the option. That is, if the payoff is known with a high degree of certainty, then an options-approach provides little value. When the results are highly volatile and the expenditures can be made incrementally, where each additional investment increases the manager's knowledge about whether the investment is likely to payoff or not, then the options-approach provides the greatest value. In short, an options-approach attempts to capture only the "upside" of an investment.

To be more specific, consider the logical consequence of an options-based valuation of R&D projects. As discussed in Chapter 2, most companies tend to spend resources on incremental improvements and focus on the short-term. An R&D organization with a focus on producing new and significant breakthroughs, however, should focus on high volatility ventures – projects that require only incremental investments, likely with a low probability of success (by nature of breakthrough technologies), but with a high potential payoff. This is shown in Figure 6, where the three dimensions are probability of success, potential payoff, and the size of the capital expenditure. The shaded regions of the cube denotes the funding strategy of companies focusing on the shorter-term revenues; the "star" projects of an innovative company, however, resides in the upper left corner.

A manager can sometimes modify the characteristics of the project to make it look more like an options-approach to project management. Consider once again the micromechanical sensors and the intelligent sonobuoy projects. Conceptually, the micromechanical project followed a path shown simplistically in Figure 7. That is, initial funding was provide for concept definition. The concept definition phase produced multiple designs; design 1 was pursued first, but after testing, was believed to not have sufficiently promising behavior. Investments were thus made toward the testing of the second design, which tested favorably, and resulted in revenues (this was the ultimate path followed by the project). In this sense, there were *options* available, since up-front investments allowed the project to choose a path that eliminated the downside and enhanced the

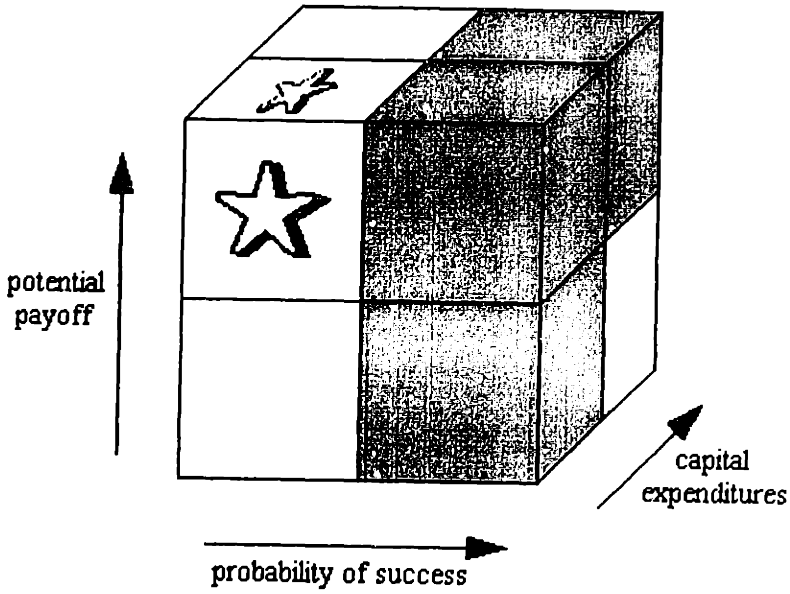


Figure 6: Characteristics of different R&D investment strategies.

upside. Contrast this with the intelligent sonobuoy project (shown in Figure 8). In essence, there was never any option -- a project must proceed in order to reach the next decision point, and if at anytime a failure occurred, the entire project had to be abandoned. As such, it's difficult to capture the upside while abandoning the downside of the project. In order to re-cast the sonobuoy project so that the value of options can be captured, a multitude of designs could have been pursued up-front, for example, which would allow an examination of the possible payoffs before abandoning the less profitable ones.

5.2 Portfolio Analysis of Draper's IR&D Projects

As with corporate managers in any research-intensive organization, R&D managers at Draper Laboratory have a compelling need to determine the positioning of every project within the corporate portfolio. From the perspective of both the mission statement and the need for fiscal soundness, the Draper project portfolio must establish a balance between the *technical impact* and the *financial impact*. In this section we present a visual depiction of the portfolio in order to convey a sense of balance among the projects.

To determine the technical impact of a project, we use the following quantities from the assessed scores of the project. Let:

- $Avg(a, b)$ denote the average of metrics a and b,

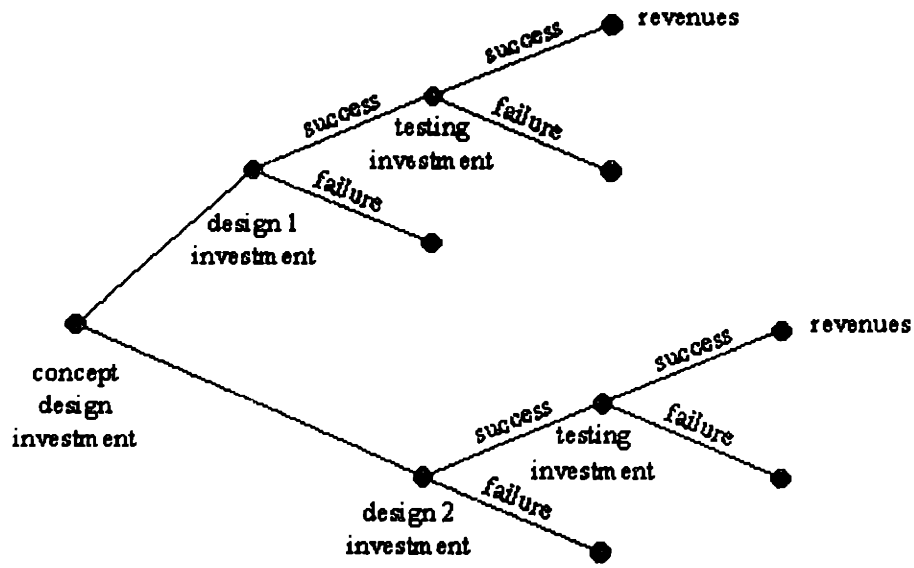


Figure 7: The investment decision tree associated with the micromechanical sensors project.

