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Evaluating and Managing the Tiers of R&D

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Abstract

Research and Development (R&D) is critical to the long-term survival of many firms. However, R&D has become a target of corporate downsizing, in part, because the costs of R&D are easily observed, but the benefits are more difficult to assess. One popular response has been to make R&D customer-driven and to ask R&D's internal customers to evaluate R&D's contribution But anecdotes and scientific evidence imply that aspects of R&D can not be evaluated easily by customers and that R&D should not be evaluated solely on market outcomes. We address these issues by combining qualitative interviews and mathematical modeling. We address the multiple roles of R&D and the implications of those roles for evaluation and management. Our research began with 43 intensive interviews with Chief Technical Officers, Chief Executive Officers, and researchers at 10 research-intensive international organizations. Those interviews suggested that there are three interrelated "tiers" of R&D -- (1) basic research explorations, (2) evaluation of research programs to match or build core technological competence, and (3) applied research projects for, or with, business units. The three tiers were evaluated and managed differently.

For tier 3 we derive business-unit-driven metrics and demonstrate why firms subsidize tier 3 projects to account for short-termism, risk aversion, and research scope. We derive formulae for optimal subsidies. For tier 2, we demonstrate that some weight should be given to (internal) market outcome metrics. However, a weight that is too large (as is implicit in popular R&D effectiveness indices) leads to significant false selection and false rejection distortions. Instead, the firm should complement outcome metrics with high weights on indicators of scientific, engineering, and process efforts. Typical indicator metrics include patents, publications, citations, and peer review. For tier 1, we suggest that the research portfolio be managed for high variance, negatively correlated alternative objectives. We also examine the implications of systems which evaluate tier 1 managers and scientists based on the ideas they originate. Even when tier 1 research enables the firm to utilize better ideas from universities and other firms, these evaluation metrics lead to systematic distortions that are counterproductive for the firm. Specifically, the metrics lead to an over-emphasis on internal research empires and a tendency to reject outside ideas ("not invented here"). The metrics may also lead to fewer ideas being investigated.

We close by summarizing the R&D metrics that are now used by firms and we use our analyses to suggest which metrics firms should use to evaluate each of the three tiers of R&D.

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We appreciate your feedback on any and all aspects of this working paper. Our goal in this paper is to provide practical insight into an important managerial problem by combining qualitative fieldwork with formal analysis. An annotated bibliography and a summary of the qualitative interviews are available from Patti Shaughnessy, Room E56-364, M.I.T., Cambridge, MA 02142. (617) 253-4936, pshaughn@mit.edu. A complete listing of ICRMOT working papers is available at http://web.mit.edu/icrmot/www/.

R&D expenditure is often a convenient target when it comes to maintaining or increasing the company dividend. If fact, with R&D expenditure roughly the same amount as the dividend in many companies, it is a significant temptation.

James W. Tipping (1993, p. 13)
Director of R&D, ICI Americas, Inc.

Pioneering research is closely connected to the company's most pressing business problems. ... Research must "coproduce" new technologies and work practices by developing with partners throughout the organization a shared understanding of why these innovations are important.

John Seely Brown (1991, pp. 103-104)
Director of Xerox Palo Alto Research Center (PARC)

Balancing Customer- and Research-Driven R&D

Research and development (R&D) provides the science and technology which firms use to serve tomorrow's customers profitably. Many managers, consultants, and researchers have argued that, to succeed in the next century, R&D should be customer driven. See a review in Griffin and Hauser (1996). John Seely Brown's comments are typical of those that we have heard from our interviews with Chief Technical Officers (CTOs). Indeed a recent international CTO task force on the evaluation of R&D opines that success is more likely if a product delivers unique benefits to the user (EIRMA 1995, p. 36).

However, it is not easy for R&D to be customer-driven. If we limit our definition of the customer to "today's customers," it might not even be desirable. R&D, almost by definition, represents the long-term technological capability of the organization. While many successful new products are developed based on customer needs (von Hippel 1988), an organization can not meet customer needs if it does not have the capability to do so (EIRMA 1995). The laser was not invented to provide high quality music or to store large quantities of data on compact disks. The US Army Research Laboratory (ARL) and their affiliated research, development, and engineering centers (RDECs) would not have been able to adapt rapidly to the post-cold-war era if they did not have capabilities in the basic research areas. By maintaining basic chemistry and chemical engineering expertise, the Hoechst Celanese Advanced Technology Group, a major producer of chemicals for automotive tires, was able to turn a chance discovery of a chemical process into a thriving pharmaceutical business. Other examples include Carothers' research on linear superpolymers that led to Nylon and Westinghouse's research on water flows through porous geological formations that led to breakthroughs in uranium mining, the evaluation of environmental impacts for real estate development, and heat flow analyses for high-temperature turbines and for below-ground heat pumps (Nelson 1959, Mechlin and Berg 1980). On the other hand, the great isolation of Bayer A. G.'s corporate research center was a failure (Corcoran 1994).

More systematic analyses of R&D also suggest that basic research can not only generate unplanned successes, but that basic research is key to the survival of corporations. Mansfield (1980) demonstrates that, holding total R&D expenditure constant, an organization's innovative output is directly related to the percent allocated to basic research. In a statistical study of new product development at 135 firms, Cooper and Kleinschmidt (1995) find that adequate resources devoted to R&D is a key driver that separates successful firms from unsuccessful firms. Bean (1995) indicates that a greater percent of research activities in R&D (vs. business units) implies more growth.

In order to reconcile the research favoring a customer-driven R&D focus with the research favoring a research-driven R&D focus, we need to understand better how research activities lead to new products. With such an understanding we can determine when and how the marketing concept applies. We can also understand when other forces such as risk, time lags, scope, spillovers, and the management of creative people mitigate a customer-driven perspective. To apply the marketing concept to R&D we must understand the balance of customer input and basic science. As the John Bush, Jr., Vice President of Corporate R&D, The Gillette Co. states: "It's the integration of activities -- from research to sales -- that is the innovative activity in a company (Corcoran 1994, p. 15)."

This paper explores how R&D laboratories produce the technology that leads to new products. Our methodology combines qualitative and quantitative methods. We began by interviewing 43 CTOs, CEOs, and researchers at 10 research-intensive organizations. See table 1. We then reviewed the public statements of CTOs, consultants, and academic researchers. (See Zettelmeyer and Hauser 1995 for more details on the qualitative interviews and Hauser 1996 for an annotated bibliography.) Together these activities led to a qualitative description of R&D's activities. We then formalized these descriptions of R&D and its functions in order to derive implications for the management of R&D

and the involvement of R&D's customers.

We also seek to provide insight on the measurement and evaluation of R&D. As our opening quote from Tipping states, R&D budgets are large and visible. They are tempting targets for downsizing. (One of our interviewees, the CEO of a $2 billion company, said that one of his main responsibilities was to protect the R&D budget from his business unit managers.) By understanding how R&D works, we can understand how to measure its output. If we can measure its output, we can value that output to determine the return on investment.

The remainder of this paper is structured into six sections. In the next section we describe the tiered structure of R&D. We then devote a section to each tier and a section to evaluating the output of R&D. We close with a summary and suggested extensions.

The Three Tiers of R&D

The firms we interviewed structured their research activities into tiers as indicated by the technology pyramid in Figure 1. In many firms, management strategies and measures of success varied by tier. Tier 1 represents basic research. We found that activities in this area are exploratory and less tied to the market -- they concentrate on understanding basic phenomena that might have applicability to new products. Tier 2 selected those technologies to develop further and in doing so it fulfilled the organizations' existing strategic directions and set new ones. Tier 3 was more applied. Research in tier 3 was usually done with funding by business units and the corporation and was often focused on fulfilling customer needs. For completeness, we might also include tier 0 -- university research on the basic sciences and tier 4 -- routine engineering for continuous improvement of products and processes. Not only is the tier structure pervasive at the firms we interviewed (for example, the US Army uses funding numbers such as 6.1, 6.2, and 6.3 to describe their tiers), but it is consistent with concepts in the R&D literature (Bachman 1972, Krause and Liu 1993, Pappas and Remer 1985, Tipping, Zeffren and Fusfeld 1995).

One firm in our sample, which uses the word "tiers," gave us a conceptual example of having to move massive amounts of 3D imaging data from remote oil fields to locations in the US for analysis. Tier 1 might scan university research and/or develop basic algorithms to code the images efficiently. Tier 2 would then select the best algorithms and develop tools (software and hardware) to implement the programs taking into account both the business unit needs and the technological competence of the firm. Tier 3, in cooperation with one or more business units, would demonstrate feasibility by using these tools and solving practical implementation issues. Routine application (tier 4) might require further engineering, but that would be done by the business units with R&D consultation.

We have structured the tiers in a pyramid to represent conceptually the amount of funding that is allocated to the tiers. For example, in a study of 108 corporations, Mansfield (1981) found that roughly 5% of company-financed research was devoted to tier 1. However, this does not mean that tier 1 is unimportant. In many ways tier 1 is the R&D lab of the R&D lab. Just as R&D develops new products for the business units, tier 1 develops the ideas and programs that R&D uses to develop new products. In the long run, R&D may not be successful without a tier 1 function. In some ways the relative effort that a firm allocates to each tier reflects a decision on a long-term (tier 1) vs. short-term (tier 3) focus. (Of course, other variables such as industry, firm size, and technology base also affect this allocation.)

In the R&D literature many words, such as program and project, are used interchangeably (Steele 1988). For the purpose of this paper we adopt Steele's terminology and use the words "objectives" and/or "explorations" for tier 1 activities, the word "programs" for tier 2 activities, and the word "projects" for tier 3 activities. This trichotomy is somewhat arbitrary, but it enables us to indicate clearly to which tier we refer.

Next, for each tier, we summarize the qualitative ideas from our interviews and a review of the literature. We then structure the qualitative ideas by formalizing some aspects to gain insight into evaluating and managing each tier. We begin with tier 3.

Tier 3. The Role of R&D's Customers

Qualitative Ideas

We heard from our interviewees that the most difficult task of tier 3 is project selection. Once projects are selected there were many monitoring and feedback mechanisms that could be used to allocate the optimal resources to a project. Many CTOs believed that the business units (the customers of tier 3) have the means and information with which to judge tier 3 projects. Furthermore, they believed that the business units were better able to judge a project's value than R&D management.

There was a major trend to make project selection in tier 3 more customer driven.

Among the statements that we heard were: "Customer satisfaction is the number one priority." "R&D has to be developed in the marketplace." "Customers have direct input on the team performance and hence on the evaluation of the technical staff." "Technology assessment is 'What does it do for the customer?'" In many firms R&D maintains its budget by "selling" projects to business units.

