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INSTITUTE FOR DEFENSE ANALYSES

Science and Technology in Development Environments – Industry and Department of Defense Case Studies

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Christopher Baker
Robert Bovey
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PREFACE

This paper for the Missile Defense Agency is in response to a task titled "Strategies and Methods for Successful Programming, Development, and Exploitation of Focused Missile Defense Technology."

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INDUSTRY CASE STUDIES

I. GE: 1900–2000

Robert Bovey and Christopher Baker[1]

A. INTRODUCTION

GE, formerly Edison General Electric Company and General Electric, has survived for over a hundred years by not only supporting research and development (R&D) activities but also leading the field consistently. After patent purchasing and outsourcing research in the late 19th century, the company began looking for people who could produce leading-edge knowledge to dominate the market rather than secondhand information good enough to tag along. Willis R. Whitney's arrival in 1900 signified a new era both for GE and business in general, one that gave more attention to scientific pursuits in search of a market application and opened up a niche for the true industrial scientist. After that time GE was the greatest technological innovator of the 20th century—based on the over 50,000 patents the company received in that century.

This paper first traces GE's history. Then it describes recent GE overall management from a science and technology (S&T) perspective and GE's management of S&T. It presents a company that is tough and demanding, an employer that insists on training its best people into an uncompromising culture of expectation of success to predefined goals.

To provide more detailed and specific focus, this paper emphasizes one GE strategic business unit, GE Medical Systems (GEMS).

The GEMS experience provides the following lessons concerning S&T management:

- Executives focused on the short term, struggling to stay on schedule and under budget, will shortchange S&T until disaster looms.
- Centrally managed research that looks beyond those needs currently recognized in the business unit or project office is essential to creating the foundation for long-term success.

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[1] The authors wish to thank Peter Cannon and Lonnie Edelheit for their inputs to and review of this chapter.

- Top executives must protect long-term S&T until such time as the business-unit managers or project officers recognize that they face disaster unless they adopt new technology quickly.

1. The Business Model

In the early 1960s, a major price-fixing scandal sent senior GE managers to prison. Much of the remainder of the decade was spent in rebuilding the idea of the firm. In 1970 the corporate structure was drastically revamped to hedge market risk through an industry-diverse group of smaller elements. These strategic business units (SBUs), over 40 in total, produced many more profit centers and operated almost separately from the central hierarchy. The internal fracture into groups consisting of several SBUs was intended to provide a start-up or small-business feel to units that also could draw upon the resources of a large corporation, mixing the best of both worlds. Funds for expansion of successful SBUs were derived from the sale of "unsuccessful" ones (see Welch principle 2 below). At about the same time,[2] GE Capital and the entertainment content businesses were expanded very rapidly.[3]

Jack Welch headed one group of SBUs from 1973–1981. He then became chief executive officer (CEO) of GE and remained CEO for two decades of its history. His 10 basic principles of management, outlined below, constituted to a large extent the GE business model in recent times. Many of them represent long-standing GE corporate management principles.

1. *Invest in People*

   Talking to them and meeting them, as well as developing people for the future.

2. *Dominate Your Market…or Get Out*

   There is no time for companies that are fourth or fifth in their market. If you can't get to the front or a close second, sell the business and look elsewhere.

3. *Never Sit Still*

   The company won't stay still or rest on its laurels.

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2 Stuart Crainer, *Business the Jack Welch Way: Ten Secrets of the World's Greatest Turnaround King,* Amacom New Media, New York, 1999.

3 Discussion, Cannon.

4. *Think Service*

   GE was a manufacturer. Now GE is a service company that also manufactures, a finance company, an information company, as well as a maker of appliances. Quality and service link its activities.

5. *Forget the Past; Love the Future*

   For a company with such a great history, GE is preoccupied with the future.

6. *Learn and Lead*

   The new model leader is not a corporate dictator. The leader is committed to learning, deciding, and moving forward.

7. *No Bull*

   Jack Welch communicates directly. Whether he is talking to workers in a GE factory, managers on a training program, or industry analysts, he speaks with passionate clarity.

8. *Kill Bureaucracy*

   Jack Welch nearly left GE after his first year due to the time-wasting of bureaucracy and hierarchy. Since taking over at the top, he has eradicated bureaucracy with a vengeance.

9. *Stick Around*

   The corporate person is supposedly dead. But Welch has excelled sticking with a single employer.

10. *Manage the Corner Store*

    GE needs to be managed like a corner store; that you are selling nuclear power plants and not candy bars is immaterial.

In sum, GE's business strategy is aggressive. The entire corporation, and R&D in particular, is very offensive minded. Key factors in this approach are taking advantage of size to take risks and never letting bulk become a burden. By market diversification and organization into divisions, GE accomplishes the feat of balancing interests and steering clear of bureaucracy. No matter what the market, GE goes all out, never short-changing the project in fear of a too-small market.[4]

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[4] Discussion, Cannon.

The GE 2001 Annual Report emphasized this approach when it asked rhetorically, "Can such a thing as a $126 billion growth company exist?" It answered,

> It does exist because GE always plays offense. We don't run this Company as a "$126 billion blob…." We run it as an $8.4 billion Medical Systems business…a $1 billion Ultrasound business within it…and as seven separate operations within Ultrasound, ranging in size between $50 million and $250 million. These operations are run by people who are obsessed with growth and achieve it by creating new markets and technology. Backing them are our systems, our initiatives, and a strong balance sheet that allows them to take risks for growth, knowing that the occasional miss or failure is not only unpunished, but is also "no big deal" in the context of a $126 billion company.

2. Why GE Does S&T

In 1900 Willis R. Whitney joined GE to pioneer the use of research scientists by industry. He employed them as such, not just to be inventors, consultants, or engineers, but making "a place where scientists can contribute to the advance of science while they put science to work." He was implementing a philosophy articulated by economist Edwin Mansfield that "industrial innovation, based largely on the work of research and development laboratories, pays both an attractive rate of profit to industry and an even more attractive 'social rate of return' to society as a whole."[5] By producing relevant technologies that conformed to business strategies, there existed a possibility of a large return rate and security/survival in the future for the companies and their laboratories.

This philosophy prevailed in GE, but the role of centrally controlled and executed R&D came under challenge in many parts of U.S. industry in the 1980s. This R&D function had to ensure its role was pivotal to corporate strategy if it was to remain on the company payroll, notably in GE. Walt Robb and Lonnie Edelheit executed the wrenching changes that ensured the survival of GE Corporate Research and Development Center (CRD), albeit in a different form, as described in the CRD mission statement presented earlier.

S&T remained largely in the fraction (about a quarter) of CRD funding received from corporate, radically reduced from what it was before 1988. Also, the focus of the reduced level of S&T conducted in the 1990s shifted toward being business driven rather than technology driven. At the beginning of the 21st century, the pendulum began

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[5] George Wise, *Willis R. Whitney, General Electric, and the Origins of U.S. Industrial Research,* Columbia University Press, New York, 1985, p. 315.

swinging back under Senior Vice President for Global Research, Scott C. Donnelly; CRD became more willing to look for a "game changer" for a whole new business.[6]

In the end, GE continued to do S&T because, after a vigorous challenge, top management was persuaded that GE could not afford to abandon it. On the one hand, some weight must have been given to arguments like Walt Robb's examples of entire markets that would have been foregone without CRD's efforts, and his calculations of the average net present values of all the transitions CRD made over a period of years. On the other hand, vigorous efforts to be useful day-to-day—"vital" in Lonnie Edelheit's 1998 description—must have taken most of the wind out of the challengers' sails. Therefore, GE continued to do a very different kind of S&T, much more constrained both in size and in technical reach. As noted above, there were signs in 2002 of a shift toward more emphasis on seeking radical innovations.

3. History of S&T at GE

The Edison General Electric Company was the result of combining multiple organizations in 1882. Edison himself was greatly interested in scientific advance, but only if it had commercial application. "We can't be like those German professors who, as long as they can get their black bread and beer, are content to spend their whole lives studying the fuzz on a bee," he said, adding, "We've got to keep coming up with something useful."[7]

When Edison left the company in the late 1880s almost no purely scientific exploration went on in the laboratories, and any that did was for economic, not scientific, gain. The research facilities and even commercial progress dwindled as no laboratory equal to Edison's Menlo Park existed any longer. Eventually competitors such as Westinghouse bought up patents and consulting from independent sources, forcing the company to take the same route at a heavy cost in order to stay afloat.

Edison General Electric and Thompson Electric merged in 1892 to become the General Electric Company, within which a centralized organization was established to direct technology and other important assets. The merger involved Elihu Thompson, a brilliant and thorough inventor—Edison's equal—who had his own labs in Massachusetts, and who, with Boston financial and legal interests, drove the merger.

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6 Discussion, Edelheit.

7 Wise, *Willis R. Whitney,* p. 68.

They created the dominant company with the aid of Charles A. Coffin, a gifted intellectual property manager. The Coffin interests wound up with a private percentage holding of the merged firm; these family associations still exist.

The managers of the 1892 merger process were of the period in U.S. capitalism where monopoly was actively sought. These monopoly capitalists were acutely aware of the need to manage intellectual property from the outset. Among the tools they used were interlocking agreements for technology licensing in exchange for ownership, along with vigorous defense of patents. For more than 100 years, these arrangements survived many court challenges, although not always. For example, through their finance people, General Electric created (an illegal) dependency of the power-generation industry on the supplier; this was broken up by the 1920s. The radio business and its patent pool were broken by 1930.

The managers who solidified the 1892 merger also were acutely aware of the need to justify monopoly through invention. Also, because outsourced patents did not support the monopoly strategy well and were expensive, there was a need for internal research to develop "commercial applications of new principles, and even for the discovery of those principles."[8] This explains, at least in part, why they provided for a laboratory for industrial research. It was probably the Coffin interests who insisted on the formation of a central laboratory and the hiring of Willis R. Whitney to lead it.

By the time of the merger, Edison's company was heavily committed to Schenectady, New York. The new laboratory was embedded in the Schenectady works, where it remained until the early 1950s, when the company and the U.S. Atomic Energy Commission developed a site at the Knolls, just outside the city, for the Knolls Atomic Power Laboratory (KAPL) and for the corporate research laboratory.

The company hired Whitney from the Massachusetts Institute of Technology (MIT) to build a facility similar to what became Bell Telephone Laboratories. In 1900 Whitney joined General Electric to pioneer the use of scientists by industry. General Electric employed scientists as scientists, and it was this policy that revolutionized the business world, allowing the pursuits of pure research and product development to intersect. Thomas Edison's dedication to invention in years past had cleared a path, but his way was heavily weighted toward economics rather than erudition.

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8 Ibid., p. 77.

Whitney demanded that the research include a variety of topics not limited to those with a commercial future. He believed that "the professional scientist can find a place in industry and remain a scientist," which became an important development in diversifying research labs around the country: "American science was becoming a compound of mutually supporting activities in pure and applied research."[9] The laboratory in Schenectady was a buzz of academic and product discovery from that point forward.

A great compromise had now been achieved to form "a place where scientists can contribute to the advance of science while they put science to work." Whitney and successors Coolidge, Langmuir, and W.R.G. Baker established the pattern of science applications that led to the company's ability to manage high vacuum technique, thus entering the rectifier "valve" and electronic tube and radio businesses.

After the late 19th century's patent purchasing and outsourcing, the company determined to lead rather than acquiring secondhand information good enough to tag along. Willis R. Whitney's arrival in 1900 signified a new era both for the company and business in general, one that gave more attention to scientific pursuits in search of a market application and opened up a niche for the true industrial scientist.

General Electric used its intellectual property and finances to hold controlling interests in worldwide related businesses (Osram, Toshiba, CFTH, etc.). After World War II, these interests were sold and the firm, which had become somewhat static, grew its R&D rapidly and diversified into consumer products, chemicals, and industrial materials, using intellectual property from the Schenectady laboratory. Examples include dominance in hermetic systems chemistry, thus ensuring reliability in refrigeration and air conditioning; the successful introduction of a series of plastics of increasing high-temperature utility; and the introduction of silicone polymers into commerce. The diversified company entered the machine tool control and supplies businesses using technology first developed at MIT and market and financial muscle to overtake the innovators. This opportunity arose from the need to machine compound curves for the USAF Century series of fighters and fighter-bombers. GE's size, scope, and reputation as a vendor were key aspects of its becoming a defense supplier, much as Bell Labs was invited into the missile business. Similarly, GE was invited by the U.S. Government to become a major player in nuclear power based on reputation and size.

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9 Ibid, p. 94.

In summary, since its formation, GE has been the greatest technological innovator of the 20th century—based on the over 50,000 patents the company received in that century. In 2001, GE received nearly 1,200 patents.[10] The model Whitney established in 1900 survives to a significant degree to the present. In the 1960s amid the post-price-fixing-scandal trauma, an attempt was made to minimize the role of the central corporate laboratory, including breakup or closure. It was clear that the corporate status was at risk; only the efforts of the managers within the laboratory to produce useful results diverted the company from these destructive acts. A second assault occurred in the 1980s; the resulting changes will be discussed in a subsequent section.

B. GE MEDICAL SYSTEMS (GEMS)

The history of GEMS from the early 1970s to the late 1990s is especially relevant for three reasons. It is an organization in a fairly specific line of business, which is defined to a large degree by technology, making it similar at the mission level to long-running public sector programs. Its interactions with CRD provide an excellent case study of the successful interaction of a corporate research laboratory and a business unit. Finally, this history illustrates the critical role played by business unit, laboratory, and senior corporate executives in driving innovation.

The roots of GEMS[11] can be traced back to the early days of General Electric producing X-ray equipment and film. In 1974 it was the leading domestic producer of X-ray equipment in the United States, but its market share was falling and its size was insignificant by GE standards.

In the late 1960s, GEMS tried to diversify into heart monitors, pacemakers, blood analyzers, etc., but failed. By the early 1970s, the profitability of its X-ray line was falling. In late 1973 Jack Welch became vice president of the GE Components and Materials Group. He quickly brought in Walter Robb (who had started at KAPL and was then general manager of GE Silicones) to turnaround GEMS. His plan was to focus on and revitalize the core X-ray business.

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10 GE 2001 Annual Report

11 The GEMS pre-1990 story is based on Joseph Morone, *Winning in High-Tech Markets: The Role of General Management*, Harvard Business School Press, Boston, 1993. The post-1990 story is based on Richard Leifer, Christopher M. McDermott, Gina Colarelli O'Connon, Lois S. Peters, Mark Rice, and Robert W. Veryzer, *Radical Innovation: How Mature Companies Can Outsmart Upstarts*, Harvard Business School Press, 2000.

In 1971, EMI, the UK company that pioneered computerized tomography (CT), proposed that GEMS be its North American distributor. Although GEMS was then still trying to diversify, it declined because it estimated the demand for CT units to be no more than 50 units over the next 5 years. By the time Robb was restructuring it became clear that EMI sales were taking off and were eating into X-ray equipment sales.

Also, two groups in GE were becoming concerned about GEMS ignoring CT. One was the executive board of Welch's Components and Materials Group. The other was CRD. Because a study had identified medical technology as an important growth area, CRD increased the share of its resources devoted to this area from 3 percent in 1968 to 7 percent in 1973. CRD began pushing CT.

By early 1974 GEMS knew it had to respond, but in the fall it still had no plan. The in-house engineering team proposed entering the CT market with a copy of the EMI machine, depending on GEMS superior marketing and service force to compete. Robb, however, overruled the team in favor of going for fundamentally different and better technology.

Roland Reddington of CRD led this effort. By June 1974 CRD had identified as the candidate a "fan" beam approach that promised faster scans than EMI's "pencil" beam technology. However, it was just a concept; to work it needed both a bank of 160+ stable and uniformly accurate detectors and a new mathematical method to build the CT picture. At about that time NIH issued a Request for Proposal (RFP) for whole-body CT scanning, to which GEMS, CRD, and the Mayo Clinic responded as a team in hope of getting substantial development funds. The proposal was not accepted. GEMS had still not funded any CT work; CRD was using its own funds.

At the beginning of 1975, a consultant from SUNY Buffalo solved the mathematical problem, and CRD found a detector invented by a small company that would be stable and reliable enough. In January 1975 Robb authorized and funded a GEMS–CRD crash program to develop a CT breast scanner for October delivery to Mayo for clinical evaluation. As the year progressed, it became obvious that the CT market was growing even more rapidly and competitors were moving faster into whole body scanning than expected in 1974.

With support from Welch, by June 1975 GE had a three-pronged (head, breast, and whole body) development program in place. The head scanner was to be licensed from a company, Neuroscan. The other two were to be developed in-house. The parallel programs seemed well on their way for some months. However, in early 1976 Mayo

Clinic trials showed the breast scanner to be no better than conventional mammography. And in the trials at the University of California, San Francisco, that began in April, the whole body scanner was faster than competitor systems, but it was producing spurious rings on its images and was inferior to competitors' systems for head scanning. A body of professionals held that the rings were intrinsic to the fan beam approach.

By November 1976 GE had figured out a way in software to take out the rings and began to ship its first fast body scanner, the CT 7800. Although it was faster than pencil beam machines, it was just equal to them in image quality. At the same time the federal government tightened enforcement of a law requiring hospitals receiving Medicare/Medicaid to get a Department of Health and Human Services (HHS) certificate of need before buying equipment costing more than $100,000. This occurred just as a flurry of new competitors was entering the market, together inducing a collapse of the CT machine market in 1977 and 1978.

At Welch's urging, Robb assembled a panel of outside experts to assess competing technical approaches to CT. In the meantime, GEMS had been working on increasing the number of detectors in the same length fan from 301 to 523. The experts concluded that if this produced as much improvement as GEMS hoped, GE should continue down this path. By mid 1977 the new array was producing dramatically better images and became the basis of the CT 8800, which was announced in 1977 and first shipped in early 1978.

To address the problem of cannibalizing the 7800 market, Welch conceived of a new policy, called "continuum," by which customers could upgrade their existing system to the next generation. As the first 8800 machines were being shipped, enhancements were announced. Incremental improvements continued for several years, which had the net effect of maintaining the CT 8800 as the "gold standard" of CT machines.

In 1979 GEMS began to develop a scanner that would be twice as fast as the 8800 with better resolution; the CT 9800 was introduced in 1981 and gradually replaced the 8800 over the 1980s.

When Robb took over in 1973, he exited a number of small unprofitable medical equipment businesses and defined GEMS as an X-ray manufacturer. By the late 1970s, with the unexpected growth of CT, Robb and Welch viewed GEMS as a diagnostic-imaging-equipment business. X-ray machines still were the largest fraction of sales, but new diagnostic imaging technologies were expected to become a more substantial fraction in the future. From this altered perspective, the leaders were actively looking for

ways to replicate the CT success. Every new diagnostic imaging technology became a new product opportunity. Nuclear and ultrasound imaging eventually grew into successful business lines, mostly using technologies developed by partners.

In the early 1980s digital X-ray development was given as high a priority as continued development of CT. However, the technology development was disappointing. Robb was quoted as saying that in retrospect, the technology simply was not available at the time to leapfrog conventional X ray, but GEMS' judgment was unduly influenced because management wanted so much for the breakthrough technology to be there.

In parallel, starting in 1979, Reddington's CRD group began pushing nuclear magnetic resonance (MR) as an important new technology for both imaging and spectroscopic chemical analyses of the body. GEMS rejected the CRD proposal to develop an MR imaging system; it refused to give MR serious consideration. Digital X ray and advanced CT were its priorities. Therefore, CRD began to pursue on its own the other application, spectroscopy. As part of this, in mid-1980 CRD ordered a superconducting magnet three times more powerful than the most powerful ones thought to be practical for imaging.

By mid-1981 the MR imaging market was growing much faster than GEMS had anticipated, and many firms were developing MR systems, some of which were producing better images than GEMS had expected. MR imaging was beginning to be a near-term threat to CT. GEMS had no choice but to move into the MR business. By early 1982, GEMS began developing an MR imaging system using the basic approach that CRD had proposed in 1979, but as a crash program because it was a year behind the competition. However, by 1982 the approach no longer offered technological leadership. It turned out that the chance for this leadership lay in CRD's spectroscopy work.

About this time the superconducting magnet, which CRD had ordered earlier, arrived. At the time no one thought it was possible to image at the high field strength it would create, but Reddington's CRD group tried anyway. They obtained images that were dramatically better than anything done at lower field strengths. This provided the basis of technical differentiation. GE announced its high field system in late 1983 and shipped its first machine a year later. The MR analog of the earlier CT market collapse occurred in 1984, and for similar reasons—overcrowding and government actions—both of which were not expected by GEMS. Supported by or even pushed by Welch, GEMS continued to spend heavily on MR product development, largely at the expense of CT. MR finally became profitable in 1988 as the overall MR market recovered. By the end of

the 1980s, GEMS had the dominant MR system in the market, having evolved in much the same way as the CT scanners with continuous upgrades and the continuum approach.

In 1986 Walt Robb left GEMS and replaced Roland Schmitt as the head of GE CRD. In 1992 Lonnie Edelheit, who had been a member of the GEMS CT team in the 1970s, replaced Robb as head of CRD. In the meantime, in 1981, Jack Welch became chief executive officer of GE and began a revolution epitomized by the principles stated earlier.

Digital X-ray research continued at a reduced level as MR was being taken to market and its market share built. Of course, the relevant technology was growing at the same time elsewhere. In the late 1980s, Jack Kinsley, the leader of a group of CRD scientists working on aerospace displays, proposed to GEMS that the technology could be applied to medical imaging. The idea was rejected. Four years later Kinsley's boss, Bruce Griffing, became convinced of the importance and began to push it vigorously. He persuaded GEMS to participate in and fund a research project on digital X ray utilizing the aerospace display technology.

However, under pressure to increase short-term financial results, GEMS withdrew its support in 1993, although the technical feasibility of a digital X-ray was pretty clear. To build and test a prototype, Griffing convinced Lonnie Edelheit, who in turn got Jack Welch's agreement to support the digital X-ray project. Griffing also repackaged the project to obtain additional funding from the Defense Advanced Research Projects Agency (DARPA) and the National Cancer Institute. He overcame a number of obstacles, including the failure of a supplier to make a critical component by setting up a small-scale manufacturing facility in CRD. Finally, the CRD team successfully tested with leading customers' prototypes that were jury-rigged systems with the functionality but not the appearance of the final system. Still GEMS remained uninterested.

In 1997, however, there was a change in leadership in GEMS. The GEMS chief executive assessed the digital X-ray project and became a strong proponent. Transition from CRD to GEMS was effected officially, but CRD continued to support the project with 50 people.

In early 2000 GEMS introduced both the Innova 2000, the first digital X-ray cardiovascular system, and the Senographe 2000D, the first FDA-approved digital mammography system. Some 400 product engineers in GEMS were assigned to digital X-ray systems, and CRD returned its focus to next-generation features and new applications.

GEMS is properly held up as a shining example of an organization that repeatedly introduced radical new technology to achieve its mission, being the world leader in medical imaging. Yet in each specific case reviewed here, the business unit resisted the new technology. Why was this the case? According to one observer, Morone, GEMS' resistance to CT and then MR was in part because they were distractions from other demanding business work. But the problem went further. Developing new product lines demanded disproportionate resources. CT development cut into X-ray development. Early digital X ray cut into CT incremental development. MR cut deeply into both. Later, digital X ray was seen as threatening overall financial performance of the division. Also, each new product threatened to cannibalize sales of older products. The reasons a business unit executive will resist new technology likely also are present in a public-sector project officer and system integrator prime contractor.

Morone also observed that the strategic consistency that gives direction to technology development goes hand in hand with managerial stability. Welch as VP and CEO, Robb as head of GEMS and CRD, and Edelheit as a senior GEMS manager and head of CRD are examples.

1. GE Corporate Research and Development Center (CRD), 1988–1997

By the late 1980s the ascendancy of financial "engineering" was clear in U.S. business. It was accompanied by an obsession with near-term financial performance measures and their effects on the stock prices of publicly traded companies. This was the era of corporate raiders and leveraged buyouts in which any component of a company that could not demonstrate a direct connection to "the bottom line" was at risk. The literature was filled with proposals for the financial measurement of industrial research.

Thus, criticism mounted throughout industry of the then-prevalent model of central research, Whitney's model. Many line managers in manufacturing and sales viewed the corporate laboratory as a luxury for which their operations were being taxed. Financial engineers saw R&D as an almost pure near-term cost-reduction opportunity with little or no downside; their financial models discounted future revenues and largely ignored strategic issues. GE was no exception.[12]

Embattled GE research managers sought to make the case for corporate-level research in a variety of ways. For example, in 1991 Walter Robb, as head of GE CRD,

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[12] The material on metrics, addressing the period 1987-1991, is drawn from Walter L. Robb, "How Good is Our Research?" *Research·Technology Management*, March/April 1991, p. 102

spoke from a traditional industrial-research perspective in describing CRD's mission. He said CRD had a dual emphasis—to increase the freedom of researchers to do exploratory research and produce new ideas while also effectively marketing R&D to the company's businesses as the source of unique, strategically important forefront technology, both to meet needs the businesses recognized and other needs beyond their current scope. Robb then presented three arguments that CRD had done well by GE:

- First, he used counterfactual thinking: Where would GE be if not for a technology developed in CRD? He argued that GEMS, GE Plastics, and GE Lighting were businesses that had recently been created or re-created by research.
- Second, Robb argued that some things could be observed that indicated how good CRD's research was. He included licensing income, royalty payments avoided, and number of patents as useful measures. In addition to gross counting of patents, he argued for counting various subsets, cost per patent in terms of patents per million dollars of own-funds R&D spending, and other more esoteric measures.
- Third, Robb argued that a rigorously conducted discounted-cash-flow analysis of CRD-created technologies transitioned to business units showed an overall 20-percent rate of return for 190 transitions in the 1982–1987 period.

Finally, Robb said in effect that none of the above had carried the day within GE. GE had changed the mechanism for funding CRD in 1988. Before 1988, one-third of CRD funding came from "contracts" with GE businesses and external agencies and two-thirds came from "assessed funds," which was money assessed by GE corporate headquarters from GE businesses according to a formula that took the sales, profitability, and technology intensiveness of the businesses into account. After 1988, three-fourths of annual CRD funding came from "contracts" and one-fourth was "assessed," the latter ticketed for exploratory work above and beyond the identified needs of GE's current businesses. Robb described the post-1988 formula as both a funding system and a measurement process, the latter because the willingness of business units to contract for research was a measure of their satisfaction. He admitted that the system did not provide a way to measure the value of the exploratory research carried out with assessed funds. He recognized that finding a way was a major challenge that would eventually be met with measurements that would complement the ones described above.

In sum, while defending the old ways, Robb announced that a sea change had taken place in GE corporate R&D.

Seven years after Robb publicly revealed the sea change in GE CRD, his successor, Lonnie Edelheit, described that change in greater detail.[13] Edelheit characterized the change as a return to Thomas Edison's principles, noting that Edison emphasized R&D as being vital to businesses and that he sought global reach, high quality, and growth through serving customers, not mere technical versatility. Although he did not say so, Edelheit could have added that Edison's principles were antithetical to Whitney's, which had formed GE CRD from 1900 until the late 1980s.

Nonetheless, what Edelheit described was a wrenching change. In general, the challenge of the 1980s was to make CRD more vital to the GE businesses, and Edelheit said that CRD had made changes to respond to business needs faster, resulting in lower cost products and services while maintaining high performance. Just how great the change was became clear as he described how it spanned the way of funding R&D, CRD's mission, and its strategy. The following sections touch on each of these areas.

2. Funding

Edelheit expanded on Robb's report of the change in funding from the pre-1988 situation in which two-thirds of CRD funding was "assessed funding" from GE CEO. He said that by 1997, one-fourth of CRD funding was assessed, one-fourth came from external sources, and one-half came from contracts with GE businesses. The inclusion of external funding and the reduction in funding by GE businesses from three-fourths to one-half suggested that CRD had shifted its priorities. Edelheit described the 25 percent assessed as enabling high-risk work in areas leading to future growth, as had Robb 7 years earlier.

With respect to the half of CRD funding coming from GE businesses, Edelheit observed that a GE business was free to acquire technology anywhere—internally, from a university, national laboratory, or competitor, or from CRD. The only limitation on GE businesses was that they could not cut CRD funding more than 20 percent in any one year. This CRD funding was allocated through an "objectives process." Working with key customers in businesses, CRD identified 100+ key objectives at the beginning of year. These objectives formed the basis for funding. Progress toward meeting them was then monitored regularly on a red-, yellow-, green-light basis. Finally, to close the loop, CRD polled its customers at the end of a year on whether its achievements toward the key

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[13] The material that follows in this section is from Lewis (Lonnie) Edelheit, "GE's R&D Strategy: Be Vital," *Research·Technology Management*, March/April 1998, p. 234.

objectives met, exceeded, or failed to meet their expectations. Edelheit emphasized that the key metric was not scientific or technical. It was GE businesses' grades on how CRD was serving them, by their ratings and by their funding. Edelheit thought that this was better than a portfolio approach in which CRD would assemble a portfolio of programs and try to sell them to businesses.

3. Mission

Edelheit observed that into the 1980s, CRD researchers identified mainly with technology peers in other research labs and spoke the language of technology, but in the 1990s, CRD was aligned more closely with values and language of GE as a whole. He then discussed the CRD mission statement areas in some detail to expand on this basic point.

1. *Teaming:* As Edelheit described it, the first component of CRD's new mission was to team with the businesses on multiple generations of products and processes. In contrast to the old sequence of separate and distinct research, engineering, manufacturing, and marketing, all these functional groups had begun to work as a team from the beginning. At the beginning of projects the teams started work on future as well as initial product generations. The GE 90 engine and the H class gas turbine were cited as examples of this sort of teaming.
2. *Game-changers:* As part of multigenerational approach, Edelheit said that teams actively looked for new technology "platforms" (i.e., the basis for entirely new products and processes). This was the second part of the CRD mission—to create "game changing" technologies that would open opportunities for major new products and services. He gave the CRD-GEMS work on digital X ray as a current example of this component of the CRD mission.
3. *Problem solving:* The third part of the CRD mission was to help solve GE's critical technical challenges. Edelheit reported that CRD specialists often worked directly with customers in fulfilling this part of CRD's mission, a major departure from the traditional view of CRD being populated by "bench scientists." As a then-current example of this kind of work, Edelheit pointed to ongoing efforts to reduce gas turbine nitrous oxide emissions
4. *Sharing across businesses:* The fourth part of CRD's new mission in the 1990s was to share technology across businesses. Performing this mission involved putting together an effort when one research program offered opportunities for many businesses. Edelheit cited learning how to utilize three-dimensional visualization as such a program.

5. *Source technology worldwide:* Edelheit noted that another part of the CRD mission was to find technology of use to GE businesses wherever it might be found. In this connection, he noted that CRD was sponsoring research at universities worldwide.
6. *Source of top technical people for GE:* The final part of the CRD mission was exemplified by the fact that about 50 people per year were moving from CRD to various GE businesses.

4. Strategy

Edelheit observed that into the 1980s there had been too much focus on technology development for technology's sake. He offered that by the late 1990s, CRD was better aligned with GE corporate strategy, two thrusts of which were (and still are in 2002) high quality and growth through services.

1. *Achieving high quality:* Edelheit reported that CRD was an important part of the GE-wide Six Sigma thrust in three ways. It was helping to improve the factories; he said that over 100 people from all 12 research labs were working in GE Power Systems on a range of manufacturing improvements from improving thermal barrier coatings and combustion to numerical control productivity and materials characterization. CRD was also working to improve product design process, in particular it was leading the company-wide "Design for Six Sigma" effort, which included reliability engineering, accelerated testing, material design for processing, product and process modeling, etc. Finally, Edelheit observed, CRD was working to improve corporate R&D itself.
2. *Leadership in services:* Edelheit described this GE corporate thrust in terms of an inverted pyramid. Before the pyramid base was product with services, manufacturing, and information resting on it. In the 1990s GE began to see the pyramid had been inverted and product had become merely the tip of the iceberg; the biggest growth opportunities might well be coming from providing services, information, etc. GE had come to see major growth opportunities in extending its businesses from simply providing products to helping customers be more productive in their use of those products. Edelheit gave a number of then-current examples: InSite, a program for medical scanner maintenance supported by remote diagnosis, contract maintenance of customer medical equipment generally, and managing patient information in the GEMS business. Use of Internet was described as an area of special interest, as exemplified by GE Tradeweb, which was then being offered by GE Information Services; it was a system for small businesses to exchange business documents with trading partners.

5. Organization for S&T

a. R&D-Specific Organization and Statistics

In 2001 CRD staff numbered over 2,000, including 1,500 scientists, engineers, and technicians.[14] In 2001 the Corporate R&D Center was renamed the Global Research Center.[15] The main locations for research:

- Niskayuna, New York—GE Corporate R&D headquarters;
- Bangalore, India—John F. Welch Technology Center (JFWTC); and
- Shanghai, China—Cao He Jing Hi-Tech Park.

Additional U.S. locations for R&D are Pittsfield, Valley Forge, Lynn, Wilmington, Cincinnati, Louisville, Syracuse, Cleveland, Pleasanton, Chicago, and Detroit; there are many other locations worldwide. Most of these additional locations are in an SBU and specialize in a technical area of that SBU's business. For example, within the GE Power Systems Division's GE Hydro SBU, the GE Hydro Engineering Laboratory in Peterborough, Ontario, performs applied research on large rotating machines, primarily for hydro generators and large AC and DC motors. Its Electromagnetics Team conducts computer-aided engineering and testing on electromagnetic performance of large rotating machines. Its Heat Transfer and Ventilation Team performs R&D in heat transfer, fluid flow, and ventilation for large rotating machines and other electrical equipment. The laboratory's Metallurgy Team evaluates metals and alloys for large rotating-machine applications. An Insulation and Chemistry Teams performs applied R&D on nonmetallic materials, sciences, and related processes, with primary emphasis on electrical insulation systems and processes, structural components, and surface protection.[16]

Funding breaks down into relatively balanced proportions—15 percent of effort/resources into technology development, 15 percent for business needs, 35 percent advancing the next product generation, and 35 percent to looking beyond the next generation. GE's total R&D expenditures were $2,349 million in 2001, up 7 percent over 2000. Of this, $1,980 million was GE funded, an increase of 6 percent over 2000. Customers, principally the U.S. Government, funded $369 million, up $43 million from

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[14] GE Power Systems press release 8 October 2001 in Schenectady, N.Y., on HTS generator development.

[15] GE 2001 Annual Report. "CRD" will continue in use as the abbreviation for the corporate R&D center.

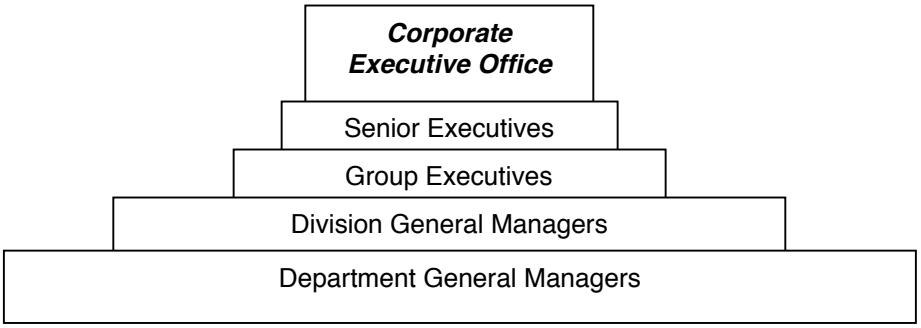
[16] www.gepower.com.

2000. Aircraft Engines accounts for the largest share of GE's R&D expenditures from both GE and customer funds. Medical Systems, Power Systems, Transportation Systems, and Plastics were the other major users of GE and customer R&D funds. In total, approximately 9,739 person-years of scientist and engineering effort were devoted to R&D activities in 2001. Product technology efforts in 2001 included continuing development work on the next generation of gas turbines, further advances in diagnostic imaging technologies, and development of more fuel-efficient, cost-effective aircraft engine designs. Services technologies include advances in diagnostic applications, aircraft engines, power-generation equipment, and locomotives. Process technologies provided improved product quality and performance and increased capacity for manufacturing engineered materials. Scott C. Donnelly has been heading up the effort as Senior Vice President, Research and Development, since August 2000.[17]

b. GE Organization from an S&T Perspective

Beyond R&D, the overall structure of the company changed often during its growth. The 1890's saw GE and many other companies turning to more structured organizations consisting of centralized, formalized management hierarchies where middle management divided by function into the various departments. At this point, securing rights to technology was difficult, so from an S&T perspective, this structure helped to control the raw material of technology.

In GE, massive decentralization in the early 1950s established dozens of profit centers to gain an economic advantage. By 1980 the operational structure consisted of five tiers of general managers with almost 200 department general managers. The hierarchy ran as follows:



[17] GE Annual Report 10K.

