SOME ORGANIZATIONS SEE the portfolio as a top-down statement of aspirations, allocating resources to broad categories. For others, the portfolio is merely a bottom-up accumulation of available projects. The place where top-down aspirations meet bottom-up realities is where the details of portfolio decisions come into play. As in architecture, portfolio strategy is a meeting of the power of the imagination and the reality of materials and construction.

Several years ago we helped a large conglomerate do a quick evaluation of a divisional R&D portfolio. This organization had, over the previous few years, acquired substantial pharmaceutical R&D capabilities. The division in question had a handful of existing products, and generous R&D funding, and aimed to be a major pharmaceutical entity.

Quantitative analysis of about twenty major projects, using portfolio tools described in this chapter, indicated that most of the division’s R&D efforts were being spent on incremental and “me-too” products. Why? We were told that management did not want to burden R&D with both establishment of a new laboratory and an attack on difficult markets.

Unfortunately, this strategy guaranteed that the division would not become a pharmaceutical major. With the high development expenses and long lead times of this industry, the only way to put this division on the map would be to gain strategic advantage in new markets. Management saw the point, and within a few months redirected most of its work toward revolutionary objectives. Only time would tell if they would make the hoped-for breakthroughs. However, the division’s initial portfolio strategy had not had a chance to accomplish the company’s larger business strategy.

The objective of good portfolio strategy is creating the most value—the same as the objective of technology strategy. R&D port-
folio strategy, however, is a balancing act (see Figure 10-1). It must reconcile the intent of business and technology strategy with a set of existing projects and new opportunities. Portfolio strategy represents a company’s choice as to which set of projects balances the potential delivery of R&D results over time. It indicates how much to focus on various segments of the portfolio (business units, technologies, markets, etc.). Ultimately portfolio strategy determines which R&D projects should be funded and at what levels.

One of the most difficult aspects of portfolio strategy is striking the right balance between innovative but risky projects and incremental projects with more certain returns. Operational business managers usually cast their votes for projects with greater certainty and more immediate returns, and R&D managers like to please them. This may explain why laboratories established to do innovative R&D quickly become the servants of short-term business needs, with the result that real innovation must be sought through acquisition.

The forces favoring incremental, short-term, high-probability projects are indeed strong, especially when the operational culture punishes failure, and when discount rates applied to R&D are inappropriately high. Ironically, the same managers who push R&D in the direction of incrementalism look to it for their company’s next blockbuster.
Portfolio strategy must ensure that all funded projects receive the resources they need to be successful. If sufficient resources are not available, either more must be obtained or some projects must be eliminated or postponed to allow full resourcing to others. Cutting resources to all projects to meet budget constraints usually destroys value because all projects accomplish too little or arrive too late to market.

Portfolio decisions are best made with a dialogue decision process similar to the one discussed in the previous chapter for technology strategy. However the requirements are different and the process needs more custom tailoring. Some of the design issues are:

1. How should we aggregate our opportunities into manageable strategic projects?
2. Who is the overall process owner and who will facilitate the analytical process?
3. How do we guarantee a level playing field and sufficient credibility that top management and project leadership will believe the results and stand behind the recommendations?
4. How will we respond to the results of a portfolio assessment? Do we simply want to prioritize the projects on the basis of value creation potential given the organization’s strategic direction? If the portfolio results have significant strategic implications, how will we initiate discussions of new direction and subsequent implementation?
5. How will business and marketing units interact in the process? Should we use one cross-functional decision team for the whole portfolio, divide responsibilities by business or technical areas, or use a multilevel review structure?
6. How will we keep the portfolio strategy evergreen?

If the process is annual, steps are usually taken at the beginning to ensure that all projects are treated appropriately, but not necessarily equally. For example, one would not demand the same scrutiny of a small project as of a large one. Also there is often provision for one or more sessions of peer or management review to crosscheck the quality of inputs for consistency. For example, one peer group might review all of the regulatory approval assessments across the portfolio, while another one might review all market assessments.

If the portfolio process is tied to stage gate reviews of individual projects, these reviews can be set against the backdrop of the entire portfolio. For example, the chief technology office of a major
biotechnology company refuses to review any project on a stand-alone basis. He always considers project decisions in the context of adjustments to the overall divisional portfolio.

