Risk Taking and Effective R&D Management

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Abstract

Several key strategies can be used to manage the risk associated with innovation to create maximum value. These include balancing the timing of investments versus cash flows, management of fads, prioritization across the company, savvy portfolio management, and a system of metrics that measure real success. Successful R&D managers will do whatever is necessary to manage the risks associated with an R&D program and stick to their long-term strategy.

INNOVATION: THE ULTIMATE GOAL OF R&D MANAGEMENT

An invention is a creative solution to a problem. The list of game-changing inventions of the past would certainly include the lightbulb, the automobile, and the telephone. And when we think of these inventions, we think of Edison, Ford, and Bell, the men who invented products that changed the world we live in. But we would be wrong. None of these men were the actual inventors of these important products. The patent for the initial discovery of each of these technologies was given to someone else at a much earlier time, but their names are largely forgotten, except by specialists or historians (1–3). What Edison, Ford, and Bell did was improve, perfect, and make available to a grateful public the inventions of these forgotten men. That is the essence of innovation.

Innovation. It is a word used by just about every major industry, and it is crucial to business success. It has many definitions. We define innovation as "invention that creates value for society." In this context, value refers to the price of the innovation. To qualify as innovation, an idea must not just solve a problem; someone also must be willing to pay for it. Innovation is the foundation of industrial research. It is our view that industrial R&D is not a right but a privilege. Corporations can exist only if they make sufficient profit to justify continued investment in R&D. The role of a company's chief technology officer (CTO) is to be the champion of innovation—of creating value for customers and earnings for the corporation.

The reason we remember Edison, Ford, and Bell is because they succeeded in execution. Edison made the lightbulb more robust and efficient. Ford automated automobile production, bringing down the price while increasing quality. Bell took an idea and created a functional product. Each created a product that people wanted to buy.

To qualify as an innovation, then, an idea must be perfected. It has to be practical, reliable, and cost-effective. Successful innovation requires inventiveness, creativity, and execution. Nothing that is new or creative is ever a guaranteed success. Therefore, R&D is, by definition, fraught with uncertainty and risk. Successful R&D management is about managing and mitigating risk.

Many in science have made a distinction between academic and industrial innovation: Academic innovation is characterized by changes in thought and industrial innovation by changes in quality of life. But the two are most certainly interrelated. Industry has not been, nor will it ever be, the sole source of invention; academic researchers also place considerable importance on meeting pressing societal needs. In addition to their own discovery efforts, industry researchers also rely on academia as a source of invention—the spark for the radical breakthrough. One important role of industry is to determine which of these radical breakthroughs has the potential to change the way we live for the better.

This becomes more challenging every year. Today's society is well served by the current slate of products and materials. To gain adoption, new innovations must present a compelling value proposition. Complicating matters further is the speed of technological innovation. Consider, for example, the history of sound media innovation. The first phonographic cylinder recording was made in 1888 (of Handel's choral music at the Crystal Palace in London). The next big advance in sound recording, portable magnetic tape, was not available until the early 1960s—72 years later! Contrast that with the mere 13-year gap between the introduction of the compact disc in 1981 and the first downloadable MP3 file in 1994 (**Figure 1**). A popular video circulating on the Internet captures the geometric nature of these trends, noting that it took radio 38 years and television 13 years to reach audiences of 50 million people, whereas it took the Internet only four years, the iPod three years, and Facebook two years to do the same (4). There is still a market for new portable entertainment technology, but it is harder to imagine the development of a disruptive technology in the industry.

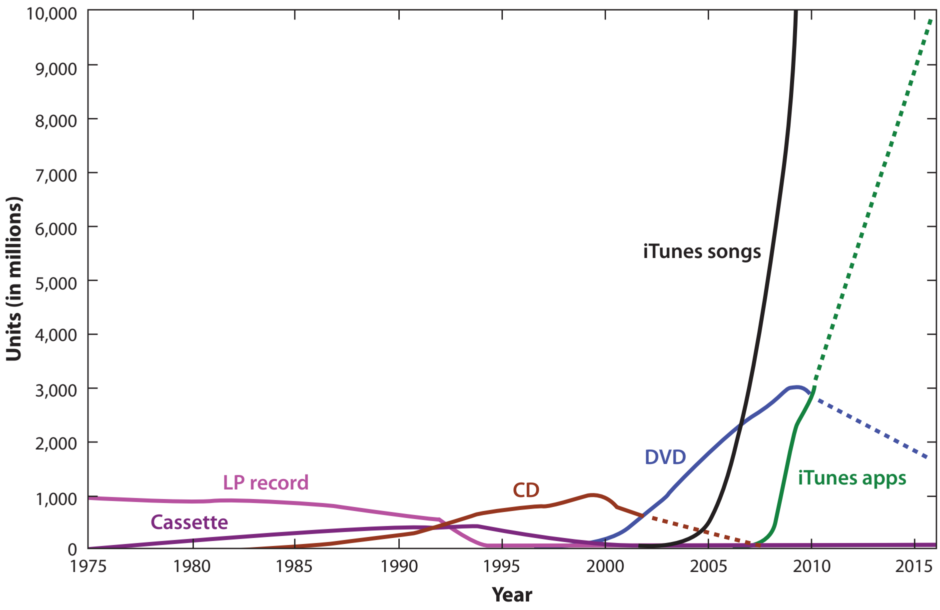

Figure 1

The rate at which media formats are changing is an example of the speed at which innovation now occurs.