GE was a "line manager's organization," with each department head responsible for his piece of the ever-important bottom line. Higher level managers were intended to use their "broader perspective" to discover new initiatives. This overall organization took advantage of diversified markets, decentralized management, and a strong corporate self-image.

The GE Board of Directors has a Technology and Science Committee. It held one meeting in 2001, during which it reviewed GE Power Systems. The members of the Technology and Science Committee included James I. Cash, Jr. (Professor of Business Administration, Harvard Graduate School of Business), Chairman; Paolo Fresco (Chairman of the Board, Fiat SpA); Scott G. McNealy (Chairman of the Board and CEO, Sun Microsystems); and Roger S. Penske (Chairman of the Board, Penske Leasing).[18]

c. Management of S&T

In GE's early years, the decision to move away from "renting" patents and contractors in favor of internalizing technological advance was a fundamental executive strategic move. Hiring Whitney to spearhead R&D began an era of GE's new, self-sustaining face; senior executives Edwin Rice and Albert Davis were key to convincing Whitney to join GE in 1900. The paradigm Whitney established at GE would have tended to maintain senior management detached from corporate S&T activities.

Whitney's belief was that "the professional scientist can find a place in industry and remain a scientist," blending the goals and methods of the corporation and an academic laboratory. He gave two addresses, "Organization of Industrial Research" and "Research as a Financial Asset," which summarized the jobs of a director: neutralizing disputes and making concessions to the personality of each employee (personal); making clear the company's ownership of products (proprietary); setting the tone for technical work ("active optimism," defined as "anything to which the fair mind seems possible is to the trained persistence permissible"); and performing general tasks, including staff reports daily in notebooks and in weekly letters to the research director, provision of proper equipment and a library of scientific journals, and any other administrative loose ends.

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[18] GE Annual Report.

It seems doubtful that this minimalist management approach was practiced in its pure form. However, senior executive involvement in S&T management was not prominent until Jack Welch appeared on the scene.

On the one hand Welch argued, "You can't grow long term if you can't eat short-term." However, he recognized that the long term could not be ignored. He observed, "Anybody can manage short. Anybody can manage long. Balancing those two things is what management is." Welch was not supportive of R&D for the sake of doing research; he approached the scientific side of business as an intelligent leader and realized its importance. Still, top executive involvement in GE's specific research activities varied greatly, depending on the project at hand. Usually this interaction was relatively low, but in some cases the executive trump was required to save a higher risk project, exemplified by Jack Welch's personal interventions in GEMS over a period of 20 years. To enable radical innovation, Welch and other high-level executives had to be risk-takers and needed to engender this attitude in their workforce.

d. Processes for Deciding Resource Allocations to S&T Overall and by Area

The year 1988 was a watershed in the processes for deciding corporate S&T resource allocations.[19] Before that year two-thirds of CRD's work was guided by decisions made in the course of the overall GE strategy-making processes described below. Afterward, other processes, mostly negotiations with SBUs and outside customers (overwhelmingly U.S. Government customers) decided three-quarters of CRD's work. Therefore, there was no single process for deciding resource allocations, even for CRD.

That SBUs are the building blocks of GE has been fundamental to S&T resource-allocation processes. SBUs were developed in 1970 to provide more "corporate direction." By 1980 there were around 40 SBUs, all of which were considered mostly independent and managed to exist self-sufficiently for the most part. If this was not the case the unit would be in jeopardy.

(i) Overall decision-making processes from an S&T perspective

SBUs worked on individual strategies, basically competing for their share of the corporate resource pie. Similar to separate companies submitting proposals for a contract, these units were required to prove their worth through visible growth, earnings, patents,

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[19] This discussion does not address how S&T resources are allocated internally in SBUs except to the extent they are involved in deciding CRD S&T activities. This means we are not addressing over 90 percent of GE's total research and development expenditures, which were $2.3 billion in 2001.

or whatever output they intended to create. Two annual reports, one illustrating basic operating expenses and the other describing capital investments, were used by upper management to determine the value of each SBU and in turn to allocate the appropriate support. This involved two budget approvals for each SBU, an operating expenses budget and a capital investment budget.

The metrics involved is a very large subject; the end result required a huge amount of effort to arrive at realistic and useful measurement systems. On the manufacturing side, the accounting of value added vs. expenditure of labor cost and benefits was a key. As was seen in the discussion of the CRD in the 1980s and 1990s, the passion to measure was applied here as well, and it produced some extraordinary efforts to measure research.

GE used portfolio analysis very early, investigating how each of its ventures was doing and only perpetuating and acquiring those that were market or technology front-runners. By applying this approach to a broad spectrum of fields and different industries, GE sought to ensure long-term survival while bringing immediate rewards.

Such a corporate strategy was intended to limit uncertainty to a great degree. The potential impact of market and resource uncertainty was lowered.

Under Jack Welch, GE strategic management was cyclical:

- Early January: Annual agenda setting session of top 400–500 executives in Boca Raton, Fla.
- March: Quarterly Corporate Executive Council (CEC), consisting of the top 30 GE executives, including the chief technology officer, meeting at Croton-on-Hudson, N.Y., for 2 days.
- April/May: CEO visits each of GE's 12 business divisions for full-day meetings to review the performance and developmental plans for GE's top 3,000 managers.
- June: Quarterly CEC meeting at Croton-on-Hudson.
- June/July: CEO meets for a full day with the leadership of each of GE's business divisions to review their 3-year strategic plans at headquarters in Fairfield, Conn.
- September: Quarterly CEC meeting at Croton-on-Hudson.
- October/November: Corporate officers' (top 100–140 executives) meeting at Croton-on-Hudson to set the stage for the upcoming Boca Raton meeting.

- October/November: CEO meets for a full day with the leadership of each of GE's business divisions to review budgets and follow up on human-resource reviews earlier in year.
- December: Quarterly CEC meeting at Croton-on-Hudson.[20]:

The formal quarterly and annual meetings preceded Welch and undoubtedly will remain in place in the future. They are the venues that allow GE to set and abruptly change the corporation's agenda and to challenge and test strategies. The CEC sessions, where GE's top 30 officers gather before the close of each financial quarter, have been described by executives as ''food fights'' and ''free-for-alls.'' They were where relatively unfiltered information was displayed, the organization's triumphs and failures were openly shared, and GE's top players were challenged and tested.

The quarterly CEC meetings, the October/November corporate officers' meeting, and the January Boca Raton meeting were the main formal venues for establishing corporate S&T strategy, as well as overall strategy. However, because GE is so diversified, S&T was typically thought of business by business.[21]

(ii) S&T-specific aspects of resource-allocation decision-making

The SBUs differ from one another. As an example, consider the differences how the various SBUs think about long term and short term. For engines, long term is 10 years. For refrigerators, long-term is next Friday. Thus, there was not a single process for CRD because of the SBU structure and because of CRD's approximately $200 million total funding, one-half came from the SBUs, one-fourth from outside (such as the government or other firms), and one-fourth from Corporate (Welch wrote a check during his tenure). There were general parameters for deciding what was to be done for these groups:[22]

- How the money from an SBU was to be used was very clear. The SBUs would tell CRD what they wanted done. The laboratory worked on their problems. The director of CRD did have some control, and there was feedback to the SBUs. CRD (1) would not waste funds on projects that did not make technical sense and (2) could tell the SBUs if their projects were too short term.

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20 "How Jack Welch Runs GE," *Business Week*, June 8, 1998 (with minor modifications of numbers and times based on discussions).

21 Discussion, Edelheit.

22 Discussion, Edelheit.

- The outside funding might be either short term or long term. Government funded research was long term. However, any money CRD took from outside had to be in a strategic area for GE.
- The one-fourth of the total money that came from Corporate was thought of in CRD as being in three piles:
  - The first pile was working on multigenerational products: doing technology work to get ahead of the competition, producing the technology the SBU wanted and needed for the future of their current product line.
  - A second pile was given to particular technical areas in the laboratory to invest in areas they thought were most likely to produce a return for the company.
  - The third pile was investment in game changers. These could be anything. They were high risk, could be very long term, but to be justified, they had to have great expected payoffs. In GE the hurdle payoff return was $200 million.[23]

The GE CRD approach is to think about multiple generations of products rather than think short term vs. long term. What often happens in business is that there is a requirement to have a product on the market. The schedule is to get something out in 18 months. Management applies people to the task. Management then puts together another staff of people to work on unrelated ideas, which they later try to insert into the product after it is on the market. In this approach there is a lack of overall coordination.

With the multigenerations approach, a team is thinking about a product and a market, and it is focused on improvements over time.

There is an overall roadmap.

- The difference from the normal research approach is that this is always focused on a product.
- The product allows a "target" for the S&T people.

The S&T people can also think better when they do trade-offs on cost/technology grounds. The decision to change and go to the next generation is made on the basis of cost, performance, and quality criteria.

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[23] Discussion, Edelheit.

(iii) Sources of concepts, project selection, tracking/oversight, accountability, and transition or termination

There is not a single system for sourcing concepts. Ideas came from a variety of sources. Edelheit noted that one part of the CRD mission was to source technology of use to GE SBUs worldwide, wherever it might be found. The SBUs have their own researchers and their own facilities worldwide, which collectively form a rich potential source. An SBU is free to acquire technology anywhere—internally, from a university, national laboratory, or competitor, or from CRD.

CRD projects and funding from the SBUs is determined through an "objectives process." Working with key customers in businesses, CRD identifies 100+ key objectives at the beginning of year. These objectives formed the basis for SBU-funded projects; further details of the project are worked out in negotiations between CRD and the SBU (e.g., the balance between the generations in "multigeneration" work). Projects for government customers are determined through marketing efforts familiar to government contractors, usually marketing efforts conducted jointly by CRD and an SBU. The research funded by Corporate is determined within the flow of the annual strategic-management cycle. For example, in early 2002 the GE 2001 Annual Report announced that molecular imaging, distributed energy, advanced composites, and sensors had been selected as areas for future CRD emphasis.

CRD must be primarily responsible for the progress that can be made in the "white space" between existing markets or technologies. One example is finding multiple applications for single technologies. A case in point is that, as a result of much innovation, GEMS introduced an open magnetic resonance imaging (MRI) system that provided enhanced image quality and patient comfort and was available to GEMS customers at a competitive price. As a bonus, a portion of the technology used in this system was leveraged by GE Power Systems in its generator designs. Another example of working in the white spaces is Discovery LS, introduced by GEMS in 2001, which combines CT and positron emission tomography (PET). Discovery LS accomplishes in one 30-minute procedure, with greater clinician confidence and patient comfort, most of the process that can take 4 to 6 weeks using traditional cancer diagnosis procedures. Even though Discovery LS has more lines of computer code than the first mission to the moon, it was launched more than 12 months ahead of schedule. GEMS used a concurrent engineering strategy and a global team to design and manufacture core components simultaneously in Wisconsin, China, and Israel. In 2001 GEMS predicted that eventually this technology will be used by oncologists, neurologists, and cardiologists to diagnose and treat a full

range of diseases. Discovery LS also will be GE's platform for molecular imaging and genomics.

CRD frequently discovers ideas with multiple applications, sometimes extending to business or corporate practices beyond technology. Conceptualizers must attempt to remain within the boundaries of the business strategy, which will create support from both marketing and developmental divisions. Edelheit argued that exploiting synergy was a major function of a corporate laboratory, which it should exercise proactively through its involvement in the multigeneration projects of many SBUs.[24] This presumably must be done through the annual negotiations on key objectives between CRD and the SBUs.

Ongoing work for outside customers is reviewed under terms of the individual arrangements. Progress toward meeting the key objectives in work for GE SBUs is monitored regularly on a red-, yellow-, green- light basis. Finally, to close the loop, CRD polls its GE customers at the end of a year on whether its achievements toward the key objectives met, exceeded, or failed to meet their expectations. Thus the key metric for such a project is the GE SBU's grade on how CRD is serving it, by its ratings and funding.

As illustrated by the several examples given here, most CRD research is done with the participation of representatives of one or more SBUs. This is intended to facilitate transition of the technology to an SBU. Project personnel often transition with their projects. It is also common for CRD to maintain considerable numbers of people on a project for extended periods after transition to ensure continuity.

(iv) What kinds of S&T are done in-house and what is out-sourced, and among the latter specific information on where

The issue of in-house work versus out-sourced labor is tangled in economies of scale, likelihood for success, and many other factors. Outsourcing requires a credible partner to develop important components of the system. During development of the Digital X-ray a few problems slowed the process. The chosen partner could not come up with the desired yield, so R&D had to take this additional research, development, and manufacturing under its wing and complete additional facets of the product itself.

The creation of R&D in 1900 showed the then-dominant judgment of the critical importance of keeping everything in-house if at all possible. Patent and consultant

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[24] Lewis S. Edelheit, "Renewing the Corporate R&D Laboratory," *Research·Technology Management*, November/December 1995, p. 14.

purchasing were helpful but inefficient and were costing GE dearly in competitive advantage. Doing R&D in-house has remained an important theme in GE strategy. A recent example is Bisphenol A (BPA), the chemical intermediate that is at the heart of the impact-resistant Lexan. It shows work being kept in a very large house stretching between India, North America, and Europe. A 25-person global team, including 16 scientists from the John F. Welch Technology Center in Bangalore, India, began an 18-month project in January 2001 to develop a new BPA process. Their goal is to take $50 million in cost out of fixed investments in the future BPA facility while developing a simpler and more environmentally friendly process. In addition to the 16 Bangalore employees, 9 employees at CRD in Niskayuna and GE Plastics in Mount Vernon and Europe are invested in the project.

Despite a preference for keeping research in-house, GE conducts R&D with other companies. Recent examples include the following:

- In October 2001 GE announced receipt of a $12 million grant from DOE to support a 3.5-year program by a GE-led team of industrial partners, utilities and national laboratories, to develop a breakthrough technology for high-efficiency generators, building on GE's extensive research into high-temperature superconducting (HTS) materials and generators. While retaining the stator design that was the current industry standard, the proposed generator would introduce a new rotor design and HTS winding unprecedented in its simplicity. Much of the development was to be conducted by GE's CRD and GE Power Systems. In addition, the GE team was to include GE Industrial Systems and GE Medical Systems. American Superconductor was named as the primary HTS wire supplier. Advanced refrigeration components were to be developed in cooperation with Sumitomo Heavy Industries and Praxair. The National High Magnetic Field Laboratory at Florida State University and the Oak Ridge National Laboratory were to conduct special studies as part of the development program.
- In April 2002, GE Power Systems and the Toshiba Corporation announced the development of new 40 in. and 48 in. steel last-stage buckets for steam turbines through a joint program spanning several years, using design and development teams from both organizations, including technology from GE's CRD and Aircraft Engines business and Toshiba's Power and Industrial Systems R&D Center. The 48 in. last-stage bucket was reported to be the largest steel full-speed (3,000 rpm) last-stage bucket in the world in terms of annulus area.
- In September 2002, GE Aircraft Engines announced the successful first test flight of the GE90–115B engine on GE's B747 flying test bed. Slated for

Boeing's new B777–300EB, the engine was developed by an industrial team that included Snecma Moteurs of France and FiatAvio of Italy.

GE does some work with universities, but GE got out of working closely with the universities in the early 1990s because it did not get enough out of the relationships. Universities tend to do what they want to do. GE continued some work with universities, but it was very focused. However, now CRD is thinking about the idea of working with universities in a much more tightly focused area, as suggested by the HTS example above. For example, MIT has a new concept that is interesting: a consortium that works on particular technical problems. Similar developments are occurring at other universities; one could pick the best schools and set up long-term relationships focused on areas of need.

e. How S&T Relates to Other Parts of the Business

GE's was the first industrial laboratory to maintain a dedicated liaison workforce of about a dozen scientists and managers on rotation from the science work. They were assigned to client divisions or SBUs and served as consultants to the general managers thereof and as sales detail workers for contract work. This force was in place as early as 1956, was cultivated by Bueche in the 1960s, and was vital to Robb when he was vice president for R&D. There is no doubt that this workforce was essential to the acceptance and survival of the research laboratory and then CRD.

The goal of being vital described by Edelheit is by its nature a commitment to close relations with the GE SBUs, a commitment met by some reduction in the long-term S&T conducted by CRD. The key objectives selection and performance evaluation processes described earlier are the formal manifestations of the fact that CRD seeks to be very responsive to SBU needs. Examples are given in several GE reports of CRD scientists working directly on SBU (and customer) factory floors to solve immediate problems. In sum, the recent relationship between CRD and GE SBUs has been close.

S&T work in CRD is credited by senior GE leadership with much of the success of GE, which can be taken as the (mostly) manufacturing segment of the consolidated financial report. Of the $126 billion consolidated revenues mentioned above, $68 billion are attributable to this segment and $58 billion are attributable to GE Capital Systems, a separate affiliate that has little interaction with CRD.[25]

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[25] For additional information, see the GE Corporate Research and Development Web site: *http://www.crd.ge.com/index.jsp*

II. IBM'S REFOCUSING OF CORPORATE S&T

Richard H. Van Atta

A. THE BUSINESS MODEL

IBM's origins stem from the U.S. Census Bureau adoption of the Hollerith Punch Card, Tabulating Machine and Sorter to compile results of the 1890 census. Its inventor, Herman Hollerith, a Census Bureau statistician, later formed the Tabulating Machine Company in 1896. In 1911 the Tabulating Machine Company merged with the International Time Recording Co. and Computing Scale Co. of America to become the Computing-Tabulating-Recording Co. In 1924, the new president of C-T-R, Thomas Watson, Sr., changed its name to International Business Machines Corporation.

IBM evolved from a calculating machine company into the dominant computer company, beginning in 1944 with the MARK I computer, "the world's first large-scale calculating computer."[1] After World War II, IBM and several competitors began to investigate the use of electronics for computers, first with vacuum tubes and then solid-state transistors. The first computers were huge machines developed under government contracts largely related to the Cold War.[2] IBM participated in some early work on applying the newly emerging electronics technology to military applications, and in the early 1950s its 701 Defense Calculator became one of the earliest fully electronic computers produced by a commercial company, although it trailed the entry into the market of the UNIVAC developed by Eckert and Mauchly from the University of Pennsylvania. Recognizing the potential of electronic computing, Thomas Watson, Jr., urged his father,

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1 The Mark I (or Automatic Sequence Controlled Calculator) designed in collaboration with Harvard University, used electromechanical relays to solve addition problems in less than a second, multiplication in 6 seconds, and division in 12 seconds. It was technically a predecessor to the electronic computer.

2 Kenneth Flamm, *Creating the Computer*, Washington, D.C.: 1988, pp. 29–69.

the president of IBM, to aggressively pursue the development of the electronic computer business as the basis for the company's future.[3]

Today IBM is an $86 billion information-technology corporation. Over the past three decades the company has survived an era of disruption that saw the industry that it dominated dramatically transformed from one of centralized mainframe computers used in narrow business and scientific applications to a world of ubiquitous computing in which individuals are interacting through multimedia information systems in nearly every facet of life.[4] However, IBM maintained a dominant position in large-scale computers, especially for business applications, and through a difficult period repositioned itself in the dynamic world of internetted computing systems.

In reflecting on the perturbed history of IBM as it negotiated this shifting environment, outgoing CEO Louis Gerstner (now Chairman of the Board) made two observations in his last Annual Report (for 2001) about key decisions made by IBM: first was a decision not to break up IBM into several pieces as some had advised; the second was to reaffirm the company's technical heritage by "revitalizing IBM research and development." In his report, Gerstner emphasized the company's history: "IBM's heritage is technology that changes how business is done, how states govern, how students learn. IBM's R&D finds its ultimate scorecard…in the impact it has on the fundamental problems and opportunities that exist in the world." His view was that IBM had to reestablish itself as "the place where grand challenges are taken on, and where paradigms are shifted."[5]

IBM has gone through a fundamental transition over the past decade—to the point where for the first time hardware is less than 50 percent of its revenue. Services is now 44 percent, and software is 13 percent. Thus the focus of research is changing. Even so IBM has filed more patents in information technology (2,800 last year) than any other firm—and more than the rest of the industry combined.

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3 Thomas J. Watson, Jr., *Father, Son & Co.: My Life at IBM and Beyond*, New York: Bantam Books, 1990, pp. 188–207, discusses IBM's entry into the electronic computer arena.

4 M. Mitchell Waldrop, *The Dream Machine, J.C.R. Licklider and the Revolution that Made Computing Personal*, New York: Viking, 2001, discusses IBM's engrained focus on batch mode mainframe computers.

5 IBM, *Annual Report*, 2001, p. 13.

B. WHY THE BUSINESS DOES S&T

IBM seeks to develop and have a leadership position in "fundamental technology building blocks." Examples of these building blocks are microelectronics and storage technology. IBM sees research at the heart of its strategic thrust across all technologies. IBM also sees the need to develop "fundamental technology building blocks" that go deep into the basic phenomenology and fundamental science underlying the technologies, not just product development. IBM has identified (and continues to review) technology areas in which it has to be world class. It establishes partnerships with world leaders in fundamental technologies and supports basic research at universities (which few other firms do). IBM sees it needs to be at the front end of these "strategic" technologies as opposed to being a "blind recipient."[6]

Early in its history, IBM's conduct of S&T was essentially a technology-push focus conducted in a closed and largely defensive posture, based on the company's dominant position in the computer industry. In retrospect, it is evident that this approach had a major defect: although many of the ideas and technologies that led to the revolution in the information age in the 1970s and 1980s were being pursued by IBM both internally and through its funding of university research, the company did not take advantage of the technologies itself, and many of these were developed and implemented by others, often based on support of the Defense Advanced Research Projects Agency (DARPA) for universities and subsequent entrepreneurial commercialization through venture capital.[7]

Note that IBM also took advantage of many of the inventions from its research labs during this time to develop and maintain a leadership position in the core business of mainframe computing and its evolution into transactional computers (see the next section). Nevertheless, many innovative concepts and technologies were "buried" in the labs, and leading technologists often grew frustrated by the company's lack of interest in pursuing their ideas in major new product areas. IBM was driven in 1964 to address time-

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6 A corollary is that the company must choose which technology areas in which to lead. This raises the question of how these areas are selected. One basis of selection is whether the company sees a path to appropriate the research results for the company's benefit. This topic is discussed below under S&T management.

7 Waldrop, *The Dream Machine*, and Arthur L. Norberg and Judy E. O'Neill, *A History of the Information Processing Techniques Office of the Defense Advanced Research Projects Agency*, Minneapolis, Minn.: Charles Babbage Institute, October, 1992. IBM in the mid-1960s was still wedded to mainframes and not eager to embrace time-shared, let alone interactive, computing. Another factor impinging on IBM's interest in venturing into new application areas, especially in the 1980s, was the environment surrounding the vigorous antitrust action being pursued by the U.S. Government.

sharing and interactive computing by concerns that a huge potential competitor—General Electric—could steal the march on it based on GE's collaboration with MIT's DARPA-funded Project MAC.[8]

A key reason to do research is that it pays—IBM received $1.6 billion in revenue from its patents in 2001. From a broader perspective, IBM has placed emphasis on linking its research activities to core business developments that are evaluated as being strategically important for the firm's future competitive position.

C. R&D AND IBM'S EMERGENCE AS THE DOMINANT COMPUTER FIRM

At the end of World War II electronics was emerging as a new arena of commercial application for what was to be known as information processing. The earliest electronic computer-related research was government funded—mostly for military purposes.[9] As a leading calculating machine firm, IBM was actively involved in providing a range of computational systems for the war effort, ranging from cryptography to weapons targeting. After the war a new defense focus on the Soviet strategic air and missile threats stimulated accelerated defense interest in developing new electronics-based computer capabilities, including the Semi-Automated Ground Environment (SAGE) air defense system. IBM was the primary provider of computers for SAGE, and this program was highly beneficial to the company's position as a leader in computer technology—especially in the engineering and production of magnetic core memories.[10] Through its pioneering of real-time processing, SAGE also had an impact on IBM's business system with the SABRE real-time airline reservation system, which placed IBM as the leader in transactional business computing.

Also significant to IBM's technological position was the Stretch Project, which began as an internally funded IBM program for a high-performance computer. IBM decided that it wanted to develop a computer that would "stretch" existing technology by a hundredfold relative to its then state-of-art 704 machine. The company went to both the National Security Agency and to the Atomic Energy Commission's Los Alamos Laboratory to get backing for the system. These two organizations had very different computing needs, and supporting both necessitated a highly flexible architecture, in

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8 Waldrop, *Dream Machine,* pp. 244–253.

9 Flamm, *Creating the Computer*, pp. 29–79, elaborates the military roots of the modern computer.

10 Ibid., p. 88.

contrast to the application-specific designs of previous computers. Stretch was a technological tour de force that included the development of high-speed core memories, high-performance micro-alloy-diffused transistors, as well as the automated design and manufacturing of standard modular-system printed-circuit cards. The basic logic circuitry—emitter-coupled logic—was invented for the Stretch computer, and the machine employed such innovations as pipelining and instruction look-ahead. A key development first employed on the Stretch system was the 8-bit byte word length and the use of bytes as basic data element for computer processing. Stretch was a seminal development in computing technology and design that was to have enormous impact on IBM's System/360—the system that was the basis for IBM becoming the dominant computer firm.[11]

In 1961 the System/360, "a new family of computers that was radically different from anything that had ever been built,"[12] was envisioned to encompass the entire range of needs in both the business and scientific worlds with a compatible line of computers that spanned a broad range of performance. The scale of the investment was daunting, on the order of $5B, reportedly the largest and riskiest business venture any firm had undertaken to date. To execute this massive effort, IBM drew on R&D facilities throughout the company, including those in Britain and Germany.[13] This was a massive job in simultaneous development of computer processors, a whole range of new peripherals, and software. The System/360 also employed integrated circuits in a commercial computer for the first time, which introduced a whole new area of technology, including the underlying production technologies for semiconductor devices, since the company decided that these components were too critical to rely on outside suppliers to provide.[14]

While the company encountered daunting technical and manufacturing problems, and especially had difficulties with the vast software-engineering job the new system required, the System/360 was an enormous success and radically transformed the industry. Underlying this success was a history of advanced technology development, much of it done under defense sponsorship, but also research and development to

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11 Ibid., pp.90–94.

12 Watson, *Father, Son & Co.,* p 346.

13 Flamm, *Creating the Computer,* p. 99–101.

14 Watson, *Father, Son & Co.,* p. 350–358. Starting with its introduction of integrated circuits in the 360, IBM became a leading producer of integrated circuits and introduced some of the major innovations in integrated circuit design and production. See Appendix, "IBM and X-ray Lithography."

capitalize on the government-sponsored developments for use in commercial markets. IBM's major competitor at the outset of the electronic computer era was Remington Rand, which has acquired the UNIVAC in 1950 from Eckert and Mauchly. In contrast to IBM's systematic, internally fostered electronic computer technology efforts, established under Ralph Palmer in Poughkeepsie and scaled up under the urging of Thomas Watson, Jr., Remington Rand had a kludge of fragmented research groups and, more critically, was reluctant to invest R&D resources into machines to succeed the UNIVAC I. Remington Rand merged with Sperry to form Sperry Rand in 1955, but its technology-development efforts were further thwarted by the departure of William Norris and his associates to set up Control Data Corporation. Nevertheless, Sperry still was able to conduct innovative technology development for the government that fed subsequent UNIVAC systems, and spin-off Control Data did the same (as did a spin-off from CDC–Cray Research).

The broad base of technology underpinning that IBM put into place, which was subsequently sustained by the firm's phenomenal growth in the 1970s and 1980s, provided a scale of technology development that no other firms could match.[15] IBM had remade itself into a high-tech firm pursuing advanced electronics to fuel new systems, which in turn transformed the computer industry. IBM shifted "to a business strategy explicitly based on a continuous investment in research, incorporated into a steady stream of new, technology-intensive products that were more advanced than those offered by its competitors. IBM's strengthened research organization was an important first step toward domination of the computer industry."[16] This emphasis on R&D showed directly in investment figures as internally financed R&D reached over 30 percent of net earnings in the 1950s and just under 50 percent in the 1960s through mid 1980s.

D. ORGANIZATION FOR R&D

In 1945, IBM opened its first research facility, the Watson Scientific Computing Laboratory, near Columbia University in Manhattan;[17] in 1952, it opened its first west

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15 Flamm, *Creating the Computer,* p. 107. IBM in 1954 trailed Sperry Rand in total revenue, $461M vs. $696M. In 1963 IBM's revenue had reached $1.2B while Sperry had fallen to $125M. A decade later IBM revenues were $8.7B, in 1982 they reached $31.5B, and in 1986 they approached $50B. Ibid., p.102.

16 Ibid, p. 85.

17 Before the establishment of the Watson Lab, IBM had supported academic research at its Cambridge Research Center.

coast lab in San Jose, Calif.[18] In 1956, T.J. Watson, Jr., recruited Emmanuel Piore, then Chief Scientist at the Office of Naval Research, to become IBM's first director of research. Piore built an organization with world-class capabilities in solid-state physics, mathematics, superconductivity, etc., and in 1961 he consolidated many of IBM's scattered research activities to form the T.J. Watson Research Center in Yorktown Heights, New York.

Today, IBM Research employs about 3,500 people in laboratories in the United States (New York, Massachusetts, and California), Switzerland, Israel, Japan, China, and India. The Watson Research Center consists of the Yorktown Heights laboratory and additional facilities in Hawthorne, New York, and Cambridge, Massachusetts. With approximately 1,700 employees, it focuses on physical and computer sciences, semiconductors, systems technology, mathematics, and information services, applications, and solutions.

IBM dedicated the Almaden Research Center, the successor to the San Jose Research Laboratory, in 1986. The company's second-largest laboratory, Almaden has a staff of about 500, who primarily conduct basic and applied research in computer science, magnetic and optical storage technology, physical and materials science and technology, and scientific and technical application software.

IBM was (and today still is) a highly vertically integrated company, and so was its research division: IBM research fed into IBM product development; those products were built in IBM manufacturing facilities (which themselves often employed processes and technologies developed in research) and sold through IBM distribution channels and supported through IBM services and support structures. This deep vertical integration was the basis for IBM's decades-long dominance of computer systems.

Given the firm's overall size and its reliance on science and technology, IBM had a larger research effort than that of almost any other firm—but the organization and management of that research enterprise were not particularly unusual:

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[18] Inspired by the National Bureau of Standards' SEAC computer, which apparently had the first magnetic disk drive, the San Jose research center developed the first commercial magnetic disk storage system (Flamm, *Creating the Computer,* p. 71). RAMAC, or Random Access Method of Accounting and Control, was introduced in 1956; it offered unprecedented performance by permitting random access to any of the million characters distributed over both sides of fifty 2 ft diameter disks. With a purchase price at the time of about $10,000, RAMAC provided a storage capability that by 1997 could be obtained for about 10 cents.

Until 1990 IBM Research typified the traditional R&D model. Its labs were set up far from its business operations, and they reported to IBM's CEO separately from IBM's business units. IBM essentially funded research through a "tax" on its business units.

Its research mission was scientific in nature, so much so that IBM Research was organized very much like a university science and engineering school. IBM had departments of mathematics, materials science, computer science and physics that hired, managed and promoted research staff. IBM competed with universities and national and industrial labs for the brightest scientific graduates. It wasn't unusual for an IBM research scientist to spend his or her entire career on the properties of one class of polymer.

Rewards reflected this approach. IBM rewarded its scientists on their accomplishments, both to the scientific community as well as to IBM itself. In those days, it was fine to excel in just one of these two areas. Researchers were advised to invest in their scientific reputations in their first years at IBM. They often declined to work on technology transfer issues to move research discoveries into IBM's businesses. They thought: "I didn't come to IBM to fight fires on the manufacturing line" or "Working out the operational details would really cut down the number of papers I could publish this year."

This approach to research generated some notable scientific achievements, including five Nobel prizes, six National Medals of Science and Technology and hundreds of other scientific awards. IBM also benefited from important technological breakthroughs that emanated from its research laboratories.[19]

E. THE CHANGING ROLE OF RESEARCH AND TECHNOLOGY DEVELOPMENT

More recently, however, IBM's organization for and utilization of science and technology have changed to reflect the "waves of change" in the business world more generally—changes that have induced corporate research organizations to engage more directly with mainstream corporate management and that have forced them to keep up with a rapidly evolving and disseminating global technology base.[20] The relationship of research to other business operations in IBM's vertically integrated structure has been

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19 Henry Chesbrough, "Old Dogs Can Learn New Tricks," *Technology Review*, July 18, 2001.

20 Graham R. Mitchell, "The Changing Agenda for Research Management," *Research-Technology Management*, Vol. 35, No. 5, pp. 13–21, September/October 1992.

transformed through a sweeping process of redefinition of both the business model of the company and the relationship of technology development to this refocused business model.[21]

Regarding the latter, IBM has made an explicit decision to retain its technology development operations, but to redefine how it does business. As a first departure from the past, the Research Division is no longer mainly centrally funded. About 30–40 percent of its funding comes from corporate. About 10–15 percent comes from government funding (including DARPA). The remainder comes from product divisions. Further, today IBM Research Division explicitly has a customer focus—the customers are those people within the firm who develop the ideas and execute them into product results.

A major change in IBM's approach to R&D has been a substantial shift toward increased linkages and relationships with the product divisions. This shift was driven by upper management and implemented by measures that directly accounted for how the IBM Research Division was materially affecting the competitive position of the product divisions. This shift was executed through the development of formal joint programs with product divisions in which it was jointly decided what the research organization and the product division would set out to accomplish *together*, and the work was explicitly divided, with a transition approach defined.

Over the last decade IBM has placed increasing focus of its R&D on *customer-focused* developments. For example, IBM developed the first transactional Web site—for L.L. Bean. Particularly since the 1990s, IBM has put emphasis on working with leading-edge customers in government, finance industry, distribution, and manufacturing.

IBM took the following steps to redefine the Research Division:[22]

1. *Leveraging intellectual property*. Since 1993, IBM has been awarded more U.S. patents than any other company. IBM aggressively enforces its intellectual property. In 2000, it received over $1.7 billion in royalties.
2. *Restructuring staff*. The academic departmental boundaries are gone. No longer can IBM researchers blithely cultivate their scientific reputations and be indifferent to their impact on IBM's bottom line.

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21 Henry Chesbrough, "Old Dogs Can Learn New Tricks."

22 Ibid.

Many IBM research managers now wear two hats. The first hat, their own area of research, remains. But the other hat is to act as a relationship manager between the entire research division and one of IBM's businesses. If that IBM business unit is seeking a research answer to a pressing problem, the job of the relationship manager is to locate someone in the Research Division who can answer it. So IBM research managers are now more than knowledge generators—they are knowledge brokers. This second role broadens their understanding of their assigned business unit and helps move research discoveries out of the lab and into the market.

3. *Changing funding*. While much of IBM's research budget still comes from corporate, a significant and growing percentage of funding comes directly from IBM business units. As a result, IBM researchers are now more sensitized to the needs of IBM's businesses. Not surprisingly, these businesses are also working more closely with IBM researchers, since these funds now flow directly from their P&L.

4. *Connecting researchers to customers*. IBM's First of a Kind program assigns an IBM research scientist to a carefully selected customer, to develop a solution to a customer problem. This solution is really a prototype, and IBM negotiates to receive the rights to the ideas that emerge in order to offer them to other customers later on. This approach boosts IBM's experimental capacity and, more importantly, links that experimentation to real customer problems. This program recently expanded into the Emerging Business Opportunities program, in which IBM Research works with customers to create advanced solutions to complex problems.

5. *Opening up to the outside*. Another critical change has resulted from IBM's rethinking of its deep vertical integration approach. IBM invented some of the fundamental computer languages, yet today it devotes over 2,400 of its staff to Java and related areas, which originated outside IBM. Similarly, the company is making a substantial commitment to the open-source Linux operating system.

   IBM continues to produce breakthroughs on its own in technologies such as copper-interconnect technology for semiconductors and giant magneto resistive (GMR) heads for disk drives. Today, though, IBM licenses or sells its technology on the open market, even to companies who compete with other parts of IBM. The technology area within IBM is one of the fastest growing parts of the corporation, along with IBM's services business, which will service and support equipment and software from any company. As Research Director Paul Horn told me, "this gives IBM more channels for its intellectual capital to get to market."

6. *Increasing the flow of ideas*. "We used to locate our labs in somewhat remote areas, where we felt we could control how much got out," says Horn. "Now we locate them near intellectual centers, in order to stimulate the flow of ideas into our labs."