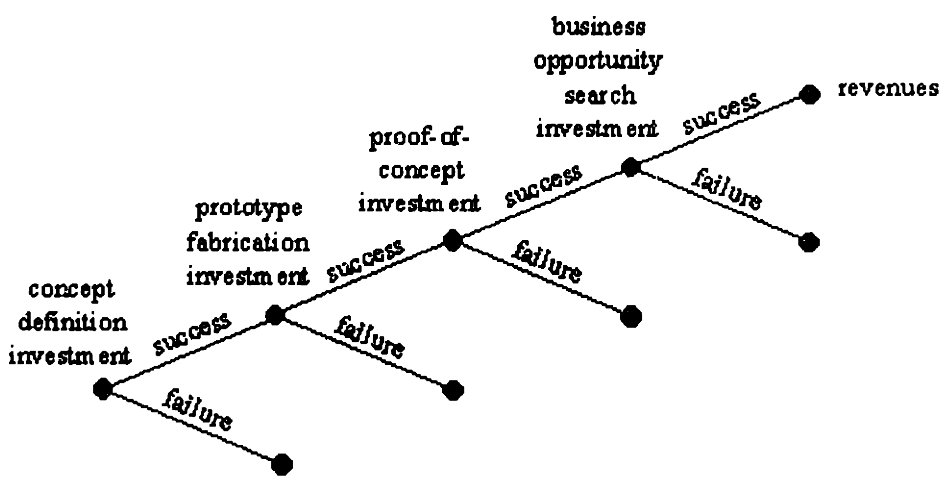


Figure 8: The investment decision tree associated with the intelligent sonobuoy project.

- $c = Avg$(innovativeness of approach, competitiveness of technology),
- $s = Avg$(opportunity for future leveraging, usefulness to the rest of the Lab),
- p = probability of technical success,
- m = ability to market product/service,
- $i = Avg$(competitiveness of product, suitability to meeting customer needs),
- r = expected return on investment,

then we define *technical impact* and *financial impact*, respectively, by

$$\tau = c \cdot s \cdot p, \tag{14}$$

$$\pi = m \cdot i \cdot r. \tag{15}$$

Intuitively, τ defines technical impact as a function of competitiveness, spillover effects, and probability of success, and π defines financial impact as a function of the marketing ability, the customer's inclination to buy (which is in turn a function of the competitiveness of the product on the market as well as the degree to which the product satisfies the needs of the customer), and the expected return on investment.

In Figure 9 we plot each of the eight projects examined earlier on a graph of τ versus π. Specifically, the labels m_j and i_j ($j = 1, 2$) denote the first and second time instances at which we examined the micromechanical sensors and the intelligent sonobuoy projects, respectively, and p_j denotes project j. Furthermore, the radius of the circles in Figure 9 are used to denote the total required investment to bring each project to completion. This plot is in essence a refined version of Figure 6.

Figure 9 shows several things. First, note that the micromechanical sensors project evolved in an expected fashion: creating greater financial impact, generating greater technical impact, but requiring greater resources. The intelligent sonobuoy project, on the other hand, did not evolve in an expected fashion: as the project matured, it lost its financial impact, without substantially gaining in technical impact.

Given Draper's corporate charter of developing pioneering technologies, the small sample of projects we examined was surprisingly low on technical impact. More specifically, given the argument put forth earlier in discussing project positioning via an options-approach, Draper should have a large number of projects that are reasonably high in technical impact (i.e., the terms c and s in τ should be very high, although p may be low) but requiring only modest

Figure 9: A visual depiction of the portfolio of the eight projects examined.