Many firms subsidized R&D with central funds. That is, the business units were asked to pay only a fraction of the cost of tier 3 projects. One CTO stated that the business units could judge research better if they did not have to pay the entire cost. For other examples of subsidies see Corcoran (1994), Mechlin and Berg (1980), Szakonyi (1990).

We found at least three justifications for subsidies: research scope, risk, and a difference between the time horizons of the business unit managers and the corporation. By research scope we refer to situations where the results of a pilot test have applications beyond those for which a single business unit pays. See also Mansfield (1982) and Vest (1995). Other business units benefit without incurring R&D costs. For example, at Westinghouse, the water-flow research was done for the mining division but was also applied to the real-estate, turbine engine, and heat-pump divisions. Scope economies apply across technological disciplines as well as business units (Henderson and Cockburn 1994, Koenig 1983). For example, discoveries in chemistry might enhance research in biology. By different time horizons we refer to the belief (expressed in our interviews) that business unit managers have shorter time horizons than the firm and favor quick fixes for their immediate problems. See also Braunstein and Salsamendi (1994), Hultink and Robben (1995), Negroponte (1996), and Whelen (1976). Holmstrom 1989 adds theoretical justification that market expectations can make it rational for management to be short-term oriented. By risk we refer to situations where a business unit manager might decide to avoid risky projects even though their expected payoff to the firm would otherwise be justified.

In calculating the net value of a tier 3 project, many firms recognize that they need only commercialize those technologies that prove profitable in pilot tests (Mitchell and Hamilton 1988). That is, the cost of commercialization is never incurred for failed pilot projects.

We now incorporate these ideas into a formal model.

Model

We illustrate the contingent nature of tier 3 decisions with the conceptual model in Figure 2. In collaboration with a business unit(s), tier 3 selects among potential projects and begins initial development. For project j let the initial development costs be k_j. If development succeeds (with probability p_j), tier 3 observes the commercial value ($t_j \geq 0$) of the project. This commercial value is modeled as being drawn from a probability density function, $f(t_j)$. If the development fails or if the realized commercial value is below a cutoff (t_c) then the firm can abort the project without further costs. If the commercial value is sufficient, the firm can exercise its "option" and apply the technology elsewhere in the business unit which participates in the research and, perhaps, to other business units. We model this research scope as if the firm can apply the technology to m_j applications at a cost of c_j for each application. Let α_j be the percent of the applications that are within the business unit that funded the research.

The parameters in Figure 2 are feasible to obtain. Many organizations feel confident in making judgments about the expected value of a pilot test ($E[t_j]$), the probabilities of success for various outcomes (p_j), and costs (both for the pilot application, k_j, and for eventual commercialization, c_j). For example, EIRMA (1995) suggests that the "3 main components that must be estimated for any project are project cost, benefits, and probability of success." See Abt, et. al. (1979), Block and Ornati (1987), Boschi, Balthasar, and Menke (1979), Krogh, et. al. (1988), and Schainblatt (1982) for discussion and methods. In tier 3 we assume that m_j and α_j are given. In the next section we address how tier 2 might determine these values.

To model the differences in time horizons we define γ_j and γ_F as the business unit and firm discount factors. These factors reflect the fact that commercial values and costs are really time streams of revenue and costs. If the business unit managers and the firm discount these time streams differently, then the net present values for the business unit managers and for the firm will be different. For example, in Figure 3 the sum of net revenue from 1996 to 2010 is $250 million. If the firm discounts this time stream at 10%, the net present value is $80 million.[1] If the business unit manager discounts this time stream at 20%, the net present value is $23 million. Without loss of generality,

[1]Figure 1 simplifies the contingent nature of R&D decisions and Figure 3 simplifies discounted cash flow calculations. We make both simplifications to illustrate the management issues. In R&D situations managers make decisions that affect risk thus the appropriate discount rate changes over the life of the project. For example, see Hodder and Riggs (1985).

we normalize $\gamma_F=1$ and treat γ_j as the value relative to the firm. For example, for the time stream in Figure 3, we calculate γ_j = *($23 million)/($80 million)* = *0.29*. For issues in the measurement of γ_j see Hodder and Riggs (1985) and Patterson (1983). We allow managers to be risk averse, but do not require them to be so.

For simplicity, we include all project costs in k_j such that t_j is positive. We illustrate the effect of $f(t_j)$ with a negative exponential distribution with expected value λ_j. Such probabilistic processes are common in the R&D literature and make sense -- the commercial values are non-negative and have their maximum value at zero. When the business unit managers are risk averse we model them as constantly risk averse with utility, $u(x)=1-exp(-rx)$, where x is monetary outcomes and r is the risk aversion parameter. For risk neutrality, $r \rightarrow 0$ and $u(x)$ becomes linear. We leave alternative $f(\cdot)$ and $u(\cdot)$ to future extensions.

Analyses

In the appendix we show that the optimal cutoff t_c equals the cost of commercialization, c_j, and that the expected rewards (to the business unit) of the decision tree in Figure 2 are:

$$\text{(1)} \qquad \textit{Expected net rewards} = \gamma_j \alpha_j m_j p_j \lambda_j e^{-c_j/\lambda_j} - k_j$$

The computations are straightforward applications of conditional probability. The term, $exp(-c_j/\lambda_j)$, appears in the formula to represent the fact that the firm need only invest further (and incur costs of c_j) when t_j is above the cutoff. The expected outcome from the decision tree in Figure 2 exceeds the naive valuation, $\gamma_j \alpha_j m_j p_j(\lambda_j - c_j) - k_j$, that would be made if tier 3 did not anticipate the "option" nature of the investigation.

If the business unit manager is risk neutral, he or she will value the project via Equation 1. If the manager is risk averse, the certainty equivalent (*c.e.*) can be approximated by:

$$\text{(2)} \qquad \textit{c.e. of expected net rewards} \approx R_j \gamma_j \alpha_j m_j p_j \lambda_j e^{-c_j/\lambda_j} - k_j$$

$$\textit{where } R_j = \frac{1}{1 + r\lambda_j m_j \alpha_j}$$

For risk neutrality, $R_j \equiv 1$. The firm values the project differently than the business unit managers because it earns the full value of all commercializations, discounts the value and cost streams with

$\gamma_F \equiv 1$, and is not risk averse. Thus, the firm wants at least one business unit to select the project if:

$$(3) \qquad m_f p_j \lambda_f e^{-c/\lambda_j} - k_j \geq 0$$

Subsidies

Comparing Equations 1 and 3 we see immediately that the firm has an incentive to subsidize projects. If the business unit is asked to pay only a fraction, s_j, of the project costs, then the business unit manager will choose the same projects as the firm if:

$$(4) \qquad s_j = \alpha_j \gamma_j R_j$$

In other words, the subsidy adjusts for the concentration of research scope (α_j), short-termism (γ_j), and risk aversion (R_j). It varies by project because both scope and short-termism vary by project. (Short-termism varies because the effect of a differential discount rate has a greater impact on projects with a longer time horizon. Research scope and short-termism, in turn, affect R_j.)

In principle, the subsidy also varies by business unit because α_j and γ_j vary by business unit. However, the firm does not want redundant project funding. It wants either a single business unit or a coalition of business units to fund a project. Because, in principle, the firm's profit is the sum of its business units' profits minus central costs, the firm does not mind if other business units "free ride" on the funding business unit's investment.

In theory, the firm can implement the subsidy with a Dutch auction, lowering s_j until one and only one business unit selects the project (with the limit that the subsidy is not so low that Equation 3 is violated). In practice, the subsidies, which varied from 30% to 90% among our interviewees, are set by a complex negotiation process that allows information to be transferred and coalitions to form. (One manager called this "tin cuping" because, like a beggar with a tin cup, she had to go to other business unit managers asking them to contribute to projects that she championed.) Some firms set an average subsidy. However, this introduces selection inefficiencies whenever there is substantial variation in α_j, γ_j, and m_j.

We summarize this section by stating the implications of Equations 1-4 as a set of qualitative hypotheses. These hypotheses can be used for empirical testing. Equations 1-3 can be used for explicit quantification of the value of tier 3 projects.

Implication 1. (a) The "option value" of a tier 3 project anticipates future decisions on subsequent investment. (b) In tier 3, firms use subsidies and implicit auctions to correct for the tendency of business unit managers to choose projects that are more concentrated in a single business unit, have shorter-term payoffs, and are less risky than the firm would find optimal. (c) Subsidies should be larger (s_j smaller) when projects have benefits that are less concentrated, have revenue streams over longer periods, and are perceived as more risky.

Tier 2. Selecting Technology to Match or Create a Core Technological Strategy

Qualitative Ideas

Our qualitative interviews and the R&D literature suggest that the primary task of tier 2 is to match expertise with strategic direction. See Adler, et. al. (1992), Allio and Sheehan (1984), Block and Ornati (1987), Boblin, et. al. (1994), Chester (1994), EIRMA (1995), Frohman (1980), Ransley and Rogers (1994), Schmitt (1987), Sen and Rubenstein (1989), and Steele (1987, 1988). As one of our interviewees said: "The customer knows the direction, but lacks the expertise; researchers have the expertise, but lack the direction." Tier 2 provides the bridge from basic research (tier 1), which has primary expertise in the scientific and engineering disciplines, to tier 3, which focuses on the needs of its (internal) customers.

To fulfill its role, we found that tier 2 selected among the tier 1 explorations and developed them to meet the firm's (strategic) needs. In selecting its research programs, tier 2 reacted to the strategic direction of the firm, provided the means to fulfill that strategic direction, and, in turn, modified the firm's core technological competence.

In tier 2 CTOs are judged both for their competence in developing technologies and for their ability to align the values of R&D with those of the firm (Steele 1987). Our interviewees said that tier 2 succeeds if it gets the programs right -- the tier 3 option-value decision tree helps determine the right amount to invest. But researchers must also have the incentives to develop the programs that are best strategically.

Net present value models of market outcomes are used in program selection, however, there are concerns that such methods favor short-term, predictable, incremental programs (Steele 1988, Irvine

1988). In contrast to tier 3, which is often evaluated on customer metrics, researchers in tier 2 (and tier 1) are often evaluated on other indicators such as patents, publications, citations, and peer review. See also Edwards and McCarrey (1973), Henderson and Cockburn (1994), Irvine (1988), Miller (1992), Pappas and Remer (1985), and Shapira and Globerson (1983). We demonstrate below the tension, when designing an evaluation system, between (1) market-value metrics that encourage managers and researchers to choose the right programs and (2) metrics that encourage managers and researchers to allocate sufficient scientific, engineering, and process effort to the program.