Beyond its research organization, IBM has also made major changes in its technology development approach and organization. It saw itself as "too slow, too costly, too insular."[23] The company engaged in a major internal management review that included how it developed products, with one major focus being on fundamental reduction in product cycle time. A major change was shifting to an acceptance of open systems as opposed to the old view of proprietary systems—a major refocusing of company perspective. IBM changed its view on how products moved from labs to market. Before the 1990s, IBM had a reputation that "IBM products aren't launched. They escape." IBM set about "reinventing" the way it creates, develops, and deploys new technologies.

This evolution at IBM is indicative of the company's desire to learn from the venture-capital model. The goal of this approach is to progress from a pure "technology incubator" role to a collaborative "hybrid technology and business incubator," which includes analyses of market intelligence and business development at an early stage in the research process.[24] The intimate relationship in collaborative teams between the research side and the business side helps transition innovations to market faster to bring customer needs into the laboratories—by building prototype solutions with a customer, for example. Today, one-quarter of IBM researchers are involved in "joint projects." IBM has placed major emphasis on linking its research to product development. As will be elaborated below, researchers are measured on how well they team with product organizations.

F. MANAGEMENT OF TECHNOLOGY

IBM explicitly looks at "global technology outlooks"—5- to 10-year projections. What is emerging? What is the vision and strategy for using this technology? What approach is needed to be the leader? What are the levers, and can high leverage points be identified? If this is where IBM must be, how will it get there? (This is laid out in the

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[23] As an example, in 1969 Ted Cobb of IBM invented the relational database, but IBM was slow to execute, and firms such as Oracle developed and dominated the market.

[24] David F. McQuenny, "IBM's Evolving Research Strategy," *Research-Technology Management,* July-August 2003, p. 20.

Technology Plan.) This is explicitly tied to a financial plan that drives resource allocation.[25]

Technology areas must be seen as being vital to IBM for the future. An example is nanotechnology and more particularly nanotubes. The key to pursuing these technologies is that they can be directly related to concerns in information-storage technology and semiconductor microelectronics. In this latter area, IBM has done fundamental research on carbon nanotube transistors. Quantum computing is another advanced-technology area that IBM has given particular attention.

Interest in these areas illustrates an IBM trait of pursuing the underlying science for areas that are judged as being "key building blocks" for its future competitive position. IBM's vision is that without having a leadership position in the basic core technologies in its portfolio, it cannot effectively compete in the information-technology industry and be a leader in technology and computer products. To support this vision, IBM has invested billions of dollars every year in science and technology ideas in its R&D laboratories worldwide, with the objective to keep itself two to three generations ahead of its competitors. "In Year 2001, IBM's R&D spending was $5.3B, that is 6% of 2001 revenue. Funding for research and development has remained relatively consistent despite the economic downturn."[26]

One of the core technology areas in IBM's portfolio has been semiconductor components. Since the evolution of integrated-circuit technology, lithography has been identified as the key process technology for advances in integrated-circuit components technology. Beginning in the 1960s, IBM has made major investments in basic technologies underlying microelectronics—especially those related to photolithography. The company determined that leadership in this technology was required to be at the forefront of the computer chips that would provide the fundamental technical lever to competing in the computer market. (For an elaboration, see Appendix A.)

An overarching driver and focus for all IBM research is that the firm is in business to make money—there is a singular goal of developing and producing products that further the company's competitive position. This is achieved through connecting research to applications and implementation. IBM has worked to develop "real measure-

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25 Interview with Kathleen Kingscott, IBM.

26 Lee Bruno, "Inside the Invention Factory," *Red Herring*, July 2002.

ment" of these linkages as part of the management process. These measurements include tangible items such as patents and publications, but include assessments of how well researchers partner with product divisions, and how well they support moving ideas into product applications. The Research Division is scored on its performance in supporting the product divisions.

> IBM Research director Horn…thinks he can minimize conflicts between research and development by involving product engineers in projects from the start.… Horn matches every dollar IBM's product groups invest in research with a dollar from his central funding, which doubles product groups' investment in their projects. "It's a simple financial trick to stimulate these partnerships," he says. "This is absolutely critical to force us to do things that are relevant." One measure of success: IBM received 2,886 patents last year—tops among U.S. companies. A third appear in shipping products.[27]

To reduce conflict that might naturally arise from the cultural gap that exists between technologists and their business associates, IBM specifically desires a management style that is both agile and "ambidextrous." This gap, illustrated in language, personalities, and ideas, necessitates that the "champions," those business experts teamed with researchers to turn science projects into moneymaking ventures, think differently in how they will pave a path for success for an emerging business opportunity. Ambidextrous managers have the foresight to apply proper metrics to products and markets at different levels of maturity; mature businesses and markets and major new growth businesses and markets require diifferent nurturing than the portfolios of experiments for long-term growth. This shift from academic-style research to a culture that values the commercial viability of a product demands a careful balance of differing personalities and units.[28]

G. RESEARCH PROCESS

New ideas usually are notions developed by bench scientists who "just start working on it." Scientists have freedom for exploratory work, but for it to progress, the next step is getting some others interested and involved. This is where some management effort is involved to decide whether a project should be made. Determining this requires

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27 Aaron Ricadela, "Reinventing Research" March 12, 2001, *Infoweek.com*, http://www.informationweek.com/shared/printArticle?article=infoweek/828/prresearch.htm&pub=iwk

28 McQuenny, "IBM's Strategy," p. 26.

laying out of costs, potential results, people needs, time to expected results, return on investment (ROI) (typically at this stage a project would entail ~$1M and 4–5 people). Most technology areas have 8–10 projects and about 50 people total.

To go to the next level, for example a ~$100M project, requires a detailed business case assessment. Most of these programs feed into existing business units as "generation-after-next" development efforts. Key members of the research team will be transferred with the technology into the product division. Transferring with the product development effort is a career/job slot move. These are managed and decided as part of the relationship between the product division and the Research division.[29] These are not "free-trips"—to return back to the Research division is another job-slot career move and requires acceptance of the Research organization.

The Research division conducts detailed evaluations of staff member performance, essentially ranking performance in areas such as patents, publications, collaborative teaming, product division, and customer support. Salary and personnel advancement decisions are directly linked to these evaluations. The Research Division has conducted this review process for 30 years. The major change in the last decade has been the strong focus on product division interaction and support and customer involvement.

Collaboration and teamwork both inside the Research division and with product groups is a key criterion. Mentoring is a major focus. Most new hires are new Ph.D.s, but IBM will hire some experienced researchers from other companies and organizations—more so today than in the past, especially in new research thrust areas.

IBM's Research Division assesses the general technology outlook to predict future trends across technology areas by asking,

- How does IBM stack up in these technology areas? If IBM is not leading in an area, why?
- What new and different technologies are emerging and what are the implications for IBM's future competitive position?

These assessments are presented to top management and help drive product division and corporate strategy. After all, a "good research lab is one that makes an

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29 There are also short-term assignments from Research into a product division that are not job-slot related.

impact on its parent company, developing a culture that values contribution to the bottom line and rapid deployment of its innovations into products and services."[30]

H. CONCLUSION

From its beginnings as the Tabulating Machine Company, to becoming the world-leader in information technology, IBM's legacy rests on its determination in resolving fundamental problems and creating previously nonexistent opportunities by uncovering breakthrough technologies to improve the quality of business, government, and interpersonal interaction. Despite ongoing changes in the nature of its research in the last decade, IBM maintains its strong commitment to promoting advanced science and technology research, as evidenced by its filing more information-technology-related patents in the past year than the rest of industry combined.

IBM's research goes well beyond product development as its labs delve into the basic phenomenology and the fundamental science behind its technologies to gain leadership in fundamental technology building blocks. In doing so, IBM seeks to establish partnerships with other leading firms throughout the world and to support university research.

Until recently, IBM's laboratories operated like a university, where scientists focused solely on discovery and were hesitant to engage in the business end of technology transfer. However, to meet the needs of a changing global economy, IBM has redefined its business model and the relationship of technology development to this model. A vertically integrated organization to this day, IBM uses research to feed its product development. According to David F. McQueeney, vice president of technology assets for IBM Global Services, IBM continues to shrink the "distance" between the research and business units: "Today, the confluence of technical innovation and business innovation requires a technology research laboratory to also be well grounded in business and well-connected to business innovations."[31] As a "hybrid technology and business incubator" with an intense customer focus, research now moves one step closer to its user.

To embrace this transition, IBM has redefined the Research Division by:

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30 McQuenny, "IBM's Strategy," p. 26.

31 Ibid., p. 20.

- leveraging intellectual property to protect royalties,
- breaking down academic departmental boundaries,
- shifting a portion of corporate funding of research to funding from IBM business units,
- connecting researchers to customers by assigning researchers to carefully selected customers,
- opening up to the outside through licensing or selling technology on the open market, and
- increasing the flow of ideas by locating labs near intellectual centers instead of the more remote locations of the past.

By bringing customer needs to the laboratory, and by bringing the best technologists to closer interaction with the business units, innovations become more deliverable to market and innovators are better informed in focusing their research portfolios. This "virtuous cycle"[32] best positions IBM to achieve its objective of outpacing its competitors by two to three generations.

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[32] Ibid, p. 26.

APPENDIX—IBM AND X-RAY LITHOGRAPHY

Vashisht Sharma and Richard VanAtta

> For IBM, the true heart of our technical and scientific heritage is in doing research and development that matter. IBM's heritage is in technology that changes how business is done, how states can govern, how students can learn. IBM's R&D finds its ultimate scorecard not in scientific journals, but in the impact it has on the fundamental problems and opportunities that exist in the world.[33]

As a major semiconductor manufacturer, IBM has always recognized the importance of lithography and has maintained a strong program of research and development. Beginning in the 1960s, IBM made many of the most significant contributions to the technology's development, often long before industry adoption. These frequently involved innovations across multiple disciplines. In parallel with its efforts in optical lithography, the main process in use by the industry, IBM has also been a major developer of alternative technologies. After more than 25 years of effort, one such approach—X-ray lithography—finally came to fruition with the production of a fully functional microprocessor.

BRIEF HISTORY OF X-RAY LITHOGRAPHY DEVELOPMENT

IBM's efforts in proximity X-ray lithography began in East Fishkill in 1968 with a small group formed to study lithography tools. IBM had a substantial science effort in X-ray lithography during the 1970s.

Even in 1980, optical lithography was poorly understood. On the basis of historical trends and current difficulties with existing tooling and technology, this approach was not thought to be capable of generating features smaller than about 1–1.25 μm. Further improvement in microelectronic integration would therefore require another approach. Because of the corporation's strategic vision to stay two generations ahead of the competition in memory-chip production, IBM scientists convinced the Corporate

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33 "Sixteen decisions that transformed IBM," *IBM Annual Report* 2001.

Technical Committee in 1980 to establish an X-ray lithography program. This decision was based on research that suggested X-ray lithography would have advantages in resolution, throughput, resist-processing characteristics, and defect rates, with the prospect of attaining higher circuit densities.

Three fundamental advances were necessary: (1) a sufficiently bright X-ray source, (2) masking materials and techniques to protect those parts of the wafer not intended to be exposed to X rays, and (3) a stepper/aligner to position the wafers so that successive irradiations could make multiple chips on a single wafer. In addition to developing a technological roadmap that could meet those objectives, the program also had to identify which processor was to be created with this new technology, determine the appropriate role for vendor assistance, and identify necessary staffing levels.[34]

1. IBM Synchrotron Development

In the beginning, synchrotron development was solely a Research Division program with no representatives from IBM's Technology Division. The program's objective was to develop a system that could be deployed in 1985–1990 for manufacturing—not device prototyping—and that had the same throughput rates as optical lithography.

IBM's initial research was conducted using a beam line on the Brookhaven National Laboratory's National Synchrotron Light Source (NSLS), which provided a sufficiently powerful X-ray source for exposing the resist material.[35] This DARPA-supported effort was particularly important in showing IBM that the technology would work.[36] While working with Brokhaven's NSLS, IBM also began to pursue the development of a synchrotron for its own facility—an ambitious project, of a scale usually conducted only by government-sponsored laboratories. During this period, IBM generated a set of design objectives and specifications for the ring; evaluated several potential vendors (all in Europe); and had experts from industry and its General Technology Division study the physics of the ring proposal and evaluate all its aspects, including tooling and manufacturing. IBM contracted with Oxford Instruments for a

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34 A. D. Wilson, " X-Ray Lithography in IBM 1980-1992, the Development Years," *IBM Journal of Research and Development*, Vol. 37, No. 3, May 1993.

35 DARPA provided support for IBM's use of BNL's beam line.

36 IBM was aware of the Hampshire X-ray point-source tool development, an alternate DARPA-sponsored project, but this tool could not deliver X rays with sufficient continuous power for device production.

superconducting dipole system known as Helios 1, which was completed in October 1990 and shipped to IBM's Advanced Lithography Facility in East Fishkill in 1991.[37] The system reached operational efficiency in 1992, demonstrating the ability to print 0.33 μm lines.

2. Stepper Development—A Cooperative Vendor Effort

At the outset of its development efforts, IBM knew that there were no vendors of ready-made X-ray lithography equipment, so to establish the feasibility of the processes, it would have to develop a stepper/aligner that could position the wafer precisely under the X-ray beam. IBM sought to use U.S. suppliers for such key systems, but when these were not available, it was open to acquiring foreign-sourced technology. IBM developed the specifications and worked with domestic (PerkinElmer) and foreign (Suss of Germany) vendors to develop this tool. Because of its financial problems, PerkinElmer was unable to deliver. Although the Suss system was delivered 1 year late, it performed satisfactorily. After that device's delivery, the IBM team had to surmount many additional technical challenges, such as holding wafers on the stepper, designing and fabricating a new pre-alignment subsystem independent of the stepper, and simultaneously focusing mask and wafers. The engineering teams within IBM's research and manufacturing organizations developed solutions to these problems. The stepper system was a combination of IBM and vendor designs and technology. It was the first X-ray stepper to be used to fabricate complex CMOS devices, circuits, and test chips using synchrotron radiation.

3. Mask Technology and Fabrication

One other key element of the X-ray program was the development of mask technology and fabricating mask blanks at IBM's Research Center in Yorktown and the later transfer of this technology to the IBM Burlington mask facility in 1987. Despite many problems and diversions, management focused on the goal of establishing and demonstrating the key elements of X-ray lithography. The goal was to make devices, not to perfect a solution to every problem. IBM researchers published technical findings in a timely manner, withholding proprietary/trade secrets that were unique. They organized

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[37] Although DARPA's Advanced Lithography Program had supported Brookhaven National Laboratory in developing a compact synchrotron, no U.S. Government funds were used for the Oxford synchrotron at IBM.

themselves by forming a self-appointed, informal team representing the different technical objectives required in mask fabrication: the e-beam patterning tool "owner," the device and mask process owner, and the substrate plating and flatness process owner, along with lab assistants. This informal team was able to deliver 2 complete sets of masks (10 levels each) to the device exposure program over a 1-year period—a remarkable achievement.

4. Status

By 1988 the program had fabricated eight-level CMOS chips with 0.5 μm feature and over 1 inch square, the equivalent of a 64–128 Mb memory chip. This accomplishment was reached before long-established optical counterpart methods had reached the same level. Yields were not 100 percent, but acceptable. The principal goal of the X-ray program had been achieved.[38] The Advanced Lithography Facility and the Advanced Manufacturing Facility (in Burlington, Vt.), both employing the X-ray technology, were functionally operational in 1992. In conjunction with DARPA, IBM fabricated and demonstrated a fully functional 512 Kb SRAM chip (with 100-percent yield) in the early 1990s. This was a key demonstration since it required a defect-free mask, a defect-free X-ray exposure process, and the full function of both the Advanced Lithography Facility and the Advanced Manufacturing Facility.[39] After two decades of substantial R&D, X-ray lithography has reached the point where it can be considered as a potential production capability. IBM, as the major U.S. sponsor of X-ray lithography, has an operating facility. It has announced plans to upgrade and equip this facility to handle 300 mm wafers and to integrate it with a leading-edge pilot line.

The major issues are not X-ray technology itself, but the production economics of its use. A key issue is the cost of the X-ray masks relative to their operational life. Moreover, the infrastructure for supporting X-ray technology as a production technology is not in place. Industry-wide acceptance of synchrotron-based X-ray lithography is impaired by concerns over the additional facility costs that are required. Although this technology is capable of producing 0.1 μm devices, it is unlikely to compete economically with another competing approach, 193 nm optical lithography, at such feature sizes.

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38 D. Seager et al., " Fully Scaled 0.5 micron CMOS Circuits by Synchrotron X-Ray Lithography: Resist Systems and Line Width Control," *Microelectronics Engineering,* 9, 97–100 (1989).

39 John M. Warlaumont, Preface, *IBM Journal of Research and Development*, Vol. 37, No. 3, May 1993.

But extending X-ray lithography below 0.1 μm is also a concern, primarily due to mask issues.

5. The Future of Lithography

At one time, X-ray lithography was the heir apparent to the lithography throne. Others said SCALPEL (scattering with angular limitation projection electron-beam lithography) would be.[40] Although X-ray lithography has been utilized in some very high-end military applications, the mainstream semiconductor market and its supply chain have forsaken it as an evolutionary dead end. Even though certain aspects of SCALPEL-related R&D may prove applicable to other electron-based technology, it has suffered a similar fate. Of the alternative technologies vying to be optical lithography's successor, extreme ultraviolet lithography and electron projection lithography are the current frontrunners. Of the two, extreme ultraviolet lithography has considerably more widespread support. It has been embraced by all three of the major lithography tool suppliers— ASML, Canon Inc., and Nikon Inc.—and the industry research consortiums in North America, Europe, and Japan.[41]

IBM funded and supported the development of X-ray lithography as a radically new technology for making semiconductor devices. At the time it embarked on the technology, the company determined that there would be a need for an alternative to using light—even deep ultraviolet—and undertook a high-risk scientific enterprise in a promising technology. At the same time, the company continued to invest in improving the dominant technology, optical lithography, through its own internal research, cooperative technology development through the government-industry consortium SEMATECH, and partnership with equipment vendors. It also had exploratory efforts in a range of alternative lithography technologies. The scale of investment by IBM in the underlying science and the advanced-technology development for X-ray lithography is unprecedented. The company clearly took a gamble on achieving a unique leadership position

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40 AT&T Bell Labs developed SCALPEL. See J. Alexander Liddle, Lloyd R. Harriot, Warren K. Waskieicz, "Projection Electron-beam Lithography: SCALPEL," *Microlithography World*, Vol.□, No.□, Spring 1997, pp. 15–20. IBM has developed PREVAIL, a projection electron beam lithography system aimed at 100 nm production, which is under development at Nikon. See R.S. Dhaliwal et al., "PREVAIL—Electron Projection Technology Approach for Next-Generation Lithography," *IBM Journal of Research and Development*, Vol. 45, No. 5, September 2001, pp. 615–638.

41 Jeff Chappell, *Electronic News,* 25 February 2002. See also Richard H. Van Atta et al., *Foreign Involvement in the Development of Advanced Lithography: Summary Report*, Alexandria, Va.: Institute for Defense Analyses, IDA Document D-2294, unpublished.

that, if successful, could have given it a fundamental advantage over its competitors. Although the technology proved successful, obstacles in production economics have kept it from becoming adopted as the substitute for optical lithography. First, optical lithography (partially resulting from IBM's own research) has extended much further than almost any experts would have thought 10 years ago. Second, key factors in the X-ray approach, especially in the costs of the masks, make it less attractive than other options. Although efforts to push optical lithography down past the 100 nm level (0.1 μm) are being pursued, it is still evident that within a another few years optical lithography will reach its limits. The alternative technologies—extreme ultraviolet lithography and projection electron beam—are still in early development and may encounter difficulties that may delay their acceptance. X-ray, while now not the favored technology, might again be considered an option. Then again, IBM is working today on an even more extreme technology—sub-100 nm interferometric lithography—for nanoscale manufacturing.

III. DUPONT CORPORATION[1]

Lee Kindberg and William Hong

A. INTRODUCTION

DuPont is the largest chemical company in the United States (number two globally) and now ranks 70th on the Fortune 500. In its 200-year history the primary product lines evolved from explosives to chemicals, then to polymeric materials and fibers. The major research emphases now are biology, work at the interfaces of sciences, and sustainability. Its unusual long-term success is due to strong and consistent adherence to its core values and culture, combined with significant ongoing investment in science and a corresponding evolution in management structure and practices. Factors in this success include the following:

1. *Significant commitment to R&D:* $1.2 billion annually (4.4 percent of sales), over 40 R&D and customer service labs in the United States and over 35 labs in 11 other countries. The Experimental Station, DuPont's primary long-term R&D and science support site, employs over 4,500 people.
2. *Technically trained leadership with significant longevity:* Over 75 percent of senior leaders hold degrees in technical fields, and most have over 20 years with DuPont in technical, manufacturing, and business roles. These leaders are intentionally developed both through training/education and experience in multiple DuPont businesses. Motivation, loyalty, and longevity of personnel at all levels are the norm.
3. *Organizational patience and long-term vision:* DuPont invests significant money and time to develop and commercialize new products and applications in existing or entirely new fields. There is broad recognition of the need for R&D with a range of time horizons, to "keep the pipeline full."
4. *Management systems to support and facilitate effective technical programs:* Key systems include Stage-Gate processes for developments and alliances,

[1] The authors would like to acknowledge the time, information, and insights provided by DuPont personnel in the preparation of this study. Discussions and internal and published analyses and information were extremely helpful in understanding the organization and its changes. DuPont personnel at all levels were very generous with their energy, time, and expertise.

the Apex Research system for the discovery process, a formal Technical Effectiveness Process (TEP) to review personnel and resource allocation and effectiveness, and Six Sigma quality processes. Corporate councils and the annual Technical Conference strongly encourage communications and networking across the company.

5. *Proven ability to adapt to and capitalize on new scientific discoveries and to continuously improve management processes through self-analysis, study of best practices, and innovative leadership.*

B. BACKGROUND

DuPont is a very large and complex global company headquartered in Wilmington, Del., and recently celebrated its 200th anniversary. DuPont now ranks 70th on the Fortune 500, with 2001 revenues of $24.7 billion, net income of $4.3 billion, and a workforce of 79,000 (approximately half in the United States). They operate 135 manufacturing and processing facilities in 70 countries.

DuPont defines itself as a science company, "delivering science-based solutions in markets such as food and nutrition, health care, apparel, home and construction, electronics and transportation." The familiar logo "Better Things for Better Living… Through Chemistry" became "Better Things for Better Living" in the 1980s. The new corporate brand identity introduced in 1999, "The miracles of science," is intended to reflect DuPont's heritage and continued base in science, while including a broader range of sciences.

The development of new business opportunities based on scientific discovery is central to the DuPont vision and mission. The commitment to R&D includes total technical expenditures of $1.7 billion annually, with $1.2 billion of that in Central R&D (CR&D), which has remained constant in spite of the recent economic downturn. This represents about 4 percent of sales. DuPont has over 40 R&D and customer-service labs in the United States and over 35 labs in 11 other countries. The Experimental Station, DuPont's CR&D facility, employs over 4,500 people in intermediate- and long-term R&D and support to business unit technical programs.

Originally, DuPont produced explosives. Their focus then turned to chemicals, polymeric materials, and energy. Today, they are organized around systems solutions to customer problems, with a corporate mission to "deliver science-based solutions that make real differences in real lives." Corporate research today centers around developments in life sciences, sustainable technologies, and the intersections of sciences

(e.g., biology and chemistry). DuPont's success comes from its ability to change while maintaining its core values:

> Our ability to adapt to change and our foundation of unending scientific inquiry enabled this two-century journey to becoming one of the world's most innovative companies. But, in the face of constant change, innovation, and discovery, our core values have remained constant: commitment to safety, health, and the environment; integrity and high ethical standards; and treating people with fairness and respect.[2]

DuPont's continued success is due to this strong and consistent adherence to its core values and culture, combined with long-term vision, significant ongoing investment in science, and a corresponding commitment to continuous improvement in management structures and practices. Although DuPont has had major acquisitions and divestitures (e.g., Conoco, DuPont pharmaceuticals, Pioneer) the DuPont culture and identity predominated and continued intact through these corporate changes.

C. THE BUSINESS MODEL

The content and style of the DuPont Vision reflect the corporate culture and commitment to excellence, science, and continuous improvement. As expressed in its Vision Statement, Dupont's long-term view enables the corporation and its people to take on significant R&D challenges not attempted by other companies:

> We, the people of DuPont, dedicate ourselves daily to the work of improving life on our planet.
>
> We have the curiosity to go farther…the imagination to think bigger…the determination to try harder…and the conscience to care more.
>
> Our solutions will be bold. We will answer the fundamental needs of the people we live with to ensure harmony, health and prosperity in the world.
>
> Our methods will be our obsession. Our singular focus will be to serve humanity with the power of all the sciences available to us.
>
> Our tools are our minds. We will encourage unconventional ideas, be daring in our thinking, and courageous in our actions. By sharing our knowledge and learning from each other and the markets we serve, we will solve problems in surprising and magnificent ways.

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2 http://www.dupont.com/

> Our success will be ensured. We will be demanding of ourselves and work relentlessly to complete our tasks. Our achievements will create superior profit for our shareholders and ourselves.
>
> Our principles are sacred. We will respect nature and living things, work safely, be gracious to one another and our partners, and each day we will leave for home with consciences clear and spirits soaring.

DuPont's strategic focus on S&T as a way to achieve and accelerate sustainable corporate growth is clear. As discussed above, market positioning and the business model have evolved from a product orientation to a focus on systems, market spaces, and solutions to customer problems. This includes some movement toward the 3M model, where a high percentage of sales comes from new products. For DuPont, this means a goal that by 2005, 33 percent of sales will be from products developed or acquired in the last 5 years (in 2001 the goal was 24 percent). In many cases, the primary commercial applications for a product are not those originally envisioned by the inventors or the sponsoring business, but are developed through the depth of DuPont's product and applications expertise and transfer of capabilities or product knowledge to other business units.

Recently, this evolution in focus has included the addition of biological research to DuPont's traditional strengths in polymer science, chemistry, math, physics and engineering. Today, biology (biomaterials, agriculture, food) and electronics represent more than 20 percent of its business.

D. S&T IN DUPONT

1. Why DuPont Does S&T

Science is central to the DuPont vision, tradition, and definition as a company, as is demonstrated by its strategy and allocation of personnel and resources. DuPont has a history of taking on bold visions to develop entirely new fields of study and then converting this research work into practical and profitable applications.

Dr. Thomas Connelly, Senior Vice-president and Chief Science & Technology Officer, defines DuPont's current strategic thrust as "Integrated Science," the leveraging of existing technical and market strengths to build for the future:

> DuPont has tremendous strengths. One that distinguishes us from other technology companies is the breadth of our involvement in, literally,

> everything from biology to traditional materials science to electronics and related applications. More and more we're recognizing that new opportunities are going to come at the interfaces of these technologies. Historically DuPont has been based in chemicals and materials and the related disciplines. For us, Integrated Science means adding biology capabilities to our traditional strengths. I stress that we're adding biology. It's not a question of trading our position in chemicals and materials for biology. It's bringing on that additional capability and then looking for opportunities where more than one science comes together. That's where we'll find our future opportunities.[3]

This vision is shared by DuPont Chairman and CEO Chad Holliday, who states that "No other company is in our league in terms of coming up with technology that leads to a product and then working the complete value chain all the way down to the end of its useful life." In the 15 April 2002 *Chemical and Engineering News*[4] report on DuPont's 200th anniversary, he observed that the company has demonstrated its ability to transform itself time and again: "You can't get to be a 200-year-old company by doing the same old thing year after year."

DuPont also recognizes the critical role played by people, the need to develop its people, and the importance of putting the right people into the right roles. The majority of DuPont senior leaders hold undergraduate degrees in technical fields, started with DuPont in technical assignments, and have over 20 years of experience with DuPont. The vision of individual corporate presidents and Chief Science & Technology Officers has been critical in setting the direction for DuPont. An example is the major R&D effort in polymers (discussed below) initiated by DuPont President Crawford Greenewalt in 1948. Appreciation for the value of science, loyalty, and significant longevity with DuPont are the norm across the corporation.

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3 http://www1.dupont.com/NASApp/dupontglobal/corp/index.jsp.

4 *Chemical & Engineering News* is published by the American Chemical Society (http://pubs.acs.org/cen/).

2. Development of Kevlar for Military Ballistic Protection Applications[5]

DuPont's long-range view of S&T is well illustrated by the development and qualification of Kevlar for military ballistic protection applications. Kevlar is a well-established product for DuPont, with military and civilian protective applications constituting a significant and visible portion of its placement in the market. The Kevlar line has been expanded, refined, and marketed over several decades, during which time the S&T organization within DuPont has evolved and changed considerably. Nevertheless, key elements of company philosophy and approach, which have remained constant throughout the process, were important in the success of the development of Kevlar and its applications.

Kevlar was one of the last major product lines to result from a corporate strategic initiative in polymeric materials instituted in 1948 by DuPont President Crawford Greenewalt. This strategic thrust led to a major expansion in corporate R&D, funded new laboratory construction, and established new university ties. DuPont's strategic goal was the development of "new nylons"—new materials to compete with or replace natural or existing traditional synthetic materials. For example, new polymer fibers such as Lycra were seen as a replacement for rubber, Dacron for cotton, and Kevlar for steel, just as nylon was a replacement for silk and Lucite a replacement for glass. Another late development of this program was Nomex, a replacement for asbestos in some applications.

At the time of Kevlar's discovery in 1964 and later development, DuPont had some 100-plus Ph.D. researchers working on fiber development at the Experimental Station, with 10–15 researchers at any given time engaged in work on the basic aramid chemistry for Kevlar. Once application and commercialization possibilities were identified, some of these CR&D personnel were relocated to DuPont's fiber plant in

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5 Sources for the early history of Kevlar development for military ballistics applications included current and retired staff from both DuPont and the U.S. Army who were present during the ballistic applications development or the subsequent qualification process or who otherwise had knowledge of the events which took place. A particularly helpful resource was Mr. Charles Williams of Sherborn, Massachusetts, retired Division Head for End Items, and lead researcher for 6.2-level technology development at the U.S. Army Natick Research, Development and Engineering Laboratory in the 1970s and 1980s. Mr. Williams worked with DuPont staff during the time that the Army evaluated and eventually qualified Kevlar for use in soldier ballistic and fragmentation protective gear. DuPont resources included Dr. Roger Siemionko, Global Technology Director for the Advanced Fiber Systems (AFS) group, and Dr. Vlodek Gabara, a DuPont Fellow in AFS, with decades of experience in the development of Kevlar.

Richmond, Va., to assist in manufacturing scale-up. This practice of first concentrating R&D efforts at Wilmington, then transferring personnel to production plants during the manufacturing scale-up process, was perhaps a precursor to the present practice of co-locating R&D personnel for a particular business at the plant location.

Kevlar's potential application to military personnel protection began to be recognized at about the same time that the primary commercialization avenue for it (automobile/truck tire reinforcement) was not materializing to expected levels. The fuel crises of the 1970s affected overall automobile and tire sales, increasing the cost sensitivities of tire manufacturers, which encouraged market shifts to less expensive materials such as PET and PBT.[6] According to DuPont, the properties and chemistry of Kevlar were felt to be of sufficient interest to warrant investigation for other possible applications. DuPont marketing and product-development personnel responsible for exploring these new market possibilities were reportedly a significant factor in this success. They were described by Kevlar users as highly knowledgeable and competent, with excellent communications skills.

Civilian applications for personnel protection (i.e., ballistic vests for law enforcement) were pursued before similar efforts were made for military use. The significant then-existing military investment in ballistic nylon systems, combined with the lengthy military specification qualification process, presented significant impediments to adoption for use in the military market.

A history of this period as documented by the National Institute of Justice (NIJ) confirms that early work on Kevlar for civilian law-enforcement applications took place in the early 1970s, followed later in the decade by work on the military side (although U.S. Army interest and participation in the early trials with the NIJ is also documented). While there is conflicting information on who originated the ballistic protection idea, a *Time Magazine* article (1980) states that an NIJ researcher first identified the possibility of using Kevlar for civilian law-enforcement protection. Nevertheless, government participation was initiated early in the applications exploration process.

More than a full decade passed from the initial interest by NIJ and the Army to eventual full qualification and production for military use. That this is much longer than typical commercial product development (e.g., fibers for the apparel market) illustrates

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6 Lee Smith, "A Miracle in Search of a Market," *Time,* 1 December 1980, p. 92.

DuPont's organizational patience in developing markets that eventually justify its investments.

Overcoming the substantial barriers to entry and qualification required significant time and effort, so a common vision of the potential advantages provided by the new technology was needed. This shared vision of the potential for significantly improved functionality encouraged both corporate decision-making on manufacturing capability and work by the key government organizations responsible for testing, standards, and compliance.

Theories of ballistic performance prevalent at that time did not predict that Kevlar would be an ideal material for these uses. After functional tests yielded positive results, additional fundamental research by the Natick scientists helped to identify the mechanism and technical characteristics of Kevlar that provided superior behavior under ballistic impact conditions (resistance to adiabatic heating).

This positive collaborative effort was reportedly a result of previous relationships formed between DuPont and the Army during the development of new nylon forms for systems such as battle dress uniforms (BDUs). These relationships and the benefits accrued through them were said to have carried over into later developments of Kevlar varieties for applications such as the composite helmets later adopted by the Armed Forces to replace steel helmets. This philosophy appears to be concordant with the current trends at DuPont and other industrial companies of tying even relatively fundamental research to end-user needs. In the case of Kevlar, this connection appears to have occurred at an earlier stage than was typical at that time, even though the original chemistry of the fiber was generated within a "traditional" central R&D venue.

In the years since, DuPont has established other applications for Kevlar products, including reinforcement for cables, belts, hoses, and composites; specialty apparel; and a continuing small-volume use in specialty tires.

3. Evolving S&T Management Structure During Kevlar Development

Kevlar's basic chemistry and the challenges of fiber production were addressed by programs at DuPont's Experimental Station; however, the further development leading to commercial products took place in an environment that more closely reflects today's practices for new applications development. Kevlar R&D personnel were physically relocated to be involved in the manufacturing scale-up process, while product-

development teams helped identify new application areas and worked with selected government and private-sector end-users to ensure product relevance to users' needs.

The current R&D management philosophy in the Advanced Fiber Systems (AFS) group is to co-locate technical, marketing, and manufacturing personnel at the production plant to maximize communication and responsiveness. Benefits are mainly cross-fertilization and awareness across organization, technical field, and project lines. Hence, the older CR&D approach (which developed a basic scientific idea, then passed it to the engineering and manufacturing groups) no longer applies within AFS and many other DuPont business units.

Other contributing organizational factors discussed include avoiding negative competitiveness in R&D structures, minimizing distractions caused by organizational restructuring, and providing structures and strategies to ensure appropriate staffing and resource levels. Although internal competition is seen as an effective spur to development in some organizations, AFS technical leaders pointed out the negatives that can arise with parallel process development and internal competition among multiple locations, plants, and technical groups. These may be exacerbated by corporate downsizing and the resultant competition for dwindling resources.[7] Frequent or superficial restructurings are also common in industry, especially with leadership changes, and may divert technical and development personnel from their main focus. Structures and strategies to provide technically appropriate personnel levels[8] and resources were considered to be particularly important. Today DuPont uses a portfolio of "Stage-Gate" processes (described later in this chapter) to rank, allocate resources for, and, if necessary, kill or shelve projects.[9]

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7 The situation as described here appears to parallel that seen in NASA over the last decade. It is characterized by the presence of multiple competing entities (i.e., the separate NASA Research Centers), which often compete for Agency-wide resources in a time when the aggregate of those resources is in decline. An example was the establishment of rival Research Center groups to work a specific technology (e.g., propulsion concepts and aerospace technologies) during a time when NASA aerospace budgets decreased by 50 percent. AFS leaders emphasized that the result of such an approach is more likely to be less rather than more, with highly destructive effects on personnel morale.

8 Technical leaders agreed that assigning a researcher to more than two major projects can reduce effectiveness.

9 DuPont's well-developed systems to retain, document, and retrieve detailed project information support this capability.

4. Organization for S&T

DuPont's SBUs include about 100 different businesses, ranging in sales quantities from tank car to micrograms. These SBUs are organized into five Strategic Growth Platforms plus the DuPont Textiles & Interiors Business. The current S&T organization is highly decentralized into the business units for short- and intermediate-term programs, and centralized for major new long-term programs and specialized support services.

The CR&D units are primarily responsible for programs in the 5+-year time horizon, many of which do not support existing business units. Support services provided by CR&D include information-technology and information services, engineering/ productivity, and major analytical capabilities. In addition, an internal consulting services unit located at Chestnut Run (the corporate technical service and applications development center) provides management process consulting, training, and support to all business units and centralized functions. Funding for these services is a non-optional component of corporate overhead (by means of allocations to business units), which pays for CR&D facilities, some centralized services, and non-SBU-related long-term programs. CR&D personnel are located at CR&D sites around the world and report to the Science Directors, who then report to the Chief Science and Technology Officer (Dr. O'Connelly).