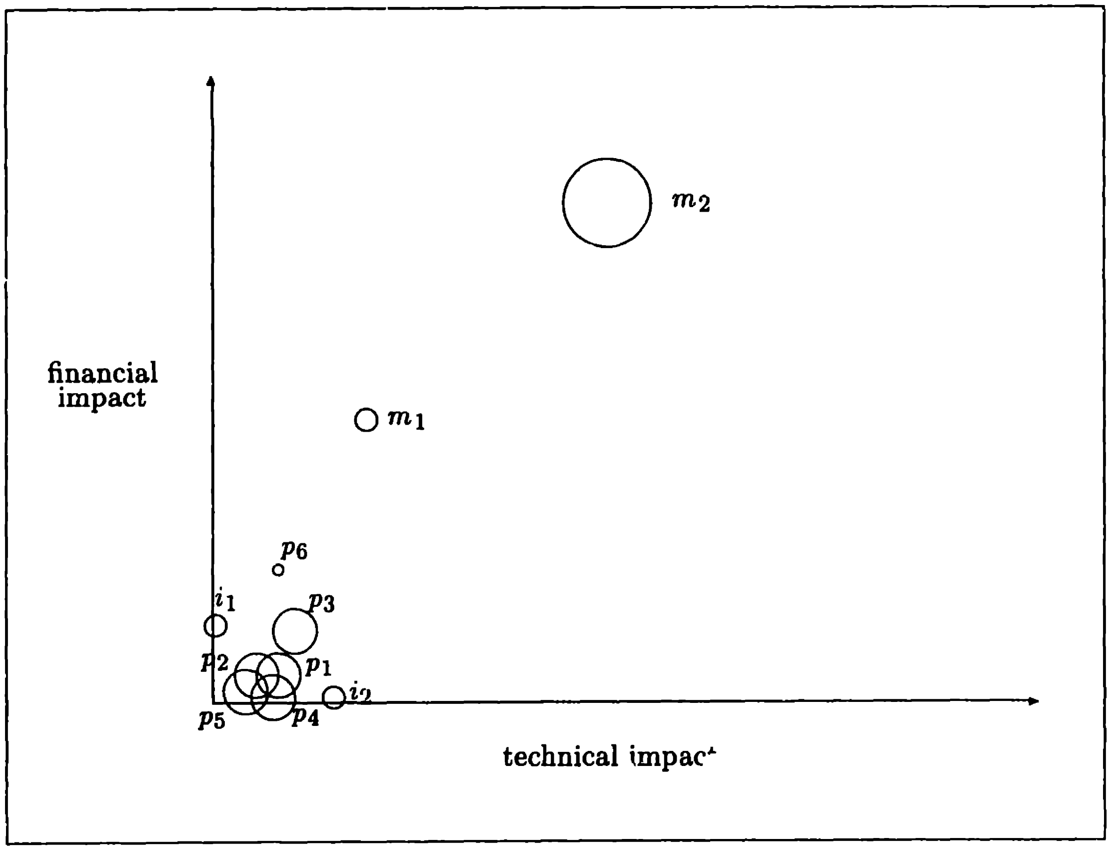


Table 30: Internal Draper hurdle rates for IR&D projects.

tier	*rate*
Tier 0	40%+
Tier 1	~30%
Tier 2	20% - 25%
Tier 3	~15%
Tier 4	10%

investments; these are the Tier 1 projects.[8] Over time, some fraction of these projects should survive and increase in technical impact as its probability of success increases. Yet considering most of the eight projects examined are Tier 2 and Tier 3 efforts, the collective technical impact of these projects as surprising low.

As a business (albeit a not-for-profit one), Draper Laboratory needs to make reasonable returns from its R&D investments. In interviews with the Draper VP Finance, we were able to estimate the internal rate of returns expected by Draper for its various projects. Specifically, we concluded the VP Finance uses a set of hurdle rates such as those shown in Table 30, based upon an assumed prime rate of ~8% (Driscoll, 1997). The profiles of the projects we examined, however do not seem to indicate a return that is likely to exceed this internal hurdle rate.

In general, we would expect three types of project positioning in the context of Figure 9. First, there are the *sustaining projects* that every firm undertakes. These are the Tier 3, more incremental developments that have fairly low technical uncertainty and moderate financial returns. Intuitively, these projects should be positioned above the diagonal that stretches from the origin to the upper right, since the financial outcomes should be less risky than the technical uncertainties (although clearly we can construct project profiles that defy this generalization). A second cluster consists of those projects that are designed for *infrastructure* improvements; these projects should have tremendously high technical spillover effects, but little financial impact since they do not directly lead to new business opportunities. We would expect these projects to be towards the bottom of the figure, and expect to see only a few of them. The more typical R&D projects, however, are those that form the third cluster. Once again, these projects should initially be positioned so that they are, say quarter- to half-way along the technical impact scale (due to the initially low probability of success), but reasonably high along the financial impact scale. Furthermore, there should be many of them, all with relatively small funding. As the project evolves, it

[8] Note however that our criterion for the selection of the 8 projects was the size of the funding over the same time period, thus the criterion tends to select the larger efforts. However, none of the 6 projects examined in lesser detail evolved from high-risk, high-payoff Tier 1 projects, thus our reservations are still valid.

should move more towards the right (as the probability of success increases), and hopefully higher as well (as greater understanding of this new market evolves). Correspondingly, the magnitude of the funding should be expected to increase as well.

5.3 Summary

In the previous chapter we demonstrated how the metrics framework can be used to guide the management of individual projects. In this chapter, we extended the usefulness of this set of metrics by applying them to the management of a portfolio of projects. In particular, we used an options-argument to dictate the preferred profile of R&D projects. We further showed that, based upon the small sample of projects examined, this preferred profile was largely unmet. The approach outlined in this chapter can provide considerable guidance in selecting projects that fit in the context of the overall R&D portfolio of the firm.

6 Generalization: Analysis of a Pharmaceutical Example

In the previous chapters we developed and analyzed a framework, based on various R&D metrics, to assist the manager in guiding, developing, and leading R&D efforts both on a project-level and on a division- or corporate-level. Although the previous chapters demonstrated how the framework can be used, and the degree to which it captures the *dynamics* of R&D projects, the application was validated using a single company – Draper Laboratory. In this chapter, we examine another company in another industry to determine the applicability of this framework.

We have chosen to examine in this chapter R&D within the context of the pharmaceutical industry.[9] Like Draper Laboratory, pharmaceutical firms are characterized by extremely high R&D intensity, with high risks and potentially very high rewards. Of the hundreds of companies represented by the Industrial Research Institute (IRI), pharmaceuticals were by far the most research-intensive group, spending on average 13% of revenues on research. (See Table 31 for comparisons among some of the most research-intensive industries represented by the IRI. Shown are research intensity as defined by the total R&D expenditure as a percentage of revenues, basic research as a percentage of total R&D expenditure, and the amount of R&D expenditure per R&D staff.) More recent data from some of the largest pharmaceutical firms are shown in Table 32.