When the goal is innovation, invention and execution are exceptionally time sensitive. Managing R&D is equivalent to managing industrial innovation. Success requires delivering game-changing, disruptive technologies, products, and processes to customers in such a way as to measurably improve their quality of life. Customers will then spend their money on your products or processes, and the result is growth for the company. All risks associated with R&D management come about from trying to achieve that goal.

Many books and journals are dedicated to specific discussions of the issues of R&D and risk management. This paper does not attempt to collect all of these into one exhaustive review. Instead we rely on their experience to focus the discussion on the essential factors leading to successful leadership in industrial research. Industrial R&D management has three key elements:

1. Determining how much money you will spend, understanding that an appropriate R&D budget that effectively manages risk will be aligned with the goals of the supporting organization;
2. Determining which projects your team will take on, which requires prioritization using a robust method and a focus on the most current technologies while managing fads; and
3. Managing these projects so that they result in execution, which includes funding and resource allocation, definition of metrics, and management of people.

Prioritization, risk management, resource allocation, and people management are fundamental to any research effort. R&D leaders who understand how to accomplish this will promote innovation, whether they are in industry, universities, or government labs.

DETERMINING THE R&D BUDGET—MULTIYEAR HORIZONS

Determining an R&D budget that the entire institution can consistently afford to support is the important first step. During much of the twentieth century, corporate R&D labs were the primary drivers of innovation. The products they created opened entirely new markets and significantly expanded applications, generating high profits in the process. This allowed the high cost of R&D to be recouped before the competition caught up. The achievements during this time, such as the development of the transistor and the laser, were significant, and many Nobel Prizes were awarded to industrial labs. But in recent decades, profit margins have diminished, whereas R&D costs have increased. A typical semiconductor fabrication can cost billions of dollars, with a single piece of the required equipment costing tens of millions. At the same time, the cost of electronics has fallen. This has resulted in increased scrutiny of research costs as compared with the expected returns.

Annual R&D budgets are typically established using the previous year as a baseline. This method has the benefits of continuity and simplicity, and concentrates on changes in a program or department. The implicit assumption is that the previous budget was the optimum to achieve institutional goals, including affordability. When more profound transformations are required or serious financial challenges exist, an alternative approach is to use zero-based budgets. In this method, there are no entitlements. Every program starts with a zero budget and must rejustify its spending. This approach requires more effort but can uncover where previous assumptions are no longer valid. Depending on the flexibility of an institution, implementing the outcome of a zero-based budget can be challenging but may be required to ensure ongoing success. For example, it might be impractical to maintain a high-cost service model for a product that has become commoditized. Dow Corning recognized this when it created its Xiameter brand of standard silicones (5). The Xiameter buying model, a powerful commerce Web site, offers a low-cost approach for products that are less specialized and cannot support a higher degree of customization or technical support.

Given the inherent multiyear nature of innovation, determining the overall budget for the current year must be done in the context of the overall costs as programs mature. Consistency in research is essential. Little is accomplished in a year or two. Starting a new program is easy; the challenge is to have enough planning and tenacity to ensure execution. It is insufficient to look at the initial costs; you must look at the expected cost over the life of a program and reconcile this with the institution's financial situation. This is complicated by the unavoidable lag between the investment in innovation and the resulting cash flows from the new product or process. For example, it can take up to 20 years to translate an inventive thought in the chemical industry into significant market penetration (6). Bringing a new material to market usually means long specification cycles with a series of key customers downstream. The materials industry does not sell a new product until the customer sells a product, and in many cases raw materials are four to five steps or more removed from the ultimate end user. Each step in the supply chain has its own new product introduction procedures, and the time line for these can range from a year for electronics products to a more than a decade for aerospace products. Additional delays can occur if the design of the end product (e.g., an automobile or wind turbine blade) changes in a way that requires a redesign of the material properties. Finally, there can be significant regulation and registration requirements that may take a year or longer to fulfill. This is particularly true for agriculture and health care products.

Further complicating budget decisions is the fact that economic cycles are now 6 to 8 years in length, whereas most materials' product development cycles are from 5 to 20 years. Developing a brilliant investment strategy is easy when the economy is strong. But it is during a weak economy that a leader's ability is really tested. The natural reaction to a downturn is to stop spending on anything that doesn't yield an immediate payback. The expectation is that when things recover projects will be reinstated, and the short suspension will have no noticeable impact on a long-term program. Unfortunately, this assumption is dead wrong. In these instances, it is not a lack of good strategy but rather a lack of tenacity that is the obstacle to success. One key tool for sustaining