SBU R&D personnel are now often physically and organizationally located with business unit personnel at SBU manufacturing, technical, or management sites rather than at CR&D in Wilmington, Delaware. The SBU prioritizes, manages, and funds its own internal R&D work and is held responsible for results based on the business plans. This model requires the following:

1. The need for vigilance to ensure that long-term projects receive proper emphasis,
2. The need to maintain current knowledge and connections to the outside scientific community, and
3. Structure focused on one business makes it more difficult to share technologies across business units.

DuPont S&T management also expressed concerns with a perceived disconnect in the 3–5 year horizon ("Horizon II"), and possible loss of research documentation input and quality with decentralization.

Note that not all business units have adopted this model at this time. The structure in the Packaging and Industrial Polymers (P&IP) group has reportedly been more traditional, where the technical staff at the sites reported to the manufacturing groups, and the technical groups at Chestnut Run reported to marketing personnel. Hence, during the time discussed, no advanced technical work was being done at the plants, and only 15 people at the Experimental Station were assigned to support the business. Therefore, most technical assets were focused on immediate problems, and no critical mass of expertise was available to work on longer term growth of the business. This also made recruiting of new technical personnel to the sites extremely difficult.

Overall, the current state of R&D evolution at DuPont might be summarized as a trend toward developing more research at the Business Unit sites (vs. the previous state of primarily development), and migration of some research activities from the Experimental Station and Chestnut Run to the plants. Although some businesses are still not integrated like the AFS group, the businesses that are doing well no longer use the older model.

Overall regard for CR&D is also reported to be improving. AFS technology leaders reported that "5 years ago, some Business VPs would have happily shut down CR&D." Now it has been determined that CR&D is needed, just as outside technical societies are needed. They also indicated that DuPont felt industrial and government lab research could not and should not reproduce the academic research environment. DuPont therefore maintains long-term relationships with academia for pure science, partnerships in R&D work, and recruiting future employees.

5. Outside Alliances

Several DuPont managers commented that in the past, DuPont's reputation was that Dupont only commercialized and produced what DuPont invented. Today, many more concepts and businesses may also be acquired or developed through partnerships.

For extremely long-range, or "blue sky," work, DuPont has long-term alliances with major universities (e.g., MIT, North Carolina State, and University of North Carolina/Chapel Hill). The President of MIT has served on the DuPont corporate Board of Directors since 1993. Academicians and outside researchers are invited to sit on the Apex Board and Technology Stage-Gate (TSG) review panels. Their purpose is to provide communications and resources from outside DuPont, ensure best current knowledge, and question assumptions.

Joint ventures and alliances with outside companies are also now more common as DuPont expands into new scientific fields and into countries in other parts of the globe. These alliances may be managed through Stage-Gate processes.

6. Management of S&T

a. Technical Effectiveness Process

A TEP is required across the corporation. This corporate process employs a group of analytical and database tools to determine and evaluate the resources applied to development projects. Managers can review the data in any number of crosscutting forms to evaluate the appropriateness of staffing and resource levels vs. strategic priorities. Marketing, finance, and technical input and involvement are required. The purpose of this process is to ensure that appropriate levels of resources, staff, and business function support are assigned, with no "pet projects" taking up too much effort. The database is closely held and access-restricted to ensure confidentiality. Annual TEP reviews of all technical programs are required, and Technical Directors/VPs must present their TEP analyses to the chairman of DuPont. The corporate impact was described as "tremendous," allowing immediate identification of fragmentation of people and improper assignment of resources.

b. Stage-Gate Technology Management Processes

Business unit managers may choose from a portfolio of project best management practices to be employed, including TSG processes and the traditional Stage-Gate process that are *recommended but not required*—SBUs are free to customize processes based on their business needs. The structural freedom allowed any particular SBU appears to depend on producing planned financial results and successful implementation of business strategies. An internal consulting group (DuPont Consulting Solutions[10]) is available to assist business units in designing and implementing appropriate management structures, providing services which would otherwise require expenditures on outside consultants. The internal group is already familiar with the organization, people, policies, safety philosophy, and product stewardship approaches, saving both time and money. External consultants are also used as needed to explore or provide training on new tools and processes or address specific SBU issues.

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[10] http://www.dupont.com/consulting/consulting/rma.html.

CR&D and many Dupont SBUs have incorporated Stage-Gate[11] approaches into their technology management processes. These management approaches build on the concept that there are natural stages and decision points encountered in any development process or project by establishing methodologies for management review and decision-making at these critical points. DuPont reports successful use of a variety of Stage-Gate processes for short- and long-range R&D, technical and business development, and outside alliances and partnerships.

We found that Stage-Gate management processes were highly recommended for managing projects with specific intermediate decision points, regardless of whether the organization is commercial, governmental, or nonprofit, and whether in-house or outsourced/contract. Programs managed by such processes appear to be more robust, being structured to withstand changes in personalities, management, and organizational structures. They also have the advantages of clear definition of resources and expectations, early and ongoing business participation, and structured revalidation of assumptions. As Ross Loeser, DuPont Fibers PACE Manager, said, "Anything you do frequently that can be divided into phases can be managed with this approach. If it is complex, you need this rigor."

Traditional Stage-Gate structures were initially developed to manage the product commercialization process, from demonstrated concept through production trials, customer qualification, and scale-up to commercialization. Traditional Stage-Gate processes are used to manage the discovery and concept-demonstration process in many DuPont business units. Three examples of these processes are discussed below.

c. "PACE"—Managing New Business Development at DuPont Fibers[12]

A traditional Stage-Gate process called "PACE" is used in the DuPont Fibers business to manage both new product development and formation of business alliances. PACE, an acronym for Product and Cycle Time Excellence, is designed to manage product development, business development, and business alliance processes. It is not intended to manage invention or discovery, but to take that discovery to commercial

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11 R.G. Cooper, *Winning at New Products* (2nd Ed.), Reading, Pa.: Addison-Wesley, 1993.

12 From interviews with Ross Loeser, DuPont Fibers Division PACE Manager and coauthor of articles cited previously on "Better New Business Development at DuPont," *Research–Technology Management, 2002*.

product/application. Different business units use a variety of front ends to manage the invention process (examples below).

PACE is described as appropriate for processes in which there are natural stages and decision points. An example is the new demand for antimicrobial characteristics in textile applications. Here, PACE was used to manage the evaluation of the available antimicrobial technologies for a variety of applications and production concepts and to carry them through to commercialization. PACE was also successfully used to manage development of new alliances in China.

DuPont purchased the PACE process from PRTM,[13] a consulting firm that spent extensive time with DuPont for over a year, training all levels of management and enforcing the process rules rigorously.

Upper management has accepted and maintained the PACE system unusually well. This was felt to be due to the strong tie to financial results, the training, positive results to date, and the clear lines of control. Note that GE also uses this system, and a number of upper DuPont managers came from GE. This is part of a transformation of DuPont to innovate and grow faster and increase revenue and margins.

The Six Sigma Process, an organized, fact-based Total Quality Management approach which is also heavily used in GE, is used to reduce costs and is now being used to improve working with customers and even to redesign pricing structures. Six Sigma practices are compatible with and often incorporated into the PACE process. Successful implementation and continued use of this approach required significant training and front-end investment in time and resources, enforcement of the process rules and structures, and upper management support of the process. An explicit effort was required to change the operational culture during the implementation period.

When a new concept enters the PACE process, a team that includes all essential areas needed to carry it through to commercialization—technical, operations, marketing, etc.—is assigned. Thus, the key inputs are involved from the beginning. The PACE team leader spends a high percentage of his/her time on the task; how much time other team members are allocated for this work varies with changing stages and needs. An extended team includes support contacts from resources areas such as safety, health, and environmental.

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[13] www.prtm.com.

The PACE teams receive resources and authority from a Program Approval Committee (PAC) composed of members from all key disciplines who can commit resources and provide direction to the PACE teams (typically business Director or VP level). PAC review is required at each Stage-Gate. PAC approval to move to the next stage includes an agreement (internally called a "contract") that defines time, deliverables, and resource commitments and that empowers the PACE team to do the work. This contract carries the project to the next anticipated Stage-Gate review, and interim reviews are rarely required unless the PACE team concludes that it cannot meet the contract.

The DuPont Alliance process now uses the PACE process. White Papers are required at each Gate, including market assessment, gap analysis, and justification for the recommended alliance. The overall China Joint Venture has a special PAC. In a global company, many alliances fail, often because of cultural differences (both organizational and geographic cultures). The structured PACE process was thought to reduce such misunderstandings through a clear definition of expectations.

Team and employee incentive systems are tied to SBU results and to successfully achieving PACE goals. The DuPont PACE Manager (who is also a Six-Sigma expert ("Master Black Belt") and a Senior Examiner in the Malcolm Baldrige Quality Award program) said that, in his experience, "while financial rewards may provide incentives for some upper-level managers, most workers are more motivated by empowerment, recognition/appreciation, and the desire to do a good job."

d. TSG—Managing the "Fuzzy Front End"[14]

The invention process has been described as the "fuzzy front end," expressing the inexact nature of the invention process. The PMDA Toolbook[15] describes the process from initial concept to commercial product as a three-phase process:

> The innovation process may be divided into three areas: the fuzzy front end (FFE), the new product development (NPD) process, and commercialization.… The FFE is defined by those activities that come before the formal and well-structured NPD process. Even though there is a

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[14] Technology Stage-Gate processes were discussed in detail with Dr. Greg Ajamian, Senior Project Manager, DuPont Consulting Solutions, and co-author of Chapters 1 and 11 of Paul Belliveau et al., eds., *The PDMA Toolbook for New Product Development*, New York: John Wiley and Sons, Inc., 2002.

[15] Koen, Ajamian, et al., "Fuzzy Front End: Effective Methods, Tools and Techniques," Chapter 1, Belliveau et al., eds., *The PDMA Toolbook for New Product Development*, cited previously.

> continuum between the FFE and NPD, the activities in the FFE are often chaotic, unpredictable and unstructured…The FFE is generally regarded as one of the greatest opportunities for improvement of the overall innovation process. Many companies have dramatically improved cycle time and efficiency by implementing a formal Stage-Gate® or PACE® approach for managing projects in the NPD portion of the innovation process. Attention is increasingly being focused on the front-end activities that precede this formal and structured process in order to increase the value, amount and success probability of high-profit concepts entering product development and commercialization.

DuPont business units use a variety of front-end processes to manage this invention process, many of which are modified Stage-Gate processes, referred to as Technology Stage-Gate. DuPont has found that research programs benefit significantly from periodic review by and support from experts and resource personnel and from periodic revalidation of assumptions. Thus, the makeup of the Technology Stage-Gate review team is different from the traditional Stage-Gate review team. The traditional Stage-Gate team is made up of Business Team and senior management, and measures are typically timing and financial. The purpose of the Technology Stage-Gate review is to provide expertise, so the team has a more technical composition and focus (Is the science right, asking the right questions, making the right assumptions?). The Technology Stage-Gate Review team often includes experts from other parts of the company, DuPont Fellows, or professors doing grant work for DuPont. Progress measures are defined in terms of cost and mission criteria instead of the ROI measures typically used in commercialization projects.

The final stage(s) of the Technology Stage-Gate process overlaps the initial stage(s) of traditional Stage-Gate processes, making the transition from innovation to commercial use flow more smoothly. Technology Stage-Gate approaches have been used successfully with outsourced development work and included as part of contracts. This formal process reportedly "makes life clearer and easier on both sides." Inclusion in the project management list for internal groups and vendors ensures communications when both use the same management process. It was also noted that there are some differences in proprietary developments, where the company owns the patents and the science and contracts work for invention; however, the Technology Stage-Gate process still adds value.

Technology Stage-Gate processes are used in Central R&D, development areas, and in many DuPont business units. Central R&D's Technology Stage-Gate process and

supporting structures are called "Apex." If an SBU in need of a significant innovation chooses to fund the work at CR&D, the Apex process will be used.

e. Apex—"Creating Transformational Growth"[16]

Very long-range and exploratory work done in CR&D is selected and managed through the Technology Stage-Gate process referred to as "Apex." Apex is described as a formal process for portfolio and project management designed specifically for use in CR&D. This corporate-funded program, which addresses high-potential/high-uncertainty research projects, accounts for approximately 15 percent of corporate R&D. The charter of the Apex is, "To establish and manage a balanced portfolio of transformational growth programs aligned with corporate direction." Apex projects may be proposed by anyone in the company; however, successful proposals have a well-defined business focus. Projects are resourced for timely resolution of uncertainties using milestones.

In addition to screening new ideas from traditional researchers, the Apex process is used to manage evaluations of concepts identified by the "Inbound Marketing" team, which was established to seek out opportunities for new technology developments. This group of six people provided 75 percent of the new ideas screened through Apex last year.

The Apex process is managed by the CR&D Science Board, which is led by Dr. Thomas Connelly, Senior VP and Chief Science and Technology Officer. This Board has oversight of CR&D resource allocation, approves program activation and staffing and movement from state to stage, and manages the research portfolio. The Apex programs are managed by Apex Platform Teams affiliated with the company's five major divisions ("Strategic Growth Platforms"). Each Platform Team is chaired by one of the three corporate Science Directors and includes business links and outside experts.

While most TSG programs may have flexible numbers of stages and criteria for movement through gates, Apex is specifically structured in the three stages felt to be appropriate to the discovery process and for screening new inventions:

- feasibility demonstration (designed to have high project throughput rate);
- focused research and value confirmation; and
- intent to commercialize.

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16 Information from a briefing by and discussion with Michael Blaustein, CR&D Planning Manager assigned as Apex Process Manager.

As projects move through the stages, expectations for ties to business and applications and financial analysis are increasingly more specific and structured.

7. Role of the Senior VP and Chief Science and Technology Officer

Dr. Thomas Connelly, the current Senior VP and Chief Science and Technology Officer, was previously Business Director for the Kevlar business, so he brings a business-management perspective to the CR&D organization. His primary goal is to improve DuPont's overall technical-management process. Discussion of the role of the senior scientist indicated that his priorities are strategic vision, championing processes and best practices, people/resource management, and ensuring adequate technical resources. These priorities may be broken down further:

- identify gaps in the technology management process;
- champion development/implementation of systems to address gaps and measure technical effectiveness;
- ensure communication by technical personnel across the corporation;
- lead career/continuity planning for technical populations;
- high-level resource allocations review and direction; and
- influence choices of major programs and long-term strategic direction.

Dr. Connelly is also currently working to reconnect the very long-term (5+ year outlooks) R&D and the very short-term (up to 2 years) operational business orientation. This means trying to grow the 2–5 year perspective on R&D and its impact on the company. Even long-range research is more project oriented now, and there is an expectation that new technologies will be leveraged across business units to optimize product performance and provide solutions to customer problems. Dr. Connelly also instituted a CR&D Board of Directors to manage the Apex process, as well as an "in-bound" marketing group, which seeks out opportunities for newly developed technologies.

Dr. Connelly stressed the importance of his role in representing all DuPont's S&T needs at the Executive Committee level (not just those parts that report to him). He strongly suggested that Chief Science & Technology Officers should "get out more," and stated that he spends his time as follows:

- one-third outside the corporation to understand customers, markets, and technology trends;

- one-third inside DuPont to understand the people, products, technologies, and processes; and
- one-third managing the CR&D organization.

8. Role of the Business Unit Technology Director

The Global Technical Director for an SBU is responsible for the personnel involved in new process and new business development, technical service, and operations/process support, including quality assurance. As discussed above, these personnel are often located physically with the teams they support (e.g., quality with operations, etc.). The Technical Director has responsibility for technical development, career development, and recruiting, as well as the broader agenda of enabling, deploying, and redeploying assets and serving as a conduit to the larger corporation.

9. Structures for Technical Communications Across DuPont

Although the business units have now been combined into five groups, cross-corporate communication is recognized as an ongoing issue. One mechanism to facilitate internal technical communications is the annual internal corporate technical conference, "TechCon," where all the DuPont business units present their own new technologies and have the opportunity to learn about others. This year's attendance also included 25 percent business and marketing staff, to provide the broader technical and marketing perspective (a Connelly innovation). Ten years ago, each technical department had its own TechCon, but it has been corporate-wide for 7 to 8 years. Personnel from acquired businesses like Herberts are also included to help integrate them into the network.

Interaction across the corporation is also encouraged through corporate "councils" such as the Corporate Technical Council (Dr. Connelly, SBU Technical Directors, and CR&D leadership) and a similar Corporate Manufacturing Council. These councils are networked and have periodic teleconferences and meetings (monthly and semiannual for technical directors), so that managers throughout DuPont can use them to look for appropriate connections and people to consult. The Corporate Technical Council is responsible for technology management for the corporation. A highlighted value was personnel development (supported by a simple but efficient corporate-wide database on personnel continuity and succession planning).

Another important resource is the DuPont Fellows Forum, which meets monthly. One of the highest honors DuPont bestows on a very small number of its scientists and

engineers, it acknowledges a career-long history of major contributions to DuPont businesses. The Fellows Forum allows these long-term technical contributors to work together to serve DuPont as a group and to play a role in S&T across the corporation.

Dr. Roger Siemionko, AFS Global Technology Director, said that the philosophy of moving managers to a variety of positions within the SBU and, as needed, across the corporation helps communications and helps the managers develop new perspectives and experiences. This also allows them to bring their current expertise to new areas. It was emphasized that top management must support this policy for it to be effective. Review of upper management career histories indicates that such experience is highly valued in career and continuity planning for promising management candidates. He also stressed that "so much depends on the people involved" and in getting the right people into the right assignments. One example cited was the need to move people both to overcome organizational resistance to change and to align skills with needs for a particular time and set of business conditions.

10. "Market Space" Structures

Maintaining ongoing scientific and technical exchange is difficult but necessary, especially for longer range research and that involving scientists outside the company. Strong focus on assigned product lines makes it difficult to migrate knowledge between business groups. For this reason, mechanisms for "keeping the barriers down" are felt to be essential. One concept being used to address this issue is location of market space units (groups with end-user knowledge and expertise) with technical service groups. In this model, technical centers are established with market-segment knowledge and end-use testing capability; they can be used by any group within the corporation. This reportedly provides much greater interaction across businesses and platforms. An example would be a technical center dedicated to apparel applications, regardless of the nature of the fiber(s) used to produce the apparel (nylon, Dacron, Spandex, Kevlar, blends).

11. Processes for S&T Resource Allocations

The absolute level of funding for CR&D is set by the DuPont Executive Committee and approved by the Board of Directors. This funding level arises from review of the long-term strategy and the recommendations and input of the Apex Board, and it is balanced against corporate business results. Although the 2001 DuPont 10K report indicates that corporate R&D spending increased both in total amount and as a

percentage of sales, this increase was due almost entirely to the pharmaceuticals business unit, which was recently sold. When adjusted for this divestiture, CR&D spending has been relatively flat both in amount spent and as a percentage of sales. Dr. Connelly indicated that he could not justify requesting an increase in spending until he could assure the CEO that current R&D spending is optimized. In 2 years in this position, he has implemented many new systems to drive the organization toward more optimal spending, but feels he has not yet reached this level of assurance.

As discussed above, business team members (Business Director, finance, marketing, manufacturing, technical, etc.) are heavily involved in resource allocation for business unit S&T work, and they are held accountable for their results both through business results vs. strategies, and the TEP. Some business unit personnel are involved in decisions on longer range programs. Resource allocations are reviewed at SBU and top management levels through the TEP, and they are managed at natural decision points through Stage-Gate processes such at Apex and PACE.

IV. THE ROCKWELL SCIENCE CENTER

Peter Cannon[1] and Lee Kindberg

> If you want to do something different, you must in fact do something different.
>
> Peter Cannon, VP & Science Center Director

A. SUMMARY

In the 1960s, North American Aviation and Rockwell Standard merged to become North American Rockwell. With subsequent mergers and acquisitions in the 1970s, the firm became Rockwell International. A legacy of the initial merger was Rockwell inheriting North American's research laboratory. The company inherited a research laboratory as part of an acquisition. Although over one-quarter of Rockwell's sales volume was in science and technology, at the time of the merger, the laboratory was less than 1 percent of the size of the firm and was dedicated to longer term research not directly tied to the businesses. Rockwell transformed its central S&T activities with tremendous benefit to both the company and its U.S. Government clients [(e.g., NASA, Department of Defense (DoD)].

In the period 1970–1990, the Science Center was brought into working partnership with the company's various businesses. Other corporate changes during this period included a radical series of acquisitions and divestitures, the Rockwell family's exit from active management, fulfillment of major national programs for which the company held prime responsibility, a fivefold expansion of sales volume, and a tenfold increase in the worth of the firm.

The key points of the case involve correcting the near-oblivion of the lab, building partnerships in an entrepreneurial environment, the codification and acceptance of

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[1] The primary author, Peter Cannon, served as vice president and director of the Rockwell Science Center from 1976 to 1989, the period covered by this study.

funding and management differentiated by type of business served, and finally, the emergence of the lab staff as a significant source of strategic planning and advice at the highest levels. Other significant learnings from the case include the following:

1. Individual leadership and interaction with the business units were critical in the transformation. The Science Center director and his subordinate Chief Technology Officers (CTOs) were given great freedom and responsibility and strong top management support.
2. A Strategic Technologies Advisory Committee (STAC) composed of the senior technical officers of all the major corporate segments provided significant guidance to the Science Center and ensured business unit interaction and support.
3. A long-range research and planning function is an important factor in avoiding operating unit overreach or unrealistic acceptance of emotion as real demand. Rockwell Science Center established and led a highly inclusive corporate approach to planning that provided routes for gathering concepts, needs, and opportunities from the entire corporation.
4. Competitive tension between the Science Center and the Rockwell Electronics Research Center required continual renegotiation of division of responsibility, but enhanced the competitiveness of both centers.

During the study period, Rockwell was a multi-industry company, with one of its significant roles being that of a major defense contractor. This role makes it unique among the private firms studied here. The Rockwell Science Center case demonstrated that interpretation of mission, preparation of action plans, and responsibility for their execution can be successfully assigned to a contractor firm that is willing to commit major competence and S&T resources. This required trust, built through personal integrity and long-term intense management cooperation, between client and executor. The role of key leaders of defense contractors in implementing and sometimes influencing or helping set national policy is significantly different from the typical corporate responsibility.

B. HISTORIC REVIEW OF ROCKWELL INTERNATIONAL

Like most conglomerates, Rockwell International was an "engineered" company with a strongly entrepreneurial corporate style and reliance on abstract, financial metrics, rather than detailed control of businesses and the laboratory. This was particularly true before the departure of the Rockwell family.

The Science Center was originally formed in North American Aviation (NAA), in the 1960s, as NAA attempted to diversify, moving away from mostly government businesses. NAA had launched propulsion, guidance and space-oriented businesses as outgrowths of the intercontinental ballistic missile (ICBM) program. The president of the firm felt that these ambitious innovations did not sufficiently provide for the future, and so the Science Center was established to provide a science base independent of immediate application requirements.

Also in the 1960s, NAA meged with Rockwell Standard Company to form North American Rockwell. Growth in the early 1970s included acquisition of Collins Radio (intending to enter the computer business) and merging interests with Rockwell Manufacturing Company and various machinery businesses to become Rockwell International. By 1973 the company had a sales volume of about $4 billion and had assumed responsibility for several significant national programs, including the B1 aircraft, the Space Shuttle, and essentially all ballistic-missile guidance.

The business model and corporate strategy were defined in terms of financial goals, and major program goals were very much the business of the individual divisions. Although most divisions in the aerospace and electronics businesses sustained vigorous advanced product-engineering programs, there was little research within the divisions. One exception was Autonetics' satellite and ballistic-missile-guidance research, which was vital to the ICBM and submarine ballistic missile programs. An integrated and secure approach to innovation in these fields demanded a captive research function at Autonetics.

North American's corporate research laboratory, the Science Center, was a novelty in the culture of the Rockwell conglomerate. By 1970, the Science Center had existed for less than 10 years and had developed few strong relationships with individual business divisions. Top corporate management was focused on operational integration and financial control of the very diverse business activities, and the Science Center received little strategic interest or concern.

In the early 1970s, the extensive corporate expansion began to adversely affect cash flow. In 1974 and 1975 the company experienced a cash crunch, and severe measures were taken to protect the ratings of the company's debt. These were successful. During the 20 years covered by this study, the company grew over 400 percent in turnover, vastly increased net income, and the stock value appreciated about 800 percent. The financial strategy was a renowned success; the firm's debt attracted the highest

ratings. This dynamic business environment provided remarkable autonomy for the Science Center, which is still reflected in the style of Rockwell Scientific today.

The Rockwell family left active management of the firm in 1979, and the highly individual and entrepreneurial style was effectively modified to emphasize execution and fulfillment of the significant workload.

Toward the end of the period, there was also a strategic shift. The period had included successful completion of the Apollo program, successful execution of the delivery of the B1B long-range aircraft, the successful survival of Rocketdyne as the remaining producer of liquid-fuel rocket engines in the Western world, the provision of ICBM hardware up through the independent multiply targeted re-entry vehicle, and prime responsibility for the space shuttle system, among many others. By the late 1980s, it was recognized that most of these programs were through their peak procurement. By the close of the B1B program, the major strategic decision to liquidate much of the firm was public knowledge. The results of these actions required that the Science Center be transformed into a vastly different operation, which is now known as Rockwell Scientific.

From 1990 to 2000 the principal elements of the company were established as separate public corporations. Because the employee savings plan owned 38 percent of the equity of the parent, this amounted to giving the company to its employees. Today's Rockwell-named companies are the automation company based on the old Allen Bradley assets in Milwaukee and Rockwell Collins in Dallas, which has returned to its traditional radio and avionics businesses. These two companies are completely independent of each other and trade separately on the NYSE, despite sharing the Rockwell name.

Rockwell Scientific is now an independent corporation owned by Rockwell Collins (50 percent) and Rockwell Automation (50 percent), Rockwell Scientific acts as the core S&T competency organization for its parent companies while serving other customers, including Boeing and the U.S. Government. About 18 percent of Rockwell Scientific funding comes from Rockwell Collins and Rockwell Automation. The rest of Rockwell Scientific funding comes from government and commercial contracts. Rockwell Scientific has about 450 people, with 230 people working in imaging—the main focus of Rockwell Scientific research.

C. THE CORPORATE ENVIRONMENT

During the study period the company's revenues grew over threefold, and the firm went through ownership and management changes. Volume increased from $4B to $14B. At the beginning, the Rockwell family was extremely important in management, contributing the chairman and almost two dozen Rockwell family members as executives. Over time, almost all family members had left the firm, and they were replaced by professional managers who had previously worked for Rockwell.

Cultural, intellectual, and geographic diversity were also issues. The Science Center and the Electronics Research Center were both on the West Coast. The managing family had been located in Pittsburgh, Pa., where they enjoyed substantial civic recognition. Management control was largely affected by continuous movements of managers on 2-day control assignments between East and West Coasts. In addition, the control requirements, style, and discipline of accounting were different for the commercial and government businesses. It was a complex company in which persons of great energy, curiosity, resourcefulness, and prior experience were needed for effective management.

Rockwell's slogan, "Where Science gets down to Business," expressed the foundational nature of science and was helpful in the transformation of the Science Center. As a NASA prime, a serious commitment was required to commercialize as much U.S. Government-paid technology as possible. This required a mechanism to identify those elements of technology that could match known needs in the marketplaces. Most of these were electronic or software related, because it was much more difficult to commercialize the real successes in heat-transfer methods that were so important in space work. There was no reserved scope for the Science Center in this work; there was some sense that the whole company was involved in this "Technology Transfer," which was very active from 1970 to 1980.

D. THE ROCKWELL INTERNATIONAL SCIENCE CENTER 1970–1990

This laboratory was created in the 1960s as the North American Aviation Science Center, with a management team first drawn from Bell Telephone Laboratories. An elite group of senior individual contributors provided leadership in technologies considered important to the company. These included nuclear and theoretical physics and electronics and excluded life sciences and most applications of materials sciences.

In 1970 there were about 150 staff members, and funding was 100 percent from corporate funds. Output expectations and style were similar to that of Bell Labs, with an emphasis on pure and applied physics. North American had merged with Rockwell, and the combination had bought Collins Radio to form Rockwell International, but these strategic moves did not yet affect the Science Center.

By 1972 the new conglomerate had started to experience cash-flow issues, and the laboratory was asked to develop its own funding sources using government contracts. The senior Science Center management was replaced by entrepreneurial-minded people, some of whom were not scientifically trained.

During the financial difficulties of 1974–1975, a further proposal to close or sell the Science Center was not executed, reportedly due to objections by the former President of NAA, who had established the Science Center. Senior jobs at the Center changed hands frequently as limited interdivisional work bargains were struck with the aircraft and space divisions of the company. The Center was operating below the $5 million total cost level with about 200 employees. Interdivisional Work Authorizations (IDWAs) were financed from the independent research and development (IR&D) funds of the airplane and rocket businesses; funding on joint contracts was yet to come. The defense electronics business had established a separate research laboratory (the Electronics Research Center) to focus on missile guidance and miniaturization (also about 200 workers).

In 1976, corporate VP Peter Cannon, formerly of GE, was persuaded to accept general management responsibility for the Science Center. He negotiated a delegation of authority that permitted him to proceed with minimal supervision from the company to heal the business issues at the Center, or failing that, to close the establishment. He was allowed an indefinitely large capital budget and freedom to replace people but no prompt expansion of the workforce.

Initial assessment of the performance of the management teams and review of relationships with the divisions and selection of strategic technologies led to replacement of half the Science Center managers, and at the end of a full year all remaining managers were replaced or reassigned. The replacement team included trusted internal personnel, people from the divisions, and some from General Electric.

Actions intended to make the Science Center credible as a business partner in contract work created tension between the Science Center and the Electronics Research Center. Negotiations resulted in dividing the field, with the Science Center's interest

primarily in optoelectronics, electronic imaging, and very high-speed circuitry, leaving topics like very large-scale integration in silicon to the Electronics Research Center. This division of responsibility was continually renegotiated over the period, and was never really comfortable, although the effect was to enhance the competitiveness of both organizations.

The new Science Center director took a direct role in marketing programs, paying close attention to the personal representations made to NASA and the Department of Defense. He personally represented the Science Center to the operating divisions of the company, modeling his role on the General Electric liaison scientist model, which reflects McKinsey partner behavior. (The liaison scientist calls regularly on client executives to provide information, solicit needs, communicate developments, and develop relationships to allow staff entrée to the executives' staff. The liaison scientist travels extensively and is expected to understand the executives' business plan in detail. This is sometimes resisted because of classification issues.)

Internal division executives were also lobbied for the Science Center's budget for the next year, and for support on major capital commitments (which were approved only at the general manager level). Research was committed to new families of infrared (IR) focal planes, a topic important in strategic surveillance, but for which the satellite systems business was not properly staffed. A program of support in computational fluid dynamics for high-maneuverability air vehicles was launched, and the Science Center reinforced the user science base for the materials science programs supporting the aircraft business in areas including the performance science of organic composites.[2] Early in 1978 these actions enabled an approximate $20 million Science Center budget, about twice that of the prior year, and useful results had started to flow to the divisions of the company and government customers.

Further redistribution of programs between the Electronics Research Center and Science Center in 1979 led to reassignment of gallium arsenide optoelectronics programs to the Electronics Research Center. In return, the Science Center was enabled to expand its work in fluid dynamics, launch a program in computational science, and expand its imaging work. In addition, the Science Center received major funding for studies of behavior of exotic alloys in turbomachinery. A 60-percent facility expansion was

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2 Selection of these particular technologies as initial changes was based in part on systematic absences in national competence observed by the Science Center director in a GE assignment 10 years earlier.

authorized (new building), and the Science Center continued to receive extremely liberal allowances for capital equipment.

Internally, the Science Center was now regarded as an important asset and its survival was assured. The Director was urged to expand the use of the Science Center in the commercial businesses of the company. A differentiated approach was developed for commercial business, where pre-project expenses and the negotiations of content and cost contributed to a high overhead load (three times the equivalent government contract cost). The Science Center sought a better way of launching R&D work for the commercial businesses. At this time, an aphorism known as "Cannon's Canon" was coined, namely, "If you want to do something different, you must in fact do something different." This was used successfully to encourage action.

Following the departure of the Rockwell family interests from the company in 1979, a STAC was inaugurated, consisting of the senior technical officers of the major segments of the Company. The STAC advised Science Center management, and STAC concurrence with the Science Center's budget and personnel actions was required.[3] This participatory management was a departure from the line style common among government contractors.

The Science Center doubled in size in the following decade and became an important executing organization for several agencies and commercial businesses. Science Center staff and management testified to Congress and provided technology appraisal briefings at the head of agency level, including the DCI. The director provided a regular "State of Science and the Science Center" brief to the Board of Directors, the Executive Committee and division heads, and management group sessions globally. This communication work was critical in providing ongoing justification for the Science Center. It was further supported by a private occasional journal, "The Sciences at Rockwell," which reported work from across the firm.

The Science Center was fully integrated into the management responsibilities for a number of major programs, including close involvement with NASA and Rockwell's space division and NASA in the shuttle programs and overarching responsibility for the Department of Energy national nuclear schedule (Rocky Flats contractor, and bid to manage Oak Ridge). The Science Center had access to many compartmented programs.

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3 The working of this group, and the management of the diverse interests, is a major subject in its own right.

The quality of the Science Center's work was reflected in Collier awards, as well as National Academy status for a number of the company's staff members. The Rockwell Science Center was widely thought to be several times its actual size, based on output. The prime public record of science achievements is the series of management communications, "The Sciences at Rockwell." In specific cases, early funding was provided by the company, closely followed by liberally written contracts from special mission agencies. Later, BAA type funds were available in competition.

In early 1986 the director was named Vice President Research and Chief Scientist of the corporation, and the Science Center General Manager role was delegated. The Chief Scientist concentrated on the relational aspects of the assignment, including communication between the chairman's office and that of the President of the United States. He wrote on science policy and metrics and traveled, opening an office in Tokyo, and ensuring the success of the Science Center Palo Alto office at Stanford. Somewhat to his chagrin, he was seen as the "Conscience of the Corporation." He continued successfully in this role for 3 years before leaving in 1988 to become CEO of a Silicon Valley company.

1. Science Center Funding and Management

After 1976 the Science Center was allowed an open capital budget and block approval of budget limits. Detail was supported by a 1-year operating plan and 5-year estimate based on overall company planning. In later years, the STAC participated in allocations, but the Center's estimates were rarely adjusted.

Rockwell senior management gave the Science Center's director and staff exceptional freedom and responsibility, especially in the early years up to 1979. By contrast, Rockwell's Electronic Research Center was significantly more structured. In general, broad and accepted evidence of the onset of maturity at a prototype level resulted in much closer attention from the operations; the Science Center basically ran by itself within its budget up to that point.

By 1990 the Science Center had ongoing partnerships with a dozen divisions and was accepted as a full partner in shared contracts with third parties. This made possible a retained profit at the Science Center, which in addition to a (capped) corporate contribution, gave the Science Center director substantial flexibility.

Early in the development of interdivision relationships, IDWAs were developed to build the required level of trust. These were handled as direct sales relationships with the divisions, with frequent reporting and liaison.

Management relationships were developed outside the firm, particularly with DARPA, where the exclusive, well-equipped attention of the Science Center was key. A successful balancing act to provide exclusivity to each client ensured success. This was achieved by the selection and training of a half-dozen subordinate managers, each of whom was extended great freedom to develop clients. They were supported by a small planning staff of some political and military competence.

In general, the Science Center was chartered to do work that might be expected to mature in 3 to 8 years. This distinction was well accepted, since the tenure of the division presidents was about 5 years, and making them happy to have longer term issues handled by an independent organization. The culture within divisions was and is intensely committed to execution of task, and most practicing engineers and scientist within the divisions were concerned with results within the 1- to 3-year frame.

In the 1972–1990 period, the firm was totally committed to major national goals as well as to a vigorous international commercial expansion. The challenge was to find people and talents to handle the vast amount of work flowing into the company.

2. Strategic and Procurement Planning

The Science Center was a partner in an unusual process to gather input on business strategies and arrive at a coherent corporate research and technology program.[4] Every responsible engineer in the Company (about 25,000) was asked each year to describe his or her technology needs and anticipated sources for the ensuing year and 5 years forward. Interactive conversations on sources and availabilities identified gaps that had to be addressed in the internal research technology programs.

Although a small corporate-level staff (six people) collaborated with Science Center management on the work, the process was intense; it involved road travel and face-to-face review for the team about 90 days each year. After financial interpretation, the consolidated plan was reviewed by the office of the president and chairman and thus became part of the corporate resource-allocation process.