Judy Lewent, CFO of Merck, has been quoted as saying that the probability of a successful drug development from identifying the appropriate compound to successful product launch is 1 in 10,000, at an average development time of 10 years at a cost of $359M (Nichols, 1994). Similarly, Basara and Montagne (1994) quote an Pharmaceutical Manufacturers Association study claiming "a drug requires 12 years and over $200M to undergo study, testing, and evaluation before it can be marketed to the public" (p. 86). There is a large variance around the average drug development cost, however; Merck's Crixivan (a protease inhibitor for combating AIDS) has an estimated development cost of $700M (Collins and Vedantam, 1996). Because of the high risk nature of pharmaceutical R&D, drug companies have developed a rigorous procedure for determining when a particular research effort should progress to the next stage. In part, however, these rigorous processes were also imposed by regulatory agencies, which demanded such rigor because of the obvious ramifications for human welfare. The relationship between these stages and our earlier

[9] We emphasize here that the purpose of this chapter is *not* to provide an exhaustive industry analysis. We will provide background information on the pharmaceutical industry only to the extent that it provides insights into the R&D processes, the risks associated with these processes, and the rationale for the structure of such processes. In particular, we will provide qualitative (instead of the quantitative details we showed in our analysis of Draper Laboratory's projects) information concerning the development of two drugs. We have chosen to perform only a qualitative analysis because the outcomes of the two drugs depended – to a very large extent – on a single metric: the quality of the *science* behind the R&D efforts. Additional details are provided in the subsequent sections.

Table 31: R&D characteristics for selected industries.

	Drugs and Pharmaceut.	*Agricultural Chemicals*	*Instruments & Related Prod.*	*Chemicals & Allied Prod.*	*Electronics & Equipment*
R&D Intensity	13%	10%	6%	5%	3%
% Basic Research	11%	1%	2%	3%	2%
R&D Expend/Staff	$195K	-‡	$130K	$145K	$95K

‡Insufficient data due to less than five respondents in this business segment.
Adapted from (Wolff, 1994).

Table 32: R&D Expenditures of Some Large Pharmaceutical Firms.

	US$M			*%Sales*		
	1994	*1995*	*1996*	*1994*	*1995*	*1996*
Abbott Laboratories	$964	$1,073	$1,205	10.5%	10.7%	10.9%
Johnson & Johnson	$1,278	$1,634	$1,905	8.1%	8.7%	8.8%
Merck	$1,230	$1,331	$1,487	8.2%	8.0%	7.5%
SmithKline Beecham	$1,008	$1,032	$1,192	10.5%	9.3%	9.6%

Sources: All data obtained from 1996 annual reports.

terminology is shown in Table 33.

Despite the bleak success rates quoted by Lewent, others have found more favorable statistics. Table 34 shows the (conditional) probabilities of success as a function of the various stages of R&D; the first of these numbers are from the baseline case of a pharmaceutical financial model study (Myers and Howe, 1996), and the second is from (DiMasi, 1995).

The different phases of pharmaceutical R&D serve distinctly different purposes. Generically speaking, the objectives of the different phases are (Basara and Montagne, 1994):

- *Discovery* – focus on uncovering pioneering molecules that have potential in treating existing

Table 33: Stages of R&D in the Pharmaceutical Industry.

	Tier 0	*Tier 1*	*Tier 2*	*Tier 3*	*Tier 4*
Technology R&D	basic research	exploratory research	applied research	development	technical services
Pharmaceutical R&D	discovery	pre-clinical research	phase I & phase II	phase III	FDA filing/ mkt monitoring

Table 34: Probabilities of success as a function of research stage.

	Myers-Howe	*DiMasi*
Discovery	60%	n/a
Preclinical	90%	n/a
Clinical Phase I	75%	75%
Clinical Phase II	50%	48%
Clinical Phase III	85%	63.5%
FDA Approval	75%	
Cumulative Success Rate	12.9%	22.9%

diseases; selection of a lead compound for development.

- *Preclinical research* – conduct animal tests, biocompatibility tests, and toxicology tests. Favorable test results lead to IND (Investigational New Drug) Application filing with the Food and Drug Administration (FDA). The FDA's approval of the IND application constitutes permission to begin clinical trials.

- *Clinical Trials Phase I* – conduct clinical pharmacology and safety tests in healthy, human subjects to determine side effects, drug interactions, dosages, preferred administration route, drug absorption mechanism, etc.

- *Clinical Trials Phase II* – initial clinical investigations of treatment effects (tolerability), safety, and efficacy in small human samples afflicted with the target disease or symptoms. The focus is on proof-of-concept demonstration.

- *Clinical Trials Phase III* – extensive safety and efficacy test are performed in large-scale human trials. Comparisons with respect to existing standards of treatment for the same condition are made.

- *FDA approval* – submission of NDA (new drug application) for review and approval by the FDA.

An interesting aspect of the pharmaceutical R&D paradigm is that the efforts of one stage are permitted to continue only if explicit tests are passed. In this sense, Hoechst Celanese, a large chemical company that also engages in pharmaceutical research, uses a similar approach in their Advanced Technology Group (ATG). Specifically, ATG divides the R&D-to-commercialization process into five stages; the objectives and tests pertaining to each stage of this *stage-gate* process are shown in Table 35 (Evins, 1996). In particular, the *tests* column in Table 35 denotes the set of tests that must be passed before an effort proceeds to the next stage.

