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TECHNOLOGY AND RISK

New Strategies for Industrial Science and Engineering

Those who become the innovative giants of our future will excel through deep technical understanding of the dynamics of technology and the science behind it.

In this era of intense international competition, new strategies for technological innovation are emerging. U.S. industry is leading this movement, stimulated by its huge and growing expenditures on new products and new manufacturing processes—that is, on innovation. An index of innovative ambitions is R&D spending. Since 1980, private industry has displaced the federal government as the primary funder of R&D in the country. Private R&D expenditures are expected to reach some $55 billion in 1985. And industry will perform, if not fund, most of the remaining $55 billion that the federal government and others will spend on R&D.

Indeed, modern R&D puts huge sums at risk. And it has become highly capital-, instrument-, and computer-intensive. President Donald Kennedy of Stanford University has pointed out in a recent *Science* magazine article that much of scientific research is becoming what we have called "big science." From industry's standpoint, R&D has turned into big business, and, like all big business, the potential for waste and inefficiency looms large. Thus modern R&D *demands* management. The old freewheeling style with its wild-eyed inventors, uncritical advocates, and enthusiastic champions is no longer accepted by the modern funders of research.

This turn of events is a mixed blessing. Productive R&D leading to innovation is inconceivable without individual creativity, and creative scientists and engineers inherently crave independence. Managers inherently crave the opposite—well-ordered, focused activity. The answer to this puzzle is for the manager to know when and where to apply hands-on management. That's easier if he or she was a research person before becoming a manager. Such a person would know that in fundamental research for industry, organized chaos is a productive mode. Or as one of my colleagues more eloquently put it—we're after research that is "random in the small, but ordered in the large." On the other hand, in product and process development, a well-planned program to produce a result within economic bounds and schedule is essential.

There is no single, simple approach to managing innovative research and development. Needs vary from industry to industry. Needs vary with rates of scientific and technical progress. But there are some overarching elements. For example, managing industrial innovation means managing risk. Not only the risk of technical or economic failure of R&D projects, but also the risk of not doing what is necessary to stay at the cutting edge of competition. Indeed, one of the most fundamental risks for an industry is being blindsided by revolutionary new technologies.

Another risk involves being too ambitious, butting up against the inherent limits to progress imposed by the natural law upon the technology itself. On the other hand, if we don't know these limits and they are less constraining than commonly thought, there is the risk of missing an opportunity. And there is the risk of creating technology that is not competitive or that is irrelevant to the business strategies of the firm; the risk of creating technology too soon or not soon enough; the risk of creating technology that won't work or that if it does work is not acceptable for environmental or safety reasons, or

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doesn't meet regulatory standards. Finally, there is the risk of failing to create an environment that genuinely fosters the creativity of scientists and engineers.

R&D will never be risk-free. But modern R&D managers are using three grand strategies for getting more innovative bang for the buck: first, through developing more precise understanding of the way progress in science and technology occurs; second, through using this understanding to link the various stages in the innovation process more closely; and, third, through utilizing the resources of basic research more systematically than in the past.

Lessons From the Past

In order to understand today's strategies for managing risk in R&D, it is useful to contrast them with the practices of the past. Of course, the technical activities of today dwarf those of yesteryear. But the past remains highly instructive. Thomas Edison set the first pattern of industrial research a little over 100 years ago when he founded his laboratories in Menlo Park, New Jersey. Of all his achievements, Edison's greatest was the invention of the industrial research laboratory itself. Thereby, he invented a method for invention, actually for innovation in the modern sense of the term, that is, carrying technical activity all the way from the laboratory to the marketplace and the factory.

Part of Edison's success goes back to an experience early in his career when he invented an automatic vote-counting machine that nobody wanted. He vowed never to invent anything again before knowing surely that it would have a market. With one or two minor exceptions, he never did. Edison's laboratories were of course problem-oriented, and they were of course Edisonian—relying heavily upon trial, error, and ingenuity. Though his lab was light on theory, he himself did not ignore it, but used it judiciously, despite what the legends might say.

Edison's example had a profound impact that persisted up to World War II. Yet while industry pursued his path, the universities, including academic science, moved resolutely in another direction. In the first part of the nineteenth century, university professors took all knowledge for their province, as in ancient times. But as knowledge began to pile up, specialization became essential, and that was eventually reflected in academic organization as disciplinary departments. By the end of the nineteenth century, the fully departmentalized university was in place. This evolution led to improved scholarship but also to a divorce between science and its industrial implications, because those tend to be interdisciplinary. The same was less true of engineering. Witness the contributions of MIT's Professor Warren K. Lewis and his colleagues in the late 1930s, which are still a principal basis for petroleum refining. Such events showed that the schism between science and application could be breached.