Some organizations use a combination of the two processes, individual project reviews when they pass stage gates and an annual portfolio review, with updates to projects that have not been reviewed during the past year.

**Understanding Project Differences: The R&D Grid**

Most R&D organizations spend only a small portion of their budgets, generally 5 percent to 15 percent, on very early stage discovery research. This kind of work takes place at the leading edge of science, where the commercial possibilities of new discoveries is largely unknown. This research supports high-level business and technology strategy decisions and develops technology and capabilities that will create project opportunities to support existing businesses or establish new ones.

The bulk of R&D budgets, the remaining 85 percent to 95 percent, is directed toward projects with identifiable paths to commercialization and value creation. Yet these have differences. Some are shorter term, others longer term. Some are more uncertain; some are less uncertain. Some constituencies favor the short-term and less uncertain projects; others favor the long-term and more uncertain projects. Achieving the mutual understanding required by alignment and empowerment requires a common process framework for comparison and discussion.

The R&D grid (Figure 10-2) shows how these different projects contribute differently to their portfolios. It is an important part of a process for reaching mutual understanding about the appropriate balance of risk and return. This grid measures projects in terms of technical difficulty and commercial potential. In our lexicon, projects are either bread and butter, pearls, oysters, or white elephants, according to their characteristics.

The grid has four quadrants, each with different project characteristics. The vertical axis reflects a project’s probability of success in overcoming all hurdles (technical, financial, regulatory, etc.). The horizontal axis reflects potential commercial value. We measure this value in terms of the expected net present value of cash flows. This axis represents the magnitude of potential value creation. Roughly
speaking, projects that produce results on the left end of this axis maintain competitiveness in existing businesses. Projects on the right create new strategic advantage, either by revolutionizing existing businesses or by creating new ones.

To use this grid, we need reliable quantitative measurements for all projects in the portfolio. It is tempting to use this grid as a qualitative focus of discussion, with project leaders simply placing their projects where they think they belong on the grid. This approach violates several principles. For example, Embracing Uncertainty requires more than a quick judgment of risk; it requires understanding the sources of technical uncertainty. Top-of-the-head estimates, and others infected by bias or wishful thinking, will not do.

**Bread-and-Butter Projects**

The upper left quadrant represents projects with high probabilities of success and good commercial value. These projects usually focus on evolutionary improvements to current products and processes in existing business areas. They are also characterized by modest extensions of existing technology or their applications. Bread-and-butter projects fulfill the need to produce regular results for existing business units and to support shorter-term profit objectives. Some examples include:
- a new form of insulin
- upgraded software tools with new features and ease of use
- product extension of an antiparasitic drug
- manufacturing cost reduction program
- one-pass truck for garbage collection and recycling

**Pearls**

The upper right quadrant contains projects with the greatest potential for *both* commercial and technical success. Pearls address revolutionary commercial applications, and they deal with proven technical advances. Ideally, R&D portfolios contain dozens of pearls, each one poised to produce long-term competitive advantage. In nature, a pearl is a rare thing and is only found by opening a great number of oysters. The same applies in the field of R&D. Examples include:

- subsurface imaging to locate oil and gas
- next-generation IC chip
- artificial heart
- phase III drug for an unmet medical need
- replacement for silver in offset printing

**Oysters**

The lower right quadrant of the R&D grid represents early stage projects designed to produce new strategic advantage. They have blockbuster potential but breakthroughs are needed to unlock this potential. Here, the potential payoff is very high but the probability of success is initially low. The majority of projects in this quadrant, in fact, are expected to fail. (In the cultured pearl business, seeded oysters have only about a 5 percent chance of producing a marketable pearl.) But those that do succeed should win big.

Over time, the uncertainties surrounding both the commercial potential of these projects and the likelihood of technical success diminish. As this happens, these projects can shift to other quadrants. For example, as the technical barriers to success of a particular “oyster” project are overcome, that project may shift to the pearl quadrant unless its commercial potential has eroded to divert it into the bread-and-butter quadrant. Some examples of oyster projects are:

- new approach to pain control
- optical computing
- technology for high-definition displays
- intelligent packaging materials
- catalyst improvements for refineries
- new kinds of plastics

**White Elephants**

According to legend, the king of Siam gave white elephants to his troublesome underlords. These rare animals were regarded as sacred and so required lavish care and feeding, and could not be required to work. Instead, they consumed many of their masters’ resources and reduced the underlords’ ability to create mischief in the kingdom.