Table 35: The stage-gate process at Hoechst Celanese.

stage	*name*	*tests*
Stage 1	idea generation and screening	• convincing business concept • plan for stage 2
Stage 2	concept development and feasibility assessment	• preliminary business plan • demonstrated technical feasibility • demonstrated market need • plan for Stage 3
Stage 3	pre-commercial validation	• detailed business plan • customer-verified competitive advantage • manufacturing process developed and verified • plan for Stage 4
Stage 4	semi-commercial development	• demonstrated feasibility of business plan • sustainable competitive advantage strategy in place
Stage 5	commercialization	

Source: (Evins, 1996).

6.1 Worldwide Pharmaceuticals, Inc.

In this section we consider in detail the drug development process at Worldwide Pharmaceuticals.[10] Of particular interest to us is the applicability of the previously described metrics (from Table 6), as well as the use of explicit stage-gates that must be passed in order to progress to the subsequent stage of R&D.

Like many other pharmaceutical firms, Worldwide has turned to collaborators and alliances for risk mitigation and greater innovations. Whereas Worldwide used to perform all discovery work in-house, there is much greater emphasis on collaborations now, including those involving genetics. In particular, Worldwide is exploiting two new developments to improve the efficiency of the drug discovery process: combinatorial chemistry, where structurally similar molecules can be developed and tested more expeditiously, and genomics, where scientific advances in the understanding of human genomes are used to identify promising new drugs and diagnostic agents (see Plunkett and Ellman (1997) and Beardsley (1996) for details).

Because of the high cost of R&D, pharmaceutical companies have been very aggressive in seeking proprietary protections. The most preferred mechanism for such protection is the patent. Patents can be obtained for a number of reasons, including (i) composition of matter (i.e., a patent

[10]The name *Worldwide Pharmaceuticals, Inc.* is fictitious; it is used in this thesis to disguise a real international pharmaceutical firm. All data and processes described herein, however, are factual. Internal processes and data were obtained via a series of interviews with employees of the firm. Clinical information were provided by both Worldwide as well as clinicians at the Massachusetts General Hospital.

on the actual compound or molecule); (ii) application (i.e., the medicinal use); (iii) formulation, and (iv) process (e.g., for synthetic drugs). Of these, the composition-of-matter patent offers the broadest protection, but is also filed the earliest – generally during discovery or preclinical stages. A trade-off thus arises as a result of the limited durability of the patent. That is, a patent is effective for 20 years after the filing date. As such, in the firm's zeal to protect itself, it also forces the clock to begin ticking well before the drug is ready for product introduction, since once the patent expires, other firms can clone the drug without incurring the several hundred million dollars of R&D costs. Therefore, there is a significant push on the part of Worldwide senior management to substantially reduce the development time of new drugs.

As is conventionally the case, Worldwide determines during the preclinical stage the general parameters associated with toxicity, bioavailability, dosage for efficacy and toxicity, drug metabolism, etc. When these indicators are positive, the project then moves into a queue that is referred to as being "on development status." A significant focus of this stage, therefore, is on the formulation of a plan of action for Phase I.

The information learned during this phase is also captured in a *product profile* that is akin to a business plan for new product development. This profile is a detailed document describing the types (and magnitude) of patient benefits, expected safety tolerances, parameters of product differentiation, riskiness of the venture, appropriateness of the timing, etc. This profile is modified as additional knowledge is gained through further experimentation and market studies in the subsequent phases. If, however, at some point the product profile has evolved into a set of specifications that indicate the drug under development is no longer an attractive investment, then the effort is terminated. In a way, this profile also acts as a contract between the R&D unit and the business unit, indicating the target performance that must be achieved for the product to be a market success.

During Phase I, Worldwide forms a team to establish the general parameters of the drug. Although not a cross functional team *per se* in the sense discussed in earlier chapters, this team is comprised of a number of clinical representatives that are closer to generalists than specialists; for example, clinical pharmacologists instead of toxicologists are represented. The purpose of the team is that, collectively, it functions as a single unit to make informed recommendations regarding the pharmacology, manufacturability, compliance, and other issues associated with this drug. Furthermore, the team can call upon other resources within the R&D Lab, and indeed the entire organization, to assist in making the recommendations. The recommendations are submitted to internal management, which decides the allocation of resources based upon several factors, including recommendations from various drug development teams, anticipated development cost and expected profits, opportunity costs, as well as considerations for the product pipeline.

By Phase II, specialists are involved in the development and assessment process. At this point, the focus is on a proof-of-concept demonstration of drug efficacy, on ensuring the safety of the patients under going clinical trials, and on preparation for the large-scale clinical trials in Phase III.

In general terms, the total development time from the decision by the Resource Management Committee to commit to the compound during preclinical, to the New Drug Application filing with the FDA, is approximately 7 - 10 years. There is large variance around this development time, however. For drugs intended to treat acute ailments, the efficacy of the treatment on a per patient basis can be determined relatively quickly, in contrast to drugs for chronic ailments where the patient must be observed for a longer period of time. Thus when the Phase III clinical trial takes place over a sample of many thousands of patients, significant differences in development time can occur.

6.2 Contrasting the Development of Two Drugs at Worldwide

Pharmaceutical R&D has, on an industry-wide level, perhaps the most rigorous process. Using the general framework developed in the last several chapters, we now contrast the development of two different drugs at Worldwide. The first of these, drug *gamma*[11], is a cancer treatment drug that was launched successfully. The second drug, *alpha*, is a medicine intended to alleviate a common ailment; this drug never gained FDA approval, and thus never generated any revenues for Worldwide, despite the nearly $200M investment.