Model

Figure 4 represents our conceptual model of tier 2 activities. In step 1, tier 2 selects programs based on the ongoing results of tier 1 explorations. Naturally, tier 2 does so anticipating potential outcomes but taking uncertainty into account. In step 2, tier 2 evaluates each program to resolve the uncertainty. In this evaluation tier 2 determines research scope (m_j) and concentrations (α_j's for each business unit). Tier 2 also clarifies any uncertainty in the value (to the firm) of the program so that tier 3 and the firm have sufficient information to estimate the parameters for Equations 1-4. If the program shows sufficient potential, then, in step 3, tier 2 invests significant scientific, engineering, and process efforts to develop the program so that it can become a tier 3 project. (Process efforts include matching technology to the customer through methods such as Quality Function Deployment [Hauser and Clausing 1988].)

Because tier 2 selects programs to develop before it knows the outcomes of that development, we model a key parameter, research scope, as a random variable, $\tilde{m}_j$. Specifically, we model the process of determining $\tilde{m}_j$ as if there were M_j potential applications within the firm. During development, tier 2 determines how many of these applications apply to the firm -- *a priori* each applies with a probability, q_j. (Estimates of M_j and q_j are based on the result of tier 1 explorations and tier 2's expertise in evaluating the outcomes of those explorations.) We define v_j as the "value" of each realized application.

We model the scientific, engineering, and process effort in step 3 with a parameter, e_j, that measures the expected result of this effort. For illustration, let the realized benefit to the firm of this effort be a normal random variable, $\tilde{e}_j$, with mean e_j and variance, σ_e^2. There is some cost to tier 2 to obtain these results and this program-by-program cost may be difficult for the firm to observe.

We call this cost, $d_j(e_j)$, and assume that it is convex in e_j. Finally, there is some fixed cost, K_j, of developing program j.

Each program might have different anticipated time streams of net revenues and tier 2 managers might be more short-term oriented than the firm. We model this by allowing each program to be discounted by tier 2 by a factor, Γ_j. We allow tier 2 managers and researchers to be (constantly) risk averse. (We set $\Gamma_j=1$ when there is no short-termism and $r \rightarrow 0$ when managers and researchers are risk neutral.)

To focus on key phenomena, we have abstracted our model in three ways. First, we set $k_j=0$ in Equations 1-3, so that tier 3 will develop all projects recommended by tier 2. Analytically, we make this abstraction to avoid the need to model explicitly tier 3's option to develop only the most promising projects. This option-value calculation would complicate the algebra for tier 2 analyses without providing any new insight beyond that contained in Equations 1-3. Second, we do not model v_j as a random variable because it would be redundant to do so. In principle, we could readily extend the model to apply to random $\tilde{v}_j$ as well as $\tilde{m}_j$. Because the variance of $\tilde{m}_j\tilde{v}_j$ is greater when $\tilde{m}_j$ <u>and</u> $\tilde{v}_j$ are random, the qualitative effects that we demonstrate in this section would be the same, but larger. Third, we model the effort allocated in step 3 but not the effort allocated in step 2. Empirically, step 3 effort is certainly the larger component of tier 2 efforts. Modeling it only in step 3 avoids redundancy and supports a simpler exposition. The same basic intuition would apply to step 2 efforts except that we would need to model the interdependence of the random variables, $\tilde{m}_j$ and $\tilde{e}_j$.

Goals

Many firms are adopting R&D metrics which evaluate tier 2 directly on market outcomes vs. costs. For example, see McGrath and Romeri (1994). (Market outcomes might be based on internal customers as well as external customers.) Such schemes are highly advocated and growing in popularity. From our interviews we believe that such schemes distort tier 2 decisions.

To analyze this trend and to understand the potential impacts of such metrics we consider a more general reward system in which the firm places different weights on different metrics. (This is consistent with the practices we have observed.) Conceptually, we represent the metrics by their ability to measure the values that result from the choice of program, the effort put into the program, or the cost of the program. The evaluation of tier 2 managers and researchers is then:

$$(5) \qquad reward = \beta_v \tilde{m}_j v_j + \beta_e \tilde{e}_j - \beta_K K_j$$

where β_v, β_K, and β_e are the evaluation weights set by the firm.[2] Metrics such as those advocated by McGrath and Romeri represent a special case where $\beta_v = \beta_e = \beta_K = 1$. The linear function suffices to demonstrate the issues. However, one might improve upon observed practice by introducing non-linear reward systems. We leave such systems to future analysis.

We must modify Equation 5 to represent how tier 2 managers and researchers evaluate the rewards. First, they recognize that the effort costs, $d_j(e_j)$, must be subtracted. Second, they may discount the time stream of benefits by Γ_j, but they do not discount the cost (which, by assumption, occurs immediately). Third, if they are risk averse they will take the uncertainty in $\tilde{m}_j$ and $\tilde{e}_j$ into account and, hence, will value rewards by their certainty equivalent. In the appendix we show that their certainty equivalent is:

$$(6) \qquad c.e. = \beta_v \Gamma_j M_j q_j v_j - \beta_K K_j + \beta_e \Gamma_j e_j - d_j(e_j) - (r/2)\{\beta_v^2 \Gamma_j^2 M_j q_j (1-q_j) v_j^2 + \beta_e^2 \Gamma_j^2 \sigma_e^2\}$$

It is immediately clear that β_v and β_e must be non-zero. Otherwise, tier 2 will select no programs for development and allocate no effort because doing so would entail costs without rewards.

In contrast to the business unit managers, the firm wants to select those programs that maximize the expected value of the program to the firm (net of the wages the firm must pay tier 2 managers and researchers).

The tension between choice and effort is complex. To understand this complexity we begin by holding effort constant and illustrating how β_v and β_K affect tier 2 decisions. We then hold research scope constant and illustrate the effect of β_e. We then discuss the joint effect of β_v, β_K, and β_e.

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[2] In practice, we reweight actual metrics to implement target values for the β's. For example, consider two metrics with weights η_1 and η_2. Suppose the first is a measure of market outcomes, $m_j v_j + e_j$ and the second is a measure of effort, e_j. Then, the effective weight on $m_j v_j$ is $\beta_v = \eta_1$ and the effective weight on e_j is $\beta_e = \eta_1 + \eta_2$. A weighting of $\beta_v = \beta_e = \beta_K = 1$ implies a reward based on market outcomes, $m_j v_j + e_j$, minus costs, K_j.

Selecting the Right Programs

Temporarily ignore d_j and e_j. Even without these effects, the presence of differential discounting ($\Gamma_j<1$) and risk aversion ($r>0$) causes the tier 2 manager's *c.e.* to differ from the firm's expected rewards, $M_j q_j v_j$ - K_j. If the weight on market outcome metrics, β_v, is approximately the weight on cost metrics, β_K, then differential discounting and risk aversion might cause tier 2 managers to reject some programs that would be profitable for the firm and to favor less profitable programs (for the firm) over more profitable programs. We illustrate these phenomena in Figure 5 for the case of two alternative research programs and for representative values of the parameters (given in the appendix). We begin with Figure 5a. The horizontal and vertical axes represent the expected values (v_j) of programs 1 and 2, respectively.

Figure 5a isolates the effect of discounting (with risk neutrality). (Equations are derived in the appendix.) If tier 2 managers and researchers discount the time stream of revenue but not initial development costs, then some programs will be falsely rejected (inverse L-shaped region in Figure 5a). If revenues from one research program occur faster than another ($\Gamma_1>\Gamma_2$), then tier 2 managers and researchers will be more likely to chose the program with better short-term prospects (diagonal false selection region in Figure 5a). We can eliminate the false rejection regions if $\beta_K=\Gamma_j\beta_v$, but eliminating the false selection region requires, in addition, that we allow β_v to vary by program. In other words, we must reward tier 2 more for selecting programs with a longer-term payback.

Figure 5b isolates the effect of risk on false rejection. (We expanded the scale in Figure 5b, vs. Figure 5a, in order to illustrate this effect.) When $\tilde{m}_j$ is a random variable and tier 2 managers and researchers are risk averse, the certainty equivalent of the tier 2 evaluation will be less than the expected value of the evaluation (see also Holmstrom 1989). For a given cost (K_j), when the value (v_j) and implied risk become large, the certainty equivalent becomes negative and tier 2 no longer finds it attractive to begin research even though the program provides a very large expected return to the firm. If the firm wants to eliminate this false rejection region, it must make β_v sufficiently small such that the false rejection region is beyond any feasible outcome, but large enough so that tier 2 prefers high-expected-return programs. Placing too large a weight on market outcome metrics leads to a tendency by tier 2 to avoid high-expected-return research programs that are risky and/or long-term.

Figure 5c isolates the effect of risk on false selection. The concept is similar to that of false rejection. In the shaded regions of figure 5c, uncertainty and risk aversion cause tier 2 managers to avoid high-return research programs when the returns are risky and/or long-term. The firm can eliminate these false selection regions by making β_v sufficiently small.

Figure 5d summarizes the effects of both discounting and risk. The regions are no longer as simple, but the phenomena are the same -- discounting and risk aversion lead to regions of false rejection and false selection when tier 2 managers and researchers are rewarded too heavily on market outcome metrics.

Encouraging Tier 2 Scientists and Engineers to Put Enough Effort into Developing a Program

In this subsection we hold the realized scope ($\tilde{m}_j$) and costs (K_j) constant and focus on step 3 in Figure 4. With only effort being analyzed, the selection of a weight (β_e) to encourage tier 2 managers and researchers to allocate optimal efforts is a standard agency theory problem. The firm will have to "reimburse" tier 2 managers and researchers for their cost of effort (d_j) and for any additional risk costs that the firm imposes by its choice of β_e. See Holmstrom (1989). In the appendix we show that the firm can choose an "optimal" β_e^* such that tier 2 allocates the scientific, engineering, and process effort that maximizes the firm's profits. The optimal weight is:

$$\beta_e^* = \Gamma_j^{-1}[1 + r\sigma_e^2 \frac{\partial^2 d_j^*}{\partial e_j^2}]^{-1} \tag{7}$$

because d_j is convex, $\Gamma_j\beta_e^* \in [0,1]$. The better that tier 2 can anticipate the effect of its efforts the smaller σ_e^2 will be and, hence, the closer $\Gamma_j\beta_e^*$ will be to 1.0. (Setting β_e close to 1.0 implies that most of the variation in returns due to tier 2's effort are given or charged to tier 2.)

We now see the tension. If market outcomes were the only metrics available then they would measure m_jv_j and e_j simultaneously. To avoid false program <u>choice</u> the firm would want the weight on market outcomes to be small, but to induce the right research and process <u>efforts</u> the firm would want the weight on market outcomes to be large. One way to finesse this tension is for the firm to find metrics that correlate with effort, but not necessarily with market outcomes. The firm can then implement a small weight on m_jv_j and a large weight on e_j by placing a small weight on market outcomes and a large weight on the "effort indicator" metrics.