Projects in the lower left quadrant of the grid are like the king’s white elephants: They consume resources, displace more promising projects, and are unlikely to enjoy technical success or produce substantial commercial value. Obviously, no rational person would select one of these beasts for his firm’s portfolio—and few would claim credit for their inceptions. But almost all companies have them. Invariably, white elephants begin life as oyster or bread-and-butter projects, but become white elephants as commercial or technical defects emerge. Examples of white elephants include:

- videotape rental vending machine as prices dropped
- immunochemistry product for a saturated market
- innovative approach to cancer that was potent but too toxic
- technical approach to match competitor’s actions
- too small an investment in microelectronics
- demonstration pollution test site that was politically correct but used obsolete technology

To executives brave enough to admit the existence of white elephants at their companies, we ask, Why haven’t you killed them? These are the typical answers:

- There is nothing else to work on.
- We are not willing to give up our sunk costs.
- They have influential backers (often a customer).
- We are unwilling to pay the costs (real and political) of shutting down.
- Hope springs eternal.

The final problem is the lack of a disciplined process to evaluate all projects and find out where the white elephants are hiding.
Managing across the Grid

We encourage our clients to do three things with the R&D grid:

1. Assign each R&D project to an appropriate quadrant based upon quantitative evaluation of the project opportunity.
2. Capitalize on pearls, eliminate or reposition white elephants, and balance the resources devoted to bread-and-butter and oyster projects to achieve alignment with overall strategy.
3. Use their understanding of grid quadrants to shape the way they manage individual projects.

Items 1 and 2 makes it possible to see at a glance how risks and potential returns are being balanced (or not balanced) in the portfolio. Item 3 helps us to make the most of the entire portfolio.

Although projects on the grid are defined quantitatively, projects in each quadrant are qualitatively different, and should be managed differently. Bread-and-butter projects are usually part of a pipeline of incremental products and processes needed to generate near-term results. They need to be managed to deliver results on time, on specification, and on budget. Personnel involved with these projects should have incentives for results and conformance to goals.

Pearls are even more valuable as projects, but managing them for short-term deliverables is usually inappropriate. Pearls represent potential breakthroughs that should be exploited in many ways and for multiple generations—often in ways that we cannot understand in advance. Thus, pearls should be managed to encourage entrepreneurship and tolerate the circumvention of rules and systems that stifle value creation.

Uncertainty is the key issue in managing oysters. Since few oyster projects are expected to succeed, it is a mistake to reward success and punish the failure to produce results. Doing so simply encourages people to drag out project time in the vain hope of producing positive results or, worse, to avoid the consequences of impending project failure. Instead, people should be encouraged to determine quickly which oysters contain pearls and which are empty and should be eliminated. This means working on the most challenging technical hurdles first. If a project team cannot find a way over its greatest hurdle, there is little point in working on the others. And failing fast minimizes resources spent on failure, which allows the project team to pursue another opportunity.

Not all white elephants should be killed—at least not immediately.
Once identified, white elephants should be examined rapidly to see how they might be redirected. If a project is technically difficult and aimed at a small market, a simpler technical approach to the same market might turn the white elephant into a bread-and-butter project. For example, researchers might be applying their favorite high-tech solution to a market that cannot afford it, when a lower-tech solution would address most of the market needs. Likewise, that same technically difficult project might be moved over to the oyster quadrant if there is some possibility of addressing the needs of a broader market. For example, researchers might be aiming high-tech solutions at easy market targets either out of the false conservatism of addressing a well-known market or out of ignorance of what the true market could be. If neither of these avenues appears promising, the white elephant project should be terminated, or possibly sold or licensed to someone else who can make better use of the technology.

**Balancing Risk**

The easy portfolio decisions involve capitalizing on pearls and repositioning or terminating white elephants. The more difficult decisions are found among the bread-and-butter and oyster projects. As described earlier, business pressures tend to favor bread-and-butter projects. It is rare that a manager has lost either job or bonus by supporting incremental R&D for established products—the politically safe thing to do. But incremental R&D does not sustain competitiveness over time. The groundbreaking work associated with oyster projects is needed to renew the business in the long run.