Before proceeding, it is first important to understand how the FDA determines whether a drug warrants approval. Fundamentally, a drug must demonstrate *patient benefits*, including a decrease in patient mortality rate, a decrease in morbidity rate, or an improvement in the patient's quality of life. *These are the only direct measures of efficacy,* or in pharmaceutical jargon, these are the only direct endpoints. Through the cumulative body of scientific knowledge, however, sometimes acceptable *surrogate markers* – proxies for the direct endpoints – are developed. As an example, there is such overwhelming evidence that a lowering of blood pressure in hypertensive patients decreases the mortality and morbidity rates that the approval of new anti-hypertensive drugs no longer requires a demonstration of benefits with respect to the direct endpoints, a demonstration that the new drug lowers blood pressure (a surrogate marker) is sufficient. The implication is that while direct benefits are harder, more costly, more time consuming, and sometimes more invasive to measure, the surrogate endpoint (blood pressure in this example) may be much easier.

[11]Drug names are also disguised in order to keep the identity of Worldwide Pharmaceuticals anonymous.

The development of cancer treatment drugs is another example of FDA's usage of surrogate endpoints for drug approval. In the last decade or so, the FDA has received considerable flack over its slow drug-approval process, especially for such life threatening diseases such as cancer and AIDS. To remedy this problem for cancer treatment, the FDA established new guidelines that provide accelerated (i.e., conditional) approval of new oncology drugs if these drugs can demonstrate efficacy not with respect to the direct endpoints, but with respect to the surrogate of reducing tumor volume. Since the reduction of tumor volume *per se* does not necessarily translate into patient benefit, this approval is conditional upon post-approval confirmatory studies that continue to monitor the patient progress after taking these medicines. The FDA retains the right to revoke the approval if these confirmatory studies do not show the patient benefits expected.

When this new FDA approval mechanism came into existence, a number of pharmaceutical firms that previously were not players in the oncology area - Worldwide included – entered this new arena. *Gamma* is the first drug from Worldwide for oncology; it received FDA approval during the mid-1990's.

6.2.1 Drug 1: *Gamma*

Two significant organizational events took place during the development of *Gamma*. First, due to the economic events of the decade earlier, Worldwide has embraced the notion of collaborations and attempted to eliminate the not-invented-here syndrome. As this culture change diffused throughout Worldwide, the opportunity to develop its first oncology drug also became an opportunity to seek outside sources of knowledge for oncology drug development. One of the strong relationships developed during this time was with the FDA – Worldwide repeatedly sought advice from the FDA during the drug development process, and this no doubt contributed to the smooth approval of *Gamma* by the FDA.

The second significant event affecting the development of *Gamma* was the lack of organizational commitment. In particular, oncology drugs are in general relatively expedient to develop. This is because such drugs are inherently toxic, and as such, would not need to be tested on healthy humans. Additionally, whereas less toxic drugs need to be tested at various doses for harmful side-effects, cancer drugs do not need to be tested to the same extent, since few side-effects can compare with the debilitating effects of cancer. Lastly, clinical trials are in general much smaller: whereas ulcer drugs may need many thousands of patients, for example, *Gamma* only needed 800. For all these reasons, the development of *Gamma* should have been much faster. As it turned out, however, the timespan between IND and NDA was still 7 years for this drug, due in part to strategic indecisiveness on the part of senior management regarding whether Worldwide should enter the oncology field or not. Had it not been for the superior performance of *Gamma* over what was already on the market – there was only one drug on the market at the time for treating this particular type of cancer – Worldwide could have lost a substantial opportunity.

As it turned out, senior management appears to have made the correct decision. Not only was *Gamma* successful in treating the currently targeted form of cancer, it is also believed to be a useful drug in inhibiting the growth of other types of tumors. As such, there is the expectation that *Gamma* can become the preferred pharmacological treatment for a number of cancers. Secondly, having had initial success in the oncology area, the *strategic intent* of Worldwide is to develop oncology into a new core competency.

6.2.2 Drug 2: *Alpha*

The development of *Alpha* was a markedly different story. In retrospect, there were many reasons *Alpha* failed, but none of these reasons were sufficiently compelling at the time to have warranted terminating the effort. And in fact, there is a tremendous tendency to continue funding late-stage, unpromising projects because of the enormous sunk cost already invested in the project.

Specifically, *Alpha* is a drug intended to combat an ailment for which the direct endpoint was difficult to assess, and there are no generally accepted surrogate markers. This implied that the drug development cost would be relatively high.

The primary competitors to *Alpha* were (i) surgical intervention; (ii) a drug that was already on the market from a very strong and savvy competitor, and (iii) a myriad of drugs that did nothing to remedy the underlying cause of the ailment but provided relief from the discomfort of the ailment (for example, drugs taken for inflammation can simply reduce the amount of pain while doing nothing to reduce the inflammation itself). In particular, although there are the common problems associated with surgery (complications, costs, discomfort, etc.), there is no doubt that surgical procedures provided immediate relief of the problems caused by the ailment. The same cannot be said of the pharmacological options.

Alpha, as well as the competitor's drug (*Zeta*) that was already on the market, was intended to directly treat the underlying cause of the ailment; both *Alpha* and *Zeta* combat the problem by inhibiting the progression of the disease process. The development of *Alpha*, however, was four years behind that of *Zeta*; in fact, the competition was gathering physicians' support for its Phase III results while *Alpha* was still in the early phases of development.[12] It is also known,

[12]Although a first-to-market is almost always the preferred position, it should be noted that the drug industry has many examples of a second-to-market that still captured substantial market shares, depending in part on the marketing campaign and the brand name of the firm. Being second-to-market may even have some advantages, including: lessons learned from, and risks mitigated by observing how the first company conducted its clinical trials,

however, that *Zeta* provided only marginal relief of the severe discomfort, for only a minority of the patients with this ailment, with no precursor indicators as to which segment of the patient population would respond positively to the treatment.