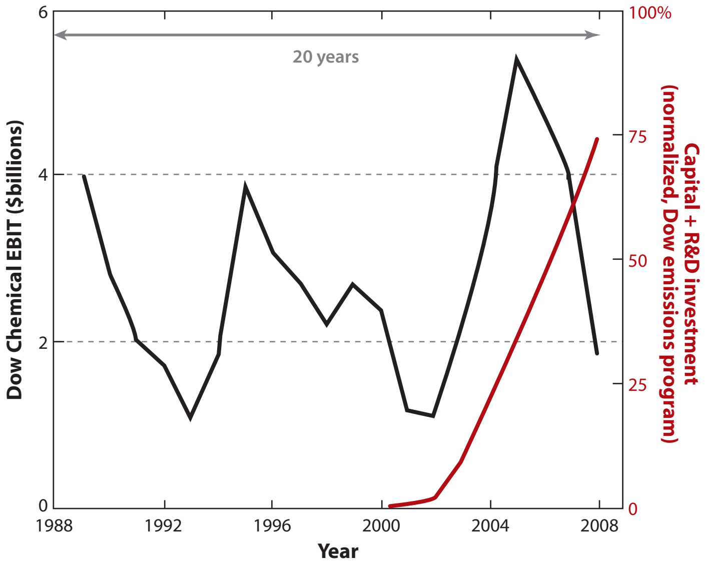

Figure 2

A plot of earnings before interest and taxes (EBIT) and normalized investment versus time for a Dow emissions program shows the mismatch between economic cycle length and materials development cycle length. Materials are a special case, as they can take up to 20 years to realize a profit.

innovation during difficult times is prioritization, which is described later. Furthermore, long-term programs require continuous investment to reach milestones. Plant construction cannot be halted midstream without incurring additional cost. The delay in completion further postpones generation of return on the capital that already has been expended. Laid-off workers cannot be expected to resume the same levels of productivity if rehired after a year or two, and R&D teams that are disbanded to other projects cannot be easily regrouped if the project is restarted. There is increasing pressure on public companies to achieve quarterly results, but successful innovation requires a long-term vision. True leadership is required to sustain that vision and ensure successful innovation. This challenge is illustrated with a specific example from Dow (**Figure 2**).

Dow developed a disruptive ceramic filter technology for vehicle emissions. After years of research and product development during the "good times," the manufacturing plant for this new product was midway through construction when the 2009 recession hit; this recession challenged the automotive industry in particular. Due to the qualification cycles in the transportation sector, this plant was not expected to generate sales until late 2010 or early 2011. But the construction of the plant was consuming cash. A common response during an economic crisis is to stop construction to conserve cash—a plan that makes obvious short-term sense. But Dow's long-term strategy is to develop products that create growth and meet pressing societal needs. The company had identified reduced particulate emissions and increased fuel economy as key societal needs. Dow decided to continue the construction of this plant with the understanding that cutting future key profit drivers today means delaying tomorrow's earnings.

DETERMINING THE PROJECT PORTFOLIO

Good R&D management requires that the innovation pipeline produce a constant and predictable profit flow so that the company will continue to excel, particularly during turbulent economic times. Managing the balance between long- and short-term projects in the pipeline is fundamental to mitigating the risk associated with R&D. To manage this at Dow, we produce, on an annual basis, the Innovation Growth Playbook. The Playbook is a compilation of one-page summaries, one for each major R&D project. Each summary includes key performance metrics such as the customer value proposition, the competitive landscape, resource requirements, expected benefits, a net-present value (NPV) calculation, and the current stage of each program. The NPV calculation is based on a ten-year horizon: Cash flows are determined and the NPV is calculated using standard accounting for the time value of money. These one-page summaries of our R&D activities are then prioritized, first within individual business units and then within the corporation as a whole. This is further risk adjusted by project stage. Early discovery programs are discounted by 90%, whereas projects approaching commercialization are discounted by 10%. When done correctly, the result is an optimal mix of long- and short-term programs that should provide the maximum return over the ten-year period.

Figure 3 is an example one-pager for a fictitious project that displays the type of information used for evaluation. In 2001, CNN reported an interesting innovation coming out of Japan—square