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4 This planning system was described by Peter Cannon in "Integrative Planning and Communication of Research," *Research–Technology Managemen,* Vol. 27, No. 3, May-June 1984, pp. 20–22.

The estimates from the process were regarded as authoritative, but were not considered to be beyond dispute. The active involvement of the CFO and the finance background of the Science Center director contributed to the sustained credibility and success of the S&T program and the process.

3. Why Rockwell Engaged in S&T

Rockwell defined itself as the company "Where Science gets down to Business." It is important to note that the divisions of Rockwell, especially on the aerospace side, regarded themselves as being in the S&T business. Any attempt to discuss the relationship of the Science Center to the divisions without recognizing that the company itself was committed to science and technology will result in false conclusions. Motivations for major commitments to S&T were clear.

At the materials and processes level, the need for technically excellent, low-cost manufacturing processes was well recognized, and proposals to do such work were usually accepted. The Science Center might be selected to provide lead concepts and proof of principle, and some divisions like Rocketdyne depended on the Science Center for such work.

To win a major competition to be prime on a government system, continued inventive leadership must be demonstrated. This work must be done in advance of the major procurement and thus is often reported as R&D. Such work requires not only new ideas, but also proof of principle and manufacturability, and usually takes 2–3 years of risk expenditures, which may be reimbursed concurrently. These expenditures are frequently large when compared with the underlying basic studies, and thus skew financial reporting. For example, the feasibility work on Peacekeeper (guidance only) came in with a $25M/year reimbursement, about the same as the Science Center's budget at the time. These large product or system study expenses can give a false notion of the corporation's commitment to long-range work, and they are frequently seen in innovative work for the government. They do reflect a high degree of risk for the executor, as they represent an uncertain use of very good people. In Rockwell, decisions to commit at this level were reserved for the corporate executive officers.

For advanced systems architecture and engineering, procurement practices frequently split system responsibility from product and subsystem procurement. This leaves system concepts and related software development with uncertain parentage and heritage. In the 1980s, two major research programs aimed at providing a capability for

thought-leader position in such systems work, were brought to maturity in the Science Center. The Science Center clearly steered divisions by doing such studies, which could be seen as examples of a "science" center migrating into a "service" center as it matured.

Client demand for such work highlighted the unusual relationships between the Executive Branch and Appointive levels in government. In at least one instance, Rockwell Science Center became a determiner of national policy on the basis of its "transparent superiority of knowledge."

4. Management of S&T

Rockwell employed two different patterns of S&T management to meet the needs of its commercial and governmental customers. All commercial businesses used relatively standard measures of rates of return and augmentation of turnover to justify product-replacement expenditures. Research was also authorized when underlying technology required change. When market needs were identified by a broad customer base, technologies were developed to be "proven and on the shelf." Such programs required funding for long-term base technology work without an immediate market need. This area functioned like a traditional research laboratory capability-building model, with corporate funding.

On the government side, much depended on approval to prepare for new missions of various systems. Approval resulted in either contract or IR&D allowance funding, sometimes for several years. As these contracts represented sales to the divisions, the entire innovative volume of business on the government side might be considered S&T. By that measurement, the proportion of total sales allocated to R&D in Rockwell in 1973 was over 25 percent.

For some work, it was not clear whether it was the result of government demand or commercial demand. An example was the negotiation about how to handle avionics, after Boeing bought Autonetics and the defense electronics business from Rockwell. The difficulty was resolved internally by observing the allocation of IR&D allowances—if you had them, you were at least in part a government-serving business. IR&D was allocated by finance with counsel from corporate engineering, a balanced division of power.

Since the Science Center was involved in both types of programs, administrative reconciliation was done by the Science Center controller with the Science Center director and the company's CFO. The Science Center was measured on the volume of business,

on its technical content, on the relevance to division business plans (sometimes acknowledged in writing) and sometimes on the management of the overall net cost to the corporation, which frequently was a single-digit percentage number of the turnover of the Science Center.

Transitions of technology from one management to another were managed jointly by the Science Center director and the division presidents involved. Specific detailed plans with milestones and schedules included links to funding.

One critical point in understanding the role and evolution of the Science Center is that partnership relationships between with the business divisions were essential. Little work was subbed completely out of the company. The partner relationships made possible the growth of the company, the survival of the Science Center, and the success of the program. In this, Rockwell International was at least a decade ahead of most laboratory-owning companies.

Incentives to encourage such arrangements included sharing the gross profit from large-scale partnered programs. This income provided a financial flexibility for the Science Center that is absent in laboratories financed by assessment or post facto by the divisions. The residual entrepreneurial climate and the drive for parity of esteem which enabled this approach could usefully be mimicked in more codified organizations.

5. Sources, Selection, and Control of Programs

On the commercial side, new programs originated in a very traditional matter. Since the company was an OEM supplier to various auto integrators, programs in those segments originated almost entirely with the end customer. The development typically involved exploring new manufacturing methods, processes, and materials. The company's role was to execute task as well and as inexpensively as possible, so union sensitivities and plant location were important aspects. In one instance, contract award success depended on moving a whole factory from Michigan to Mississippi in just 90 days. The role of the Science Center evolved, to specialize in methods, processes, and materials innovation, working through the company's customers (typically GM, Ford, and Chrysler) with the consent of the relevant business division.

Program sourcing and origination was significantly different in the government businesses. Historically, such work was handled on a highly entrepreneurial basis, which might hinge on a single individual's ideas or commitment of resources. Charismatic

management style and total emotional and physical commitment qualified one to steer the company's interests.

In the study period, Rockwell assembled portfolios of capabilities for customers cutting across disciplines, while retaining line managers to oversee specific disciplinary activities (e.g., materials, chemistry, etc.). Some personnel were assigned to conduct research in core competency areas Rockwell believed would have long-term payoffs. Rockwell encouraged discretionary spending or research by matching division S&T funding on a 1-for-1 basis. This ratio was negotiated at the highest level, and it was expressed as "percent SC discretionary program choice." Researchers could also submit proposals to the Science Center GM for IR&D funding or seek independent funding.

Given the detachment of the firm's Science Center from the active market, it was clear that a linear process of research, development, test, and engineering (RDT&E) would not lead to new products. Immediately following his appointment, the Science Center director focused on cultivating research contracts to build partnerships with technology-intensive businesses. Particular targets included the unmanned satellite business and expansion of the materials interests of Rocketdyne. There was ready acceptance, and within 3 years this approach had almost tripled the annual volume of the Science Center. This could not have occurred without highly skilled individual researchers who led specific tasks. The Science Center staff included "a few hundred of the world's finest applied scientists," whose skills and expertise were the stock in trade. The active participation of those researchers in planning and execution of the total task had to be facilitated, without allowing them to be overwhelmed with day-to-day minutiae. This required that "administration" was seen as "support."

6. Rockwell Scientific Today

Today Rockwell Scientific is effectively a contract research organization and reportedly conducts little discretionary S&T. Most work is specifically oriented toward meeting a customer's needs, and core competencies are maintained only when customers provide funding for such activities. Rockwell Scientific customers have shifted to out-sourcing and strategic partnerships to access research that is not part of their core business, and they benefit from multiple organizations funding common activities and needs. This allows the parent organizations of Rockwell Scientific to have access to Rockwell Scientific research while providing only a portion of the funding.

V. CORNING INCORPORATED

Gerald L. Epstein

A. BACKGROUND

Corning describes itself as a global, technology-based corporation. Tracing its origins to a glass business founded in 1851, the corporation has pioneered a series of innovative products and manufacturing processes, at least three of which—involving electric lighting, television, and fiber-optic communications—have helped transform modern society. For most of its history, the company's core business has focused on glass technology, through which it has pursued applications as diverse as consumer products (e.g., Pyrex, Corning Ware, and Corelle), construction materials, optical components, chemical-reaction substrates, and aerospace. Along the way, Corning diversified into a number of non-glass-related business areas such as electronics, medical diagnostics, laboratory services, and biotechnology. Many of these businesses (and even some core ones, including the consumer products division that was the source of much of the corporation's public identity), however, have since been divested.

Corning today is a Fortune 500 company (ranked 289 in 2001 with over $6 billion in revenues) that pursues three primary business areas:

- Telecommunications, which produces optical fiber, components, and communications equipment;
- Advanced Materials, which pursues specialty materials based primarily on glass, ceramic, and polymer technologies; and
- Information Displays, which manufactures glass components for televisions, flat-panel displays, and video systems. Corning prides itself on its history of successfully bringing innovative products to market, and it continues to look to technology, product innovation, and its commitment to R&D to underpin its competitive edge.[1]

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[1] Corning Incorporated 2001 Annual Report, p. 1.

B. S&T IN CORNING

From its origins in the 1850s, Corning has emphasized R&D. In the 1870s, through persistent experimentation and involvement with outside scientists, Corning developed railroad signal lenses of a composition and design that represented considerable advances over current practice.[2] In 1908, Corning hired a chief chemist and opened up one of American's first corporate R&D laboratories. This move was controversial within the founding Houghton family, whose senior member—Corning President Amory Houghton, Jr.—worried that bringing in scientists in-house would threaten family control of the secrets of Corning's craft-based operation. However, protecting a firm's advantage through secrecy became riskier in an era when professional physicists, chemists, and engineers were increasingly interacting with industry. Amory's sons Arthur and Alonson argued for establishment of the laboratory, which institutionalized research at Corning and helped establish the firm's "distinctive research culture,"[3] in which

> generations of scientists and engineers were given the freedom to wander down paths where their inquiries and talents took them, although they were also encouraged to stay in touch with developments in manufacturing and production. Many of the inquiries proved to be dead ends. But through the years, Corning researchers made hundreds of scientific breakthroughs and frequently developed their findings into commercial applications.
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> While many of the discoveries were the result of patient, systematic inquiry, others were the result of serendipity, luck, happy coincidences, and interactions among different sets of talents residing in the laboratory, the factory, the executive suites, the community, and an extensive network of professional contacts and associations. The company, time and again, displayed an ability to take advantage of opportunities that presented themselves. Discoveries in one generation frequently led to breakthroughs in another.[4]

By the 1930s, Corning had implemented a research philosophy along the lines articulated by distinguished English organic chemist Edwin Mees, who established Eastman Kodak Corporation's research laboratories in 1913. Mees' "laboratory of the middle way" featured researcher freedom and the free flow of information, together with

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2 Davis Dyer and Daniel Gross, *The Generations of Corning*, New York: Oxford University Press, 2001, pp. 53–54.

3 Margaret B.W. Graham and Alec. T. Shuldiner, *Corning and the Craft of Innovation*, New York: Oxford University Press, 2001, pp. 40–41.

4 Dyer and Gross, *The Generations of Corning*, p. 8.

discipline and an awareness of the company's strategic objectives. At Corning, researchers were free to pursue lines they thought most useful, but the corporation exerted strict control over information. No written communication of any kind pertaining to scientific information left the laboratory without the company approval.[5]

The "linear" model for innovation. World War II's blockbuster scientific and technological achievements, such as radar and the atomic bomb, were brought to fruition through a concentrated application of technical resources. This experience helped give rise to a "linear model" view of innovation in which a fundamental scientific breakthrough would proceed more or less directly through successive stages of development to yield large-scale new businesses. Corning's technical leaders sought to organize the company's technical activities along these lines. They added to their research staff, built new self-contained research facilities, and created a comprehensive organization that consolidated all corporate technical effort.[6] Corning sought to pursue radical innovations, which it called "big hits," by hiring exceptionally creative people and giving them the resources they needed. At the same time, the company was engaged in a major, directed development effort to develop color television bulbs that consumed a substantial share of the companies R&D resources; it was also dealing with technical issues involving its ongoing black-and-white television bulb business.[7] Whereas Corning researchers had once pursued many small projects, of which an unpredictable number would mature to significant business lines, by the late 1950s Corning sought to manage more of its innovations from the top down, concentrating a greater share of its resources on those appearing to have the best prospects.[8]

One "big hit" driving Corning's television business was the development of a centrifugal casting process that permitted efficient mass production of larger bulbs. Perhaps more than any other single innovation, centrifugal casting was responsible for Corning's success in the black-and-white television bulb market. Another "big hit" also

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5 Graham and Shuldiner, *Corning and the Craft of Innovation*, pp. 65, 75.

6 Ibid., p. 222.

7 Ibid, p. 233; Joseph Morone, *Winning in High-Tech Markets: The Role of General Management; how Motorola, Corning, and General Electric have built global leadership through Technology*, Boston, Mass.: Harvard Business School Press, 1993, p. 131. Corning had what it referred to as 125 percent of the U.S. black-and-white television bulb market in the late 1950s and early 1960s: it was the exclusive supplier to American television manufacturers, who, due to breakage, had to purchase five bulbs for every four television sets they made. Dyer and Gross, op. cit., p. 271.

8 Ibid., p. 256.

stemmed, although indirectly, from television research: the invention of a photosensitive glass that could be chemically etched into shapes determined by a pattern of exposure to light. Although never used in production of color television bulbs, which had motivated its development, this discovery led to a series of independent Corning products and additional research efforts. The most important of these, in turn, resulted from an additional—and quite serendipitous—finding. When a sample of this chemically machinable glass was heated, a malfunctioning furnace allowed the temperature to overshoot the intended 600 °C by an additional 300 °C. Surprised to see that the plate still had sharp corners and had not become a puddle of molten glass at that temperature, Corning researcher Donald Stookey removed it from the oven—only to drop it. He was even more surprised to hear the plate "clang like a piece of steel," rather than shatter. The glass had turned into a ceramic which eventually became known as Pyroceram—a material that the company realized was a breakthrough, and one it exploited in products as diverse as missile nose cones and Corning Ware ovenware.[9] Its success reinforced the notion that "a new product with radically superior properties could be expected to create its own markets."[10]

The 1960s. Corning prospered financially during the 1960s. Tom MacAvoy, who was an R&D manager and then a division vice president during that period, recalled that "everything was growing like mad, and we thought we were causing it."[11] However, the firm's success depended on a relatively small number of big successes, such as its television business and Pyroceram. In what has been described as an "unfettered pursuit of the technological 'big hit,'"[12] Corning managers sought to replenish their larder, turning the 1960s into "a golden age for research at Corning. Scientists were given wide latitude and substantial resources, and they were encouraged to pursue their interest—regardless of the immediacy of a financial payback."[13] A "succession of very expensive projects"[14] ensued, with mixed results. While many were failures, some succeeded modestly, and several "laid the foundation for the new businesses that would drive Corning's resurgence 20 years later."[15] Predicting which projects would pay off, of

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9 Ibid., p. 257.

10 Ibid., p. 260.

11 Morone, *Winning in High-Tech Markets*, p. 130.

12 Ibid.

13 Dyer and Gross, *The Generations of Corning,* p. 279.

14 Graham and Shuldiner, *Corning and the Craft of Innovation,* p. 260.

15 Morone, *Winning in High-Tech Markets*, p. 132.

course, was the challenge. "If you want any successes at all," recalled Bill Armistead, Corning's research director during much of this period, "you've got to have the failures too. I could never figure out which was which in advance."[16]

One project that did not succeed at the time but that ultimately paved the way for a major new business line was development of a new safety windshield for automobiles. Building on some 1961 patents, researchers in 1964 developed a new fusion process for making thin glass that did not need grinding or polishing. This was then strengthened to the point where it flexed, rather than shattered, under strain, and it was then shaped into final form. When the window was forced to shatter, it would do so in relatively safe pieces and in a way that preserved the ability to see through it. Despite significant investment in the late 1960s, Corning was unable to interest automakers, who were able to procure an alternative and considerably cheaper form of safety glass from other vendors, and it finally discontinued the effort in 1971. However, this technology formed the basis for Corning's major business since the 1980s, making high-quality class for computer displays.[17]

Retrenchment in the 1970s. The 1970s, however, marked a very different era for Corning—rather than one of growth and optimism, it was characterized by declining profitability, recurring crises, and retrenchment, in no small part due to rising import competition that eventually displaced Corning's television business. Corning's approach to science and technology changed as well. That approach—and the linear model to innovation on which it is based—had provided little incentive for the company to reach outside the laboratory for ideas. In optimizing its search for "big hits," Corning's technical staff's division over time became "absorbed in its own self-generated technologies"[18] and isolated from the technical stimulation the firm had historically derived from the rest of the company (including the factory floor) and from customers.

New business development within Corning took on a "much more selective, focused character."[19] Incoming president Thomas MacAvoy described the situation just before he took office in 1971 as one in which "we probably had overcapacity relative to the total organization in the creative technical part of the business," a situation that "led

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16 Ibid., p. 132

17 Graham and Shuldiner, *Corning and the Craft of Innovation,* pp. 29, 264–267.

18 Ibid., p. 277.

19 Morone, *Winning in High-Tech Markets*, p. 141.

to a great frustration among the operating managers of the company and the division managers"[20] (one of which had been himself). Notwithstanding his own history of having come up through Corning's research laboratory, MacAvoy believed that the company's technical community was shaping company strategy without sufficient exposure to economic realities.[21]

As early as 1968, research director William Armistead had instituted a "technical request system" through which the operating divisions could ask for assistance from, and provide guidance to, the laboratories. In the 1970s, additional management tools were implemented in the lab, including a system that established objectives for research projects and tied them to particular operating divisions and specific products. Economic recessions in the early 1970s and related business difficulties forced Corning to institute layoffs in 1975 that extended to the R&D organization, which had traditionally been spared. Corning began implementing various corporate strategic and portfolio planning methodologies to focus business-development efforts. The new climate in the research labs, with decreased emphasis on long-term research and increased support for development and engineering, frustrated researchers who had been hired expecting to have more control over what they worked on. According to one Corning biologist, the new environment tended "to stifle creativity, since it does not allow for failures and discretionary investigations. As productivity falls in terms of research the control becomes tighter and innovation is reduced even further."[22]

Meanwhile, operating managers complained that they had insufficient influence in R&D decision-making and even less over the corporate engineering staff, which they viewed as too expensive. However, senior corporate management resisted their desire to break up the centralized technical structures, believing that the firm's key technical capabilities—and especially those underlying process technology—were inherently centralized.[23]

Although more tightly focused, and with more attention given to cost-reduction activities, technical developments continued in the Corning laboratories. Two in

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20 Ibid., p. 141.

21 Graham and Shuldiner, *Corning and the Craft of Innovation*, p. 347.

22 Ibid., p. 370, quoting Corning biologist Ralph Messing.

23 This discussion draws on Morone, *Winning in High-Tech Markets*, p. 141, and Graham and Shuldiner, *Corning and the Craft of Innovation*, p. 348, 369–372.

particular led directly to some of Corning's most important future businesses: ceramic substrates for automotive emission-control catalytic converters (Celcor) and fiber optics. Tracing the origins of these businesses illustrates the importance to Corning of building and sustaining its core technical competencies. Doing so shows Corning's approach to achieving technical and competitive leadership.

Celcor. Celcor originated in work Corning had done to develop ceramics suitable for high-temperature gas-turbine heat-exchanger applications. Although this work was originally done primarily for General Motors, Corning executives realized by 1970 that automakers were unlikely to implement gas turbines anytime soon. However, GM expressed interest in high-temperature materials that would be suitable for use in the catalytic converters that they anticipated using to meet prospective emissions regulations. After checking with the other principal automakers, Corning realized not only that there was a need for such a technology, no other firms appeared positioned at the time to fill it.[24]

The company mobilized a high-priority R&D program to develop the appropriate materials and manufacturing process to produce a ceramic substrate—which it named Celcor—on which the catalyst in a catalytic converter could be deposited. In final form, the substrate would be a honeycomb ceramic structure about the size of a coffee can with 200 rectangular cells per square inch and the surface area of a football field. Championed by the highest levels of corporate management over substantial internal resistance, this program soon became the corporate lab's biggest project, consuming at its peak one-quarter of the lab's resources and putting a squeeze on other R&D activities.[25] Researchers and engineers worked on development, production, and scale-up issues to meet the auto industry's deadline for equipping 1975 model cars. This schedule required that production begin by January 1974, which in turn required Corning to begin building manufacturing facilities in 1973—before the production process had been finalized. Process development, testing, facility construction, negotiation with customers, and ramp-up to full production all occurred more or less simultaneously. Once the product

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24 This discussion based on Graham and Shuldiner, *Corning and the Craft of Innovation*, pp. 351–358, Morone, *Winning in High-Tech Markets*, pp. 142–147, and Dyer and Gross, *The Generations of Corning,* p. 323.

25 When R&D head Tom MacAvoy advised company president Amo Houghton of the pressure he was getting from managers whose projects were being sidelined, Houghton replied "Well, Tom, you just tell them that if they can come up with a project that has the potential that this one has, we'll do theirs, too." Morone, *Winning in High-Tech Markets*, p. 144.

was launched, the R&D labs turned to improvements in manufacturing efficiency as well as next-generation product development. By 1994, various generations of the product had generated $1 billion in sales.

Optical fiber. Development of Corning's fiber-optics business was, in a sense, the reverse of Celcor's. Here, Corning had the technology but no expectation of a near-term market. As early as 1966, Corning started to explore possibilities for transmitting information in optical waveguides. At the time, light traveling through 1 km of an optical fiber made of the best glass then available would suffer a loss in intensity of 1,000 db, or a factor of 10^{100}. A practical communications system could not tolerate losses of more than about 20 db/km, requiring an improvement of 98 orders of magnitude. Corning researchers realized that no amount of incremental improvement in optical glass would suffice. Since the late 1950s, a Corning group had studied glass-based lasers—a line of work that at the time had led to "nothing—no business," according to Tom MacAvoy.[26] "But the result was that we had about 20 people who understood quantum optics. We really understood it." Through this work, researchers realized that the intrinsic losses in optical glass were so high that no amount of purification or refinement could reach the required level of transparency. Instead, they explored the possibility of using fused silica, the purest known form of glass, which a Corning researcher had developed a vapor-deposition process to formulate back in the 1930s. By the fall of 1970, researchers broke through the 20 km/db threshold in a fused-silica optical fiber—a necessary, but far from sufficient, condition to make optical communication a reality.

Even when researchers developed a fiber with only 4 db/km loss, telecommunications companies showed little interest. Although optical fiber promised a vast increase in telecommunications capacity over existing cable, AT&T—with a massive existing investment in telecommunications cable capacity and 80 percent of the U.S. telecommunications market—said that it would be 30 years before the capacity of optical systems would be needed. By then, they would have developed fiber optics for themselves. U.S. copper cable manufacturers were similarly unimpressed.

Nevertheless, Corning persevered. It licensed its fiber technology to foreign cable producers to develop components and cabling for actual systems and to encourage demand for such systems in those countries. Recognizing that it was thereby creating

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26 Morone, *Winning in High-Tech Markets*, p. 135. The remainder of the fiber optics discussion is based on pp. 134–136, 148–153, and 186.

potential future competitors, Corning resolved to stay ahead by continuing to advance the state of the art, ensuring that its foreign partners would remain dependent on Corning if they wanted to stay abreast.

Corning pursued a three-part strategy in developing fiber optics. First, it continued to push cable costs down. Second, it pursued continuous improvements in performance (in terms of reducing losses and increasing bandwidth, or the amount of information that could be carried by a single fiber). Third, it built production capacity ahead of demand so that when a market finally developed, Corning would have the capacity to achieve economies of scale and to retain customers who might otherwise switch to utilize alternate communications technologies such as satellite.

After years of continued scale-up and development, what broke open Corning's fiber-optic business was the divestiture of AT&T in 1982. Corning was approached by MCI, which expressed interest in a massive fiber procurement to build a nationwide network to compete with AT&T. MCI's needs were challenging: it wanted a new type of fiber Corning had not developed past lab prototype; it wanted a volume that—despite Corning's efforts to keep ahead of demand—was 50 percent greater than Corning's annual production capacity; and it was willing to pay a price that Corning could meet only by deploying a new generation of production equipment that was still under development. Corning pulled it off and began, for the first time, to recoup its cumulative, multiyear investment in developing fiber-optic technology.

The pace of progress in fiber product and process technology has required Corning to implement seven generations of production equipment in 15 years—each obsoleting its predecessor. Phasing in successive generations required substantial concurrency: while undertaking continual incremental improvement on the current production technology, Corning would be preparing to deploy that technology's successor, while simultaneously developing the next generation after that.

Evaluating opportunities. The magnitude of the "bet-the-company" corporate investments in Celcor and optical-fiber development—particularly during a time of business turmoil—could only be sustained with the active support and involvement of the highest levels of corporate management. Indeed, Amo Houghton—chairman of Corning's Board during these developments, championed both Celcor and optical fiber. Money was tight for other R&D programs during the turbulent financial times in the 1970s. This support was based on faith in the technologies' promise; neither case was very amenable to standard corporate financial and market analysis tools management approaches such as

discounted present value calculations. According to David Duke, the Celcor program manager, "I didn't believe most of them. In fact, in a lot of the big ones that I went through, the financial people cranked out the numbers and we were never going to make any money. But those of us who were close enough to it knew that if you changed two or three assumptions, you could get the numbers to come out any way you wanted.[27]"

"Resurgent Corning" in the 1980s. Despite Celcor's establishment in the marketplace in the early 1980s, Corning's earnings and particularly its operating margin had dropped significantly by the time James Houghton, in the fifth generation of the founding Houghton family, assumed chairmanship of Corning's Board from his brother Amo in 1983. He placed great emphasis on reducing costs, improving productivity, and especially on instituting total quality management. Several lines of business, particularly mature, low-margin operations, were divested.

The abortive safety windshield development, which had been terminated in 1971, proved to be invaluable a decade later as Corning adapted its fusion production process to develop high-quality glass substrates for computer liquid-crystal displays. This business took off with the growth of the LCD display industry in the late 1980s and has continued ever since to be a mainstay of Corning's operations. In its earlier stages, this business was developed with the same philosophy used for Celcor and fiber optics: pursue continuous improvement in product performance and quality; reduce cost by continually improving manufacturing processes; and create manufacturing capacity ahead of demand.[28]

Less successful in the long run were Corning's investments in industrial biotechnology, which it had begun in the 1970s when it discovered that porous glass could immobilize enzymes that could perform commercially significant reactions (e.g., convert corn starch to corn sweetener, etc.). By the mid 1980s, Corning realized that developing a strong industrial biotechnology business would require an investment comparable to what had been needed to develop optical fiber—a price it was not willing to pay, given the concurrent ramp-up in optical-fiber investment and the fact that biotechnology was further from Corning's core technologies. By the end of the decade, Corning was completely out of the industrial biotech business.

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27 Ibid., p. 191.

28 Ibid., p. 174. The description of Corning's business in the 1980's as "resurgent" is also from Morone.

Corning also grew a medical diagnostics and laboratory services business in the 1970s and 1980s, a business line that could be traced back to Corning's development in the 1960s of glass electrodes for medical instruments. This business became quite profitable and had diversified to include operation of medical laboratories. By the end of the 1980s, however, this business line had been spun off into a wholly owned subsidiary because it was so different in nature from Corning's glass and ceramics-based core business.

Organization of S&T. Relationships between the central technical staff and Corning's operating divisions weathered some strains during the early 1980s. At that time, CEO Jamie Houghton was placing high priority on instituting Total Quality Management through the company—an approach featuring aspects such as development of quality measurement systems, increased in-house training, and worker participation in group quality-improvement processes. This program met resistance in the R&D community, which viewed many of the aspects of this approach as irrelevant. (What some may see as an "error" a researcher may see as a serendipitous source of innovation.)[29] With time, the researchers began to perceive some benefit from this approach, including the greater interaction with other parts of the company when researchers would serve on "quality improvement teams."

However, the operating divisions continued to see the central technical organization as too expensive and insufficiently responsive or creative. This sentiment was not unique to Corning, and in response, many firms at that time decentralized either the R&D structures themselves, or their control. Corning, however, continued to believe that specialty glass and materials research warranted centralized, focused attention. The corporate R&D lab formed much of the firm's institutional memory and also provided a pool of technical experts that could be mobilized to focus on major new opportunities (such as Celcor or optical fibers). However, greater financial control was devolved to the operating divisions for engineering and other technical areas important to their businesses. In 1984, Tom MacAvoy, who the year earlier had become vice chairman of the corporation with special responsibilities for technology, commissioned an "innovation task force" to find out why the rest of the company was not happy with R&D and what could be done about that.

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[29] Graham and Shuldiner, *Corning and the Craft of Innovation*, p. 405.

The task force issued a "frank and devastating report" that charged that, except for a few projects that had been protected by top management and some other exceptions, most of Corning's R&D and engineering program had fallen into a "state of neglect."[30] It concluded that Corning's defensive moves in the 1970s and early 1980s to emphasize development at the expense of research, to improve productivity, and to pursue low-risk product and process extensions had set up a cycle of "diminishing returns," with efforts at real innovation succeeding only by "acts of heroism or by fighting the rest of the company." A chart mapping out Corning's technology portfolio according to how rapidly the field was evolving and how Corning's capabilities compared to competitors showed that Corning had lost ground in many of its key technology areas. A major part of this task force's continuing effort lay developing an explicit description of Corning's historical way of innovating. After a year's work, the task force rolled out this Innovation Process at a 2-1/2-day Innovation Conference in 1986.[31]

1. Corning's Innovation Process

This Innovation Process systematized the more qualitative, experienced-based procedures that senior Corning executives had long used to decide when opportunities warranted further investment, given the difficulty in applying standard cost-accounting models to innovation. According to David Duke, who had grown both the Celcor and optical fiber businesses, the criteria for determining whether Corning had a sustainable competitive advantage in some potential new business included

> Do we know anything about the market? Do we see access to customers? Do we have some sort of real advantage—some unique process or unique product that we think gives us that advantage to win over not just the first year, but over the 10 or 20 or 30 years or the lifetime of the product? We look at these questions and we analyze them.… Is it real? Can you win? Is it worth it?[32]

Tom MacAvoy provided much the same interpretation: "If it's important and you can be the leader, and the market is going to happen, and it builds on your strengths, do it."[33]

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30 Ibid., pp. 411–412. Quotations are not from the original Task Force report.

31 Ibid., p. 411, 416.

32 Morone, *Winning in High-Tech Markets*, p. 192.

33 Ibid., p. 193.

According to an interview with Don Keck, former head of the Corning R&D Division and one of the Corning scientists most responsible for the discoveries that led to optical fiber, the Innovation Process was based on Corning's optical-fiber development effort.[34] As modified during a later period of corporate self-analysis in the 1990s, the Innovation Process describes a systematic approach toward establishing an overall strategic direction, developing a project portfolio consist with that strategy, and defining gates through which individual projects must pass. Setting the strategy, in turn, depends on creating a technology road map, which identifies likely future technology trends, matches them against Corning's strengths, and defines areas for Corning to compete. This road map also identifies the successive technical steps needed for each of Corning's businesses to reach its goals, and it estimates the people and resources needed to do so.[35]

The five gates in the Innovation Process are

1. *Build Knowledge*.
2. *Determine Feasibility*—Applied research and/or proof of concept demonstrations; feasibility experiments.
3. *Test Practicality*—Pilot development with limited production.
4. *Prove Profitability*—Pilot production.
5. *Manage Life Cycle*—Manufacturing and commercial operation.

According to Keck, the company's technology leaders drive gates 1, 2, and 3, but they are also involved in 4 and 5. Manufacturing specialists share gate 3 with the technical community and drive gates 4 and 5. Business managers are involved from gate 2 on.[36]

2. Corporate Role for S&T

Science and technology have always been viewed as integral to Corning's business. Indeed, one observer has noted that Corning's technology strategy is "so thoroughly tied to its business strategy that it is difficult to distinguish one from the other. The pursuit of cost and performance leadership in its markets hinges on technological leader-

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34 The names used for the five gates following those presented in Graham and Shuldiner, *Corning and the Craft of Innovation,* p. 416; the descriptions of those gates are from an interview with Donald Keck conducted by Julius Harwood and Yevgeny Macheret on 4 June 2002 in Corning, N.Y.

35 Graham and Shuldiner, *Corning and the Craft of Innovation,* p. 437.

36 Harwood and Macheret interview, 4 June 2002.

ship."[37] The founding Houghton family, actively involved in the firm for five generations, considered R&D to be essential for the company. According to Don Keck, Corning's top business managers are asked to visit the lab to better acquaint themselves with the research. Lab management therefore has direct contact with senior corporate management.[38]

Corning has viewed the establishment of core competencies in specialty materials to be integral to its business, and it has maintained centralized corporate research facilities to foster and enhance this competence. Corning's consistent, long-standing support of research in this area and its long-term retention of staff have built an institutional memory that has served the company well as a source of further innovation. On occasion, Corning has pursued business lines outside of these core areas—into electronics in the 1960s and industrial biotechnology and medical diagnostics in the 1970s and early 1980s. Each time, Corning

> recognized the significance of an impending technological revolution… and positioned itself to take advantage of it well in advance of other firms. Nonetheless, the company was never able to build the technological capabilities needed to achieve a leadership position in these new fields, and one of the consequences seems to have been a reinforcement of the historical strategic focus [on glass and specialty materials].[39]

According to Keck, the company has had little interest in outsourcing R&D and in buying start-up firms, approaches that do not contribute to building corporate experience. Neither has the company adopted the approach of establishing stand-alone "skunk work" organizations to develop innovative products, independent of the parent company. Corning pursued such a path but abandoned it a few years later.[40] Corning has, however, had a long history of entering into joint ventures with companies that complement Corning's glass and materials expertise by contributing expertise in complementary areas (e.g., cabling for its optical-fiber-communications business; recombinant DNA for its venture into biotechnology) or that provide marketing and distribution capabilities to capitalize on Corning's technological leadership.[41]

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37 Morone, *Winning in High-Tech Markets,* pp. 184–185.

38 Harwood and Macheret interview, 4 June 2002.

39 Morone, *Winning in High-Tech Markets*, p. 176.

40 Harwood and Macheret interview, 4 June 2002.

41 Morone, *Winning in High-Tech Markets,* p. 221.

Keck's conclusions regarding Corning's success at innovation, which are consistent with those of other observers and participants, are that

- Strong support for R&D should be embedded in the company's culture. Long-term R&D is absolutely essential for the survival of the company and should not be viewed as a discretionary, adjustable expense.
- Support from the highest corporate management levels is necessary to protect a program or project during periods where the goal may seem technically unattainable, and
- Successful technology transition from the laboratory to the marketplace requires sustained attention, and regular communication, on the part of business, manufacturing, and technology leaders.

VI. SUN MICROSYSTEMS

Richard Van Atta and Donald Goldstein

A. INTRODUCTION

Sun Microsystems presents a classic case study in the development and exploitation of serial and radical technological innovation. Headquartered in Mountain View, Calif., Sun is global in scope. Sun Microsystems provides products, services, and support solutions for building and maintaining network-computing environments. Sun sells scalable computer systems (including high-performance supercomputers), high-speed microprocessors, and a line of high-performance software. With $18 billion in sales in 2001, this company dedicates around 10 percent of its revenues toward R&D. Last year it spent around $2 billion on R&D, most of which was spent within product-development organizations. However, about 2–3 percent of that (around $50 million) was allocated directly by the Chief Technologist, Greg Papadopoulos, for advanced technology developments not related directly to current products.

Sun's origins trace back to DARPA funding of the "Stanford University Network" as part of a broader program at Stanford supporting research into advanced computing. From these academic roots Sun Microsystems was a venture-capital-funded startup in what became known as Silicon Valley. DARPA played a key role in encouraging the commercialization of Sun's reduced instruction set computing (RISC) processor-based workstations, including providing the initial market for these machines by supporting their purchase by several universities conducting research for DARPA's Very Large-Scale Integration (VLSI) project.[1]

Innovation for Sun translates into "technology transfer" from corporate-level R&D labs into product-engineering programs. Sun institutionalizes innovation by CEO and Chief Technologist oversight over both corporate-level and project-level R&D.

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[1] Richard Van Atta, et al., *DARPA Technical Accomplishment—An Historical Review of Selected DARPA Projects, Vol. II*, Alexandria, Va.: Institute for Defense Analyses, IDA Paper P-2429 II, April 1991, pp. 17-B-8.

Bill Joy, one of Sun's founders, is the company's Chief Scientist. In this role, Joy pursues beyond-cutting-edge S&T concepts with a long-term perspective.

Sun places a major emphasis on technology transfer from its labs to product development and uses the technique of pushing corporate-level advanced-technology personnel off into the project divisions at a rate of 10–20 percent per year. The CTO is allowed to use some of his allowance of corporate R&D funds to stimulate, reinforce, and reward promising project-level R&D. While ultimate responsibility for all R&D spending remains under the CEO, there is a close and continuing relationship between the CEO and the CTO.