The third competitive factor was that of older and cheaper alternatives that provided relief of the discomfort. The major implication of this factor is that, unless *Alpha* demonstrated tremendous efficacy, it would not be able to demand a high price on the market. In other words, the revenue potential of *Alpha* was capped.

While the drug development process went smoothly, it was also fairly expensive – costing nearly $200M. *Zeta* was approved on the basis of two clinical trials using placebo tests.[13] Both tests, however, only showed marginal improvements over the placebo. Worldwide, following the same steps, also conducted two placebo tests; one showed marginal improvement, the other one did not. When a third, more limited trial was conducted using a more invasive measurement technique also failed to demonstrate efficacy, Worldwide terminated the development effort. *Alpha* never made it to market.

6.3 Summary

The pharmaceutical industry is a highly research-intensive, competitive, and regulated industry; the R&D that is performed is extremely expensive and risky. Furthermore, it is sometimes the case that one does not know whether a development process will pay off until the most expensive stage – Phase III – is performed.

To mitigate the investment risk, as well as risk to patients, a very rigorous R&D process has been set in place. This process uses explicit stage-gates to determine if an effort warrants proceeding to the subsequent stage or not. Furthermore, although proprietary information offers competitive advantages and is closely guarded, the clinical trials themselves are very well communicated within the industry, so that preliminary results are known to everyone, adding to the overall spillover effect. The risky nature and the need for a continuous pipeline of new products have also made collaborations, alliances, and even acquisitions much more commonplace.

In the context of the R&D evaluation framework proposed in earlier chapters, many of the considerations still apply despite the rigors of the pharmaceutical R&D process. Consider the following:

gained FDA approval. and launched its marketing campaign; current patient satisfaction, etc.

[13] A placebo test is a double-blind experiment of the drug under development against a placebo. To gain approval, the drug must show a statistically significant efficacy over the placebo.

- With respect to personnel factors, although cross functional teams *per se* are not used by Worldwide, the multi-disciplinary nature of such teams are present within the generalists on the initial development team. Also, given the expense of conducting R&D, the internal allocation of sufficient resources demands the voice of a strong champion in order to succeed.

- The technical factors, while somewhat different for pharmaceutical R&D, still have the same flavor. In particular, it is clear from Worldwide that the monitoring of outside activities – emerging technologies, innovations, competitive products – play a vital role. Additionally, the necessary infrastructures for undertaking drug development in a new area is a major concern. Lastly, the depth and breadth necessary to take a new drug from research through clinical trials and finally to product launch is also clearly an important factor.

- The strategic factors play an important role for any expensive venture; Worldwide is no exception. Although the organization hesitated before committing to oncology as a new strategic direction, Worldwide was fortunate that the lost time was not more costly for the development of *Gamma*. For drug companies such as Worldwide, with many efforts in the pipeline, every new development must be considered within the context of the investment strategy, the corporate strategy, and the R&D portfolio. *Gamma*, in particular, was a good illustration of using a new product to help shape a new competency.

- Project management factors play important roles in two ways. First, as a large, international firm, large-scale R&D efforts need to coordinate personnel and expertise on a global scale. Second, when the development process reaches Phase III where potentially tens of thousands of people (including patients) are involved in administering, monitoring, collecting, and analyzing the data, project management expertise is a must. Additionally, Worldwide has focused on the continued shortening of development cycle time as a strategic directive, further demanding project management know-how from the product managers.

- Marketing factors, including the firm's ability to educate and communicate with the medical community and the regulatory agencies, to provide demonstrated patient benefits, and to understand the market potential for the new drug, are vitally important to the success of a new drug. Marketing in the pharmaceutical industry, however, is complicated by the set of purchasing decision makers that must be appealed to, including HMOs, physicians, patients, drug distributors, and hospital administrators.

- Financial factors are clearly crucial to the R&D effort. With a $200-$350M investment per new drug, the return on investment and the breakeven period are vital parameters. Furthermore, as has been demonstrated with various biomedical products, the potential liability of any new drug is a serious consideration. Indeed, much of the technical rigor that is applied up-front is used to mitigate the liability potential on the back-side.

In short, it would appear that despite the inherent differences between high-tech and pharmaceutical R&D, the same set of issues applies, and presumably a similar set of metrics applies. The explicit stage-gate process, however, appears to eliminate the need for the dynamic metric framework, since the figure of merit of any stage is the success with which the development process progresses to the next stage. In the final analysis, the factor that dominated the fates of *Gamma* and *Alpha* is the science – *Gamma* was a strong product with demonstratable benefits to patients; *Alpha* was not. In this sense, the technical merits of the product is a meta-metric – a significant emphasis must be placed upon this metric.

7 Summary and Conclusions

The purpose of R&D is to contribute to the overall level of knowledge within a firm, so that the firm can compete more effectively through greater ability to assimilate new information, produce more innovations, create better products/services, and adapt to uncertain changes. The evidence reviewed in Chapter 2 of this thesis clearly indicates the positive relationship between R&D strength within a corporation and its overall performance.