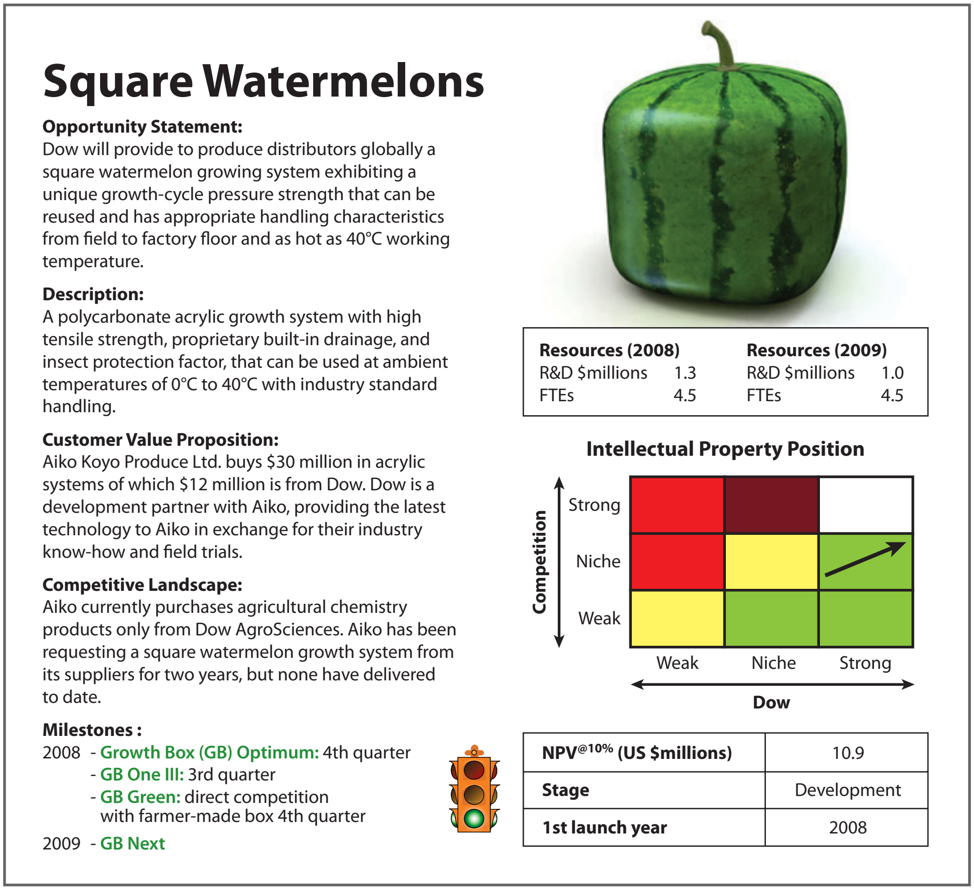

Figure 3

An Innovation Growth Playbook one-page summary for the fictitious Square Watermelons R&D project. Abbreviations: FTE, full-time equivalent; NPV, net-present value.

watermelons (7). Japanese consumers were looking to maximize refrigerator space, and a square watermelon does just that. If our R&D organization were to support a project for the production of square watermelons, this one-pager would be used to decide its place in the prioritized deck. Actual execution documentation uses more detailed project summaries, specifications, customer requirements, testing schedules, and protocols. In addition, a detailed stage gate process regulates program execution and assures that all critical risks are recognized and mitigated.

The term "playbook" is a quite accurate description of the final compilation of one-pagers. Just as a football coach has a playbook describing the individual tactics and plays that the team will use collectively to win a big game, the Innovation Growth Playbook is the collective strategy the company will follow to win at innovation. It is also a key tool used to allocate resources. Although details will vary by organization, the ability to define a strategy and allocate resources is essential. To successfully manage risk and obtain the greatest return on investment for R&D dollars, it is crucial to deploy those limited resources against the opportunities that have the highest potential rate of return. This will never happen if leadership simply aggregates the individual prioritized program decks from each business or individual. Successful portfolio management requires intervention. The playbook allows the leadership team to compare the lowest ranked funded program in one area with the highest ranked unfunded program in another. If the unfunded program in business two has a higher risk-adjusted return, resources are allocated from business one to support business two. The same protocol also applies to new business development. A proposal for an entirely new business generates a business plan that details the costs and expected earnings. These are risk adjusted, and if the returns exceed those of an existing program, funds are reallocated. In some cases a decision not to deemphasize an existing business is made, and incremental resources are committed, all within the confines of the overall corporate affordability model. Thus, a successful playbook facilitates portfolio management across the entire corporation.

EXECUTING PORTFOLIO MANAGEMENT

Resource Allocation and Funding Mechanisms

Prioritization is only one part of the R&D risk management process. Once the leadership team has agreed on project prioritization, funding and resources must be allocated appropriately. And the only way to manage R&D successfully is to be ruthless in allocating resources. Recent turbulent economic times have forced many R&D organizations to reduce and reallocate resources, and many strategies can be utilized for this. These can be thought of as on a continuum between two extremes: the haircut and the lifeboat.

To reduce spending in times of crisis, the most common strategy is the classic haircut: a small cut in overall funding across the entire project portfolio, for instance, a 10% cut from every ongoing project. The rationale for this strategy is that it is fair. Everyone takes the same hit, and although management and personnel might be unhappy with the cut, they feel comfortable because all projects are supported and continue to move forward, just more slowly than before. The upside is that this socialist strategy feels safe and does not require any additional consideration from project to project.

But this plan of action has a major complication: All projects are not equal in value; in other words, life is not fair. This strategy presumes that the lowest ranked program has the same importance as the highest ranked program, which completely destroys the purpose behind the prioritization exercise. This haircut method of spending reduction using equal cuts across all programs as a resource allocation strategy represents, in the authors' view, a lack of leadership.

If a situation requires a 10% overall budget cut, rather than cut 10% from every program, a better strategy is to cut 100% of the least attractive 10% of programs. This can be thought of as a lifeboat because decisions on programs are binary. A project is either above the line (in the boat) and safe with all resources required, or below the line (outside the boat) and dead. The downside of this approach is that the project managers of the least attractive, lowest rated programs are not going to be happy. But the chemicals and materials industry is cyclical, and there will always be a program at the bottom of the list. It is impossible to imagine a scenario in which it would be a good strategy to sacrifice the top programs in any way to keep the bottom performers on life support. Having already determined that it can take 20 years to go from idea to deep market penetration, it is difficult to see why one would choose to intentionally delay a program with high potential return. Of course, this assumes that the risk-adjusted prioritization was correct.

Programs that grow organically or new initiatives within a business represent a particular challenge. Breakthrough projects typically take a long time to develop. The business units generate earnings, and therefore most resource decisions need to occur within the business units to ensure accountability. However, businesses are by their very nature preoccupied with quarterly results (the same might be said for public corporations). The system is biased toward the short term, so the long term may suffer, particularly in times of financial crisis. Some management system must be in place at the corporate level to ensure that the long term is sustained and balanced against the inherent short-term bias of the business.