Indeed, World War II and the years following brought dramatic revelations about the role that science could play in the innovation process. Physics and mathematics proved their power to open up whole new realms of technology: nuclear weapons, nuclear power, radar, computing, jet aircraft, spacecraft, and multiplex communication. These successes provided justification for the doctrine of federally inspired innovation. This doctrine led to the vastly increased role of Washington as a funder of research for government in the universities and of R&D in defense industries. But many civilian industrial purposes were left out, widening the gap between science and basic industry. However, the chemical industry did apply the findings of chemistry *without* the direct help of the federal government: nylon and orlon are but two of the more famous innovations of the time.

In the euphoria of the 1950s and the 1960s, some of the rest of private industry came around to the view that "science is wonderful." Many companies set up laboratories in the ivory tower image, and then sat back, waiting breathlessly for breakthroughs. Within the short attention span of the founders and funders, these laboratories failed to deliver. Disillusionment inevitably set in.

However, the idea of basic research apart from application persisted. In addition, other concepts developed that tended to compartmentalize the innovation process. Terms such as "advanced development," "final development," "product development," "applied science," "design," "design for manufacture," and "commercialization" gave evidence of stage-by-stage thinking about innovation, with each stage largely independent. While such divisions may be organizationally neat, they impede participation by all the required functions in the innovation process. As research departments and development departments grew, so did the difficulties of achieving synergy between them.

The result of all this is that we have reached a situation today where nationally we are paying greater attention to the *connections* between stages and organizations than to the *stages* themselves. An example is the wide effort to establish industry-university relations. But there is much ferment of the same kind within industry itself. So we are coming full circle: from Edisonian wholism to specialized functions and now to reintegration into entrepreneurial or "intrapreneurial" units like Edison's. It is this overall objective that is leading to new strategies for industrial science and engineering.

Understanding How Progress Occurs

Now something about the substance of the new R&D strategies. To begin with, they are exploiting keener understanding both of the way that science and technology interact and the way that technological progress occurs. Any particular technology improves gradually, in evolutionary fashion. Indeed, most industrial scientists dislike the term "breakthrough" for that reason. A breakthrough—for example, the invention of the transistor—is usually accepted broadly as revolutionary only years later. It takes years of gradual improvement and success before people notice that *one* invention marks a critical branching in the tree of technological progress.

There *are* recognizable features at the break point, however. For example, the technology being supplanted—say, the vacuum tube—has usually improved to a point of sharply diminishing returns imposed by the laws of nature. Looking back through history, examples of the same kind are easy to find in the petroleum refining business—such as the swift rise of fluid catalytic cracking technology supplanting thermal cracking, or the transformations that occurred in the reforming technologies that make possible today's unleaded gasolines.

One technique for recognizing this phenomenon early is called learning curve or "S" curve analysis. The curve first traces slow initial improvement in an embryonic technology, then exponential improvement, and, finally, a tapering toward a limit to progress. The trick is to use research guidance studies to discover where your technologies are on the curve and adjust your R&D strategies accordingly.

In this effort, mathematical science can offer important clues. A classic example is Claude Shannon's mathematical formulation of information theory. Using the theory, the system parameters in any electronic communication can be related to one another, providing an upper bound on just how efficient a given system can possibly be. Obviously, if a company finds that a system has *not* approached this bound, it has reason to look ambitiously for ways to approach it. Conversely, if the system *has* approached the upper bound, the company should not persist in trying to improve the system. Instead, the company should seek out entirely different technologies. This sort of analysis helps define the risks of being blindsided by new technologies and the risks of missing opportunities.

Similar analysis is possible for refining and chemical processes. Several years ago, we at Exxon did a thermodynamic analysis of the chemistry of converting coal and steam to methane. The analysis indicated that efficiencies of 80 percent were theoretically possible, compared to the 50 percent efficiencies in existing technologies. Eventually, we developed the Exxon catalytic coal gasificaton process, which boosted these efficiencies to around 65 percent. Similar analysis of technologies for making hydrogen from coal and steam indicated that there was not much room for improvement in the chemistry of existing processes, so we have not put much effort there.