B. BACKGROUND

Sun Microsystems Inc. was founded in February 1982, based on initial funding from DARPA at Stanford University and subsequent commercialization of the university-based research through a venture-capital startup. Twenty years later it has approximately 43,000 employees in more than 170 countries. Its self-stated corporate vision is to ensure that network services are available to anyone, anywhere, anytime, using any device. It sells both hardware and software, such as network servers, data-storage systems, engineering workstations, desktop appliances, microelectronics, software systems, e-commerce applications, and cross-platform technologies. It also provides consulting and support services. It is recognized as a market leader in telecommunications, financial services, manufacturing, government, education and research, retail, health care, digital media, and entertainment. In its 2001 fiscal year it had revenues of about $18.25 billion, ranking 125 on the Fortune 500 index.

Sun regards itself as a relentless innovator, having been the first to bring a successful RISC-processor-based workstation to market.[2] The company has a 20-year history of bringing innovative ideas to market. It has attempted to institutionalize forward-thinking technology, which it claims enables its customers to get the most out of their existing network environments and take advantage of future opportunities. Sun is known for its avowed business philosophy that open standards and open programming interfaces increase the value of Net-based solutions and create a larger market for all players.

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[2] Ibid., pp. 17-B-9–17-B-10. The original Sun workstations used Motorola 68010 processors. Sun incorporated RISC technology with the SPARC workstations, which were first shipped in 1987.

The official mission statement for Sun Microsystems' Laboratories, presented below, represents not only the corporate philosophy, but also provides an insight into a model of managing research and development with potential applicability to other domains faced with rapidly evolving technological competition:

> Since 1990, Sun Microsystems Laboratories' charter has been to transform brilliant ideas into tangible technologies that can become powerful new products, and even spawn whole new industries such as the Java technology has done.
>
> Turning research into products is difficult, complex, and a social as well as a technical problem. We view the process of technology transfer as a key area of competency and we apply our creativity to be the best industrial laboratory at transferring as well as developing technology. As a result, Sun has been very successful at translating promise and potential into products.
>
> Researchers at Sun Labs are working on projects that are significant to the evolution of technology and to our society's future—asynchronous and high-speed circuits, optical interconnects, 3rd-generation web technologies, sensors, network scaling and Java technologies, to name a few.
>
> Although many companies have R&D groups, Sun Labs can claim one of the highest rates of technology transfer, i.e., the incorporation of Labs' technology into future products. However, the Labs also pursues high-risk projects, those with the most dramatic potential, knowing that some will not work out, while a few will have significant payoff. After all, you only need a few dramatic successes to shake up the world.
>
> Sun Labs' investigations into novel technologies and methodologies will continue to set Sun apart from competitors and help to define the future.
>
> Sun Microsystems Laboratories has a corporate mandate to search for the undiscovered. We look for novel approaches and methodologies. And we take on the projects that product groups can't, such as ideas that won't be practical for years, projects with high risk or uncertainty, or concepts outside the mainstream of Sun's current focus. Even though our research may push the boundaries of what is possible, we work hard to keep our development focused on what is practical and profitable.

1. S&T at Sun Microsystems

Dr. Greg Papadopoulos, the CTO of Sun Microsystems, notes that one of his most important roles is always to be looking outside.[3] He stated that one of Sun's mantras is "Innovation happens elsewhere." He sees his role to "build the impedance matching filter to exploit outside innovation."

Dr. Papadopoulos has direct oversight over all R&D spending, both through budgetary reviews with the Chief Executive and through rotation of his staff to product units. He is responsible for managing Sun's technology and architecture, standards, the Science Office, global engineering architecture, and associated advanced-development programs.[4] He also provides leadership and consistency for hardware and software architectures across Sun.

Technology transfer is viewed as a very important mission at Sun. Moving a technology from advanced development to product development is characterized as being "a contact sport." In Papadopoulos' view, this contact must be continuous. While he personally directly manages around 2.5–5 percent of Sun's total budget for advanced development, it is corporate policy to encourage distributed development.

Sun's main approach for achieving this is to move people. Each year about 10 to 20 percent of the Advanced R&D people move out into the product divisions. Product-division staff replace them in turn. Not every person on the Advanced R&D staff is expected to move with a project—some are seen and valued as "idea people" who would not be effective in a product organization. At Sun, principal investigators (PIs) sometimes make several career loops into and out of the labs as their projects move from the research to the product-development stage. Returning to the lab is not automatic. Former PIs have to make the same sort of application to re-enter the labs as they did to make initial entry.

Papadopoulos emphasizes that it took a concerted effort to convince product managers that R&D should not be viewed as an external activity, used "merely" for

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3 Much of our initial information for our case study came from interviewing CTO Greg Papadopoulos. We supplemented this with an on-site visit to Sun's Mountain View, Calif., laboratory facility and a review of corporate literature.

4 Dr. Papadopoulos is an MIT Ph.D. He also was an associate professor of electrical engineering and computer science at MIT, where he conducted research in scalable systems, multithreaded/dataflow processor architecture, functional and declarative languages, and fault-tolerant computing. Dr. Papadopoulos was formerly a director of Sun Laboratories.

generating ideas. Sun pursues an active program of education and seminars to refresh the organization. His staff puts together sessions around themes such as Java integration into an operating system. These programs are run out of labs. Twice a year his staff runs a technology leadership conference designed by the CTO. There is a great demand for an invitation to these, but only about 250 people are allowed to attend each event. Invitations are allocated to each division. Not only is the content viewed as an attraction, but the meetings are viewed as a chance to network and meet peers. In addition, Sun maintains an internal Web site for free-form collaborative work by researchers on projects that have not yet been officially approved. One Sun researcher described this pre-project phase as "a loose federation of tribes." This pre-project phase is an important part of creating a consensus for launching a formally supported project.

Sun's corporate literature outlines the rationale behind maintaining free-standing R&D centers:

- one of the most successful research labs in the world at "technology transfer"—moving ideas from the drawing board into products that actually ship;
- a magnet for technical talent, attracting expertise to Sun;
- an "intellectual trading post" for exchanging technology and know-how within and outside the company; and
- a source of expertise for internal consultancy within Sun.

2. Priorities and Focus

We asked Papadopoulos how the CTO identified technological gaps at Sun; how they were addressed, and how areas of importance were targeted. He noted that everyone knew that he was interested in reports on chip development, especially in area of reliability. Competitive analysis is done on about six areas of focused technology. Topics are generated by both the product divisions and the CTO staff.

Sun has an "advocates program" for facilitating and stimulating the interaction of the labs with the more focused product-development organizations. PIs and senior analysts are expected to bring new problems back to the labs from the product groups. The advanced-development staff is expected to "churn the waters."

3. Long-term Research

Sun has spelled out its vision of the time horizon and unpredictable nature of husbanding long-term R&D in its public characterization of the first decade of the laboratories' achievements:

> People often imagine that the process of innovation is fluid and linear. The reality of applied research does not bear this out. Projects can take 10 years to yield tangible results, as was the case with asynchronous technology, now an extremely promising alternative for designing faster circuits. Sometimes the road to shipping a product is bumpy and circuitous, as was the case with Java technology. And sometimes the development of a technology involves many people from many places, not a single individual working in isolation.

Therefore, it is more accurate—and more interesting—to view the impact of Sun Laboratories not simply in terms of "inventions" but in terms of technologies, products, and people influenced over the years.

4. Organization for S&T

Sun Laboratories was first established in 1990. At the time the CEO of the firm, Scott McNealy, had some reservations about the prospective return on investment that might flow from advanced R&D.[5] Nevertheless, he approved the establishment of the organization. A decade later the company credits the laboratories with a key role in the growth of the company

Sun Labs employs a staff of more than 200, including 180 scientists and engineers, with facilities in California, Massachusetts, and Grenoble, France. It also maintains collaborative relationships with universities, entrepreneurs, government, and other research institutions. A global center of cooperative, results-oriented innovation, Sun Labs has developed core technologies instrumental in creating breakthrough products, including

- The Java software platform,
- NETRA carrier-grade servers,
- SUN RAY desktop appliances,
- JINI networking technology,

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5 Interview with Sun executives.

- SUN CLUSTER 3.0 software,
- SUN STOREDGE networked data storage products, and
- ULTRASPARC III.

5. Process Management

At Sun the CTO reports to the CEO for keeping the company technologically competitive. Dr. Papadopoulos states that he that provides the CEO "a systems-level view of where the problems are and level of effort is required." At Sun this is done face to face. The CEO is ultimately responsible, but he delegates responsibility to the CTO to make sure company maintains technological competitiveness. The primary responsibilities of the CTO in reviewing technology developments are development standards, progress monitoring, and the maintenance of integrity.

The CTO looks at all the product division technology road maps to assess their competitive potential. For example, are there potential "disruptive technologies" in areas such as processor design and the network management? Is it possible to speed cheaper processors to the market? Should Sun take the lead on a strategic acquisition to acquire a needed technology? What are the new challenges for product competitiveness?

Project termination is handled as a simple zero-sum game, governed by ranking of projects and adjudicated by the CEO and the CTO. However, Sun tries to make sure that lab PIs have more than one project to lead, so that termination of an individual project does not reflect personally on the career or ego of the PI.

In an interview with us, Dr. Jim Mitchell, Sun Fellow and Vice President, Director of Sun Labs, noted that

> The starting point is the research strategy. At Sun Labs, our strategy is to develop technologies that are relevant, that have the potential to solve real customer problems, that seem feasible given the constraints of time, money, and technical staff resources, and that will directly benefit Sun. Our work then focuses on the objective of technology transfer…. That is not a traditional research strategy.
>
> In the past, many research labs took on projects that had no direct relevance to the company's business or that had no set limits in terms of funding or time frames. We want to see results that make a direct impact on Sun and its customers.

Sun's Web site describes the corporate intelligence function of the Laboratories:

> Even though Sun Labs' researchers are sensitive to Sun product and customer needs, Sun Labs was founded with the mandate to be the "eyes and ears" for the company. It's the Labs' job to keep an eye on the horizon and to evaluate technical trends. Some current projects are organized around technological themes (for example, Java development, tools and chips, networking). Some reflect a researcher's vision of future technological directions and some a unique solution to a technical challenge. But all are driven by the convergence of technologies, researchers' interests and skills, and relevance to Sun's future business.

C. CONCLUSION

As the economic climate for high-technology companies has become stormier, Sun has turned to the laboratories to conquer its problems. While it may have become more selective in the projects it funds, SUN still views its laboratories as a key part of its corporate strategy. The laboratory is expected lead to gains in markets by using advanced R&D to enhance core competencies and create revolutionary technologies that will lead to success. Sun's management appears confident that real scientific breakthroughs will produce favorable outcomes, even in the current economic environment.

VII. LIBERTY, THE DAIMLER-CHRYSLER ADVANCED-TECHNOLOGY ORGANIZATION

Julius Harwood and Yevgeny Macheret

A. BACKGROUND

Liberty was founded by Chrysler in 1983, after GM and Ford started their own advanced-technology divisions to help them compete with the Japanese automakers. Liberty is an autonomous platform, which means it has its own charter and budget. Liberty's goal is innovation, and Liberty has been the source of a variety of products.

Liberty has about 50 Chrysler people and 35 contract employees. Liberty's budget figure is negotiated between Tom Moore (Chrysler Group Vice President, Liberty and Technical Affairs) and Bernard Robertson (Chrysler Group Senior Vice President of Engineering Technologies and Regulatory Affairs). It is determined by how hard Tom Moore argues and defends his projects and the current financial performance of Chrysler. Annual budget has been steady, about $25–35 million per year (for 2002 it was $27 million). Liberty's budget is officially a part of the total engineering budget.

1. Liberty's Mission, Culture, and Organization

Liberty's mission is to develop advanced innovative technology for automotive applications. That means looking 5–10 years ahead and inventing new technologies for the future. Liberty pioneered many products that are currently in the market: die-cast magnesium instrument panels, hybrid power train, and automatic tire-pressure monitoring and control system, to name a few.

Liberty was started from scratch. Tom Moore handpicked his own people. As a result, a company with unique culture focused on innovation was created. Moore said that corporate culture in Liberty is similar to that of skunk works.

According to Moore, the innovation process in Liberty is simple: you get an idea, sell it to the management, and go do it. Most projects are internally generated. No formal system for ranking and selecting projects is in place. Moore personally approves or

disapproves new projects. The organizational culture and relatively stable budget encourage an open forum: people are not afraid to take risks, and anybody can criticize anybody (and they do). Moore says he gives his young engineers three chances to criticize a new project. After the three strikes, if they did not prove the point, they have to start working on the project even if they disagree with the idea.

The organizational structure of Liberty is flat. Moore described it as a "tip of the iceberg structure," that is, a small group of people (the tip) who get a lot done. The reason for the high productivity is that Liberty utilizes resources of many other organizations outside Daimler-Chrysler (the iceberg), particularly of supplier networks. Liberty often establishes a cost-shearing agreement, with suppliers contributing parts and hardware for the project. The advantage to the supplier lies in obtaining an "in" to Chrysler's advanced technology. Even though the Liberty organization is small, people in Liberty are ranked high in the Chrysler organization.

Liberty is also engaged in technology transfer: after a product is developed in-house, people who developed it go to the production platforms and help implement it. They typically stay there for some time to oversee the implementation and then return to Liberty.

To make sure that Liberty has expertise in many different technology areas, Liberty looks for people with solid foundation in science and engineering. The people also have to be quick learners and able to work well in teams. Tom Moore also directs the Chrysler University Research program, with a budget of $1–2 million per year. This program gives Chrysler a broad look into the academic advanced research world.

Other organizations within Daimler Chrysler do R&D as well. For example, the Advanced Technology group in Germany also looks at the long term (5–10 years), with some 250 projects underway. Moore asserts, however, that there is no duplication of efforts. The projects are different. Liberty is unique in its approach to R&D: although people at Liberty are developing advanced technologies, they do not stop working until their solution is ready for implementation (e.g., Liberty's electric car is ready; Liberty is waiting for a battery to power it).

2. Advanced Technology Example: Fuel-Cell-Powered Car

Most major automakers are currently developing fuel-cell-powered vehicles. GM, Ford, and Daimler-Chrysler invested in Ballard, a Canadian company, to develop fuel

cells to power their cars. Chrysler has 25-percent equity invested in Ballard (Ford has about 20 percent). Ballard is working on making a fuel cell with appropriate costs and performance characteristics. Liberty's role is to adapt the fuel cells to Chrysler's needs.

The big unknown with fuel-cell-powered cars is how to supply hydrogen. The current alternatives are (1) create a hydrogen infrastructure or (2) on-board hydrogen generation. The former alternative necessitates on-board hydrogen storage—a difficult technological problem. The latter alternative is currently addressed by developing reformers to convert gasoline to hydrogen—not an optimum solution from cost and efficiency standpoint.

Since it is not possible to predict which alternative is going to win out in the end, Liberty mitigates the risk by pursuing technologies for both alternatives simultaneously (such an approach is typical for Liberty). For hydrogen storage, Liberty is investing in developing ultrahigh-strength low-cost fibers. These fibers would be used to make high-pressure cylinders for hydrogen storage. For on-board hydrogen generation, Liberty is working on sodium borohydride source of hydrogen in cooperation with Millenium Ev and Dow Chemical. In fact, Liberty has already developed a fuel-cell sodium borohydride car, called Natrium, with a 300-mile range. One billion dollars has already been spent on fuel-cell development.

B. CONCLUSIONS

We offer the following conclusions based on Liberty's experience in creating a successful innovative organization:

- An independent autonomous organization is needed to create innovative products. Such a stand-alone organization can support innovative culture that is not restrained by existing bureaucracy, so that application and production people do not inhibit the innovative, long-term development work.
- Top-management support and championship are important factors for long-term stability of the innovative projects.
- A protected stable budget is needed for a stable work environment and to encourage people to take risks.
- Pursuing several technologies simultaneously can mitigate long-term technology risks.
- Successful transition of innovative technologies into commercial applications requires original developers and application engineers working together.

Throwing a new product across the fence over to production people and hoping they will pick it up is unlikely to work.

DoD PROGRAM CASE STUDIES

VIII. S&T IN THE NAVAL REACTORS PROGRAM, 1949–1959

Robert Bovey[1]

A. SUMMARY

In the Navy and Atomic Energy Commission (AEC) joint Naval Reactors (NR) office, the mission and management was a seamless web encompassing R&D, acquisition and construction, and plant operation and maintenance. NR's vision of its reach was as broad. It saw itself as responsible for creating or providing materials, processes, and qualified people. The first two responsibilities required a great deal of fundamental research.

The salient features of the NR experience in managing S&T research in the course of building the first nuclear-powered submarines were as follows:

- Selecting highly qualified people and training them intensely were passions.
- A strategy of pursuing alternative technologies simultaneously (at several levels, from overall concepts to specific materials) involved fundamental research and reduced long-term technical risk.
- Clear definition of program performance goals and systematic, strict evaluation of the projects led to well-defined technology gaps, focusing research where it was most important to the overall goal.
- S&T project progress and results were scrutinized frequently and judged on technical grounds, after often tough, sometimes bruising, debate.

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[1] The author would like to thank the following people who reviewed the final draft of this report. Their comments were invaluable. Any errors in the report are solely the author's.

Captain John W. Crawford, Jr., U.S. Navy (Retired), who served in NR for nearly all the period covered by this report, much of it as Admiral Rickover's principal deputy.

Dr. Francis Duncan, Admiral Rickover's biographer and historian of NR during much of the period covered by this report and for many years thereafter.

Dr. David F. Winkler of the Naval Historical Foundation, who has written on a number of DoD development programs.

Dr. Yevgeny Macheret and Mr. Andrew W. Hull of the Institute for Defense Analyses.

- Clear program technical and schedule requirements were set early and, in turn, drove S&T decisions on how much research was enough.
- Requiring research to support development schedules was instrumental in delivering working systems on time.
- Budget stability was sought, and largely achieved, to foster consistent efforts in addressing difficult technical problems.
- Research management at NR headquarters was under the directors of technical groups (physics, materials, etc.), not project officers.
- Long-term personnel tenure was achieved. The knowledge accumulated through experience in the headquarters technical groups and dedicated laboratories was invaluable. Many difficult technical problems required long-term commitment.
- The director of NR was intimately involved in managing research. He defined the technical areas of research concentration, reviewed individual projects, and actively managed the research portfolio.

NR's management philosophy was summarized in the phrase "Demanding Customer." NR would identify very specific deficiencies and demand their correction but generally would not tell the research contractor how to do so. NR saw itself as the conductor, orchestrating the research work of the instrumentalists to ensure that each was being directed so that collectively they produced useful and timely results.

B. OVERVIEW

This chapter will examine NR from the perspective of S&T. It will not attempt to be comprehensive. General comments are included in the text when the evidence suggests that they apply to S&T as well as more generally. The paper will cover five topics:

- History of submarine nuclear power to set 1949–1959 in context;
- Several concrete research cases to set a basis for discussing S&T management;
- Relevant organization;
- Management philosophy and practices; and
- Key features of NR, still from an S&T perspective.

Submarine nuclear-power development lay on the intersection of the development of nuclear power over time and the world of submarine technology generally. The focus of this review is 1949–1959 and is not entirely arbitrary.

In 1949 the Naval Reactors Branch of the AEC was established, headed by the same man who had earlier been appointed head of the Navy Bureau of Ships office, Code 390, for the same purpose. The name changed several times over the years, but the combined office was usually referred to as "naval reactors," or "NR." In 1959 the *Skipjack* (SSN 585), a hull form optimized for submerged performance and powered by a "standard" S5W nuclear power plant, went to sea. Nuclear submarines had reached maturity. For this and other reasons, 1949–1959 was the decade on the time continuum when nuclear power moved from a fuzzy idea to a mature industry.

Although developing nuclear power was crucial to creating a true submarine, it was only one part of the submarine technology continuum. Without nuclear power, earlier submarine hulls had to be designed in recognition that the ships spent most of their time on the surface. At the same time nuclear propulsion was being developed, however, the Navy was conducting parallel developments in several submarine-related areas, including designing a hull form optimized for high-speed submerged operation and testing it extensively at sea in the *Albacore*, starting in 1953.[2] Therefore, the program described here is only a partial picture of a much more complex reality.

C. HISTORY: THREE PHASES OF SUBMARINE NUCLEAR PROPULSION DEVELOPMENT

1. Pre-1949

In January 1939, in a conference in Washington, D.C., Niels Bohr and Enrico Fermi announced that Otto Hahn and Fritz Strassman had split the nucleus of a uranium (U) atom.[3] Ross Gunn of the Naval Research Laboratory (NRL) heard this presentation and "became immediately convinced of the importance of quickly initiating navy research…toward the goal of nuclear power plants for submarines.…"[4] A few days later, Gunn asked Rear Admiral Harold G. Bowen to initiate work at NRL. Bowen allocated

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2 Richard G. Hewlett and Francis Duncan, *Nuclear Navy,* University of Chicago Press, 1974, p. 267.

3 Ibid., p. 16.

4 Vincent Davis, "The Politics of Innovation: Patterns in Navy Cases," Vol. 4, No. 3, University of Denver, 1967, p. 24.

$1,500 to Gunn, "the first government money spent on the study of atomic fission."[5] NRL began research into the technology of gaseous diffusion to enrich uranium in the fissionable isotope, U-235, for fueling such a submarine. The Manhattan Project adopted this gaseous diffusion technology in 1944 to produce the highly enriched uranium (HEU) for the Hiroshima atomic bomb.

On 2 December 1942, Fermi's University of Chicago experimental group achieved the first controlled and sustained nuclear chain reaction, 10 years and 4 months before NR's Mark I initial criticality. During World War II, three reactors were built for producing nuclear weapons materials. These and five small research reactors were operating in 1946. The technology that existed for developing a reactor that would produce usable power was scattered and buried in classified files, not at all readily available.

In June 1946, a group of Navy officers and civilians were assigned to Oak Ridge to learn about the state of nuclear technology. In August, General Leslie Groves of the Manhattan Project approved a contract with the General Electric Company for a paper study of a liquid-metal-cooled reactor for a destroyer. Earlier, General Electric had agreed to "operate the plutonium production plant at Hanford, Wash., in exchange for a promise that the government would provide a nuclear development laboratory for the company at Schenectady…."[6] This laboratory became the Knolls Atomic Power Laboratory and eventually, over the period 1950–1955, was subsumed under the NR program. In sum, a good deal of research began shortly after World War II.

The five officers and three civilians studying at Oak Ridge facility developed an initial pool of information and concepts. They then toured the country, visiting laboratories and experts to refine their ideas. The team leader, who later headed NR, developed the initial research agenda to fill gaps in scientific knowledge required to support what he saw to be essentially an engineering program

2. 1949–1959

R&D within NR broadly followed three parallel tracks—pressurized water reactor (PWR), liquid metal (sodium) reactor, and gas-cooled reactor. Gas-cooled reactors were

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5 Vice Admiral Harold G. Bowen, USN (Ret.), *Ships, Machinery and Mossbacks*, by Princeton University Press, 1954, pp. 182–3, cited in *Politics of Innovation,* p. 24.

6 Hewlett and Duncan, *Nuclear Navy,* pp. 38–9.

abandoned early (1949) by NR for naval use,[7] although the issue was revisited from time to time. For example, in a 12 April 1957 hearing of the Subcommittee on Research and Development of the Joint Committee on Atomic Energy, the director of NR was being pressed by several members who clearly were enthusiastic about gas-cooled reactors. In the context of civil reactors, he responded to Representative Chet Holifield's question, "If you had the privilege of naming the reactor you would like to go into, which one would you select?" with "Gas cooled."[8] Indeed, gas-cooled reactors have been pursued subsequently for land-based applications.

Liquid-sodium-cooled reactor development preceded the formation of NR. In 1946, under AEC contract, General Electric had begun designing a sodium-cooled breeder—a reactor that created more fissionable material than it burned during operation. The sodium-cooled reactor was pursued through a full-scale operating, land-based prototype and into operation in the USS *Seawolf* (SSN-575), which went to sea in 1957. In 1959, the *Seawolf* was converted to a PWR after a series of debilitating maintenance problems directly related to the sodium coolant.[9] However, liquid-metal coolant R&D was continued under AEC, the Energy Research and Development Administration (ERDA), and Department of Energy (DOE) sponsorship in the liquid-metal fast-breeder reactor (LMFBR) program until it was terminated by President Carter in 1977 on the grounds that production of fissionable material was inconsistent with efforts to stop the proliferation of nuclear weapons. The LMFBR program is of some interest in the current context because the NR R&D management approach was applied with more formality, and hence more visibility, than it had been in the early NR program itself.[10]

The PWR turned out to be the dominant technology to emerge from the NR program. The specific examples below are therefore from PWR-related research. NR's success in naval propulsion led the AEC to task an NR-led team to design and construct the PWR at Shippingport, Pa. This PWR became the world's first purely commercial nuclear power plant in December 1957, when its generators transmitted electricity to the Duquesne Light Company grid. The Shippingport reactor was not only larger than the

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7 Joint Committee on Atomic Energy, Various Subcommittees, "Progress Report on Naval Reactor Program and Shippingport Project," 85th Congress, 1st Session, 7 March 1957, p. 10. Hereafter referred to as JAEC 3/7/57.

8 Ibid., p. 93.

9 Ibid., pp. 7–8.

10 Discussion with Captain John C. Crawford, Jr. U.S. Navy (retired).

Nautilus one, it also employed a seed-and-blanket design in which a central cylinder of HEU was surrounded by an annulus of natural uranium.[11] The PWR remains the dominant nuclear power technology in the world.

R&D has continued worldwide on gas, liquid-metal, and water-cooled nuclear power plants to the present.[12]

3. Post-1959

Also during the 1949–1959 decade, two significant variants of the submarine PWR plant were conceived. One involved substituting an electric propulsion motor for steam turbines and using a reduction gear to turn the submarine propeller(s). It will be addressed later. The second involved creating a reactor plant in which the water would flow through the reactor, without using pumps, by natural circulation because of density changes in water as it is heated. This spanned the science-engineering boundary. Whether it would work at sea was questionable, and this could not be fully established through calculations and small-scale tests. Therefore, like the *Nautilus* and *Seawolf* plants, a fully operating plant was built and tested inside a steel cylinder representing a submarine hull. This prototype was moved by a hydraulic system to mimic roll and pitch at sea.

Unlike the *Seawolf* liquid-sodium plant, both PWR variants [the natural-circulation reactor in the *Narwhal* (SSN-671) and electric propulsion in the *Tullibee* (SSN-597) and a later system in the *Glenard P. Lipscomb* (SSN-685)] operated successfully at sea for the life of their submarines. While technically successful, the operational importance of these innovative designs was overtaken by a steady march of incremental innovations in the main-line PWR, steam turbine, reduction-gear propulsion system.

Most recently, the current director of NR announced in June 2002 that in 2003 work would begin on a "transformational technology" reactor, which would provide a 30- to 50-percent increase in power as part of "significant but classified propulsion-plant innovations" for the planned Virginia-class submarines.[13] If this is achieved without

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11 Francis Duncan, *Rickover and the Nuclear Navy: The Discipline of Technology*, Naval Institute Press, Annapolis, 1990, pp. 194–8.

12 James A. Lake et al., "Next-Generation Nuclear Power," *Scientific American*, Vol. 286, No. 1, January 2002.

13 Andrew Koch, "USN SSN Force Faces Overstretch," Washington Bureau Chief, *Jane's Defense Weekly,* 19 June 2002.

increasing the volume of the reactor plant, it will represent a radical innovation in power density, arguably the most radical innovation since the *Nautilus* reactor.

D. ILLUSTRATIVE CONCRETE CASES OF RESEARCH IN NAVAL REACTORS OFFICE

1. Introduction to Reactor Terminology

As an introduction to terminology used later in specific projects, Figure VIII-1 illustrates some of the main parts of a PWR. The top view shows an arbitrary number of fuel cells, long boxes made up of fuel plates with interspersed channels through which water flows. Each fuel plate is a sandwich of a uranium alloy fuel element, covered on both sides by cladding. In the fuel element, the fission process produces heat and radiation. The heat passes through the cladding and is absorbed and carried away by the water coolant.

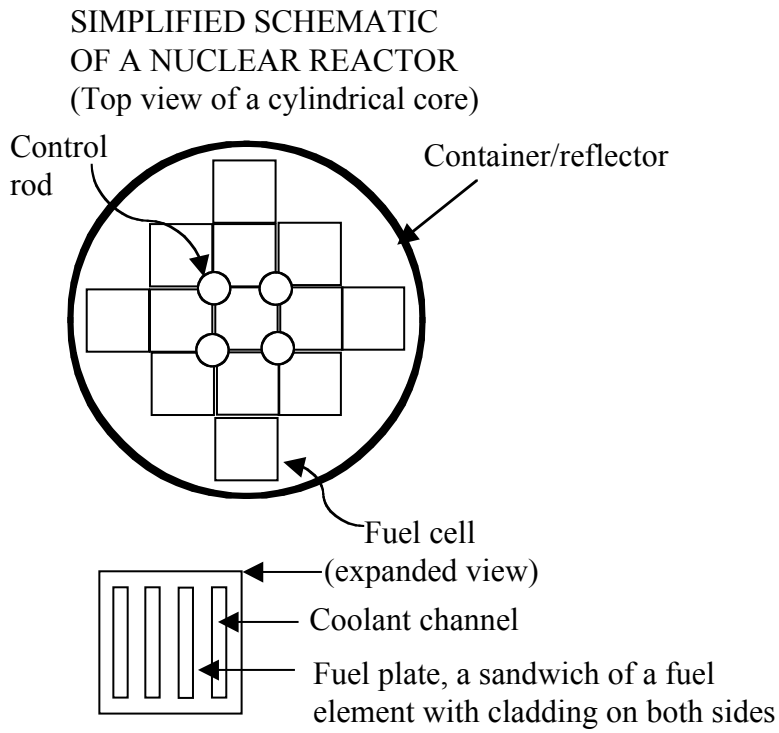

Figure VIII-1. Simplified Schematic of a Nuclear Reactor

Within the reactor core, a chain reaction is maintained because relatively high-energy neutrons produced by one fission interact with solid materials and coolant, losing energy to a point that they can be absorbed by another uranium nucleus to produce

another fission. Real-time control is exercised by withdrawing/inserting control rods in the core. Because a control rod contains material that absorbs neutrons very effectively (i.e., it has a large "neutron capture cross-section"), insertion squelches the chain reaction. Such materials are called "poisons." Fuel depletion over the life of the core is optimized by using "burnable" poisons (not shown) distributed in minute quantities throughout the core to reduce the fission rate in parts of the core that otherwise would be very active early in core life.

2. Illustrative Cases

Research of a fundamental sort was required to support the basic engineering development and to discover means to overcome the limitations of the reactors that were feasible at first. Five cases are discussed. The first case discusses early research and addresses the assembly of basic scientific knowledge of properties and phenomena. The second case discusses the development of zirconium as a structural material and is the prime example of research performed hand-in-hand with ongoing engineering development. The third case discusses the use of hafnium as a control rod material and illustrates how serendipitous discoveries were turned through R&D into engineering applications. The fourth case, burnable poison, is a case of a research effort directed to achieve a radical innovation in core design. The fifth and final example, about secondary systems, discusses NR's use of technologies invented elsewhere.

a. Early Research

Early in the Navy team's stay at Oak Ridge (June 1946–June 1947), it concluded that the necessary technology base for designing propulsion reactors did not exist. Each team member took a subject area and set out to read, listen to, and question Manhattan Project personnel about it. Each member also wrote a series of papers, which were reviewed by his colleagues. These initial papers were the first step in creating the necessary database. Adding to this database systematically became a primary function of NR.[14]

The striking feature of the research initiated by NR in the late 1940s and early 1950s was its elementary nature, its attention to the sorts of basic measurements and analyses that physics and engineering students perform in class. It was exactly the kind of

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[14] Hewlett and Duncan, *Nuclear Navy,* p. 137.

work that many scientists and graduate engineers disdain; yet, it was precisely the kind of information needed before the reactors could be designed. For example, in reviewing existing data on water, NR was surprised to discover how little was known about the properties of water itself or its effects on materials.[15] Over the years, NR coordinated a variety of laboratory studies on corrosion and wear in water systems.

Throughout the 1950s, NR sponsored a series of reactor engineering handbooks that were the foundation of the nuclear industry as a whole. The series included the *Liquid-Metals Handbook* (1950), *The Metallurgy of Zirconium* (1955), *A Bibliography of Reactor Computer Codes* (1955), *The Metal Beryllium (1955), Reactor Shielding Design Manual* (1956), *Corrosion and Wear Handbook for Water-Cooled Reactors* (1957), *The Metallurgy of Hafnium* (not dated, post-1957), and the three-volume *Physics Handbook* (1959–1964).[16]

b. Support of Engineering Development—Zirconium as a Structural Material

One development within the PWR materials track serves to illustrate two points about the interplay between scientific research and engineering development that was commonplace within the program. First, research was often done to understand the properties of materials that seemed attractive based on preliminary knowledge. Second, research sometimes unexpectedly uncovered possibilities that demanded further R&D to exploit.

By December 1947, Oak Ridge had completed a very preliminary design of a PWR.[17] One of the problems in building a PWR was to find a material that would be strong enough and workable to support and clad the uranium fuel elements, had little tendency to absorb neutrons, and resisted corrosion by hot water under high radiation. Many materials, including stainless steel, aluminum, and beryllium, were studied.

An Oak Ridge engineer, Samuel Untermeyer, had suggested zirconium (Zr) because of its mechanical, metallurgical, and corrosion characteristics; however, it had two big disadvantages. Zr had never been produced in quantity, and it seemed to have a large neutron capture cross section. However, in late 1947, Herbert Pomerance, an Oak

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15 Ibid., p. 136.

16 Ibid., p. 139.

17 Ibid., p. 58.

Ridge physicist, had discovered that the large cross section recorded in earlier tests was mostly the result of a hafnium (Hf) impurity in the Zr test material. Therefore, removal of the Hf would make the Zr neutron capture cross section quite low. However, the removal of this previously undetected alloying material might also degrade Zr's mechanical, metallurgical, and corrosion properties.[18]

Based on the evidence accumulated by the end of 1947, the future director of NR committed to Zr as the metal for fuel-element structural material and fuel-plate cladding. This decision set in motion four parallel tracks of materials scientific research and engineering work. One path was to verify the properties of pure Zr and perhaps discover alloying materials to improve them. The second was to mass-produce Zr. These two tracks converged onto the third, which was to design, test, and manufacture hundreds of fuel elements. The fourth track concerned Hf and is addressed in the following section. Each of these tracks involved iterative but overlapping scientific research and engineering problem solving.

Although Zr was selected in 1947 as a reactor structural material for PWRs because of its favorable nuclear properties and corrosion resistance, it was not until March 1950 that Argonne and Bettis laboratories decided it would be feasible to assemble a fuel plate consisting of a U-Zr alloy fuel element clad with Zr.[19] Research continued to improve the performance of Zr, and out of this, an alloy named "zircaloy" was developed. Zircaloy was less expensive than pure Zr and had improved corrosion and mechanical properties. However, after deciding to use zircaloy as cladding for UO2 fuel elements in the Shippingport reactor, in-pile and out-of-pile tests revealed unexpected Zr properties. Zr tended to absorb hydrogen (H) from high-temperature water systems. Irradiation affected this, and the behavior of H dissolved in Zr was not initially understood. Both in-pile and out-of-pile tests were used to study the redistribution of H in Zr under thermal and stress gradients. Together, they provided a basis for explaining and predicting the migrations. Further research revealed the role of nickel (Ni) contained in zircaloy in accelerating or increasing H absorption and pointed the way toward a class of Zr alloys free of this injurious feature.[20]

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18 Ibid., p. 59.

19 Ibid., p. 145.

20 Joint Committee on Atomic Energy, Various Subcommittees, "Progress Report on Naval Reactor Program and Shippingport Project," 85th Congress, 1st Session, 12 April 1957, pp. 48–9. Hereafter referred to as JAEC 4/12/57.

In the meantime, R&D was carried out to produce Zr and zircaloy. For example, in 1948, U.S. Zr production was about 86 pounds at $135–235 per pound, all by the Foote Mineral Company.[21] In 1955, the AEC signed 5-year contracts with three producers to produce a total of 2.2 million pounds Zr per year at $4.50–8.00 per pound.