The recent history of the Swiss watch industry provides a clear example of the limits of bread-and-butter R&D. Swiss companies dominated the world watch business for generations and continued to make incremental improvements to their timepieces. They broke new ground in the 1960s with their development of quartz technology, then settled back into their traditional pattern of incremental improvement.

Asian watchmakers, primarily in Japan, adopted quartz technology, and incorporated it into families of inexpensive watches that offered superb quality and customer-pleasing features at prices the Swiss could not match. All through the 1970s, the Swiss conceded one market tier after another, beginning with the low-priced mass market. By the late 1970s, Swiss producers—now clinging to small
luxury niches—could lay claim to only 44 percent of the North American and European market, down from 80 percent. Even in these high-priced enclaves their dominance was threatened.

No amount of bread-and-butter R&D could have reversed this situation for the Swiss. What did reverse it was R&D conducted by ETA S.A., a subsidiary of Société Micromécanique et Horlogère (SMH), an entity into which two failing Swiss watch companies had been merged. ETA developed a simple and reliable product platform with fewer than half the number of parts used in comparable Asian quartz models. This platform was capable of supporting tremendous product variation. A parallel R&D effort created breakthrough molding and assembly processes for low-cost mass production of this platform and the entire watch assembly.

The outcome of these R&D efforts was the Swatch Watch, a product family that returned industry dominance to the Swiss. High-risk R&D projects, like the one that produced the Swatch Watch, are required for long-term competitiveness, but need to be balanced with incremental R&D.

Understanding the Risk of Oysters

People misperceive the uncertainty of a well-diversified set of oyster projects. A portfolio of sixteen projects that each have a 25 percent chance of success is expected to produce four successful projects, and there is only about a 1 percent chance that all will fail. These numbers should be reassuring to even the most risk-averse, but one annoying uncertainty remains: We cannot identify in advance which four or so will be the winners.

People confuse their inability to pick winners with financial risk. Embracing uncertainty in this context means being able to live with not knowing which projects will succeed because you are quite certain that the portfolio will have a sufficient number of successes to pay off well.

A Case of Too Few Oysters

The core business of a Fortune 500 manufacturing company was under siege from worldwide overcapacity, foreign competition, and technology substitution. The company’s centralized R&D laboratory served several business areas and pursued many new technologies.
Given its situation, top management favored allocation of resources to new high-growth businesses, and R&D was expected to follow this strategic shift. To do this, the company’s entire R&D portfolio (over $100 million annually) was analyzed to determine how portfolio decisions, and budgetary reallocations, could improve future R&D results. One of our colleagues was asked to help with this challenging undertaking.

Over a period of six months, each of the firm’s forty-five major projects or programs was examined. This work included quantitative evaluation of the probability of overcoming each technical hurdle. In addition, commercialization paths were identified for each project and estimates of customer, industry, and market conditions were incorporated into NPV estimates that would follow in the wake of technical success. The result of this analysis was displayed in a portfolio grid like the one in Figure 10-3.

Figure 10-3 indicates several interesting findings. First, the portfolio has seven pearls, which should lead to long-term value creation.

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This portfolio contained many projects of strategic importance and good expected value. But the grid made clear at a glance that it contained too few high-risk/high-potential return projects to generate and sustain long-term growth.

Figure 10-3 indicates several interesting findings. First, the portfolio has seven pearls, which should lead to long-term value creation.
This number, however, is surprising in view of the fact that there are only four oysters! Since many oysters are needed to produce a single pearl, we have to ask ourselves two questions: Where did the seven pearls come from, and what will be the sources of future pearls? We would not expect this portfolio to renew its supply of pearls from so few oysters. There seemed to be a lack of systems thinking about the current portfolio.

Looking elsewhere on the grid, we observe a reasonable set of bread-and-butter projects, enough to deliver results to the business units in the near term. But why are there so many low-value, low-probability white elephants?

We eventually found answers to our questions about the odd shape of this portfolio by digging into the history of the company’s R&D establishment. It had been headed by an individual who understood uncertainty and the dynamics of R&D investments and had aggressively funded oyster projects. These projects produced the pearls we later observed. However, when this individual retired, the company directed his successor to reorient the department to the short-term R&D demanded by the business units. The principle of systems thinking about R&D was not broad based.