In tier 2 firms do appear to complement market outcome metrics with "effort indicator" metrics.

In particular, many firms use metrics such as patents, publications, citations, and peer review. Such metrics have proven to be correlates of incremental value (and by implication) scientific, engineering, and process effort. See Griliches (1990), Koenig (1983), Miller (1992), Stahl and Steger (1977), and Tenner (1991). Indeed, if more than one such measure of effort is available, the firm can do better by using a linear combination of measures (Holmstrom 1989). When the measures are independent indicators, the "optimal" weights are inversely proportional to the variance of the measures (see appendix for equations). Thus, when metrics such as patents, publications, citations, and peer review, are indicators of tier 2 effort, then the firm should weigh these metrics more heavily than market outcome metrics in an evaluation of tier 2 efforts. If these indicators can be observed before market outcomes and if the measures are less uncertain for tier 2 managers and researchers, then such metrics avoid distortions due to short-termism and risk aversion.

Selecting the Right Programs and Allocating Sufficient Effort to Develop Them Further

If returns to effort vary by research program, then, in step 2, for a given set of β's, tier 2 will select among programs anticipating the effort that it will allocate in step 3. This changes the expected value and the variance of tier 2's rewards. Technically, we incorporate this effect by redoing the analyses that led to Figure 5. If the firm selects the reward system to maximize profit, then the firm's expected rewards become $M_j q_j v_j - K + e_j^* - d_j^*$ minus risk costs, where e_j^* and d_j^* are the result of the firm choosing the optimal β_e^* and tier 2 responding with the optimal efforts. Similarly, we replace e_j, d_j, and β_e in Equation 6 with their optimal values. For each potential program, these optimal values do not depend upon the realized value of the research scope because $\tilde{m}_j$ and $\tilde{e}$ are independently distributed (and managers are constantly risk averse). When we work through the algebra, these changes reinforce the qualitative lessons above. That is, the firm can encourage the correct choice of projects, reduce the false rejection and false selection regions, and encourage the optimal amount of scientific, engineering, and process effort by placing a small weight on market outcome metrics and a much larger weight on effort-indicating metrics.

Summary and Implications for Tier 2

The primary responsibility of tier 2 is to select the right programs based on their applicability to the core technological competence of the firm. A secondary responsibility is to develop those programs so that they become viable tier 3 projects.

Our analysis demonstrates the tension between reward systems that are focused on encouraging tier 2 to select the right programs and reward systems that are focused on encouraging tier 2 to allocate the right amount of scientific, engineering, and process effort. The firm can do both if it can identify metrics that are correlated with tier 2 efforts. It can place a high weight on effort indicating metrics while placing a lower (but non-zero) weight on market outcome metrics.

The firm can also attempt to develop metrics that measure directly the ability of tier 2 to choose the right projects. For example, some firms reward tier 2 managers and researchers for "strategic vision" and for decisions that are aligned with the firm's goals (Steele 1987).

Our analysis is contrary to calls in the popular press for greater market accountability of tier 2 and is contrary to many of the schemes advocated (but not yet fully evaluated) in the R&D literature. We predict that a simple comparison of market outcomes and research costs (e.g., McGrath and Romeri 1994) will lead tier 2 to avoid long-term and/or risky programs. (Indeed, one senior manager, who has used market outcome metrics, indicated to us that the measures at his firm have increased in the short-term, but may now be decreasing.) Instead, we support the practice of weighing metrics like publications, citations, patents, and peer review more heavily than market outcome metrics.

We summarize our analyses with some testable implications.

Implication 2. Tier 2 should be evaluated on market outcome metrics such as profits, revenues, or business-unit evaluations, but the weight on those metrics should be small. Otherwise, tier 2 will favor short-term projects with less risk. On the other hand, metrics such as publications, citations, patents, and peer review should have a much higher weight if those metrics correlate with the amount of scientific, engineering, and process effort that goes into developing a tier 2 program into a tier 3 project.

Tier 1 -- Exploring and Recognizing New Objectives

Qualitative Ideas

Tier 1 explorations provide the raw material for tier 2 programs. Tier 1 is more likely than the other tiers to be funded from corporate coffers; more likely to located in central laboratories; and more likely to focus on long-term concepts. See also Chester (1994), Krause and Liu (1993), Mansfield (1981), Mechlin and Berg (1980), Reynolds (1965), and Szakonyi (1990). It is more often organized by scientific discipline than by markets served (see also Chester 1994). It accounts for roughly 5-15% of R&D spending, but appears to be the seed for new ideas.

One key problem articulated by our interviewees was the selection of the right portfolio of tier 1 objectives. However, "inventors are unlikely to know the value of their inventions in advance" (Griliches 1990) and even the best people may be working in an area that does not prove profitable. We observed that tier 1 managers tried to keep many explorations going so that the winning programs provided a high return. In fact, one CTO told us that tier 1 learns as much from failures as successes and that a researcher can succeed by identifying an area in which not to invest further.

A second important problem mentioned was the need to maintain expertise in the scientific disciplines in order to identify ideas from universities, from other firms in the industry, and from other industries. This activity was called "research tourism." One of our interviewees stressed that their competitive advantage was to identify and develop outside ideas better than anyone else in their industry. Research tourism opens "new fishing grounds" for corporate development (Griliches 1990) and spillovers can be quite large (Acs, Audretsch and Feldman 1992, Bernstein and Nadiri 1989, Griliches 1992, Jaffe 1989, Ward and Dranove 1995). In an econometric study of 1700 firms, Jaffe (1986) suggests that, while the direct effect of R&D spending by competitive firms lowers profitability, the indirect effect of spillovers is sufficiently large to make the net effect positive.

However, research tourism is not easy. A common problem at many tier 1 laboratories is a "Not Invented Here (NIH)" attitude (Griffin and Hauser 1996). The outputs of internal explorations are easier to measure, hence it is tempting to evaluate tier 1 based on the number of internal ideas rather than the total number of ideas.. This is perpetuated by evaluation systems (e.g., Galloway 1971) that trace successful new products back to their idea source. Other firms encourage work within the organization to avoid "buying" technological results (Roussel, Saad, and Erickson 1991). Incorporating spillovers and spin-offs appears to be one of the weaknesses of current evaluation systems (EIRMA 1995).

Evaluation of tier 1 often focused on identifying the best, most creative people who were likely to develop new ideas (e.g., Steele 1988). We observed that management provided these people with sufficient protected space and discretion in which to innovate. This included special privileges, such as "Research Fellows" at IBM and 3M or "Man on the Job" at the US Army, that are not unlike the tenure system at research universities. In some instances fame, recognition, and salary appears to depend more on ideas that a researcher originates than on ideas that are "arbitraged" from outside sources.

We begin with models of portfolio management. We then address research tourism and NIH.

Model

Let $\tilde{w}_j$ be shorthand for the value of a program that is passed to tier 2. The distribution on $\tilde{w}_j$ represents the outcome of the exploration. Let n be the total number of explorations.

Manage for High Variance and Negative Correlation

Our qualitative interviews suggest that tier 1 should look at many different ways to solve a problem. If, at the end of the explorations, tier 1 chooses the best program to pass to tier 2, then the value of the portfolio of n such explorations is the $max\{\tilde{w}_1, \tilde{w}_2, ..., \tilde{w}_n\}$. If $\tilde{w}_j$ is normally distributed, then this selection process is well-studied. See David (1970), Gross (1972), Gumbel (1958), and Stigler (1961). The value of the portfolio is an increasing, concave function of n and is proportional to the validity and reliability of the measures that are used to select the best program.[3] The value is proportional to the standard deviation of $\tilde{w}_j$. In addition, it is not difficult to show that the value of the portfolio increases if the $\tilde{w}_j$'s are negatively correlated. See appendix. We state these testable hypotheses formally as implication 3.

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[3]The referenced derivations are based on independent and identically distributed normal variates. The results are approximate for other distributions. In modeling tier 2 we assumed that $\tilde{e}_j$ is normally distributed and we have approximated $\tilde{m}_j$ by normal variates, thus $\tilde{w}_j$ should be approximately normal (subject to distortions introduced by effort allocation and risk management). Reliability is defined as the variation (variance) in exploration value divided by the total variation (true score plus error) of the measure. Validity is the correlation of the measure with outcomes.

Implication 3. Tier 1 managers should encourage risky (high variance) explorations and manage their portfolio to investigate alternative solutions that are negatively correlated. The metrics with higher reliably and validly, in terms of indicating which exploration from a portfolio should be advanced to tier 2, are the better portfolio metrics. Such indicator measures are likely to be more relevant to managing tier 1 than magnitude measures of the ultimate market outcomes.

The Right Reward System Encourages Research Tourism; the Wrong Reward System Encourages NIH

The R&D literature and our qualitative interviews suggest that many firms focus on the ideas that are created internally. On the other hand, our interviews and the literature suggest that more and better internal research provides a greater ability to identify and use outside ideas (e.g., Cohen and Levinthal 1989). We now analyze whether or not a focus on original ideas is detrimental to the firm.

Let h be the number of internal explorations. We model spillovers by assuming that, for every internal exploration, the firm can also identify μ ideas from the outside. Let κ_i be the cost of an internal exploration and let κ_o be the cost of each external idea that is brought into tier 1 from external explorations. Naturally, $\kappa_o<\kappa_i$. Let $V(n)$ be the value of the best technological solution that results from n total explorations. Based on the discussion above, V is a concave function of n and $n=h+\mu h$.

The potential for spillovers ($\mu>0$) decreases the cost per idea, hence, for concave V, the optimal number of ideas increases when spillovers are possible. However, even though spillovers make internal explorations more efficient, this efficiency might imply fewer internal explorations. In the appendix we show formally that this means that the optimal number of internal explorations might actually decrease. We summarize this analysis as testable implications.

Implication 4. When spillovers are possible, (a) the optimal number of ideas increases but (b) the optimal number of internal explorations might decrease.