In sum, research into some very fundamental physical phenomena continued in parallel with engineering design and even manufacturing. Research was the bootstrap that pulled the engineering development forward.

c. Incremental Innovation—Hafnium as a Control Rod Material

The third process, set in motion in 1947, was to investigate the use of Hf to capitalize on its very high neutron-capture cross section. For control rods, Argonne Laboratory had decided to use an alloy of silver (Ag) and cadmium (Cd), which would be bonded by hot rolling to strips of stainless steel, and the main development work continued with this technology. However, as Zr was being manufactured, a growing stock of pure Hf was accumulating. Therefore, research was conducted to investigate its mechanical, metallurgical, and corrosion properties and to design test control rods using Hf. In late 1952, when the *Nautilus* prototype reactor was almost complete, shifting from Ag-Cd to Hf control rods was found to be necessary.[22]

Research continued on Ag alloy because of concerns that a civilian nuclear-power industry would need more control-rod material than could be provided in the form of Hf and because of a conservative approach that acknowledged the possibility of an unanticipated weakness appearing in Hf. These investigations of potential resulted in the development of an Ag-alloy containing about 15-percent indium (In) and 5-percent Cd. To establish the feasibility of this alternative, a significant amount of research was conducted. For example, an analytic technique was developed to account for the metallurgical changes produced by neutron absorption in the rods. Also, the release of radioactivity from such control rods because of plant accident conditions involving dissolved air in the coolant was measured. The problem thus characterized was circumvented by the development of high-quality Ni plating procedures.[23]

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21 Hewlett and Duncan, *Nuclear Navy,* p. 140.

22 Ibid., p. 146.

23 JAEC 4/12/57, p. 49ff.

This illustrates a typical situation in which a serendipitous discovery led to a new application while other technologies were being investigated. Insurance-type investigation is an approach to incremental innovation that keeps options open. It also requires judgment as to when a line of investigation has become what one expert called "polishing the cannonball."[24]

d. Radical Innovation in an Ongoing Program—Burnable Poison

On 30 March 1953, the *Nautilus* prototype reactor went critical, achieving a self-sustaining nuclear chain reaction, at the Idaho Test Site. The *Nautilus* reactor went critical in the *Nautilus* on 30 December 1954.[25] It was designed to operate for about 2 years. The limiting factor was the uneven burning of U fuel in different parts of the core. The fuel elements in the low central section of the core experienced many more fissions than those in the outer annulus. Because control rods have to move from bottom to top, the fuel near the top of the core still had experienced relatively few fissions when the fuel in the bottom central section had had too many for continued operation.

Alvin Radkowsky[26] oversaw physics research from NR headquarters and, contrary to NR policy, was an innovator in his own right. Among the many new design principles he originated, the most important one addressed this problem of uneven fuel burnout. Radkowsky's concept was to intermix an isotope of boron (B^{10}) in the fuel, ranging from small amounts in fuel that was most subject to fissioning early in core life to none in sections active only late in core life, and thus smooth out the depletion of fuel across the reactor core.

From the concept to execution, of course, involved considerable theoretical and experimental work to verify the physics, metallurgy, and mechanical properties of the doped fuel. Dr. Sidney Krasik, Manager, Central Physics Department, Westinghouse Electric Corporation (Bettis) testified in 1957 about the history of doped fuel:

> The suggestion was originally made and presented late in 1952, as I recall, after the first *Nautilus* core had been completed. There was nothing we could do about incorporating it in the first core then, but we proceeded to factor the information from the *Nautilus*—the Arco [Idaho] operation—

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24 Discussion with anonymous sources.

25 Hewlett and Duncan, *Nuclear Navy,* pp. 184, 216.

26 Alvin Radkowsky was a Bureau of Ships civilian employee. He earned a doctorate in physics at Catholic University, then spent a year training at Argonne, and joined NR in October 1950.

> into that and during 1953 that principle was examined analytically. It was then incorporated into the prototype. That prototype was fully tested…and now it is ready to be used in the *Nautilus* and [to] demonstrate its worth.[27]

The *Nautilus* was refueled in the first quarter of 1957. Its first core had run for just over 2 years. On 8 March 1957, the first director of NR quantified the improvements of later cores:

> We just finished installing the third core in the *Nautilus*. That core will be capable of steaming about 120,000 miles, compared with 60,000 on the first core, yet the cost of this new one is 20% less…With the type of operations we are doing now, we sure should get five years of operation…The goal we now have is to design a reactor which will last for a war.[28]

In the end, burnable poison and other incremental innovations extended core lives by many years. Initially, however, calculating how to orchestrate the use of burnable poisons, control rods, and other means to control reactivity across a core throughout its life was inexact at best. Therefore, NR became an early and enthusiastic innovator in the use of computers to perform physics investigations. In 1959, the director of NR observed how computers had helped with the evolution of reactor design:

> Another advancement has been our ability to use computing machines to help us design nuclear cores. We spend several millions of dollars per year developing and using new computing machine methods…We just would not be able to develop our advanced reactor cores without these machines and the techniques we have developed for using them.…
>
> The most important development from a technical standpoint [since creating the first power reactor] is the considerable increase in the life of the nuclear cores, which has greatly extended the cruising range of our nuclear-powered ships…We can now get cores that last two or three times as long as the first one….[29]

Of course, there were other radical innovations during this program—the basic idea of using a steam plant in a submarine was an engineering innovation rather than a

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27 JAEC 4/12/57, p. 62.

28 JAEC 3/8/57, pp. 2, 22, and Joint Committee on Atomic Energy, "Naval Reactor Program and POLARIS Missile System," 86th Congress, 2nd Session, 9 April 1960, p. 28. Hereafter referred to as JAEC 4/9/60.

29 Joint Committe on Atomic Energy, "Naval Reactor Program and Admiral Rickover Award," 86th Congress, 1st Session, 15 April 1959, p. 31. Hereafter referred to as JAEC 4/15/59.

scientific one, but radical nonetheless. Once that was accomplished, the use of electric vs. mechanical propulsion created a technology tension that continued throughout the life of NR.

e. Outsourcing Innovation, Secondary Systems

Most nuclear power plants have primary and secondary systems. The primary system includes the reactor and all means to carry energy to a heat exchanger, where water in a secondary system is turned to steam. This steam drives turbines, which, in turn, propel the ship and turn generators that produce electricity for the ship. Most of NR's S&T work was devoted to reactors and the primary systems. For secondary systems, NR relied primarily on R&D being conducted elsewhere, in the Bureau of Ships and beyond.

Throughout the 20th century, submarine propulsion had been based on internal combustion (usually diesel) engines that turned generators to produce direct current electricity, which, in turn, powered electric motors that turned propellers, supplied shipboard needs, and charged large storage batteries. These batteries powered the motors when the submarine was fully submerged. NR's substitution of turbine and reduction gears for submarine propulsion was a radical innovation, dependent of course on nuclear power. However, the advantages of an all-electric ship spurred continued interest throughout the Navy.

From 1949–1959, a long history of experimentation with electric propulsion continued. Motivated in large part by noise considerations, a decision was made early in the decade to build an all-electric submarine. This decision sacrificed power density. Because of the state of existing electric-power technology, the electric-propulsion system was bigger and heavier than the equivalent turbine and reduction gear system.[30]

In 1954, DoD requested the AEC to develop a small reactor for what was to be a large class of relatively small, very quiet, all-electric submarines. The prototype was built at Windsor, Conn., and achieved full power on 19 December 1959. The *Tullibee* (SSN-597) was launched on 27 April 1960 and operated for many years. No follow-on ship of the class was built, but the sensor and weapons innovations proved in the *Tullibee* were incorporated in later SSNs, all of which used turbines and reduction gears for propulsion.

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30 Duncan, *Discipline of Technology,* p. 23.

Electrical system R&D continued in the Navy and beyond, and NR followed it closely. By the late 1960s, an accumulation of incremental innovations had improved efficiency and size enough that the Navy decided to try again. The *Glenard P. Lipscomb* (SSN-685) was launched 4 August 1973 and operated successfully at sea for about 20 years. While these innovative electric designs were successful, their operational importance was overtaken by a steady march of incremental innovations in the main line steam turbine, reduction-gear propulsion system.

Electric propulsion was widely regarded as a blind alley of technology innovation. However, improvements in electrical power technology continued. On 12 April 2002, the current director of NR said that the *Tullibee* and *Glenard P. Lipscomb,* in some ways, may have been ahead of their time:

> Those two experiments, or those two prototype submarines…a lot of things have changed since we tried those, specifically in the field of power electronics. It's another example of Moore's Law these days, in the generation-jumping that power electronics have taken. Motors themselves now are much more power-dense than before, and much quieter and much smoother-operating. But more importantly today, if indeed we're going to execute some of the ideas that we're fairly keen on—about off-board sensors, and unmanned underwater vehicles being launched from our submarines; large-volume unmanned aerial vehicles being launched from those large volumes—these new payloads, these new off-board sensors are going to require electricity that we don't have today, electricity that we don't have because we reserve about 75 percent of the reactor's power and energy—power—for the main engines…So putting all that output of the reactor into turbine-generators, and then drawing from an electrical grid the power that you need to move the submarine, and yet having it available for powering these other payloads and sensors, may be very important and just around the corner. That's why we're looking afresh at the idea of electric drive.[31]

These examples illustrate the range of research conducted by NR and its relationship to the overall system-development process. The following sections describe the organization and management processes employed for this R&D. A later section will address the key issues involved.

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[31] "The Education of Hyman G. Rickover," Naval Historical Foundation, Washington, D.C., 2002, p. 23.

E. ORGANIZATION FOR S&T

This section first describes the overall organization structure of the nuclear reactors enterprise from Congress, through the Executive, and to the contractors, with emphasis on R&D management. Second, it describes the headquarters (HQ) organization for R&D. It concludes with a discussion of NR R&D funding.

1. Overall Organization

The NR organization evolved from a loose network of interested individuals in 1947, which largely ignored an existing Navy office, to a formal organization in January 1949. This formal organization was unusual because the director was dual-hatted (in the Navy and the AEC). While there were many changes over the years, for this discussion, a simplified organization chart (see Figure VIII-2) will do.

For S&T management, the left leg was more important because almost 90 percent of NR R&D funding, in the neighborhood of $100 milllion in FY 1958, flowed through it. [32]

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[32] JAEC 3/7/57, p. 18.

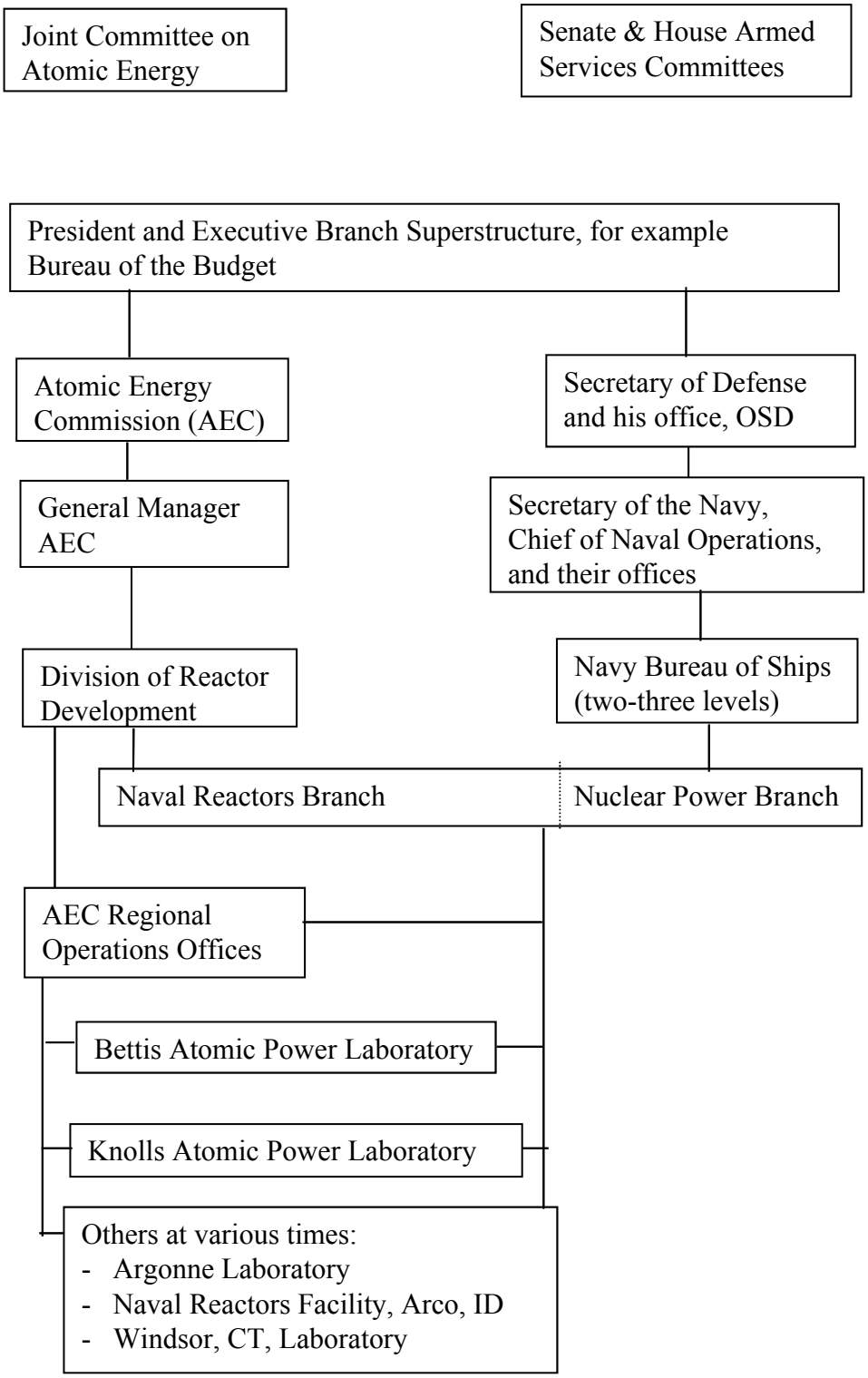

Figure VIII-2. NR Organization

The NR HQ in Washington grew to about 90 scientists and engineers, both officers and civilians, by 1957. These people worked interchangeably for the AEC and Navy. In addition, somewhere between 150–180 people in the Navy Bureau of Ships worked with NR almost exclusively.[33] By 1959, the NR HQ had grown to about 120 scientists and engineers.[34]

At the beginning of the decade, the main sources of science support to NR were first Oak Ridge and later Argonne Laboratory in the AEC system. The importance of this support declined by the early 1950s because NR built its own laboratory system. Two main AEC laboratories were established during this decade: Bettis Atomic Power Laboratory near Pittsburgh, Pa. (established in 1949 and operated by Westinghouse Corporation), and Knolls Atomic Power Laboratory near Schenectady, N.Y. (assigned to work for NR on 12 April 1950 and operated by General Electric Company).[35] Most of the R&D work done on naval reactors was performed in these two facilities, and no other work was done for other government or private programs. At the end of the decade, a third, smaller laboratory was established at Windsor, Conn. (owned and operated by Combustion Engineering). Together, these facilities employed about 2,000 scientists and engineers, plus supporting people.[36] Bettis alone employed about 5,300 people, of whom 1,300 were scientists and engineers.[37] Reactor prototypes operated at the Schenectady and Windsor sites, but most were at the AEC facility in Idaho. These prototypes were used for conducting engineering tests, training submarine crews, and conducting physics and materials research.[38] At the same time, many other scientists and engineers who worked for the subcontractors were designing equipment for the naval nuclear program.

The laboratories reported administratively through an AEC field office, in which NR representatives were posted, and a program field office was located at each site to carry out functions such as budgeting, contracting, administrative control, etc. The relation between NR headquarters and the laboratories was usually direct on technical

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33 JAEC 3/7/57, p. 26.

34 JAEC 4/15/59, p. 30.

35 Francis Duncan, *Rickover: The Struggle for Excellence,* Naval Institute Press, Annapolis, 2001, p. 113.

36 JAEC 3/7/57, p. 32.

37 JAEC 4/12/57, p. 89.

38 JAEC 4/15/59, p. 30.

matters of most interest to an examination of S&T. Communications with the Navy and the AEC were conducted through NR headquarters.

2. Headquarters Organization for S&T

Early in this decade (1950s), NR had made a point of not having a formal organization. However, from its establishment, NR was organized into project officers and technical groups, as a practical matter, and this NR organization was quite fluid. Specific projects and, to a lesser extent, technical areas shifted. Each project and group reported to the director of NR. The project officers were responsible for everything having to do with their project(s). The technical groups were involved in reactor design, materials for reactor systems, radiation shielding, and component design for all projects. The HQ technical groups usually did not do actual technical work but, rather, oversaw contractor work. These were the same contractors being overseen by project officers. This situation often led to friction or creative tension, depending on one's viewpoint.[39]

Responsibility for S&T generally lay first with the technical group directors. They were responsible for understanding where research gaps or opportunities existed, based on inputs from their staffs and the laboratories, and for proposing research projects.

3. Funding

Most nuclear power plants have a primary and secondary system. The primary system includes the reactor and all means to carry energy to a heat exchanger, where water in a secondary system is turned to steam to drive turbines. According to an AEC–DoD agreement, the AEC paid for reactor and primary plant R&D. The AEC also financed all R&D for the first plant of a type. In addition, the AEC financed the construction and operation of the prototypes, with minor exceptions. The Navy paid for any R&D on the secondary plant beyond the first of a type and for the ships themselves and the cores that went into them. At least some of the AEC and Navy R&D funds were interchangeable under an arrangement approved by the Joint Committee on Atomic Energy.[40]

As of March 1957, the AEC was to provide $86 million in R&D, and the Navy was to provide $11 million to NR in FY 1958. The first director of NR pointed out that he

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39 Hewlett and Duncan, *Nuclear Navy,* p. 129.

40 JAEC 3/8/57, pp. 20–21, and JAEC 4/12/57, p. 89.

had asked the Navy for $7 million more, but that had been cut. Prompted by a member of the Joint Committee, he then said he could use another $20 million, $10 million from the AEC and $10 million from the Navy in FY 1958. This report does not address whether he got his plus-up in this case, although he often did. For present purposes, suffice to say that the NR R&D budget had grown by the end of the decade to something over $100 million.[41] Of this, most of the money came from the AEC.

Although the numbers certainly varied dramatically during the 1949–1959 decade, the fraction of the NR budget devoted to S&T is not clear. It is, however, reasonable to think that after the program matured, 10–15 percent of the NR R&D budget was devoted to S&T work.[42]

4. Management of S&T

This section presents the philosophical underpinning and then the practice of S&T management in NR.

5. Philosophy

NR's management approach was its conviction that it was in the business of building fully operational ships and, in the case of Shippingport, a commercial reactor. NR saw itself as an engineering enterprise, and its vision was encompassed in a design philosophy and a management philosophy.

a. Design Philosophy

NR's design philosophy represented one of an infinite number of balances that might have been struck between power density, personnel radiation exposure, safety, reliability and maintainability (notably, in this last respect, to increasing the length of core lives so as to minimize the number of times a ship had to go through the arduous refueling process), and other operational considerations such as noise emissions into the water around a submarine.

By way of example, the following illustrates NR thinking about safety. The director of NR described his philosophy:

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41 JAEC 3/8/57, pp. 18, 20–21.

42 Anonymous sources, discussion.

> In a good engineering design you try to include three safety features for each possible casualty…even if a man "goofs" in one feature nothing will happen…Even if he "goofs" on three nothing will happen. But it has happened that there have been more than three mal-operations simultaneously; then something may happen. Now, if you try to design for every contingency or combination of contingencies you introduce so many new gadgets into the plant that these gadgets themselves may render the plant unsafe…you can design for about three "goofs" in a row and that's all. You must violate all three to get into trouble, but it can happen. This is why careful training and constant direct supervision by responsible and competent people is essential."[43]

The balance NR struck was strongly criticized throughout the life of the nuclear-propulsion program, almost always by those who favored a balance that gave much higher weight to power density than the combination of other factors. The NR balance being criticized gave considerable—perhaps near-equal—weight to the other factors. These are matters on which reasonable technical specialists could disagree.

b. Management Philosophy

Admiral Hyman G. Rickover, the first director of NR, believed firmly that

> In a complex development effort involving a new technology and a tight schedule, the government could not simply place an order and expect the contractor to fill it. Unless the government officials themselves had sufficient technical competence to evaluate specifications, contractor performance, and the quality of the product, the government's interests were not likely to be protected. Creating and maintaining that kind of technical competence in a government organization was a back-breaking task….[44]

To reiterate, the NR management philosophy emphasized individuals rather than processes. Admiral Rickover personally selected each scientist and engineer in the HQ staff, among many others, and followed his training. Officers and civilians were interchangeable based solely on technical and managerial ability.

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43 JAEC 4/9/60, pp. 19–20.

44 Hewlett and Duncan, *Nuclear Navy,* p. 152.

> From the beginning each member of the staff had definite responsibilities and was held personally accountable for every aspect of that responsibility even when it overlapped assignments to others (as it usually did).[45]

> In his own organization [the first NR director] could demand full accountability from each of his staff; among contractors he had to depend upon his leverage as the customer. At Bettis he was largely successful in imposing his principle of full personal responsibility. At Knolls he had only limited success after many years of argument. At Argonne the relationship was terminated before this issue was resolved. But in every case [the NR director] put the relationship in the personal context. Organization, reputation, or system did not determine the quality of a laboratory…Quality was the…sum of the talents of the laboratory director…and each member of his staff.[46]

The concept of a "demanding customer" is discussed later in the section on key features.

The NR management philosophy rested on acquiring top people, training them hard, and subjecting them to tough demands.

> In [the NR director's view] an area as new as reactor technology, the unknowns were so great and the possibilities so intriguing that the lure of research was irresistible to many scientists. In [his] mind, research meant investigation and exploration. Engineering meant creating something new to reach a fixed goal. Research was vital, but in his program it had to be controlled…[47]

The director of NR saw this control as fundamental to progress in research as well as development, acquisition, construction, and operation. He described his views:

> If you want to get a job done and have it done on time and have it work, then you better tie the guy down within boundaries…otherwise, he is going to take easy, dilatory methods. I think you will ultimately get more progress—much more progress out of this hard regime than you get where somebody just builds something…When you force scientists and engineers to stick to something that has got to work in exactly that way, then there is something happening every day and night. They have got to find out why…these things are…That is the reason we are developing a

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45 Ibid., p. 385–386.

46 Ibid., p. 386–387.

47 Ibid., p. 102-3.

> science of reactors and reactor technology because we have had to find out why it works.[48]

Balance requires noting that, while NR clearly saw itself as an engineering enterprise, it recognized its responsibility to conduct research for which an immediate application might not be evident. The director of NR articulated this view in 1957:

> One of the reasons for the considerable expense is the vast amount of instrumentation that we are putting in so we can learn [from, sic] future design. We have not looked at any of our plants as jobs you just build to make it work. We have always borne in mind that we had to develop lessons for our own future and for the future of other people in the game…We have always been conscious that while we were trying to get a particular job done we had to learn lessons for the future. We had to find out why things were happening. Naturally, you never do as much of that as you want to, but there has been a considerable amount of effort and money spent to develop basic information.[49]

A specific application of this aspect of the NR R&D management philosophy on research is illustrated by comments made by Dr. Benjamin Lustman, Manager, Metallurgy Department, Westinghouse Electric Corporation (Bettis), in 1957. Describing the previous years, he said, using "development" in the sense of both R&D,

> because of the lack of knowledge which existed…hot water corrosion resistance, thermodynamic stability of the material, specification from purity contents, method of melting, casting, rolling, bonding with zirconium cladding—even the development of sources of metal to use as an alloy and in addition of suitable purity—all these questions had to be answered…within the time period…available for development of the reactor. Certainly many development questions have had to remain unanswered because this time period is much too short. Decisions have to be made and concurred with between Bettis personnel and Naval Reactors Branch technical personnel to permit the job to go on within the required scheduled period.[50]

F. MANAGEMENT PRACTICES

This section discusses management practices at four levels: overall, at HQ, over the contractors' work, and finally the project level.

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48 JAEC 4/12/57, pp. 63-4.

49 JAEC 4/12/57, p. 54.

50 JAEC 4/12/57, p. 71.

1. Overall S&T Management

As with organization, general management responsibility statements were avoided in the early days. Later, matters became more formal. By 1982, an NR report stated,

> In the areas of research, design, development, and specification, the program performs the following functions:
>
> Conducts all research and development related to the current and future application of nuclear propulsion for ships of the U.S. Navy.
>
> Designs and develops all components, equipment, systems, and related parts of the nuclear reactor and its primary plant, including associated biological shielding and…all components, equipment, systems, and related parts for the entire propulsion plant of all ships and craft which are the first of their class, and for all naval nuclear propulsion plant prototypes…
>
> Designs, develops, fabricates and operates prototype reactors and other test facilities and mockups for the purpose of conducting research and development…
>
> Concurs in…any other research, development and design work done on other nonreactor plant equipment, systems, equipment arrangements, modifications and concepts for nuclear powered ships where such items may have an effect on the reactor plant or personnel radiation exposure…" [etc.][51]

At least by June 1947, the future director of NR had identified the most important target areas for research as shielding, construction materials, reactor controls, coolants, and heat exchangers.[52] More specifically, in November 1946, he had reported that designing an effective shipboard shield to protect personnel from the enormous amounts of radiation generated in the reactor was an obvious need that would demand original research. The land-based production reactors built in World War II had ample space to install huge shields; however, a ship or submarine was a different matter.[53] As the program progressed, research priorities shifted because of progress that was made and new needs that were uncovered. The management processes for this are discussed below.

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51 Joint Economic Committee, "A Description of the Naval Nuclear Propulsion Program, January 31, 1982, A Joint Program of the Department of the Navy and the Department of Energy," 97th Congress, 2nd Session, Part 1, 28 January 1982. Hereafter referred to as JEconC 1/28/82.

52 Hewlett and Duncan, *Nuclear Navy,* p. 46.

53 Ibid., p. 38.

Within those priority areas, for balancing short- and long-term research, the NR time horizon breakdown tended to be as follows:

- Contractors—short term, applied;
- Universities—long-term possibilities; and
- Atomic power laboratories—a mix.[54]

While the precise way in which short-long term balances were struck remains unclear, it is clear that NR always drove toward concrete performance goals. In practice, the execution of this management philosophy was often dramatic. The approach taken at the beginning of the 1949–1959 decade to jump-start PWR development by Argonne and Bettis illustrates the intensity with which the "tie the guy down within boundaries" philosophy was applied.[55]

> Early in 1949 [the director of NR] realized he would have to assert his authority quickly at both Argonne and Westinghouse if work at both sites was not to flounder. The key issue, as he saw it, was the type of reactor to be developed for the Navy…Without a firm goal Argonne might well drift off into years of speculative research…Westinghouse might take a similar course. But if Argonne could be forced to concentrate its efforts, Westinghouse would follow…
>
> He asked Argonne to determine which approach would be the best if the choice were to be made at that time. The reply came back on March 21, 1949…On the basis of existing knowledge, the water cooled approach was most promising. [Based on its knowledge of the state of research at Argonne, NR] could have expected no other answer.
>
> [The NR director's] action was crucial. His purpose was to make certain that Argonne and Westinghouse would do engineering—not research. It was a point he was to hammer at many times.

The practice of making a firm decision to proceed down an engineering path when it was judged that enough, but not nearly all, of the scientific knowledge was in hand characterized NR throughout the following decade and beyond.

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54 Crawford, discussion.

55 Hewlett and Duncan, *Nuclear Navy,* pp. 102–103.

2. S&T Management Practices at HQ

In the early days, the NR director was, in effect, the project officer for innovation. Research work was generally subject to the same system as other work. Within the HQ, a form of daily reporting was practiced. The NR director received "pinks," a carbon copy of everything every secretary in the HQ typed each day, and he scanned these and annotated them with questions and instructions to the responsible scientist or engineer. Beyond the "pinks," there were weekly, biweekly, and monthly reports from laboratories described in the following section. Finally, the NR director reviewed each technical director's program, including its S&T component, at least annually. He demanded fresh approaches to the extent that, if a technical director did not cut some research project, replacing it by another, the NR director would cut that research budget 10 percent.[56]

Day-to-day management of S&T was a vigorous activity in NR HQ. At the beginning of the 1949–1959 decade, the person responsible for supervising specific research at a university, for example, would debate alternative approaches personally with the NR director. These debates were often argumentative.[57] Of course, there was less of this one-on-one debate as the program grew. As time passed and for broader decisions,

> The creative process of design took place in the discussion involving [the NR director] and his senior staff—those spontaneous, probing, challenging, and usually argumentative sessions in which the validity of ideas was tested. Here each participant, including [the NR director], stood on his own feet and depended on his own knowledge, skill, and wit to advocate what he believed was right in a technical sense. Only the technically qualified took part these discussions…of technological innovation…The method placed the stress on the unknown, the undecided and the unresolved. It laid every assumption open to question…the validity of ideas was the only measure of merit.[58]

How the first director of NR could be project officer for innovation as well as everything else is a credit to his phenomenal work schedule and tremendous knowledge. At his retirement, he said,

> I continue to work an 11- to 12-hour day plus weekends…I am an engineer and before I make any…nuclear engineering decision, I go into

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56 Anonymous sources, discussion.

57 Crawford, discussion.

58 Hewlett and Duncan, *Nuclear Navy,* pp. 385–386.

> all aspects. Furthermore, I believe I have a unique characteristic—I can visualize machines operating right in my mind…I do not think there has ever been anyone in the U.S. Navy who has had as much engineering experience as I have had. So that is one of the reasons I am able to do my job.[59]

Few who observed him doubted the essential accuracy of this statement.

Later, at least under successor directors, a project officer for advanced submarine propulsion technology was established. His job was like other project officers. He maintained a corporate overview of research gaps and opportunities, but he controlled no part of the NR budget. Only technical directors had budgets. The project officers acted as critics from the perspectives of their projects. The advanced technology program officer acted as a critic for focused research. The normal experience reportedly was that the advanced technology project officer did not need to push the technical branches to do research. Rather, he had to "hold the reins" so they did not go too far in some esoteric area. The technical branch, in turn, was holding the reins on the laboratories.[60]

After the advanced technology project officer was established in NR, in his concurring role, he was tasked to give a broader perspective to the technical directors. The NR concurrence process was rigorous and involved getting into the business of the technical branch to make sure that advanced technology professional perspective was taken into account.[61]

Overall, the technical areas in NR started with last year's budget and then argued why areas' budgets should change to be more or less. Base technology work was scheduled during this process and could well go up as the budget was worked. When someone presented a new idea, NR collectively ruminated to reach a conclusion. When the new idea got a priority, everyone looked around for what to cut. Everything was up for grabs—not just research. In the end, the project officers all made their cases, and the NR director ultimately decided.[62]

In the early part of the decade, the process for evolving broad research priorities was informal. In the LMFBR Program, the NR approach was formalized and

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59 JEconC 1/28/82, pp. 60–72.

60 Anonymous sources, discussion.

61 Ibid.

62 Ibid.

systematized. Argonne Laboratory was the focal point for R&D planning. It was to formulate what R&D was needed for both near term and long term. It laid out plans in physics, materials, components, instrumentation and control, and in other fields. There were eight to nine fields in all. This is what was done in NR HQ, but because it was so much smaller, the process was far more informal. As part of its charter, Argonne was responsible for identifying known or suspected gaps in scientific knowledge of potential importance to the program and documenting what actions, if any, were underway to remedy them.[63]

3. Day-to-day Management of R&D Execution

The approach begun in 1949 with Argonne and Bettis illustrates the NR management practices employed to implement the "tie the guy down within boundaries" philosophy, practices that were employed not only in the early research-heavy days, but also throughout the history of NR.

> [The director of NR] from the start [in 1949] insisted upon continually appraising contractor performance…Through his own representatives he learned—daily if necessary—what was happening at each of the laboratories…From scanning this material he could detect potential trouble spots. As these began to form a pattern, [he] would send one of his Washington staff to investigate. If the situation appeared serious, he would make the trip himself....
>
> In all aspects of the project [the NR director] saw the need for more personal contact, not just between Argonne and Bettis, but also with… Washington…Whether at Argonne or Bettis, [the director's] methods of appraisal were the same. He inspected facilities and saw the work that was being done. He and his staff followed contractors' efforts closely and knew key personnel. [He] and his men would question the scientists and engineers in detail about their work. Occasionally the process was bruising….
>
> The impact of conferences did not end when the participants adjourned. Members of [the] Washington staff…took extensive notes at each meeting and consolidated them into a formal report. Later [the NR director] discussed the report with the individuals involved to make sure they understood the problems and what they had agreed to do about them. The

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63 Ibid.

> conference and the report soon became an effective and distinctive tool of the naval reactors branch.[64]

Research work was generally subject to the NR system of frequent written reports. Daily reports from the field were sometimes used, and weekly, bi-weekly, and monthly reports were common. The information in these reports had been made known earlier through telephone discussions among participants, especially information seen to be important. It was not unusual for the NR director's use of the HQ "pinks" to escalate the importance of a matter being discussed among contractor, field office, and HQ personnel.

4. Advanced Technology Research Project Management in the Laboratories

In 1957, Dr. Walter Esselman, Manager, Advanced Development Group, Westinghouse Electric Corporation (Bettis), described the management of S&T projects in some detail. He began by emphasizing the practical focus of his research group. He said,

> The objectives of this group…are all based on trying to achieve reduced cost of plants and cores, reduced weight per shaft horsepower and, third, maintenance and improvement of the reliability of the nuclear reactor plant…[65]

He then employed a chart similar to the following (see Figure VIII-3) to indicate how an idea is developed.

Dr. Esselman described the process as being based on collecting information from a variety of sources on a variety of possibly relevant topics, Step I. He noted, as an example, that years of experiments had been performed at Bettis to understand the amount of heat that could pass through a metal surface under various water flows and temperatures without causing meltdown.[66] These experiments had begun in universities and AEC laboratories before Bettis was formed.[67]

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64 Hewlett and Duncan, *Nuclear Navy,* pp. 103–104.

65 JAEC 4/12/57, pp. 77ff.

66 JAEC 4/12/57, p. 79.

67 Crawford, discussion.

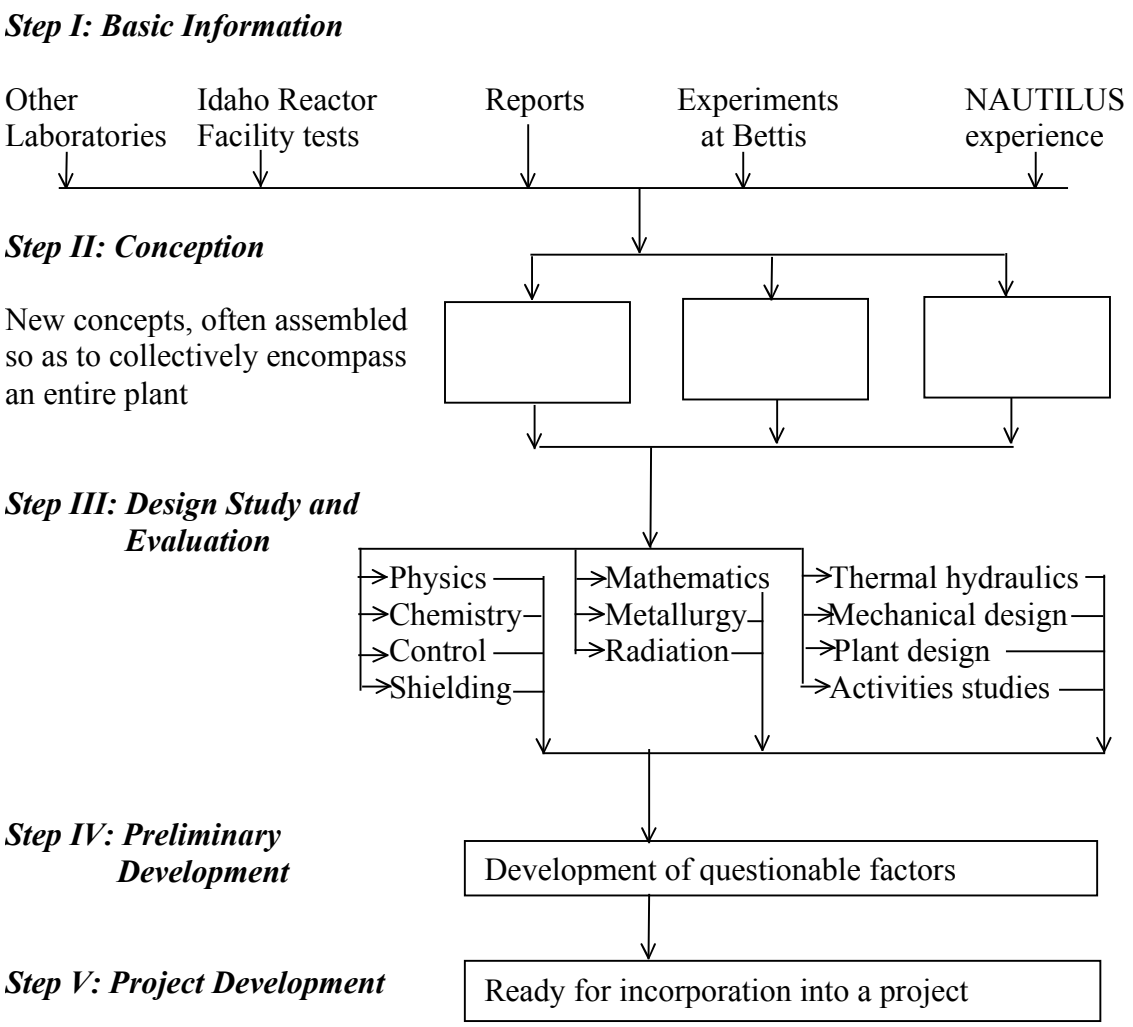

Figure VIII-3. How an Idea Develops

Dr. Esselman could well have added foreign intelligence to his source list. An NR report published 25 years later observed that the NR program,

> Evaluates intelligence information on other nuclear propulsion plants and reactor systems as part of the program's continuing assessment of potential future areas for development…

Dr. Esselman went on to say,

> When we get this information, the next step is to the conception of new ideas. Actually the conception of new ideas is not a difficult part of this process. The difficult part is to evaluate the ideas that you have to determine which are good…to limit…studies we want to make…to a number which can be accommodated with the few people we have. At some time we combine several ideas…and discuss…starting a design

study on a typical type of ship. We discuss with the AEC Naval Reactors Branch.…

We are making at this point a very practical study of a design concept. We are making a desirability study of a plant for a particular ship. The third step then is the design study and evaluation. The number of technologies that are involved are indicated. Actually there is probably only one of these various technologists performing this study [so] this is a relatively small organization. The more scientific aspects require the work of physicists, mathematicians, and metallurgists. The engineering aspects are covered by thermodynamicists and thermal hydraulic mechanical design, control engineering…we will make an actual plant layout…If we were making a general study and were not confining this to a particular type vessel…this changes the concept…

At the end of the study we now have established that a particular concept is desirable. I hesitate to use the word "feasible" because at this point what we have come up with is a knowledge that a certain type plant would have a certain weight per shaft horsepower. It will have certain operational characteristics, but there are usually a number of problems that require development. The development of these is then undertaken on a small scale.