One mechanism to address this short-term bias is to move the funding allocation for critical or strategic initiatives outside the business units, which allows the pursuit of breakthrough innovation without burdening the business units. This new funding model is currently used to fund the Dow Powerhouse solar shingle. The building-integrated photovoltaic (BIPV) project, which will ultimately move into the Building and Construction Business, was initiated and currently is overseen by the CTO in consultation with senior executives. The program is funded outside the budget of the business unit. As the product line matures and positive cash flows materialize, the product and R&D effort will transition to the business, and at that point it will have to compete with other programs for funding. Other potential mechanisms, such as adjusting earning expectations to account for growth initiatives, are possible, but all require some sustained and direct intervention by leadership to maintain the appropriate short-term/long-term balance.

A model is also required for technologies and expertise that are valuable to more than one business across a corporation. Process engineering, catalyst design, analytical services, and high-throughput research are examples of R&D expertise that ideally are shared across all businesses and research units. The costs of developing these capabilities are very high and would be too much for any one unit to absorb. Additionally, the capabilities are too valuable not to be leveraged across the corporation. These capabilities and expertise centers are therefore incubated in a central or core R&D organization. In essence, businesses hire the central research group, leveraging this expertise for specific projects as needed.

This discussion leads to a very important question regarding R&D management: Can you manage basic research? Some might argue that you cannot, that management is reasonable for applied research, because the problem is known and success is defined by finding a solution to the problem, but that basic research is focused on the solution when you do not know what the problem is. However, basic research is not a synonym for accidental research. All R&D projects are based on a hypothesis of some sort, so a projected project outcome exists. If the resulting information potentially can be used to create value, then the project is a viable R&D effort. If it is impossible to define a realistic application of value, or if finding such an application seems a stretch, then the R&D manager must think hard about whether or not the project is worth supporting. Albert Einstein once said, “If we knew what we were doing, it wouldn’t be called research”

(8, p. 35). To that we might add, "But if we don't know why we are doing it, it cannot be considered valuable, at least in the realities of a corporation."

Staying Current: The Industry-Academia Relationship

Successful R&D management requires a deep and thorough understanding of what is new on the frontiers of science. Although industrial R&D success is not measured by the number of prizes or papers, the most valuable new products and technologies must, by their very nature, spring from and employ the latest discoveries.

To be successful in today's fast-paced R&D world, industrial research has to be aware of the breakthrough science of the best and brightest academic researchers. Similarly, academic research looking at solutions for major societal problems such as energy, health, and clean water will require commercial entities for implementation. Even government and private funding agencies want to know that the monies they provide to academic labs will ultimately lead to a discovery that has some sort of measurable benefit.

A study by Mansfield (9) looking at industrial output from 1975 to 1985 found that, on average, more than 10% of new products and 9% of new processes in a variety of industries would not have been developed without substantial delay in the absence of academic research. These numbers were 4% and 6% for the chemical industry and 27% and 29% for the pharmaceutical industry, but interestingly enough, the numbers for all of the industries studied were statistically similar when corrected for R&D intensity as defined by R&D as a percent of sales. The mean time lag between academic invention and industrial product was approximately 7 years. A similar study by Gellman (10), looking at industry between the years of 1953–1973, came to nearly identical conclusions with respect to lag time.

So how does this relate to managing the risk associated with industrial R&D? Simply put, industrial R&D leaders are responsible for constructing systems that derive the most benefit from significant academic breakthroughs. The classic way to achieve this was to hire students from these cutting edge laboratories, bringing the talent into your space. Make no mistake: This is still important. It always will be true that the strength of an R&D organization starts with the strength of the people who drive it. But an even better way to ensure that an industrial R&D group has access to breakthrough technologies is through formal partnerships.

Although the largest challenge for industrial-academic partnerships is control of intellectual property, in principle there is no conflict. The goal of industry is to develop new and valuable products, and the goal of academia is education. Research is the vehicle to achieve both of these goals, and this is where the conflict can arise. For research to ultimately provide value to society, it must produce practical applications. This can require decades and millions or even billions of dollars. The question of fair compensation and an adequate recognition of the difference between invention and innovation can be contentious and often derail collaborations. This has been particularly problematic in the United States. By comparison, European and Asian universities historically have primarily focused on education and have been content to leave the costs and benefits of technology commercialization to industry. However, many U.S. universities have recently recognized that as federal R&D funding declines, private industry can be an important source of research funding. In addition, industry is taking on increasingly challenging problems, and to succeed, it must engage the best academic minds. National labs are also a source of experience and knowledge. Collaboration has always occurred between academia, industry, and government agencies, and recent trends suggest it is expanding. The current initiatives for energy storage research are some of the best examples of this accelerated collaboration (11). But even as federal spending on energy research ramps up, programs with clear paths to commercialization have priority.