Based on Implication 4 we can see why tier 1 managers might encourage an NIH attitude. If a manager's status is based on the number of tier 1 explorations, then encouraging spillovers might decrease his or her internal empire. To illustrate this more formally, suppose that the firm can evaluate tier 1 on either internal ideas alone (the size of the research "empire") or on the total number of ideas that are identified -- whether or not they originate in tier 1. Specifically, the firm either evaluates tier 1 by $g_h(h)$ or $g_n(n)$. To investigate what happens when tier 1 managers can encourage or not encourage attention to spillovers we allow tier 1 managers and researchers to select policies that are equivalent to choosing μ from the set $[0,\bar{\mu}]$. Let μ^o be the value they choose (in their own best interests). If $\mu^o=0$ then this is equivalent to NIH; if $\mu=\bar{\mu}$, then this is equivalent to research tourism. To illustrate the effects of the reward system suppose that $g_h(h)=V[(1+\bar{\mu})h]$ and $g_n(n)=V(n)$. The firm would choose this $g_h(h)$ if it fully expected managers and researchers to explore spillovers and rewarded them accordingly, but did not anticipate that $g_h(h)$ would affect μ^o. (We obtain related results with $g_h(h)=V(h)$ and $g_n(n)=V(n)$. If the firm were restricted to using h, but could anticipate μ^o it would choose $g_h(h)=V(h)$; if it were not restricted to h, it would choose $g_n(n)=V(n)$ as the reward function.[4])

We compare $g_h(h)$ to $g_n(n)$. (See appendix for formal derivations.) When tier 1 is evaluated on n, the reward structure of tier 1 is similar to that faced by the firm. The cost per idea decreases with μ, thus, like the firm, tier 1 will find it in its own best interests to set $\mu^o=\bar{\mu}$. Thus, its objectives parallel those of the firm implying that tier 1 will choose the optimal number of explorations. However, when tier 1 is rewarded on h, the cost per unit gain in $g_h(h)$ increases as μ increases, hence tier 1 will want to keep μ^o small. With $\bar{\mu}>0$ and $\mu^o=0$, tier 1 is rewarded as if there were spillovers, but its costs are incurred as if there were no spillovers. Because rewards are concave, this leads to more internal explorations. This does not necessarily imply more ideas. That depends upon the relative costs of internal and external explorations. We state these testable results as Implication 5.[5]

Implication 5. (a) If tier 1 is evaluated on all ideas, including those identified outside the firm, it will encourage research tourism by setting $\mu^o=\bar{\mu}$ and will invest in the "optimal" number of explorations. (b) If tier 1 is evaluated on internal ideas only, it will adopt an NIH attitude by setting $\mu^o=0$. It will work on more internal explorations and may develop fewer ideas than would be "optimal" for the firm.

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[4]We could analyze this as a formal agency problem, in which case, the firm could obtain maximal profits by paying tier 1 via $V(n)+\bar{u}+(\kappa_i+\kappa_o\mu)/(1+\mu)n^*-V(n^*)$ Because we have abstracted from risk in this section (it is covered in previous sections), this makes tier 1 managers the residual claimants. Alternatively, we could restrict the firm to rewards of the form $g(h)+constant$ In this case, the optimal rewards would be $g(h)=V(h)$. This case is analyzed in the appendix. It provides similar, but not identical, results. We have chosen instead to compare two reward systems that we have seen in practice. This allows us to illustrate intuitively why a common reward system might be counter-productive. We leave the analysis of tier 1 decision making with risk aversion to future extensions.

[5]If tier 1 is evaluated on $g(h)=V(h)$, then the equivalent result is that tier 1 will develop fewer ideas and may work on fewer internal explorations.

Summary and Implications for Tier 1

Our interviewees suggested that tier 1 explorations are highly speculative, uncertain, and long-term. Only a few explorations succeed, but failures benefit other explorations. Our analyses suggest that CTOs should encourage risky, high-variance explorations and should balance explorations within a portfolio so that alternative (negatively correlated) approaches are taken.

Our analysis of spillovers and NIH suggests that the common practice of rewarding managers and researchers for original ideas leads tier 1 to (1) ignore ideas that were "not invented here" and (2) build "research empires" by investing in too many internal explorations. This may lead to fewer ideas. The firm can be more profitable if it rewards tier 1 for ideas created <u>and</u> brought in from the outside.

Metrics to Evaluate R&D

Arthur Chester (1995), Senior Vice President for Research and Technology for GM Hughes Research Laboratories, states that: "measuring and enhancing R&D productivity or R&D effectiveness ... has gained the status of survival tactics for the R&D community." R&D evaluation is an important policy issue in Japan (Irvine 1988) and Europe (EIRMA 1995). Good measures enable CEOs and CTOs to evaluate people, objectives, programs, and projects and, in many ways, determine the size of the corporate investment in tiers 1, 2, and 3. Metrics serve many purposes including pre-evaluation, monitoring, and post-evaluation (EIRMA 1995, Irvine 1988).

The analysis of specific measures and the derivation of quantitative weights on those measures is an empirical question and is beyond the scope of this paper. However, our analyses provide qualitative insight into what should be measured.

First, it is clear that metrics must vary by tier. While customer metrics make sense for tier 3, they make less sense for tiers 1 and 2. Effort indicators such as publications, citations, patents, and peer review make more sense for tier 2 than for tier 3. Tier 1 is even further from the market, hence indicators of the quality of the people become more important.

<u>Tier 3</u> can be customer driven if scope, discounting distortions, and risk are taken into account. Equation 3 suggests explicit post-evaluation measures for tier 3. Equation 2 provides the value to a business unit. These metrics include measures of scope, discounting, and risk, and apply across

business units -- they capture more than the willingness of a business unit to pay for the R&D. To evaluate the incremental investment in tier 3 we use Equation 3 recognizing that there are also input costs associated with the combined output from tiers 1 and 2. To the extent that the outputs of tiers 1 and 2 are internal to R&D, Equation 3 minus tier 1 and 2 costs provides a post-evaluation measure of R&D. For pre-evaluation and monitoring we can use an internal market system (with the business units as customers) if the subsidies (Equation 4) are set appropriately.

Tier 2 metrics might include customer-driven measures, but they should not be weighted too heavily (β_v positive but not too large). Tier 2 should rely more on effort indicators such as patents, publications, citations, and peer review. Because risk costs and discounting distortions might vary based on whether the focus is pre-evaluation, monitoring, or post-evaluation, the weights on the β's might change by focus.

Tier 1 should be the least customer-driven. Metrics for tier 1 should capture portfolio issues through "max of n" calculations that take explicit account of variances and correlations. The portfolio rather than specific objectives should be the focus of the metrics. Furthermore, the firm should evaluate tier 1 based on all ideas, whether they originate inside or outside the firm.

Table 2 provides a list of the metrics that were used by our interviewees. We have indicated the tier for which they are most appropriate. Notice that some are explicit attempts to measure incremental value (e.g., economic value added), but many are surrogates for incremental value (e.g., customer satisfaction, time to market, revenue from new products). This reflects the reality and the difficulty of actual measurement. It is an exciting and wide-open research area.

Summary and Future Research

Our analyses have attempted to model those aspects of tier 1, 2, and 3 that our interviewees indicated to be important. In tier 3 we focused on corporate subsidies to account for business units' tendency to undervalue long-term, risky projects with less concentrated benefits. In tier 2 we focused on the tension between program selection and program development to demonstrate how managers might balance direct measures of downstream value with indicators of effort. The right balance avoids false selection and false rejection while encouraging sufficient scientific, engineering, and process effort. In tier 1 we focused on exploration portfolios and on the need to discourage NIH attitudes. Together these analyses provide the basis with which to assess alternative R&D metrics.

We feel that we have made progress toward a tiered theory of R&D evaluation and management, but there are many interesting areas that have yet to be addressed. We suggest a few.

Once a project is selected, the firm must motivate tier 3 researchers to allocate the optimal effort to a project. To address this issue one might combine the output measures of Equations 1 and 2 with internal customer evaluation systems. See Hauser, Simester, and Wernerfelt (1996). This might be extended to a fully integrated "optimal" reward system that covers all of the interrelationships among the tiers.

Self-selection on risk aversion is an important phenomenon for researchers and managers. If we reward tier 3 researchers on outcome metrics such as those in Equations 1 and 2, then the uncertainty of the reward system imposes risk costs on tier 3 researchers. If we use less risky effort indicators in tiers 2 and 3, then we might find that the reward system encourages risk averse scientists to avoid tier 3 research. See also suggestions in Holmstrom (1989). We might also explore whether an internal-idea metric helps the firm select which researchers to assign to tier 1.

Our analyses suggest one way to set research subsidies. However, our interviewees suggest that the means by which subsidies are set involve a negotiation process among business units, CTOs, and CEOs. If some business units have better information about a project than others, we could image more strategic behavior by those business units. Some business unit managers may withhold support from promising projects while waiting for another business unit to fund the project or may skew information to redirect the firm's focus (Rotemberg and Saloner 1995). Future research might address this complex gaming and explore issues such as internal patent systems or research tournaments (Taylor 1995). Similarly, we might extend our analyses to strategic gaming between firms.

Our construct of scope (m_j) is related to the growing interest in platform management -- the use of the same core technology across a variety of products (Utterback 1994). One example of a successful platform design is Hewlett Packard's use of ink-jet technology in a full range of printers, fax machines, and other document hardware. Many of our analyses can be modified to study platform management.

Some of our interviewees described an interesting dynamic phenomenon: "growing technical managers." They indicated that one core competence of the firm might be a corporate system in which R&D acts like a crucible in which technical managers survive if they understand both technical issues (tier 1) and business issues (tier 3).

Throughout this paper we have made a number of simplifications to illustrate key phenomena.

We believe that these phenomena generalize to more complex models. Generalizations to other utility functions, other probability density functions, more complex decision trees, etc. could prove interesting.

Finally, there are many personal and cultural issues in a research community. Many scientists are driven by an inherent need to know and many scientists believe strongly in a research culture. Some of these beliefs might be the result of self-selection while others might be a "thoughtworld" cultural indoctrination. Hopefully, our analyses are complementary to these sociological and anthropological approaches to R&D management.