Under its new head, the R&D organization did, indeed, redirect its focus. However, its tradition of technical excellence was so ingrained that it could not let go of its many technically interesting projects. R&D lacked a value creation culture. Almost all of the former oysters migrated into white elephant territory. Despite the limited commercial potential of these projects, R&D personnel worked diligently at solving their technical challenges. They transformed a few into bread-and-butter projects, and a few were redirected in other useful ways. But most former oysters simply absorbed staff time and money without producing tangible results.

The better way to reorient the R&D effort toward short-term results would have been to eliminate most of the technically difficult work! Of course this would have meant dismantling a technical capability it took decades to build.

By not understanding the dynamics of R&D, and by succumbing to the short-term business pressure, this company’s management had destroyed much of the value of its excellent laboratory. In the process they created an ineffective mix of bread-and-butter and white elephant projects, and had almost eliminated the company’s potential for future renewal through R&D.
The R&D Productivity Curve

Part of the problem was R&D’s lack of a value creation culture. This had led management to tighten the reins and focus on short-term projects. This organization had no way to obtain mutual understanding among employees about what created value for the organization. The R&D productivity curve, illustrated in Figure 10-4, moved their discussion to a new level and focused both sides on value creation from R&D. This curve measures cumulative R&D contributions to firm value (in terms of net present value) as a function of cumulative expected remaining R&D costs. It is a measure of the incremental productivity of R&D investment. To create this curve, first order projects by the ratio of expected value to expected cost, starting with the highest ratio. Beginning at the origin, each point of the plot adds these two components for the next most productive project in the portfolio.

The curve in Figure 10-4 shows a typical case of diminishing returns for increasing R&D investment. It follows the usual 80/20 rule: 80 percent of the value of the lab is produced by about 20 percent of the R&D investment. And the last few projects are costly and barely produce a one-for-one discounted return. These projects are mostly the big white elephants from the R&D grid.
For the company in our case, the challenge is clear; many of its projects are contributing very little. How can this lab revise or trade the projects on the right side of its grid for ones more like the projects on the left? Step one is to provide a wake-up call about many of these nonperforming projects.

Reallocating Resources

Further analysis of segments of the portfolio showed that projects dedicated to applying new technology to existing markets—a favorite focus of R&D work in this company—had an average productivity of 2:1, while finding new markets for existing technology had an average productivity of 14:1. Other similar insights were generated. These insights guided the reallocation of scarce resources to projects with high productivity. Laboratory management eliminated some of the projects and reallocated resources to new projects that, according to their own evaluation, resulted in a 30 percent increase in the total expected return from R&D, which translates (at an average 3:1 productivity ratio) into $90 million in extra value creation per year from the same $100 million annual R&D budget.

One important result of these strategic reallocations was that top management gained confidence in the R&D laboratory and, at a time when most other functions were suffering deep cutbacks, increased its overall budget by 40 percent over two years. The process helped the entire organization toward the principles of smart R&D: alignment and empowerment through mutual understanding of choices, embracing the uncertainty in the R&D portfolio, a systems understanding of the long- and short-term impact of R&D, and a focus on value creation.

Maximizing Portfolio Value Creation

Managing R&D in support of both short-term needs and long-term competitiveness is one part of the portfolio balancing act, but finding completely new value is another.

The pharmaceutical industry is increasingly aware of the importance of portfolio management. Several majors have embarked on multi-year efforts to learn better methods for managing their global R&D portfolios for maximum value creation. This case, based on a
composite representative “company,” demonstrates the typical increase of about 30 percent in R&D return.

This company wanted to go beyond making yes/no decisions about each project in the portfolio to considering reallocation of resources among all of its existing and potential projects. Some skeptics felt that each project was highly constrained by technical considerations. Others thought that additional resources applied to some projects, and programs would generate far more value than the cost of taking these resources away from other projects. To make these kinds of comparisons in a geographically and culturally diverse organization would require building high credibility in the treatment of each project and fair comparisons of the value-generating potential across projects. That is, there had to be a “level playing field.”