Naturally, when you perform such analysis, you must make sure the mathematics and the science are fundamentally sound. Young electronics engineers are often reminded that in 1924 the AT&T physicist John Carson *proved* mathematically that FM radio was impossible; in fact, Carson later concluded that "noise, like the poor, will always be with us." As *Electronics* magazine has told the story, the flaw in Carson's mathematics was *not* in his calculations; it was in his solving the wrong problem. Carson correctly proved that *narrow*band FM did not eliminate noise. Later, Edwin Armstrong, who trusted his intuition more than Carson's mathematics, invented *wide*band FM and ushered in the day of exchanging bandwidth for signal-to-noise ratio, which eventually led to essentially noiseless pulse-code modulation and digital communication generally.

Ultimately, human judgment rather than rote application of scientific principle must decide where a technology lies on the learning curve. Many other factors should be brought to bear—economic analysis, research experience, study of the rates of progress in competitive technologies, environmental issues, and so on. But industry forgets one fact at its peril; for every Carson, there is an Armstrong. Again and again, companies that stick to improving the old technologies at the top of their "S" curves are clobbered by competitors with new technologies at the bottom of their curves. Thus, not one of the 10 leading manufacturers of radio tubes became a leading manufacturer of transistors. Silicon Valley grew up to take their place. There are multitudes of other examples too painful to recite.

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The Need for Connections

Preparing for dramatically new technologies—just preparing for technological change—is not easy. Companies have difficulty adapting for many reasons. The skills required may be different. The organizational functions required may be different. The culture required to exploit new technology may be different. The markets may be different. The urge to preserve embedded capital is strong. Connections between the stages of innovation are the key to overcoming such barriers. As I have said, each of the steps in the innovation process that can be named—basic research, proof of technical feasibility, development, engineering, design, commercialization—each represents a potential lock on the forward movement of an idea. But what can management do about it?

We can get some ideas by considering the conventional lore that smaller companies are more innovative than large companies. That is so because small companies typically center on the entrepreneur. By definition the entrepreneur is a person who can put it all together—from fundamental discovery or technical concept through production and finance to the marketplace or the factory. By now everyone realizes that this nation is experiencing a renaissance of entrepreneurship. In fact, there has never been a time with more opportunity for people who can put it all together. About 600,000 new businesses are being formed each year. Only about 30,000 are failing each year. New businesses are being formed at *seven* times the level of what we used to consider the roaring 1950s and 1960s. Large corporations are seeking to ride the same wave through "*intra*preneurism."

Central to *entre*preneurship and *intra*preneurship is fostering a common sense of responsibility among researchers and managers. It goes without saying that excellent R&D requires excellent scientists and engineers. For them to work purposefully, the organization must also instill within them a deep sense of mission by example and by precept. At Bell Labs the mission is "to improve electronic communications"; at Exxon Research and Engineering Company it is "to improve the value of hydrocarbon resources." But even with excellent people and a sense of mission, the technology that emerges from R&D will seem irrelevant to the business unless there are tight connections between the R&D organization and the rest of the business.

Most companies recognize that moving people is one of the best ways to move ideas and develop a widely shared sense of responsibility for R&D. At Exxon, we systematically introduce fifth columns and subversive influences—for example, cadres of scientists into our engineering groups and cadres of engineers into our basic research groups. We find that the result is not only mutual understanding of scientific and engineering issues, but a willingness to cross organizational boundaries to get things done.

The shared sense of responsibility immensely facilitates the most important and difficult aspect of R&D management—namely, budgeting. It is in the crucible of debate between affiliates, research companies, and corporate management that companies really answer the questions: "What should we do?" and "How much is enough?" Someone has said that the one virtue of being condemned to hang in the morning is that it concentrates the mind so wonderfully. I can assure you that for the funders of R&D, only the thought of hanging concentrates the mind as wonderfully as the thought of spending money. In other words, the budget process imposes severe discipline, forcing people to put tangible resources against intangible promises.

Essential to R&D budgeting are detailed studies of proposed developments, both early and late in the process, to probe technical limits, economic limits, competitive limits, social and environmental limits, and so on. I won't elaborate on budgeting any further, but you can well believe that connections within industry are profoundly influenced by the courses through which money flows to R&D.

New Importance of Basic Research

You can devise a strategy based on the *top* of the technological "S" curve and the natural limits to progress for a particular technology. You can devise a strategy that takes into account the dangers of being blindsided. You can devise a strategy for ensuring that your R&D people are excellent, have a sense of mission, and are so connected in all manner of means with the rest of the organization that they both respond to and influence the corporate objectives. But what about the task of finding the *bottom* of the "S" curve, the mother-lode technologies with a rich future?