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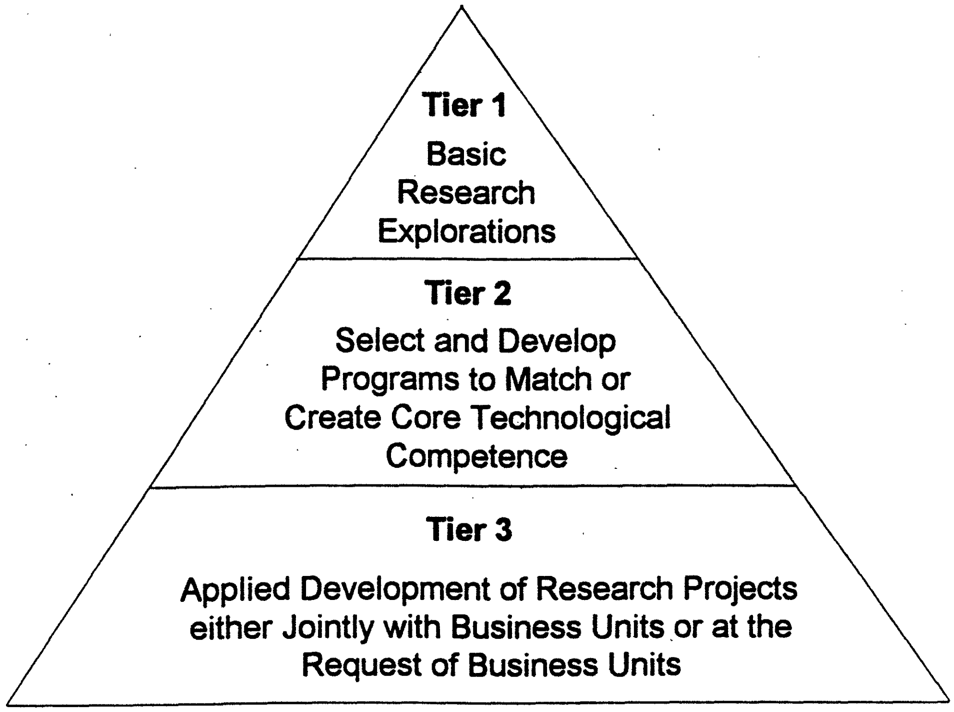


Figure 1. Technology Pyramid

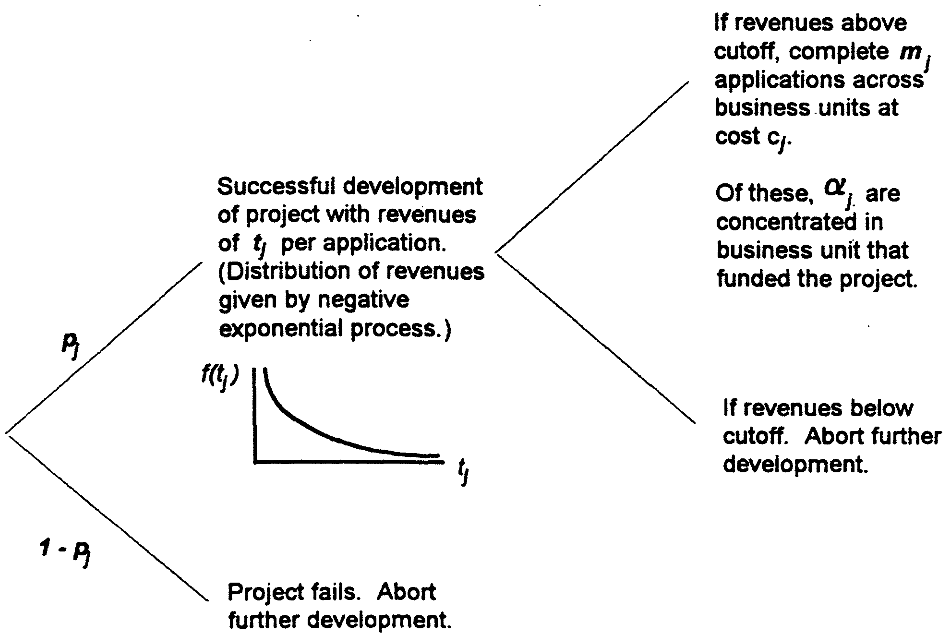


Figure 2. Decision Tree of Tier 3 Options

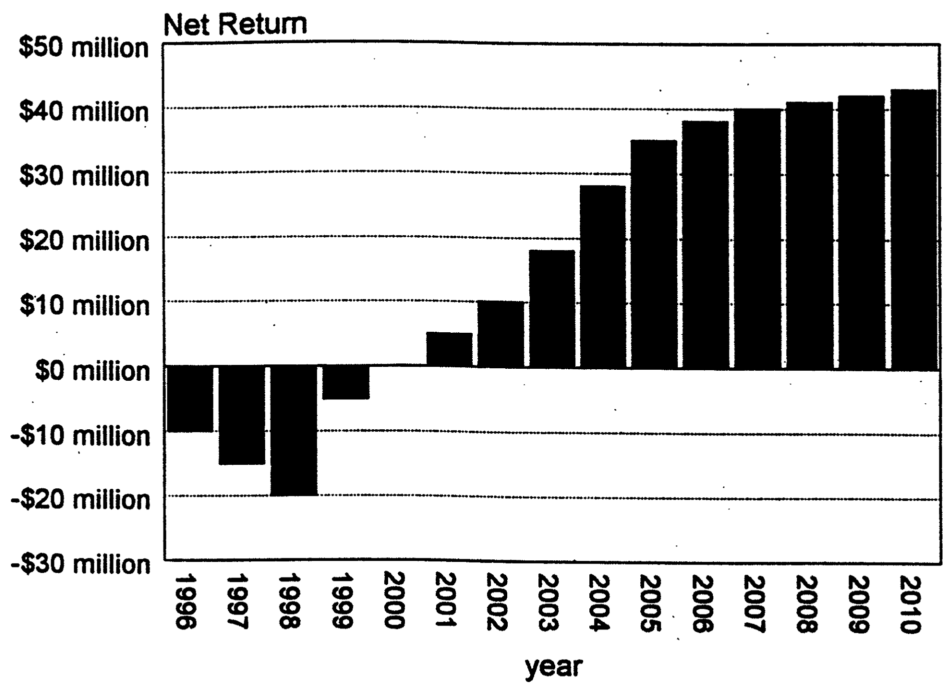


Figure 3. Example of Time Stream of Net Revenue

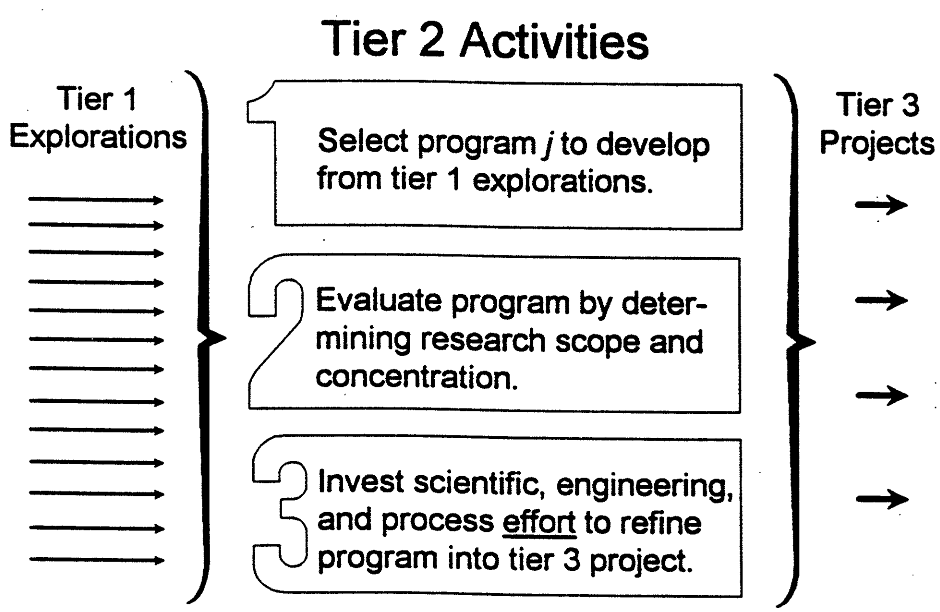
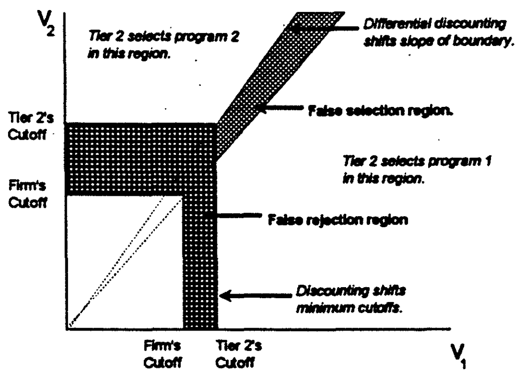
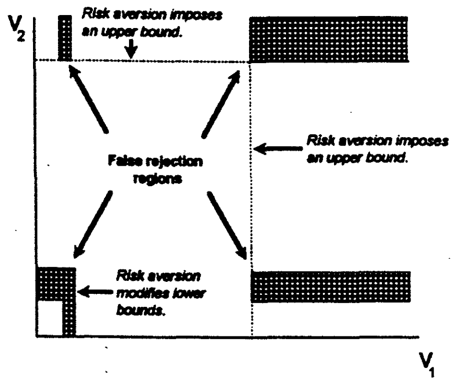


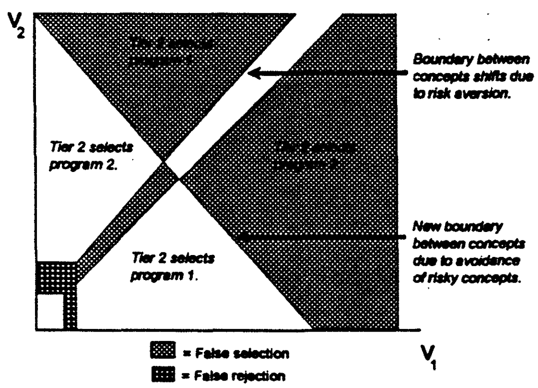
Figure 4. Tier 2 Activities



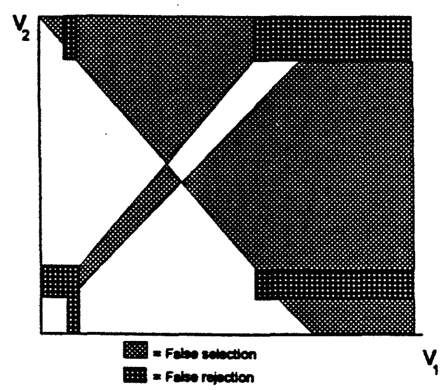
a. False rejection and selection due to discounting.



b. False rejection due to risk.



c. False selection due to risk.



d. Combined false rejection and selection

Figure 5. Tier 2 False Selection and False Rejection of Programs

Table 1. Managers Interviewed
(A total of 43 managers and researchers were interviewed. This table lists some of the titles.)