This company sought alternative ways of carrying out each project, and then optimized the overall portfolio by optimizing the resourcing of each project for maximum incremental returns. To do this in a credible way, the R&D staff designed a dialogue decision process tailored to gain understanding and buy-in from different therapeutic areas and from top management (Figure 10-5). The process meets the dual needs of top management and each therapeutic area to be part of the portfolio decision process and committed to its results. By establishing an effective and credible process at the beginning, they laid the groundwork for powerful strategic decisions at the end, with the commitment to make them stick.

In the first month of the project, therapeutic area and project leaders were asked to respond to the following questions:

- What are your current development plans and what are the resources required to carry them out?
- If you were given a significant budget increase (say 25%–50%), how would you use the added funds to get the most additional value out of your project?
- If your budget were significantly decreased, how would you cut back your project while preserving as much value as possible?
- What budget would be required to terminate your project while still capturing its “salvage value” (e.g., through out-licensing partially developed work)?

Answering these questions forced managers to think deeply and creatively about their projects, and to develop explicit alternatives for both increased and decreased funding. Table 10-1 is a sample of
Figure 10-5. A Specially Tailored Dialogue Decision Process
### Table 10-1. A Sample of Alternatives with Different Levels of Funding

<table>
<thead>
<tr>
<th>COMPOUND</th>
<th>CURRENT</th>
<th>EXPANDED</th>
<th>REDUCED</th>
<th>MINIMAL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alpha</td>
<td>▲ Target launch for hypertension ▲ Initiate long-term outcome study in Phase IIIb ▲ Conduct Phase IIIb comparator studies</td>
<td>▲ In addition to Current, two alternatives: (A) Pursue PLE with patent extension (B) Accelerate NDA filing</td>
<td>▲ Current, but eliminate Phase IIIb long-term outcome study</td>
<td>▲ Stop all development activity after Phase IIb ▲ Pursue license-out opportunity</td>
</tr>
<tr>
<td>Beta</td>
<td>▲ Target launch for prostate cancer in U.S. and Europe 3Q99 ▲ Start a comparator study vs. major competitor (not to be included in regulatory file)</td>
<td>▲ Current, but add mortality study</td>
<td>▲ Not applicable</td>
<td>▲ Stop all development activity ▲ Pursue license-out opportunity</td>
</tr>
<tr>
<td>Gamma</td>
<td>▲ Target file date for Alzheimer’s: Sept. 1998 ▲ Pursue seven Phase IIIa studies in parallel ▲ Pursue one comparator study</td>
<td>▲ In addition to Current, three alternatives: (A) Perform two disease modification studies starting Jan. 1997 (B) Perform two disease modification studies starting Oct. 1997 (C) Perform two disease modification studies starting April 1998</td>
<td>▲ Two alternatives: (A) Current, but perform two fewer Phase IIIa studies (B) Current, but drop comparator study</td>
<td>▲ Stop all development activity after Phase II ▲ Pursue license-out opportunity</td>
</tr>
</tbody>
</table>
the kinds of alternatives created for three projects (compounds), indicating how they could be changed according to different levels of funding.

Each project was then analyzed and reviewed through the dialogue decision process. In the end, the shareholder value (SHV) potential was established as estimated by the net present value of increased cash flows (net of investment) for each project and each alternative, as shown in Figure 10-6. These values indicated, in the absence of resource constraints, how each project could be funded to achieve optimal performance. For example, the SHV of project Alpha is optimized at current budget levels. In contrast, SHV for projects Beta and Gamma improve measurably with expanded funding. To maximize shareholder value, an organization should be prepared to fund the best alternative for each project as long as its shareholder value is positive.

Using the R&D Productivity Curve

The next step in the pharmaceutical company’s process was to determine the extent to which optimizing increments of project funding across the entire portfolio would increase portfolio productivity and value creation. As a baseline, a productivity curve was developed assuming all projects are funded at the current levels (Figure 10-7). Overall the current portfolio was expected to generate high shareholder value, achieving over $8 billion in SHV on some $200 million in incremental investment (that is, not counting the many years of sunk costs it took to arrive at this enviable position). However, as we
might expect, the 80/20 rule appears to be at work. That is, most of the value appears to be generated by a minority of projects.