One real benefit of these collaborations is the support of multigenerational product planning that dividing responsibilities between institutions permits. A recent example is the strategic relationship between Dow Solar Solutions and the California Institute of Technology. Dow and Caltech have partnered to discover and develop the next generation of thin film photovoltaic materials based on abundant elements. Current Dow thin film photovoltaic technology utilizes cadmium, indium, gallium, and selenium (CIGS). But indium is a semirare element; there is, in fact, not enough indium in the earth's crust to meet the world's electricity needs using only thin film CIGS. Dow's current priority is to develop a reliable BIPV based on CIGS (Dow Powerhouse solar shingle). Ultimately, however, the world will need an improved product. Applying Caltech's world-class expertise in chemistry and physics to invention of a new class of thin film materials offers several advantages for both parties. Caltech gains additional funding to support fundamental materials research and can continue to educate students while contributing to a significant solution to meet the world's energy needs in a more sustainable manner. The benefits to Dow include the ability to focus resources on the commercialization of the current CIGS-based product while also participating in the development of the next-generation product. It is a true collaboration with free exchange of information between the parties. These relationships between industrial and academic leaders ensure that the company is on the cutting edge of invention and align the innovation strategy to the latest advances in the field while simultaneously accelerating academic research through commercialization, which ultimately helps society.

This type of strategic relationship is a key example of one of the pathways to success in today's fast-paced marketplace: collaboration for innovation. It is increasingly unlikely that any one entity can generate all of the best ideas and assume all of the risk and still maintain the fastest speed to market. In their book *Innovation to the Core*, Skarzynski & Gibson (12) refer to this as "bringing the outside in" and state that this mixing of intellectual gene pools is a key to stimulating new nonlinear thinking and avoiding an overly internal focus. In addition to bringing together the best talent, some corporate collaborations are necessary to reduce risk to an acceptable level. For example, Dow and BASF are the two largest chemical companies in the world but chose to collaborate on an entirely new process for propylene oxide production that utilizes hydrogen peroxide to replace the chlorine route (13). Similarly, General Electric and Pratt Whitney are jointly developing the next generation of military jet engine (14). These programs cost hundreds of millions of dollars; the cost of failure is so high that collaboration is the only way some research projects can proceed. Collaboration between customer and supplier (for example, Procter & Gamble's Open Innovation) is another risk mitigation technique (15). The concern associated with such partnerships involves ownership of the value created, so one of the keys to successful R&D management is to have a specific agreement in place that clearly states what each participant contributes to and gains from the collaboration, thus ensuring equitable treatment.

Managing the Fads

Although a key challenge in managing a portfolio is deciding what to work on, sometimes deciding what not to work on is as important. It is human nature to be drawn to a new trend or exciting new field of inquiry. In the midst of these programs to maintain technological currency, there is an important caveat, particularly with respect to R&D risk management: It is critical to manage the fads.

Fads are particularly prevalent when determining customer needs. The successful R&D manager maintains a clear view of the consumer landscape and manages the interest in fads. This requires ongoing conversations with marketing experts to understand consumer trends. The customer, not the researcher, always determines value, so keeping a finger on the pulse of the customer is critical to success. However, relying solely on customers' desires can result in work on incremental problems while missing the industry disruption. This is not a new phenomenon; even Edison experienced it as his company worked to perfect the cylindrical recording device, realizing too late that the flat recording disc was the media consumers favored. More recently, work to perfect the audio quality of the compact disc blinded researchers to the impending decline of physical media distribution and the rise of the digital music revolution. Christensen discusses this well in his book *The Innovator's Dilemma* (16).

Technological and social fads can also impact the direction of an R&D organization. This can be deadly, for the popularity and urgency of the fad can often mask scientific realities. A timely example is the current interest in environmentally sustainable technologies, particularly those that impact the field of energy. The current financial crisis and resource crises have received significant media attention. This could be a catalyst for innovation, as it has created a renewed interest in alternative energy technologies. For example, concerns over petroleum availability and pricing have led to increased interest in alternative fuel sources. But amid the hype, it is important to remember that these fads cannot drive good R&D decisions. Scientific fundamentals always must be the number one force driving decisions regarding investment in new technologies.

As straightforward as this sounds, fundamentals are ignored surprisingly often. Take, for example, the ethanol boom. When the cost of petroleum reached $150 per barrel, the idea of an alternative to gasoline gained wide support. Since 2000, ethanol production in the United States has quadrupled. But there is a fundamental problem with any plant-based energy project: photosynthesis.

Photosynthesis does a relatively poor job of converting solar energy into chemical energy via plant metabolism (as measured by the heat of combustion of dry biomass). In fact, the conversion efficiency of plants is only a small fraction (typically 0.1% to 1%) of the input solar flux, and that includes the energy captured in the roots, leaves, etc. By comparison, solar panels routinely achieve 10% to 20% efficiency. The low energy efficiency of biomass results because plants have evolved to perform one overriding function: reproduction. Although energy conversion efficiency is important, it is not the parameter that nature has chosen to optimize. Thus, any plan to capture the energy created by photosynthesis is fundamentally limited by the poor efficiency of the process.

There are additional losses in the fermentation process. To make ethanol, biological systems reduce oxidized, lower-enthalpy carbon atoms in carbohydrates to ethanol, a process that sacrifices 33% of the carbon atoms by oxidizing them to carbon dioxide. This results in poor carbon efficiency overall. Furthermore, the ethanol yield from U.S. corn is only 330–424 gal $acre^{-1}$ (17). The inefficiencies of photosynthesis, the land and water requirements for production, and the significant capital costs for conversion lead to only one conclusion: Ethanol via corn fermentation is not the long-term solution to U.S. energy needs. However, there was a time when bioderived ethanol was a key element of energy policy and received considerable popular press. Over the past several years, ethanol has enjoyed mandated price supports in the form of subsidies. Without these supports, the profitability of ethanol production never would have satisfied fundamental investment economics. Ultimately, when the price of oil collapsed, so did the price of ethanol. The result: bankruptcies. VeraSun, Aventine, E3Biofuels, Cascade Grain Products . . . these are only a few of the well-known casualties.