In an intensely competitive world, the major strategy for overcoming the risks of missed opportunity is fundamental science and mathematics. However, I am speaking about fundamental science linked strongly to the purposes of industry. I am talking about fundamental science conducted by industry, or by industry consortia, or through industry-university connections in research.

This strategy is not a reprise of the 1960s. The organizational approaches are different, with the federal government in an important but background role. Still, the process that began with the application of new physics, mathematics, and chemistry during World War II is reaching into the rest of industry, despite the large doses of empiricism and handbook engineering that still play a vital role in much of industry. The reason is that with the development of advanced instruments and computing, science can characterize and manipulate vastly more complex "Messier" systems than in the past. This is a lucky development, because modern technology has grown so complex that creating or improving it without fundamental understanding has become more and more risky.

Consider the sweeping challenges posed by computer optimization. As you know, the petroleum and petrochemicals industry blazed the path that the hardgoods manufacturing industries are now following in computer process control. For the refining industry, the next step in computer control is to begin developing methods for optimizing online not just a single processing unit, but groups of units, entire refineries, and even groups of refineries. But as we attempt these tasks, our difficulties are compounded and highlighted again and again by our incomplete knowledge of our processes. Petroleum refining processes incorporate complex, messy phenomena that in many cases are just now becoming amenable to fundamental study. Some of the issues include the kinetics of main reaction pathways, mass transfer effects, and operability phenomena like coking, foaming, fouling, and agglomeration.

Similar things are true about the task of scaling up processes from the laboratory bench to commercial units. With more fundamental understanding, it is already proving feasible to limit the number of steps in scaleup. This gives us hope that eventually we may eliminate the multi-million-dollar costs associated with building large-scale pilot plants. The ideal, of course, would be to use computer simulation to devise, develop, and prove out alternative processes without connecting two pipes together. Obviously, the first company that puts up a billion-dollar plant on the basis of a computer simulation will be very brave indeed. It may also be hundreds of millions of dollars richer—or poorer!

What kinds of things has fundamental research done for Exxon's businesses up to now? I could list some specifics such as multimetallic heterogeneous catalysts. In 1979, one of our scientists, John Sinfelt, was awarded the National Medal of Science for his work in this area. However, the real task of our fundamental research is to find the bottom of the "S" curve—to open, find, and bring in-house entirely new areas of technology that can provide multiple applications. We are doing substantial amounts of this generic work—for example, on associating polymers, metallic clusters, quasi-crystals, new chemical reactors which improve solid-gas interactions, fractals, and new principles of catalysis. We are participating, too, in bioengineering, including recombinant DNA techniques. Any industry that is not undertaking such initiatives is at peril.

But the past is mere prologue to what basic research can help attain for the petroleum industry. I have already mentioned the radical challenge to optimize whole refineries and to eliminate pilot plants through computer simulation. There is also the radical challenge to learn enough about catalysis that we can remove it from the empirical realm, where it now largely is, and take it to the realm of predictive science and engineering discipline.

There is the challenge to develop new on-line sensors and analyzers to the point that on-line compositional and physical data will permit interactive modeling of processes. There is the challenge to learn enough about the structure and chemistry of coal, oil shale, and other heavy hydrocarbon materials that we can take them apart and reassemble them at costs no greater than for bringing existing petroleum fuels to market. And in the area of petroleum exploration, there is the challenge to learn enough about satellite scanning and seismic wave transmission that we can image the interior of the earth. All of these, and other possibilities we haven't thought of, are likely to become reality.

Conclusions

To sum up—the R&D strategies of yesteryear have gone the way of many good things. Small was beautiful. But with the strategies evolving now, big need not be bad. Big will *always* be risky. Today's multi-billion-dollar R&D *demands* careful, inspired, farsighted leadership. The nation has been blessed with many great models—including Thomas Alva Edison, Vannevar Bush, and the man whose name honors our proceedings today, Arthur M. Bueche.

Those who become the innovative giants of our future will excel through deep technical understanding of the dynamics of technology and the science behind it. Our future Edisons will have a genius for integrating the parts, for overcoming the institutional and cultural barriers that spring up wherever people coalesce in divisions and departments. They will be Renaissance men. They will know how to utilize the nation's supreme abilities in basic research. They will know how to bring the universities into the effort. They will know how to enlist the support of governments.

Where will we find such leaders? How will we recognize them in their youth and educate them for such a demanding role in their maturity? In closing, let me leave that challenge to our educators here and to our great educational institutions, including MIT. For it is only on the shoulders of knowledge and wisdom that true leadership can stand.

Ultimately, human judgment rather than rote application of scientific principle must decide where a technology lies on the learning curve.