But appearances are not always reality—a systems thinking approach is required to understand where the value is really coming from. Projects on the steep side of the curve are often close to reaching the market; the projects on the shallow side may be early stage projects, some of which will eventually migrate to the steeper side over time. Simply cutting all of these less productive projects would endanger the future. Deeper analysis is required to draw firm conclusions about whether some of the projects on the shallow side of the curve are the oysters that could turn into the next-generation pearls.

The next step was to build a new curve based on all possible combinations of funding levels for every project in the portfolio. Unfortunately there are about $10^{12}$ (one quadrillion) possible combinations. However, there are mathematical methods for determining the optimal set of combinations for any resource level (called the efficient frontier in investment portfolio theory). Figure 10-8 plots the optimal set—considering shareholder value versus costs—for the company's total R&D portfolio.

Shown against the base case productivity curve, adoption of the optimal combination of project funding levels would produce an
additional $2.4 billion in shareholder value, a 30 percent increase, for the same total R&D investment. This optimization depends on the ability to generate and evaluate multiple alternatives. When this is done, the optimization is a straightforward task.

To implement these recommendations, we would have to assure ourselves that we have the right kind of staff, availability of facilities, and so forth. With a little fine tuning, most of this value should be achievable. So far this approach answers the question of how to allocate resources to create maximum value within the financial constraints.

The Case for Increased Funding

One and a half billion dollars in added value is a very good return on the cost of doing one's homework. But for our pharmaceutical company, the story did not end there. The expanded set of project alternatives generated through the study indicated additional high-return possibilities if more development resources could be released. In the months that followed, the organization considered the implications of adding these potential projects and requested an increase in R&D
funding to pursue them. The discipline of the portfolio process focused attention on where it belonged: Why were there constraints in the face of excellent prospects for value? A value creation culture works to remove the constraints.

To appreciate the logic of this proposed funding increase, take a systems perspective on the entire enterprise and ask what returns are offered by incremental investment in R&D compared to other areas of the company. Figure 10-9 shows the level of returns offered by R&D. It shows the slope of the productivity curve (i.e., the ratio of incremental SHV to incremental investment) at several points along each curve. In the current portfolio curve, the lowest incremental productivity is 3:1; this occurs at the $200 million investment level. The optimized portfolio curve indicates incremental productivity of 10:1 at this same investment level. This suggests that additional funds could be productively invested in the optimized curve, perhaps up to the original 3:1 level, which would occur at the $310 million level. The fundamental value creation question is, Does the firm have other opportunities that have (long-term) returns of this magnitude? If not, R&D is the highest and best use of incremental resources.

“No way!” was top management’s initial response to R&D’s re-
quest for added funding. A major product was going off patent and management did not want to consider anything that would increase pressure on current earnings. The stock analysts had already been assured that there would be no drop in earnings. Management preferred the plan to hold the budget constant and pick up an added $2.4 billion in NPV through portfolio optimization. To them, it was a “free lunch.”

Debate on this issue—which pitted short-term concerns against long-term competitiveness and shareholder value—continued for several months. It is the same debate that takes place in virtually every company that relies heavily on R&D. In the end, the long view prevailed—they expanded the funding level by over 50 percent, which created another $0.8 billion of shareholder value. Management recognized that its obligation to shareholders required added R&D funding. In fact, they held special meetings with the stock analysts to explain the value hidden in their R&D portfolio and their obligation to the shareholders to increase funding. Withholding investment from such excellent R&D opportunities, they concluded, would actually destroy shareholder value.

This new way of managing the R&D portfolio did more than simply rationalize the portfolio and increase its funding. The process helped everyone to think deeply about value and the way it is created. It required everyone to find and evaluate alternatives. It gave many individuals the opportunity to gain experience using a rational decision process and tools such as SHV. And it required top management to come to grips in a systematic way with the perennial tension between the present and the future. In making the difficult choice between long-term value and short-term results, top management affirmed value creation as the most important organizational imperative.

Many industries are coming to realize that smart strategic management of their R&D portfolios is their only hope for long-term renewal and strategic success in their base business. It has become obvious that taking calculated risk is the basis of successful competition; companies that fail to learn will fall by the wayside. Successful companies have recognized the need to develop a common approach across global organizations with diverse histories and cultures.