The lesson is clear: you cannot cheat thermodynamics, and thermodynamics will ultimately drive economics. The fundamental concepts of energy and mass balances never change. Unfortunately, this is not the only example of an alternative energy strategy that attempts to ignore the laws of thermodynamics that is crossing the desk of many an R&D leader. They are called laws of thermodynamics for a reason. Overlooking the basic truths of the physical world to embrace what is merely a fad is never the solution to a problem and is by definition a poor investment of R&D resources.

R&D Metrics

There is an old management adage that says, "You can't manage what you can't measure." A proper system of metrics is crucial to ensuring that an R&D strategy is producing the desired results. Ultimately, the only real measure of innovation is company earnings. However, there are two challenges in relying solely on earnings. First, many things affect earnings, such as price, volume, and cost of the existing product. Second, as mentioned above, the time lag between investment and earning can be as long as a decade. Few shareholders will accept a "trust me; the earnings are coming" rationale.

Developing a useful set of metrics is particularly important when measuring innovation, because the goal is to bring focus and discipline to what is, at the outset, a creative process. R&D leaders have to remember that existing metrics that have been developed for R&D and new product development do not necessarily offer a complete view of a company's overall innovation capability or health. Some companies that excelled based on historical R&D metrics no longer exist.

In the recent past, several industrial R&D institutions have become famous for their output. Bell Labs, from its very beginnings through the 1980s, was defined as the pinnacle of R&D success. It has generated more than 33,000 patents since 1925. Bell Labs employees won seven Nobel Prizes, nine U.S. National Medals of Science, and seven U.S. National Medals of Technology. By all obvious measures, Bell Labs was achieving R&D success. But this success in research did not promote its overall financial health: A foreign company ultimately bought it. A *Washington Post* story about MIT PhD Lawrence Rabiner illustrates this (18). Rabiner arrived at Bell Labs in 1967. His assignment was to "go to the farthest point somewhere on the frontiers of science and take the next step." Rabiner's work was a success as measured by the research standards of the day: He wrote papers, patents, and books; registered 34 patents; and was elected to the National Academy of Sciences. But Rabiner also said in an interview that he cannot recall anything he did in his first 15 years at Bell Labs that had any impact on the company or on its customers.

Industrial R&D has to exist to create value for society; patents must lead to products that the consumer wants and needs. Discovery for discovery's sake is not a sustainable enterprise, whether in academia or industry. And university researchers have recently embraced the market-driven approach to R&D. In 1986, Richter (19) reported that an estimated 3.3% of all academic scientists consulted for industry or owned their own business. These academic entrepreneurs have an increasingly important role in the technology transfer between universities and industry. For instance, in companies founded on the basis of MIT intellectual property during 1980–1996, nearly one-third of the lead entrepreneurs were the university inventors (20). Recent, unpublished data report that this number rose to 36% in 2007 (R. Fini, N. Lacetera, & S. Shane, unpublished manuscript).

Historically, industrial R&D has used the output metrics shown in **Table 1** to monitor success. But in the current state of the industry, it is not obvious that these metrics actually measure the qualities associated with R&D success. For instance, the quantity R&D spending as a percent of sales is a measure only of how much money is being spent. Although it takes into account whether the R&D budget is in line with the company's profits, it does not consider whether or not R&D efforts are being translated into sales. The ratio of new product sales to R&D is a better measure of research productivity and provides a way to assess research effectiveness.

Measuring new product sales is also deceptive because it is based on the notion that a new product is always better and that people will be willing to pay more for it. But this is not necessarily true. These days many new products do not deliver a better financial return. Green products are a real example. In a 2009 study, Grail Research (21) found that although 65% of consumers consider purchasing green products, the top reason for not considering green products among

Table 1 A comparison between traditional industry metrics and those designed to measure industrial innovation

Historic Metric	Disadvantage
R&D as % of sales	Maximizes spending, not productivity
New product sales	Says nothing about margins
Number of patents	Does not measure whether patents create a competitive advantage
New Metric	**Advantage**
New product sales/R&D as % of sales	Measures productivity
Margin on new products	Determines whether new products are accountable for expanded earnings
Patent advantaged sales	Determines whether patents are protecting sales

the remaining 35% is that they are too expensive. In the same study, 67% of consumers said that green products need to be of the same or higher quality and offered at a comparable or lower price than the conventional product for them to consider making a purchase (21).

One of the authors (Banholzer) personally experienced this scenario when involved in a \$500 million business with 55% of sales coming from products introduced in the past five years. This is a very large number; in most cases 30% new product sales represent a world-class innovation. But sales within this particular business were flat for more than three years with absolutely no margin increase during that time period. The reason was that the new products were just tweaks of existing products. They were not disruptively new and therefore led to no growth in earnings.

Margin on new products is a better measure of innovation because it answers the question "Is the customer willing to pay more for your new product?" To grow earnings, the margin on new products must be higher than the products that they are replacing. If it is not, then the resources used to develop the new product are not going to generate sufficient returns.