R&D is expensive, however, and it is tempting to reduce R&D expenditures in order to boost short-term profits. Furthermore, the results of R&D are frequently difficult to assess, due to the inherent time lag between R&D investment and the resulting benefits, as well as the difficulties associated with attributing tangible benefits to specific R&D innovations. Simply spending more money on R&D is no panacea, however, as evidenced by the variety of studies showing the lack of direct correlation between R&D intensity and profits. The compelling need is therefore not for greater R&D spendings, but instead for better management of R&D.

In this thesis we took a bottom-up perspective in determining the major issues pertaining to the management R&D. Through a fairly extensive review of the R&D management literature, we created a set of metrics that appears to capture the "healthiness" of an R&D project. The set of metrics cover a range of issues, including personnel, technical, strategic (or long-term organizational), project management, marketing, and financial returns. This set of metrics can serve as a checklist for project managers to ensure that sufficient attention is being paid to various relevant factors. This notion of a checklist serves a valuable function since managers, in their day-to-day operations, have a tendency to focus on events demanding their immediate attention, and frequently fail to look at R&D projects from a broader perspective. However, there is also an inherent danger in treating the proposed set of metrics as a checklist merely to be checked-off, without understanding or appreciating the implications of the metrics.

From the list of metrics, a scoring function that alters the placement of the emphasis depending on the maturity of the R&D project was proposed. This dynamic metric framework is used to recognize the evolutionary nature of such projects. The functionality of the framework was then demonstrated on a set eight IR&D projects within Draper Laboratory. It is shown that the scores from the metric correlated highly with the subjective assessment of various managers with Draper Laboratory.

The metrics were then used to demonstrate how corporate portfolio considerations can be quantified. Specifically, we demonstrated the use of the metrics in two ways: first, as a means of determining whether a project is "improving" or not over some timespan. Second, the metrics were used to construct a financial-impact versus technical-impact plot so as to convey where projects are positioned.

In short, we believe there are many uses for R&D metrics, and we have demonstrated several of them within this thesis:

1. *Project management*: the list of metrics serves as a checklist of issues to be managed by the R&D project leaders.

2. *Benchmark*: scores on the proposed set of metrics can be used to compare various aspects of how well an R&D project is managed.

3. *Historical trends*: a project's score over several years indicates the direction that the project is headed. In the projects we examined, the micromechanical sensors project improved between the two time periods, whereas the sonobuoy degraded over the two time periods.

4. *Predictor*: the scoring function proposed herein utilizes the metrics as components in a predictor function that accurately characterizes the degree of success as perceived by the management.

5. *Portfolio management*: the metrics provide convenient ways of examining the positioning of individual projects within the R&D portfolio.

The thesis also attempted to see how well a dynamic metric framework would generalize to other research-intensive industries. By examining pharmaceutical R&D, one of the major insights is the extent to which the use of stage gates adds to the rigors of the process. Many parallels were found between the pharmaceutical and high-tech industries, despite their differences in the scientific component.

The metrics approach proposed herein is not without its weaknesses. Foremost among these is that the metrics are highly subjective. At present, we would argue that no convincing and infallible technique for assessing the merits of R&D has been found. As such, a small committee of project reviewers whose members each independently makes an assessment of the score on each metric comes as close to the best solution as we can hope for.

Above all else, this thesis has sought to devise and demonstrate a set of tools that assist both project managers and corporate managers to manage R&D more effectively. Despite the weaknesses of the metric system, it must be recognized that it is not the *scores* themselves that are important, but instead it is the *process* of thinking about the various relevant issues and making judgments about the progress being made, and taking intelligent actions based on these judgments, that is most crucial to successfully managing R&D.

7.1 Future Work

The work described in this thesis can be extended in a number of ways; we describe some of these in this section.

First, the sample size of the projects examined was very limited. We examined in total only eight projects, of which only two were examined at different time periods. Furthermore, we examined only IR&D projects, whereas it would have been highly desirable to look for similarities as well as differences with contract R&D programs. Lastly, because of the sample size limitations, we were unable to obtain useful data on *outcomes* of the R&D effort, such as success rates in transitioning technology to customers, customers' level of satisfaction, etc.

Second, it would have been informative to extend the study to other firms and other industries. That is, Draper is rather unique in its non-profit status; how would the emphasis on various metrics change if we examined a for-profit company instead? Similarly, we examined only one pharmaceutical firm; how do other firms perform R&D? Had we examined firms in other industries (e.g., aircraft manufacturing, consumer electronics), what differences would we have found? How well does the dynamic metric framework apply to these other firms and industries?

Third, despite our best attempts to derive a comprehensive set of metrics, there will always be some that we have not anticipated. In part, the difficulties come from that fact that although we have tried to be bottom-up in our approach (i.e., we started from the literature review), the *granularity* of the metric is highly subjective. For example, cross functional involvement was identified as one of the key principles within the literature; but how does one judge this involvement? Is frequency the best indicator? Are there issues related to the quality of the involvement? Should these items be explicitly noted in our list of metrics or are they part of the overall scoring of cross functional involvement?

Fourth, in this thesis we have not performed a rigorous financial valuation of the "worth" of an R&D project. Such an analysis would include calculations of the weighted average cost of capital, the value of assets tied up in the R&D projects, marketing research to determine potential market size, etc. Such an analysis is beyond the scope of this thesis.

In short, despite the numerous ways in which this thesis can be improved, we believe there is considerable utility in using a system such as the one proposed herein. We believe the greatest utility of a scoring and monitoring system is achieved when it is applied systematically across all projects. This thesis is but a first step in defining a framework that permits more rigorous management of R&D processes for the benefit of the firm.

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