Organization	Managers Interviewed
Chevron Petroleum Technology	President, Head of Strategic Research, R&D Portfolio Manager
Hoechst Celanese ATG	President, VP Technology, VP Commercial Development, VP Technology & Business Assessment, Director Innovations
AT&T Bell Laboratories	VP Administrative Systems, Director of R&D Programs, Director of Information Applications Architecture
Bosch GmbH	Senior VP for Strategic Planning, Head of Corporate Research
Schlumberger Measure. & Systems	VP Director of R&D, Director of Engineering Process Development, Director of European Tech. Cooperation
Electricite de France	Associate Director R&D, Director of Division
Cable & Wireless plc	Federal Development Director, Director of Technology (HK), Group Strategic Development Advisor
Polaroid Corporation	CEO, Director of Research
US Army Missile RDEC and Army Research Laboratory	Associate Director for Science and Technology, Associate Director for Systems, Deputy Assistant Secretary for Research and Technology/Chief Scientist
Varian Vacuum Products	VP, General Manager

Table 2. R&D Metrics Reported by Interviewees

	Category	Metric	Most Relevant
Qualitative Judgment	Strategic Goals	Match to organization's strategic objectives	Tier 2
		Scope of the technology	Tier 2
		Effectiveness of a new <u>system</u>	Tier 2
	Quality/Value	Quality of the research	Tiers 1, 2, 3
		Peer review of research	Tiers 2, 3
		Benchmarking comparable research activities	Tiers 2, 3
		Value of top 5 deliverables	Tier 3
	People	Quality of the people	Tier 1
		Managerial involvement	Tiers 2, 3
	Process	Productivity	Tier 3
		Timely response	Tier 3
	Customer	Relevance	Tier 3
Quantitative Measures	Strategic Goals	Counts of innovations	Tier 2
		Patents	Tier 2
		Refereed papers	Tiers 1, 2
		Competitive response	Tier 3
	Quality/Value	Gate success of concepts	Tier 3
		Percent of goal fulfillment	Tiers 1, 2
		Yield = [(quality*opportunity*relevance* leverage)/overhead]*consistency of focus	Tiers 2, 3
	Process	Internal process measures	Tiers 1, 2
		Deliverables delivered	Tier 3
		Fulfillment of technical specifications	Tier 3
		Time for completion	Tier 3
		Speed of getting technology into new products	Tier 3
		Time to market	Tier 3
		Time of response to customer problems	Tier 3
	Customer	Customer satisfaction	Tier 3
		Service quality (customer measure)	Tier 3
		Number of customers who found faults	Tier 3
	Revenues/Costs	Revenue of new product in 3 years/R&D cost	Tier 3
		Percent revenues derived from 3-5 year old products	Tier 3
		Gross margin on new products	Tier 3
		Economic value added	Tier 3
		Break even after release	Tier 3
		Cost of committing further	Tiers 2, 3
		Overhead cost of research	Tiers 1, 2, 3

Appendix: Derivations and Proofs

April 12, 1996

For ease of exposition, we temporarily drop the j subscript in the derivations for tiers 2 and 3. We assume that all functions are thrice differentiable and, when appropriate, all maxima are interior.

Tier 3: The Role of R&D's Customers

Equation 1. Following the decision tree in Figure 2, we obtain *expected net rewards*$=\gamma[(1-p)\cdot 0+p\cdot Prob\{t<t_c\}\cdot 0+p\cdot Prob\{t\geq t_c\}\cdot\{\alpha m(E[t|t\geq t_c]-c)\}]$ minus the costs, k. Using the properties of the exponential process we obtain $Prob\{t\geq t_c\}=exp(-t_c/\lambda)$ and $e[t|t\geq t_c]=\lambda+t_c$. Alternatively, we obtain the result by direct integration of $f(t)=\lambda^{-1}exp(-t/\lambda)$. Thus, by substitution and simplification, the *expected net rewards*$=\gamma\alpha mp(\lambda+t_c-c)exp(-t_c/\lambda)-k$. Differentiating the *expected net rewards* and setting the derivative to zero yields $t_c=c$. Finally, substitution yields Equation 1.

Equation 2. Consider first the rewards. $E(u)=(1-p)\cdot 0+p\cdot Prob\{t<t_c\}\cdot 0+p\cdot Prob\{t\geq t_c\}E[u(\gamma\alpha mt-c)]$. Dropping terms that equal zero, substituting definitions, and using the properties of the exponential process, we obtain:

(A1)
$$E[u] = pe^{-t_c/\lambda}\int_{t_c}^{\infty}(1-e^{-r\gamma m\alpha(t-c)})\lambda^{-1}e^{-(t-t_c)/\lambda}dt$$
$$= pe^{-t_c/\lambda}\left[\int_0^{\infty}\lambda^{-1}e^{-x/\lambda}dx - e^{-rm\alpha\gamma(t_c-c)}\int_0^{\infty}e^{-(rm\alpha\gamma)x}\lambda^{-1}e^{-x/\lambda}dx\right]$$

Recognizing the first integral as an integration over the range of a probability density function and the second integral as the Laplace transform of the exponential density, we obtain:

(A2)
$$E[u] = pe^{-t_c/\lambda}\left[1 - \frac{e^{-rm\alpha\gamma(t_c-c)}}{1+rm\alpha\gamma\lambda}\right]$$

Solving for the optimal cutoff $(t_c=c)$ and substituting yields:

(A3)
$$E[u] = rm\alpha\gamma\lambda pe^{-c/\lambda}\left[\frac{1}{1+rm\alpha\gamma\lambda}\right]$$

For the constantly risk averse utility function, *c.e.* $= -(1/r)log[1 - E(u)]$. If we substitute Equation A3 into the expression for the *c.e.* and if we approximate $log[1-z]\approx -z$ + *second order terms*, we get the result in the text. Because the utility function is constantly risk averse, we just subtract the certain costs, k. When the approximation does not hold, we use Equation A3 directly.

Equation 3. The derivation of value to the firm follows that for the business unit (manager) except that $\gamma_F=1$ and $\alpha=1$. Equation 3 derives from (risk neutral) condition that *expected value* $\geq$ *costs*.

Equation 4. With subsidies, the conditions for the business unit (manager) to select a project are $R\gamma\alpha mp\lambda\ exp(-c/\lambda) \geq sk$. If $s=\alpha\gamma R$, then this is equivalent to Equation 3. If the business unit manager is risk neutral, then $R=1$.

Tier 2: Selecting Technology to Match or Create a Core Technological Strategy

Equation 5 is a definition.

Equation 6. Following the text we assume that the scope, $\tilde{m}$, results from M independent draws from a Bernoulli process with success probability q. Thus, the expected value and variance of the tier 2 (manager's) rewards are:

$$E[rewards] = \beta_v \Gamma M q v - \beta_K K + \beta_e \Gamma e - d(e) \tag{A4}$$

$$var[rewards] = \beta_v^2 \Gamma^2 M q(1-q) v^2 + \beta_e^2 \Gamma^2 \sigma_e^2 \tag{A5}$$

We use the DeMoivre-Laplace Theorem (Drake 1967, p. 219) to represent the Bernoulli process outcomes with a normal approximation. For normally distributed outcomes and constantly risk averse utility functions, the *c.e.=expected value-(r/2)(variance of outcomes)*. (The result is also approximate for other density functions.) For both results see Keeney and Raiffa (1976, PP. 161, 202). This gives Equation 6 in the text.

Figure 5a. To demonstrate the effect of discounting we, temporarily, ignore e, assume risk neutrality and focus on *E[rewards]*. The minimum cutoff for the firm is then, $v \geq K/(Mq)$. For the business unit managers (BU), we rearrange equation A4 to obtain the cutoff as $v \geq (\beta_K/\beta_v)K/(\Gamma Mq)$. The conditions for choosing program 2 over program 1 are then:

$$\textit{BU:} \quad v_2 \geq \frac{M_1 q_1 \Gamma_1}{M_2 q_2 \Gamma_2} v_1 + \frac{\beta_K}{\beta_v \Gamma_2} \frac{(K_2 - K_1)}{M_2 q_2} \tag{A5}$$

$$\textit{Firm:} \quad v_2 \geq \frac{M_1 q_1}{M_2 q_2} v_1 + \frac{(K_2 - K_1)}{M_2 q_2} \tag{A6}$$

Figure 5b. We now allow the BU to be risk averse. The firm's cutoff value does not change, but the BU minimum *c.e.* condition becomes: $\beta_v \Gamma M q v-(r/2)\beta_v^2 \Gamma^2 M q(1-q) v^2 \geq \beta_K K$. This quadratic equation will yield both a minimum cutoff (v too small) and a maximum cutoff (v too risky). That is:

$$v \geq \left[1 - \sqrt{1-2\beta_K K r(1-q)/Mq}\right] / [r \beta_v \Gamma (1-q)] \tag{A7}$$

$$v \leq \left[1 + \sqrt{1-2\beta_K K r(1-q)/Mq}\right] / [r \beta_v \Gamma (1-q)] \tag{A8}$$

Figure 5c. The conditions for choosing program 2 over program 1 become $\beta_v \Gamma_2 M_2 q_2 v_2 - (r/2)\beta_v^2 \Gamma_2^2 M_2 q_2 (1-q_2) v_2^2 - \beta_K K_2 \geq \beta_v \Gamma_1 M_1 q_1 v_1 - (r/2)\beta_v^2 \Gamma_1^2 M_1 q_1 (1-q_1) v_1^2 - \beta_K K_1$. This is a quadratic equation which will yield hyperbolic boundaries in (v_1, v_2)-space. For Figure 3c we have used the special conditions of $K_1=K_2$ and $M_2 q_2 / M_1 q_1 = (1-q_2)/(1-q_1)$. These conditions reduce the boundaries to straight lines to demonstrate the regions more clearly. The intuitive reasoning for Figure 3c is that, for a given v_2, as v_1 gets very large, program 1 becomes less attractive due to risk. There will be regions where the BU prefers a less risky program 2 over program 1 even though program 1 has a higher expected value.

Figure 5d. The effects from both figures 5b and 5c are plotted. The specific values used are $K_1=K_2=2$, $M_1q_1=10$, $M_2q_2=8$, $\Gamma_1=.9$, $\Gamma_2=.6$, $\beta_v=\beta_K=1$, and $r=2$ (Figures 5b,c,d) or $r=0$ (Figure 5a).

Equation 7. Following the arguments above, the certainty equivalent for a given e is given by: $c.e.=\beta_v\Gamma Mqv-(r/2)\beta_v^2\Gamma^2Mq(1-q)v^2-\beta_KK+\beta_e\Gamma e-d(e)-r\beta_e^2\sigma_e^2/2$. Since v is given, this reduces to $c.e.=constant+\beta_e\Gamma e-d(e)$. The tier 2 manager will choose e such that $(\partial d/\partial e)=\beta_e\Gamma$. By the Implicit Function Theorem, this implies $(\partial e/\partial\beta_e)=\Gamma(\partial^2d/\partial e^2)^{-1}$. In equilibrium, the firm must reimburse the tier 2 manager for effort and risk costs, thus the firm will maximize $\{e-d(e)-r\beta_e^2\Gamma^2\sigma_e^2/2\}$. Recognizing that e is an implicit function of β_e, we solve this maximization problem to obtain Equation 7. We find the optimal efforts, e^*, by solving $(\partial d^*/\partial e)=[1+r\sigma_e^2(\partial^2d^*/\partial e^2)]^{-1}$.