Industry cannot sustain its innovation if inventions and innovations are not protected by patents. Once a new product is introduced, competition will attempt to copy it if it is not protected by a patent. Historically, the number of patents was used to signify the strength of an organization's innovation engine. However, the number of patents generated is insufficient to ensure financial success or even the strength of a pipeline. It is possible to obtain a patent on any new idea, but the patent office does not determine whether or not that idea has any value. By extension, having a large number of patents does not ensure that those patents are protecting margins. Instead, it is important to measure the percent of current sales directly protected by patents (what we term "patent advantaged sales"). This is a quantitative measure of product commoditization and is far more indicative of true innovation than simple patent count. It also takes into account the time lag between granting of a patent and the resulting sales.

Many national and international patent indices attempt to determine the strength of a company's patent portfolio. One that Holger Ernst of the Otto Beisheim School of Management recently published has as a component the competitive impact of the portfolio (22). The two determinants of competitive impact, technology relevance (based on citations) and market coverage (based on the actual market size covered by valid patents and pending applications), are synergistic. Great technology combined with large market coverage creates substantial value.

Ensuring Execution

Once the programs and spending allocations are complete, the majority of leadership time is spent ensuring execution. There are considerable literature and numerous forums on tollgate and other processes designed to improve innovation execution. We believe that Bossidy & Charan's 2002 book *Execution: The Discipline of Getting Things Done* (23) is one of the better references. No matter what specific process is implemented, it is critical that cross-functional teams have an operating system to constantly revalidate the value proposition and progress against measurable milestones. To be effective, milestones must reflect results, not effort. For instance, a milestone that reads "Finalize product formulation for antifouling" is more likely to drive results than one that states "Explore possible product formulations for antifouling."

A successful R&D leader understands the time constraints for innovation. In particular, an R&D leader must understand when to champion and provide air cover for a struggling team or project as well as when to discontinue a program. Solving tough problems requires both sufficient tenacity to work through challenges as well as the judgment to avoid excessive spending without tangible results. Experience has proven to the authors that the scientific process is an effective tool to determine when to continue and when to terminate a program. First, a quantified problem statement is developed. Next, a hypothesis describing the solution to that problem is formulated. That hypothesis is tested, the results processed, and if a solution is found, success is claimed. If a solution is not found, another hypothesis is generated, and the process is repeated. This process is repeated as long as necessary to generate a legitimate hypothesis for potential solutions. When no more hypotheses are possible, it is usually time to stop the program. Just because a problem is important doesn't mean that you should work on it if you don't have a solid hypothesis for how you will solve the outstanding issues. The key is to know how long to allow teams or an individual to continue when they are searching for the next hypothesis.

Managing People

People are the most essential element of the innovation process. No innovation process can overcome a lack of talent, and creative, organized, and intelligent people are often successful in spite of the bureaucracy that the innovation process has the potential to create. Thus, any discussion of innovation execution must start and end with people.

In many ways, an R&D team is analogous to a sports team. Invariably the team with the best talent (and the best coach) wins more often than the teams with marginal players. Every leader's first and foremost responsibility is to recruit and develop world-class people. R&D leadership is no exception, whether it is industrial or academic. Few would debate that it is not the quality of the facilities but the quality of the faculty that defines world-class research. Most faculties would agree that the quality of their graduate students and postdoctoral fellows has a profound effect on the amount and quality of research that they can produce.

Effective execution demands regular and detailed assessment of the manpower in an organization. People must be measured against goals; those who excel must be appropriately rewarded, whereas those who miss the mark need appropriate, constructive coaching. There is no place for socialism in a high-performance team. This is not a new concept: Students are graded, premier institutions don't accept every applicant, and not all junior faculty receive tenure. These organizational realities apply equally to industrial and academic institutions. The authors firmly believe that in R&D management, nothing is as important as talent management.

CONCLUSION

Robert Jarvik, the inventor of the artificial heart, said, "Leaders are visionaries with a poorly developed sense of fear and no concept of the odds against them" (24). This is wrong. A more powerful definition is that "leaders are fearless in their vision and are fully aware and respectful of the possible obstacles to success." There is no secret to managing the risk associated with R&D. It is ultimately about choosing the best team, determining the best projects, allocating resources appropriately, and faithfully measuring progress toward well-defined goals. The R&D leader or leadership team that can consistently determine a budget that is in line with the goals of the institution, develop a process to determine which projects will be top priority, pay caution to fads and balance an interest in what is current with a healthy skepticism for what is scientifically unsound, and finally develop a plan for execution that involves putting together a world-class team and supporting and monitoring their progress will promote innovation. This is true for leaders in industrial, university, or government labs.

SUMMARY POINTS

R&D is, by definition, fraught with uncertainty and risk. Successful management of R&D is about managing and mitigating risk. Managing industrial R&D has three key elements:

- Determining how much money you will spend, with an understanding that an appropriate R&D budget that effectively manages risk will be aligned with the goals of the supporting organization;
- Determining what projects your team will take on, which requires prioritization using a robust method and a focus on the most current technologies while managing fads; and
- Managing these projects so that they result in execution, which includes allocating funding and resources, defining metrics, and managing people.

DISCLOSURE STATEMENT

The authors are not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

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