Effort Indicators. Suppose that y and z are effort indicators, such as patents, publications, citations, or peer review, and suppose that y,z are jointly distributed as independent normal variates with variances, σ_y^2 and σ_z^2, respectively. Both have means of e. Holmstrom (1979) demonstrates that the optimal contract is linear in y and z. Using this fact, we derive the tier 2 manager's optimal e for a given set of weights, a_y and a_z. This yields $(\partial d/\partial e)=a_y+a_z$. (Note that $\Gamma=1$ for the indicators since the tier 2 manager is paid now rather than later based on the indicators.) The Implicit Function Theorem yields $\partial e/\partial a_y=\partial e/\partial a_z=(\partial^2d/\partial e^2)^{-1}$. The firm will then set wages to assure that the tier 2 manager participates, that is, the $c.e.$ of the wages will at least equal the tier 2 manager's reservation wage. Because, in equilibrium, it must reimburse for effort and risk costs, the firm will maximize $\{e-d(e)-ra_y^2\sigma_y^2/2-ra_z^2\sigma_z^2/2\}$. Recognizing that e is an implicit function we solve this maximization problem to show that $a_y\sigma_y^2=a_z\sigma_z^2$. This is the result quoted in the text.

Tier 1: Exploring and Recognizing New Objectives

Variance and correlation. Gross (1972), among others, demonstrates that for $W=max\{\tilde{w}_1, \tilde{w}_2, ..., \tilde{w}_J\}$, $E[W]$ is proportional to the variance of $\tilde{w}_j$ when the $\tilde{w}_j$ are i.i.d. normal variates. To prove that larger negative correlation leads to larger expected values we work with the cumulative density function for W. That is:

$$F(w) = \int_{\tilde{w}_1=-\infty}^{W}\int_{\tilde{w}_2=-\infty}^{W}\cdots\int_{\tilde{w}_J=-\infty}^{W} f(\tilde{w}_1,\tilde{w}_2,...,\tilde{w}_J)d\tilde{w}_1d\tilde{w}_2...d\tilde{w}_J$$

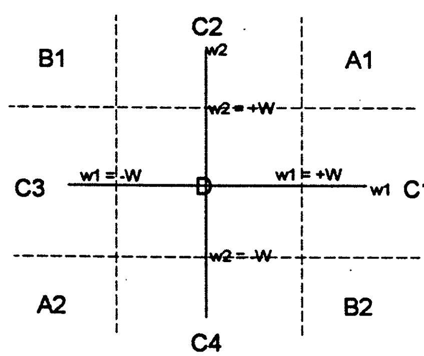


Figure A1. Visualization of Proof

We then divide the space by the hyperplanes for $\tilde{w}_j = \pm W$ for all j. This yields a series of regions. The formal proofs are tedious, but we can sketch the idea for $J=2$. For example, for $J=2$ we get the regions in Figure A1. For zero-mean variates with $\sigma_1^2=\sigma_2^2$, we switch limits to demonstrate that integration over the C and D regions are unaffected by correlation, but integration over the A_2 region increases as correlation increases. Thus, $F(W)$ increases as correlation increases. Finally, this implies that $E[W]$ decreases as correlation increases. To see the result more intuitively, consider the special case of the bivariate correlation equal to $+1$ or -1. In the former case, it is as if there were only one draw, hence $E[W]=0$. In the latter case it is if we drew only the absolute value of $\tilde{w}_1$, hence $E[W]=E[\tilde{w}_1 | \tilde{w}_1 \geq 0]>0$.

Not Invented Here

<u>Implication 4. When spillovers are possible, (a) the optimal number of ideas increases but (b) the optimal number of internal programs might decrease.</u>

(a) We show $\partial n^*/\partial\mu>0$. The firm wishes to maximize $V(n)-\kappa_i h-\mu\kappa_o h$ with $n=h+\mu h$. Hence, the firm maximizes $V(n)-[(\kappa_i+\mu\kappa_o)/(1+\mu)]n$ which implies the optimality condition of $\partial V(n^*)/\partial n=(\kappa_i+\mu\kappa_o)/(1+\mu)$. Implicit differentiation yields $\partial n^*/\partial\mu=[\partial^2 V(n^*)/\partial n^{*2}]^{-1}(\kappa_o-\kappa_i)/(1+\mu)^2$. Thus, $\partial n^*/\partial\mu>0$ because $V(n)$ is concave and $\kappa_o<\kappa_i$. (The firm prefers μ to be as large as possible because $\partial\{V[h^*+\mu h^*]-\kappa_i h^*-\mu\kappa_o h^*\}/\partial\mu= [\partial V(n^*)/\partial n-\kappa_o]h^*=[(\kappa_i+\mu\kappa_o)/(1+\mu)-\kappa_o]h^*>0$ for $\mu>0$.)

(b) To prove the result we must only establish that an example exists such that internal programs decrease. We establish existence with the example $V(n)=V_o log(n+1)$. Notice that $V(n=0)=0$. For this example we show that $\partial h^*/\partial\mu$ is ambiguous. In terms of h, the firm maximizes $\{V_o log(h+\mu h+1)-\kappa_i h-\kappa_o\mu h\}$. Differentiating and solving for h^* yields: $h^*=V_o/(\kappa_i+\mu\kappa_o)-1/(1+\mu)$. For $h^*>0$, this requires $V_o/(\kappa_i+\mu\kappa_o)>1/(1+\mu)$. Differentiating again we obtain: $\partial h^*/\partial\mu=1/(1+\mu)^2-(\kappa_o/V_o)(V_o^2/[\kappa_i+\mu\kappa_o]^2)$. For $\kappa_o\rightarrow 0$, $\partial h^*/\partial\mu>0$. For $\kappa_o\rightarrow\kappa_i$, $\partial h^*/\partial\mu\rightarrow[1-V_o/\kappa_i]/(1+\mu)^2$, hence $\partial h^*/\partial\mu<0$ whenever $V_o>\kappa_i$. This last condition is necessary for $n^*>0$. (If n^* were not positive, there would be no need for tier 1.) □

<u>Implication 5. (a) If tier 1 is evaluated on all ideas, including those identified outside the firm, tier 1 will set $\mu^o=\bar{\mu}$ and invest in the "optimal" number of explorations for the firm. (b) If tier 1 is evaluated on internal ideas only, it will adopt an NIH attitude by setting $\mu^o=0$. It will work on more internal explorations and may develop fewer ideas than would be "optimal" for the firm.</u>

(a) We first consider the case when tier 1 managers and researchers are evaluated on $g_n(n)=V(n)$. Tier 1 managers and researchers will select μ^o and n to maximize $\{V(n)-\kappa_i n/(1+\mu^o)-\kappa_o\mu n/(1+\mu^o)\}$. Differentiating, we obtain: $\partial\{V(n)-\kappa_i n/(1+\mu^o)-\kappa_o\mu n/(1+\mu^o)\}/\partial\mu=n(\kappa_i-\kappa_o)/(1+\mu^o)^2>0$. Thus, $\mu^o=\bar{\mu}$. With $\mu^o=\bar{\mu}$ and $g_n(n)=V(n)$, tier 1's objectives match those of the firm.

(b) Following the text, we now consider the case when tier 1 managers and researchers are evaluated on $g_h(h)=V[(1+\bar{\mu})h]$ where $\bar{\mu}$ is announced by the firm as a parameter of the reward function. Tier 1 managers and researchers select μ^o and h^o to maximize $\{g_h(h)-\kappa_i h-\kappa_o\mu^o h\}$. Since $\partial\{g_h(h)-\kappa_i h-\kappa_o\mu^o h\}/\partial\mu<0$, tier 1 managers and researchers will set $\mu^o=0$. The revised optimal h^o is given by $\partial V(h^o+\bar{\mu}h^o)/\partial n=\kappa_i/(1+\bar{\mu})$. The firm's optimal is given by $\partial V(n^*)/\partial n=(\kappa_i+\bar{\mu}\kappa_o)/(1+\bar{\mu})$ where $n^*=(1+\bar{\mu})h^*$. Hence, $\partial V[h^o(1+\bar{\mu})]/\partial n<\partial V[(1+\bar{\mu})h^*]/\partial n$. Since $V(\cdot)$ is concave, this implies that $(1+\bar{\mu})h^o>(1+\bar{\mu})h^*$, hence $h^o>h^*$. We establish the ambiguity of the comparison of n^o with n^* by using the example from Implication

4 to prove existence. Because tier 1 managers and researchers set $\mu^o=0$, $n^o=h^o$. We compute $h^o=[V_o(1+\bar{\mu})-\kappa_i]/(\kappa_i+\kappa_i\bar{\mu})$ and $n^*=[V_o(1+\bar{\mu})-\kappa_i-\kappa_o\bar{\mu}]/(\kappa_i+\kappa_o\bar{\mu})$. As $\kappa_o \rightarrow \kappa_i$, $h^o>n^*$. As $\kappa_o \rightarrow 0$, $n^*>h^o$ by the condition that $h^*>0$. □

<u>Footnotes 4 and 5. If $g_h(h)=V(h)$ then tier 1 may work on more internal explorations and will develop fewer ideas than would be "optimal" for the firm.</u>

The reasoning of these footnotes covers the case where the firm rewards only on internal ideas, but anticipates that tier 1 will adopt NIH and set $\mu^o=0$. Under these restricted conditions (and no risk aversion) the firm will select $g_h(h)=V(h)$. With $\mu^o=0$, tier 1 maximizes $V(h)-\kappa_i h$, hence $\partial V(h^o)/\partial h=\kappa_i$ and $n^o=h^o$. The firm's optimal is given by $\partial V(n^*)/\partial n=(\kappa_i+\bar{\mu}\kappa_o)/(1+\bar{\mu})$. Thus, $h^o=n^o<n^*$ because $V(\cdot)$ is concave and $\kappa_i>(\kappa_i+\bar{\mu}\kappa_o)/(1+\bar{\mu})$. We establish the ambiguity of h^o vs. h^* with the example of Implication 4. We first compute $h^o=(V_o-\kappa_i)/\kappa_i$ and $h^*=[V_o(1+\bar{\mu})-\kappa_i-\kappa_o\bar{\mu}]/[(\kappa_i+\kappa_o\bar{\mu})(1+\bar{\mu})]$. As $\kappa_o \rightarrow 0$, $h^*>h^o$ and as $\kappa_o \rightarrow \kappa_i$, $h^*<h^o$. □