Step IV can be undertaken on a small scale to determine whether some of these features that we consider problems really exist. Finally after we have done this it is ready for incorporation into a project. If we have enough knowledge of the plant which we are designing…we would not have to do too much development in this step IV. It could go directly into a project.

I do not wish to indicate that the development is anywhere nearly completed at this point. It is really only a beginning. What we have established to the point of incorporation in a project is that an idea will probably be feasible when the development in the project is completed. We have merely sifted the various ideas which were presented to those that are probably capable of solution and should be incorporated in a reactor plant…

Dr. Esselman concluded,

We are looking at various types of reactors. The advanced development group's thoughts are quite broad in scope. We are looking at improvements in water-cooled reactors as well as other types of reactors. I think the setup which we have whereby the various people in the study group are so closely related to the people in the projects results in a very practical approach. The fact that the people in the study group realize that the ideas they are trying to sell will very soon be in a project forces a practical

> approach. Also the people in the group are the ones who have had experience with the design…or operation of a plant so that the personnel we have will certainly take the practical approach to any design study.[68]

Dr. Esselman's presentation somewhat skimmed over the difficulties of establishing that an idea probably would be feasible. The testimony of Dr. Lustman, the metallurgist, indicated clearly that experiments often presented daunting management challenges. For example, he observed that for fuel elements and fuel materials under reactor operating conditions,

> To a large extent there is no theoretical background which permits prediction of the effects of radiation on these fissionable materials…no background exists to guide us as to which type of experiments ought to be carried on…We are getting knowledge, but there is no background of metal physics or solid-state physics such as exists in other fields which gives you a lead or which tells you some things can be ignored, but other things have to be investigated more closely…Certainly we have made a lot of wrong experiments.

Dr. Lustman added,

> Another factor here is the very restricted space which is available in reactors for doing this sort of testing…the very high radiation fields…and the very high radioactivity of the test space…after they [test samples] are discharged from the reactor, the limitations of the types of examinations which you can perform on these materials…We have to handle them remotely behind 6-foot concrete walls. You are restricted to just the simplest types of examinations—things we wouldn't even think of doing if we could physically handle the samples ourselves.[69]

G. KEY FEATURES IN NR'S S&T

1. People

When one asks an NR alumnus for the important factors influencing the conduct of S&T by NR, the first answer is "people." This was rooted in the NR emphasis on individuals rather than processes. The first director of NR required each staff member to have definite responsibilities and to be held personally accountable for every aspect of

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68 "Description of Program, January 31, 1982," JEconC 1/28/82, Part 1.

69 Ibid.

those responsibilities. To achieve a staff that could succeed in such an environment, NR devoted extraordinary attention and energy to selecting and training people.

The first NR people engaged in independent study and research for the June 1946–June 1947 year as a team at Oak Ridge. A second group trained at Argonne National Laboratory. Other additions followed a course of supervised independent study in the NR office. By June 1949, NR had negotiated with MIT to extend a longstanding naval architecture and marine engineering course to include a year of nuclear physics and engineering for Navy engineering duty officers sponsored by NR. In March 1950, NR and Oak Ridge began the Oak Ridge School of Reactor Technology, which had trained over 100 NR, Navy, and contractor employees by 1956. The school eventually provided hundreds of trained engineers for the nuclear power industry.[70]

In the meantime, universities were graduating physicists and materials scientists. The major "people" thrust after the 1949–1959 decade was the selection and training of officers and enlisted men to operate nuclear-powered ships, although the renewal of the NR staff continued to receive great attention. From the early 1950s, the NR approach for the laboratories was different. It was up to the contractor to select and educate its people, but NR evaluated these people and demanded replacement of those found deficient in capability or dedication.

In his later years, the first director of NR became a well-known critic of the American education system generally and scientific/engineering education in particular. However, in this decade and later, NR training programs focused on meeting its own needs for managers and operators.

2. Focusing Research on the Mission and Central Control

In the beginning, the NR director insisted on focusing on specific projects that would lead to a practical nuclear power system. He was ruthless in eliminating research that did not contribute directly to these projects.[71] Later, the focus was broadened somewhat, as discussed below. Still, NR wanted to be in control of R&D—to tell the

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70 Hewlett and Duncan, *Nuclear Navy,* pp. 123–126.

71 Ibid., p. 137.

researchers what was to be done. The NR director wanted advice, but in the end he wanted relevance and sensible work.[72]

The general view was that when an HQ pushes a laboratory, the lab will say that the HQ is not competent to judge. However, that was not the point of NR's philosophy. It believed the laboratory is like a violinist in a symphony orchestra. HQ should not tell the R&D contractor what to do (how to play his violin), but the government office must be the "conductor," telling all the instruments what to play, what aspects of research on which to focus, etc.[73]

NR believed that it must not get into the dangerous situation that it regarded as usual for government, where the researcher does whatever he thinks is fun without knowledge of overall system issues. An example drawn from the LMFBR Program was also illustrative of NR experience. The program was having serious civil heat exchanger problems. The program director ended up in a fight with a talented academic who wanted to work on some esoteric aspect that probably would never have an application (but was frittering money away), to get him to work on the real problem.

In general, the view from NR was that most government people overseeing science are not managerial oriented. They tend to be sympathetic to the "laissez faire" approach of the labs and contractors. The NR view was that when they look at R&D, they need to ask, "What is mission value?"[74] In other words, R&D had to be mission oriented, and it had to be the government who judged. To do that, talent was needed. Hence, the focus on people for the HQ organization.

Mission focus moved from a management precept to a crusade for the first director of NR. From 1974 though 1982, he embarked on a campaign against the system for the contractor IR&D then in effect and for those who administered it. The NR director debated with senior political appointees in the Navy and Office of the Secretary of Defense (OSD) and took his case to the General Accounting Office (GAO) and Congress. His fundamental issue was that much of the work being funded by the government in contractor organizations had no relation to military needs. He opened his argument at high levels on 21 June 1978 with a memorandum for the Secretary of Defense (SECDEF)

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72 Crawford, discussion.

73 Ibid.

74 Ibid.

via the Secretary of the Navy (SECNAV). He recommended that IR&D reviewers be guided by the technical evaluations of proposals, that only experts in the proposed work evaluate proposals, that proposals in which the benefits to the government did not warrant the cost be rejected, and finally that the entire system be changed so as to finance worthy R&D by direct contract so the government could supervise the work and retain appropriate rights to the resulting intellectual property. On 24 November 1978, the Under Secretary of Defense for Research and Engineering, William J. Perry, rejected these arguments.[75]

Having said this, the focus was not entirely consistent. First, the NR director interpreted his nuclear-power charter broadly where research was involved. Speaking of the many technical publications of NR, he said,

> By having these books available you get the people in the universities and in other places starting to think about the problem and making improvements…You will find that today these are the standard books in the United States on this subject…There are not any others with detailed scientific and engineering information in this field.[76]

NR was also more relaxed with university research than with industrial research. The money involved was much less, and it was good Congressional politics to have research going on in many places. As a practical matter, NR found that it could get good results from universities because it was possible to press the faculty principal investigators to do good work without incurring Congressional ire, so long as the money kept flowing. University research, however, was undertaken with some reticence because of the folklore that just when the research reached the point that NR needed it, the professor would go on sabbatical.[77]

3. Demanding Customer

One of NR's main points was that it internalized the matter of responsibility. For research and other work performed through contracts, NR distilled from this the concept

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75 All correspondence noted is included in the record of JEconC 1/28/82, Part 6.

76 JAEC 3/7/57, pp. 24–25.

77 Crawford, discussion.

of the "demanding customer." The following description of this concept is extracted from Appendix A.[78]

> Direction and guidance provided by the customer for contractor activities can take different forms. In many instances, the customer will arrange with contractor organizations to perform specific functions like research and development, design, procurement, construction, testing, and quality assurance, but will retain management of the total effort. In other instances, the customer will enter into arrangements where managing the total effort will be assigned to a selected lead contractor. The latter may still perform functions like those cited or have them provided by other organizations. Depending on the organizational arrangements involved, there will be one feature common to all—the need for the customer to exercise management across a customer-contractor interface.
>
> The key principle is that management and other capabilities of the customer's organization should be used basically for one function: namely to require and otherwise bring about effective management by the contractor organization or organizations to assure performance in accordance with the contract. The decisive test for any action contemplated by the customer is whether it is conducive to this objective. The principal pitfall is that the customer will use its capabilities to compensate for continuing weaknesses of the contractor. Like other management principles, this one is logically compelling but difficult to apply.
>
> A second principle is that the customer should set forth technical requirements in sufficient breadth and depth to assure that the product will meet customer objectives, but not in such degree as will stifle contractor management, initiative, and innovative capabilities. A corollary is that the customer needs to be able to adjust requirements, as practicable, to accommodate difficulties being encountered.
>
> The prerequisite need in applying these principles is that the customer have "in-house" capability as measured by technical competence among its own employees to shape, guide, direct, and assess the activities and operations of its contractors… If the customer organization lacks technical strength, the contractor will not feel the same pressure to achieve excellence.

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78 Appendix A of this chapter is an extract from "An Assessment Concerning Safety at Defense Nuclear Facilities: The DOE Technical Personnel Problem," March 1996 (DNFSB/TECH-10). It was written by John W. Crawford, Jr., who was among the first dozen men in NR. He served there from 1950–1963 and was deputy director 1960–1963.

> Having cited the need for strong customer technical capability, it is important to caution against its misuse. The general caution is that it should not be used to do work or perform functions for which the contractor is being paid… Many customer personnel would not perceive this as happening; some would not find it objectionable if they did. Such individuals find professional satisfaction principally from making a contribution to the solution of problems…It takes a firm hand to keep them from subverting the larger interests of their own organization.
>
> A demanding customer will insist on developing clear, mutually agreed-upon understandings about relationships with the contractor. True responsiveness by the latter always obliges the contractor to use his own good judgment in questioning suggestions made [by] the customer staff if the contractor believes them to be ill-advised. Responsiveness is to be measured, not by the extent to which the customer responds automatically to guidance from customer representatives, but rather by the degree of responsibility exhibited in analyzing such guidance and then in acting on it or recommending reconsideration as appropriate. It is also to be emphasized that differences in important matters are not to be held unduly long at lower levels, where they foster animosity and weaken cooperation. Instead, they should be raised promptly to higher levels of management for resolution. The objective to be sought is open, constructive dialogue between the parties, giving the primacy to objective technical and other considerations and suppressing personal predilection and bias….
>
> The need for the demanding customer to have "in-house" capability emphatically should not be taken to imply that the numbers of personnel be large. A customer operating in a sound managerial relationship vis-à-vis a contractor should be able to provide the needed managerial oversight with far fewer numbers than the contractor is obliged to use…the objective should be to keep competence up and the numbers down.

4. Stability

In NR's view, organizational funding was important to good S&T. An organization needed to have, as NR had, mission funding, which provided a steady diet. Organizations that did funding task-by-task ended up just "feeding the tourists," those who came around evaluating projects for continued funding. Also, project officers were seen as risk averse. They would not support S&T.[79]

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[79] Anonymous sources, discussion.

Recalling that NR's budget was nearly all R&D, most of it from the AEC and quite stable overall mission funding, controlling the dollars available then became an issue. In NR, the project officer had no money. He had to concur with plans of the technical branches. The technical area director had the money and covered the spectrum in his technical area. For example, reactor engineering covered current production, operations, and technology development, both to fix current problems and for the next generation. The project officers crosscut the technical directors. They were critics. Otherwise, inertia would be in control, and the technical branch would just keep working down a particular line. This implied that the advanced technology project officer was often in the position of arguing, "You guys are 'polishing the cannon ball'; it's time to shift money to something else." These money shifts could take place across technical branches.[80]

5. Continuity

The first director of NR "…held that it took years to train a man to be proficient in the peculiar kinds of technical and management problems faced in the nuclear project…" In particular, he viewed the idea of rotating officers after a 3-year tour, "…as the height of folly. Virtually all his senior staff agreed that the navy's rotation system… made adequate control of technological development [impossible]."[81] Building and maintaining a management team for the long term was a major objective—one that was achieved to a large degree.

For example, a head count taken as of 1982 indicated that there were 21 section heads (technical groups, project offices, and support sections) at NR headquarters. Of these, 12 had joined NR in the 1949–1959 decade and the remaining 9 had joined in the 1960–1970 decade.[82]

Because of this continuity, NR had a stable of strong advocates in its technical directors. They knew they were responsible for the whole spectrum, including the next generation, which had to be better than the last one. Furthermore, they would still be in

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80 Ibid.

81 Hewlett and Duncan, *Nuclear Navy,* p. 390.

82 Duncan, *Discipline of Technology,* Appendix 2.

NR to take the responsibility. In NR, the technical director had a much longer life than the technical leader in a normal Navy organization.[83]

While the issue of tenure in NR ended up being a positive with respect to S&T management, controversy continued throughout the life of the program about the negative impacts of Navy rotation policy (applied to officers outside the NR program) on the program generally. For example, in 1960 Congressman Price observed, "With the attitude of the Navy in regard to…it would indicate to me that perhaps they are considering nuclear-powered submarines and Polaris-type submarines as conventional a little too early…which might adversely affect you." The first NR director responded, "Nuclear power has brought many novel problems with it. The people in the Navy rotate very quickly. Nuclear power is hard to understand so they try to force it right back into the old system, which they do understand."[84]

6. Top-Level Executive Support

In the years 1949–1959, judging top-level government executives' support of naval nuclear propulsion R&D (as contrasted to their support for shipbuilding plans, personnel decisions, and other matters that were related to, but different from, R&D) is difficult because of the many and tangled threads that ran through the decade.

In *The Politics of Innovation: Patterns in Navy Cases,* Vincent Davis took strong issue with

> account[s] in which [the first NR director] is generally portrayed as the clear-cut hero, and all others in the plot are either his helpful accessories or his villainous opponents…which made it appear as if [the first NR director] had been forced to wage a one-man campaign against a Navy high command generally unenthusiastic about developing nuclear-powered submarines.[85]

Davis saw the decision to send the team to Oak Ridge in 1947 as, "…representing the triumph of the nuclear power enthusiasts within the Navy with respect to a firm Navy commitment to press ahead into research and development on nuclear propulsion for submarines. All remaining problems were ultimately resolved, in large part because the

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83 Anonymous sources, discussion.

84 JAEC 4/9/60, p. 10.

85 Davis, *Politics of Innovation,* p. 27.

highest officials in the Navy Department, including the Secretary and Chief of Naval Operations consistently gave this project their strong support."[86]

Others emphasize the difficulties in getting and keeping the highest officials engaged.

> In January 1947 [the Chief of Naval Operations, Fleet Admiral] Nimitz himself had approved a recommendation supporting development of a nuclear submarine…Two years of planning and discussion had…all but stifled the idea that seemed so promising…No one in a responsible position in the Navy really opposed the idea of nuclear propulsion…In a larger sense the issue was…whether the potential impact of nuclear power on the Navy warranted more than routine development.[87]

The judgment was made more difficult by the fact that two organizational superstructures stood over NR. Also, top managers in this management structure changed over the years as it coalesced and later evolved. The first director of NR was a masterful bureaucratic politician and played the two parts of the organizational superstructure over him to marshal support for the nuclear reactor program. Generally, the DoD superstructure was instrumental in overcoming early AEC reluctance and inertia to begin serious R&D into nuclear propulsion. Later, the AEC superstructure became far more important for NR R&D—most R&D funding flowed through it—while relations with the DoD superstructure were often acrimonious over matters other than R&D. However, by the end of the decade, the director of NR could bypass both legs of the superstructure to a large degree, at will, and was empowered by Congressional connections, primarily with the Joint Committee on Atomic Energy, in R&D and many other matters.

H. CONCLUSIONS

The success of NR from 1949 through 1959 was demonstrated by the performance of its product—the nuclear submarine—and speed with which it was developed and built. This success was even more impressive considering that the nuclear reactor technology and several supporting industries did not exist and had to be developed starting from almost zero. The reasons for such an astonishing achievement were many. This review has not attempted to account for all the factors that played a role. It has focused on NR's S&T research, which was a major factor in the success achieved during that decade.

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86 Ibid., p. 27.

87 Hewlett and Duncan, *Nuclear Navy,* p. 51.

What seem to be the key relevant considerations in NR's management of S&T research are summarized below.

- Based on its reliance on individual responsibility as a central management principle, NR regarded hiring highly qualified people as a central task. The training and education of its HQ personnel was given first priority. By June 1949, NR sponsored a course in nuclear engineering and physics at MIT for the Navy engineering duty officers. In March 1950, NR opened the Oak Ridge School of Reactor Technology, which provided basic fundamental as well as reactor-specific training to hundreds of engineers for the nuclear power industry.
- NR, in its management of government-owned/contractor operated (GOCO) laboratories, universities, and contractors performing research, was a demanding customer.
- Clear definition of program performance goals and systematic, strict evaluation of the projects led to well-defined technology gaps, focusing research where it was most important to the overall goal. The NR program benefited immensely from having highly qualified personnel set technical requirements in sufficient breath and depth to ensure that research products would meet its performance objectives.
- In addition, these highly qualified NR personnel were able to use sound technical judgment in evaluating project results and determining its progress. S&T project progress and results were scrutinized frequently and judged on technical grounds, after often tough, sometimes bruising debate.
- Clear program technical and schedule requirements were set early and, in turn, drove S&T project decisions on how much research was enough. Requiring research to support development schedules was instrumental in delivering working systems on time.
- NR, in its quest for solutions to an entirely new set of technical problems, maintained a strategy of pursuing several technologies simultaneously, thereby reducing long-term technical risk. The strategy was applied at several levels, from overall concepts to specific materials and from fundamental research through engineering development and operations at sea. Best known is the search for the best reactor cooling configuration, in which parallel efforts on PWRs, liquid metal (sodium), and gas-cooled reactors were conducted. Another example of this strategy is simultaneous work on Hf and Ag alloys for control rod material applications.
- NR R&D (including the S&T component) also benefited from stable budgets, most of which came from the AEC.

- Research management at NR HQ was under the directors of technical groups (physics, materials, etc.), not project officers. The internal NR budget-setting process ensured that decisions to cut or fund additional projects were viewed as those affected the overall program goals. When a new idea got a priority, "everything was up for grabs, not just research." That meant that the S&T research budget was not automatically cut when additional money was needed elsewhere. In addition to the budget stability, the long tenure enjoyed by NR technical leaders encouraged them to think long term, since they would still be around to benefit or suffer from the effects of S&T research management decisions.
- Long-term personnel tenure was achieved. Typically, people stayed with NR for many years, in sharp contrast to the then-normal personnel rotation in the federal government. In addition to encouraging senior people to take the long view, it resulted in NR possessing an extraordinary "corporate memory." The knowledge accumulated through experience in the HQ technical groups and dedicated laboratories was invaluable. Many difficult technical problems required long-term commitment.
- The director of NR was intimately involved in managing research. He defined the technical areas of research concentration, reviewed individual projects, and actively managed the research portfolio.

APPENDIX A—EXTRACTS CONCERNING S&T AND INNOVATION FROM "THE EDUCATION OF ADMIRAL HYMAN G. RICKOVER"

Edited by: David F. Winkler, Ph.D., Naval Historical Foundation
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PROGRAM

Welcome and Introduction of Speaker...Vice Admiral Robert F. Dunn, USN (Ret.)
President, Naval Historical Foundation

"The Education of Admiral Rickover"...Dr. Francis Duncan, author, Rickover, *'The Struggle for Excellence'*

Panel Discussion:

Moderator: Vice Admiral Dunn

Panelists:

Admiral Kinnaird R. McKee, USN (Ret.)

Director, Naval Nuclear Propulsion (1982–1988)

Admiral Bruce DeMars, USN (Ret.)

Director, Naval Nuclear Propulsion (1988–1996)

Admiral Frank L. "Skip" Bowman, USN

Director, Naval Nuclear Propulsion

* * * *

DUNN: …Did his long tenure in the job prevent innovation?

McKEE: I'll talk about that for a moment. First of all, the question is: What's innovation? Innovation is generally a word used by people with no responsibility for innovation. I don't think very many people realize how many things he started in those early years. By the time Nautilus went to sea he already had four or five other nuclear plants in design, with specific ship applications. He did natural circulation. He did liquid metal. He did a number of very innovative things. And by the time I got there, even though he'd been there for a long time, there were a lot of very innovative ideas, particularly in instrumentation and control, that made a tremendous difference in the way we were able to operate the ships, that were well underway. New turbine generator sets, for example.

But he also resisted innovation for its own sake. And I think if you really pull the string far enough, that's generally what the people who grumbled about the lack of innovation meant. A good example is the Soviet Alfa submarine. "Why didn't you build a submarine like they did?" Well, we didn't want to personify the ocean, among other things. You remember that the Russians had a term—I can't remember how you say it in Russian—but what the sailors would call pay was "Have no babies" pay. I'm serious. They had such terrible problems with radiation. Also you always have to remember that a submarine's a blimp. It's got to have neutral buoyancy. So when you talk about changing the way things are done, you have a set of boundary conditions that simply cannot be avoided.

BOWMAN: Bob, let me also take a stab at that last question about innovation. We are beginning in these last couple of years the thirty-fifth design of a reactor. These thirty-five reactors have gone into what will be twenty-five different reactor plants over the course of these some fifty-two years or fifty-three years of the Naval Reactors program. Now, some people who haven't been there would argue that these thirty-four or thirty-five different reactor plants are thirty-three or thirty-four of the same one, just done a little bit differently. But from the war-fighter's point of view each one of these changes, each one of these different reactor plants, brought real value to the skipper, to the war-fighter. Whether it was a more reliable plant, a longer-lived reactor core avoiding taking the ships off line for refueling, a reactor plant that was simpler to operate and therefore easier to fight in war, each of these innovations—and I call them that on purpose—are innovations to the war-fighter. Not necessarily innovation to those who haven't been there.

DUNN: I was intrigued in part of Frank's book where, when the subject of whether Kennedy should be nuclear-powered or conventionally powered was addressed, that Admiral Rickover had in mind, or perhaps had designed, a four-reactor ship. But it never came to fruition because of largely political reasons. And then when Nimitz came out we had a two-reactor ship. Rickover argued for the four-reactor ship, as I understood it, on the advantages of redundancy. I do remember steaming in Nimitz—I made several cruises in Nimitz—and I early came to the conclusion that I was steaming in a two-boiler ship, which I didn't like very well. Now, the proof of the pudding is that these two-boiler ships have done quite well over the years. But I think Admiral Rickover was certainly right there, and I think that's kind of an indication of his unwillingness to reach out further than you really have to.

DeMARS: I think the other way to look at the innovation question also is: Compared to who? What Navy has done better with nuclear power than the United States? We have the best submarines. We have the best aircraft carriers. And at the heart of why they're the best is nuclear power. So you don't have to go much farther than that, and most people understand that, I think.

DUNN: Well, that leads into another question I had here, but I can't find it in this stack right now. Dr. Duncan, did you look, as you were doing your research for the book, into any of Admiral Rickover's reactions regarding the Soviet nuclear development?

DUNCAN: No. And the reason I didn't was—two reasons. Partly, by that time I was writing as an individual. I had no clearance. The other thing was I could suspect from what little I did know that it was still such a sensitive topic when I was writing the biography that I couldn't have handled it…

DeMARS: But I thought, Frank, in your history of the nuclear power program you talked about when Rickover went aboard the Russian icebreaker early in the program. They were going to give him a tour of the berthing spaces and the bridge or something, and he ended up in the engine room, of course, opening doors and continuing to go down. That's where he got a real feel that they were so far behind, because of their pipes and their valves and their components. And he said: We're ahead and we have to figure out how to stay ahead. But I think that's the only thing.

And of course he, as we did, always got all the intelligence briefings that went on in the Navy that had to do with what the Russian navy was doing, so we knew what the delta was between us and them.

DUNN: This question comes from the floor, but it ties into a question somebody wanted me to ask as well. Could you make a case that the continuity—that Naval Reactors is a model for other advanced technology programs? For example, if Rickover had been chosen to head the space program vice the nuclear Navy, would we have—somebody has written here, I'm sorry I don't know who you are because we didn't ask for names—would we have made it to the moon? Hey, we did make it to the moon! But the second part is, would Star Wars be a reality today? Anybody?

McKEE: I think that's a little bit hypothetical.

DUNN: Okay. Hypothetical question.

DeMARS: But the tenure thing, I think, is always held up as a model, and other branches of the military try and strive for that. But it's overwhelmed by the quest for jointness and, to a degree, careerism. It just makes it very hard to stabilize very important jobs in all the services. And they're not just technical jobs. They're technical jobs; they're jobs that have to do with a lot of money; they're jobs that have to do with important policy. Director of the National Security Agency—he serves two or three years and then they get a new guy, and on and on and on. So it goes across the areas. The thing that the Navy has is very unique, but somehow that model hasn't propagated to other places, for a number of reasons that are good, bad, or indifferent.

DUNN: Right. Did you have a comment, Dr. Duncan?

DUNCAN: In doing the research on the very early days, when he came up from Oak Ridge and so on, before the program was really started, and in the years thereafter, so many circumstances combined to make his program possible. For instance, there was the cold war. But there was the importance of the detonation of the Soviet weapon program and so on, that let him be able to get through some of the material that he'd been searching for, trying to get. He couldn't get it. But once there was that military reason behind it, he could get it. But it took fighting all the way.

Another thing was the unique circumstances in Congress at that time, and the ability, the art, he had of managing Congressional things. I'm not sure whether you can start out, you can have a program like that that is imposed, or whether it has to grow. My belief from what I've written, obviously, is that it's a number of events that coalesced that made it possible.

DUNN: Well, I like especially the way you wrote about Admiral Rickover working directly with the Congress and really not caring about what anybody else might say. And

I'm mindful of the episode in Secretary Nitze's office, where Secretary Nitze said: You're not supposed to tell the Congress anything we haven't approved. And he says, go peddle your papers; I'm going to go ahead and tell the Congress anything I want. I know that's the way Admiral Bowman does it every day.

BOWMAN: No comment! [*laughter*]

Bob, there was an interesting article in the New York Times financial section just this Monday on this tenure issue that is not directly to the point, but close enough. This article reads, "Of the 4,058 equity portfolios tracked by Morningstar, just 54, less than one percent, avoided losing money in any of the years from 1992 through 2001. Of those 54 funds, 31 also succeeded in beating the Standard and Poors each year over those ten years. You need a rather unique fund to pass these tests," it said in the article. "A pretty elite group. Members of it do share some characteristics, most notably the long tenure their portfolio managers...These funds generally take a mild-mannered, risk-conscious, yet forward-moving approach." Pretty close.

DUNN: Good. That's good dope…

DUNN: I have a question here specifically for Admiral McKee. What do you believe Admiral Rickover would have thought of the electric-drive warship?

McKEE: Well, again, that's a theoretical question. He spent a lot of energy on electric propulsion in nuclear submarines, as you know. He built the Tullibee, a small, relatively slow direct-drive electric submarine; then he built a big one, Glenard P. Lipscomb. Neither of those came anywhere near the performance of the 688 class. And certainly, they don't even approach the performance of the Seawolf or the Virginia that will follow. There are great advantages to electric propulsion, but I think they're not as real as a lot of people think they are.

BOWMAN: Let me say just a couple of things, Bob. Those two experiments, or those two prototype submarines, are first of all evidence that Admiral Rickover didn't shy away from innovation and didn't shy away from testing the next generation.

But to Admiral McKee's point, a lot of things have changed since we tried those, specifically in the field of power electronics. It's another example of Moore's Law these days, in the generation-jumping that power electronics have taken. Motors themselves now are much more power-dense than before, and much quieter and much smoother operating. But more importantly today, if indeed we're going to execute some of the ideas that we're fairly keen on—about off-board sensors; and unmanned underwater

vehicles being launched from our submarines; large-volume unmanned aerial vehicles being launched from those large volumes—these new payloads, these new off-board sensors are going to require electricity that we don't have today, electricity that we don't have because we reserve about 75 percent of the reactor's power and energy—power—for the main engines.

So putting all that output of the reactor into turbine-generators, and then drawing from an electrical grid the power that you need to move the submarine, and yet having it available for powering these other payloads and sensors, may be very important and just around the corner. That's why we're looking afresh at the idea of electric drive.

DUNN: Good. This is a question that, not being a graduate of Admiral Rickover's university, I don't understand. But I'm just going to read it. Discuss the development of rapid reactor recovery post-Thresher.

DeMARS: Why?

DUNN: Why? Okay, I understand...

DeMARS: No, let me give a quick three-sentence answer. The reactor was not the cause of the loss of the Thresher. In response to the loss of the Thresher, Admiral Rickover and his staff accelerated their hard look at what could be done to make sure that the reactor contributed to helping ships recover. So they changed the procedure to reduce the time it took to get the reactor back on the line after it scrammed. And during that period you were maintaining propulsion. So that was an adaptability thing. And the rest of the Navy, of course, did the same things in all their areas. But it was learning from a very tragic lesson and employing that very quickly, training the whole fleet.

I'll always remember: I had been on the George Washington and we did it the old way. I went to a tour of shore duty. I went to the Snook, and they said: Well, you've already been an engineering officer of the watch; just go back and do a couple of drills and you'll be re-qualified. The first drill was this new procedure. The sailors were well-trained, everything happened right, and we got back on line very shortly. So I said: God, that was amazing.

So it was an amazing response to a very serious issue. And that's part of the growing of the Program, I think.

DUNN: Okay. This question goes back to something we were discussing earlier. Do you believe that the Navy would benefit from the Secretary of Defense, the Secretary of the

Navy, the CNO, and/or other senior civilians and naval personnel serving eight-year terms?

BOWMAN: I can think of one of those that would benefit. And I'm not going to tell you.

My answer to that is, this tenure, this longevity in the position, gives the leader of the organization the time to not be impulsive, to not think that he has two years or three years to make his or her mark, and I think that contributes dramatically to the success of the organization. I think that those institutions in our government that have had long tenures have been, on the whole, more successful than those that rotate people through every two or three years. So it's a thought, to do that with political appointees. It would be difficult, though, because of the very nature of the political appointment. You'd first have to have a President that would last that long, and agree that these are the right people to put in for that period.

McKEE: I think also you have to think about the people that are working for you. What you need in a demanding technical organization, that not only designs and invents things but builds them and has the responsibility for maintaining them—in that regard, this program is almost unique—you've got to have people in the staff that are going to be there for a pretty long period of time, and are going to be highly regarded in the disciplines for which they're responsible. If you've got Christmas help running the place, they're not going to stick around. They're going to look for something else to do.

While I was there we re-established some of the contacts we'd had with the Royal Navy in the nuclear submarine world. And I took three of my top people with me to look at what they were doing and how we could do some things together. And two of the three were offered roughly four times the salary if they'd just stay. But they all stayed with us. I think if this were a short-term sort of deal, you wouldn't have that kind of people.

DeMARS: But on the tenure thing, I think you have to look at why. It's just not necessarily to make organizations run better, because long tenure does a lot of other things. It slows down the pipeline, all of those sorts of things. It's the imperative of the proper operation of nuclear power that makes it the right thing to do in this business. You have to look at all the other businesses where they want to make the tenure longer, and make sure that there's an imperative there that requires it. It is the imperative in nuclear power. I mean, a couple of bad mistakes and you shut down the entire Navy. So that's a very strong imperative. And so that drives the eight-year tenure in this business, I think…

DUNN: I have just one more question here, and then two people in the audience have asked to make short statements. Then I'm going to ask each of you if you have anything further you want to say to kind of wrap things up, and then we will wrap it up.

The last question—if you'll bear with me because this is one of these things I read about in the book, but I'm not sure I fully understand. In 1967, there was a difference of submarine design concept between Captain Don Kern and his "conform" design, and Admiral Rickover. Can you comment on this? And did the alleged letter of agreement between Captain Kern and Admiral Rickover, in which the admiral agreed to support Captain Kern's design, ever surface? Any comments? You wrote about it in the book.

DUNCAN: Yes, I think the question's addressed to me. As far as I know, the situation is this. There were two competing designs. There was the one of the submarine desk, which was Captain Kern's, and that was called the "conform" design, because it was a design in which there were several input studies. The "concept formulation" was the way it was brought out. And the admiral's approach was a different one, and it was on the propulsion plant, but again, of course, the other things couldn't have been left out. The statement is made, and I think it's in Pat Tyler's book, that there was such a letter, in which they would agree to which one would become first at a given point to get the money for a prototype. To the best of my knowledge that letter has never surfaced. And I think I tried to cover that. I don't want to bother looking it up, but it's in there someplace.

DUNN: Okay. Any of you have anything to say? Okay, now for two short statements please… [Captain Bing Gillette's comment on nuclear sailors skipped.]

DUNN: Okay. Comment? And Jack Crawford?

CRAWFORD: One of the most important questions asked here tonight was the question, "Was Naval Reactors ever used as the model for any other government program?" Now, before I answer the question, which is "yes," I'm going to say that I spent fifteen years in the Naval Reactors program and then about twenty-five in various government agencies—the Atomic Energy Commission; ERDA, Energy Research and Development Administration; DOE; and then lastly two tours in the Defense Nuclear Facilities Safety Board.

Going back to the question. A very strong attempt was made to apply Naval Reactors principles to the program of the Atomic Energy Commission in developing civilian reactors. What happened was, Congress was irate. They demanded changes, they demanded order of the kind that they had experienced in the Naval Reactors program. So

they arranged for Milton Shaw, project officer on the carrier Enterprise and a masterful manager, to come over and head up the division of reactor development in the Atomic Energy Commission. What he did was recruit, elicit, invite, all the Naval Reactors alumni of competence that he could into the program. Most of the assistant directors, for example, were NR graduates, some of the very best.

Over a period from about '64 to about '73, when Shaw was sacked for being too good at the thing, we did straighten out the civilian reactor program of the AEC. I didn't say the civilian reactor program of the utilities, but we straightened out the reactor development program of the AEC. We had the whole works from Naval Reactors—some of its best people, a newly instituted intern program, interviews, the lore—you name it, we had it. And over the course of about seven years we developed the breeder reactor program to the point where a fast flux test facility operating on liquid metal coolant was put into operation out in Richland, just being shut down now some forty or so years later. So, I say, the program lasted for a substantial amount of time and proved that it can work.

Now let me shift to the downside of the story. The Rickover model was attempted, at least in my observation, in a number of areas in the AEC—more modest attempts in the AEC—in ERDA, and also in DOE. But never with the force that it was done in the division of reactor development. The fact is that those organizations resist, with all the power and force and manipulation they can, the introduction of Rickover principles.

How do I know that? Because of my role, because of my experience with them, and because of my more recent experience as a charter member of the Defense Nuclear Facilities Safety Board. That board was given by Congress the job of raising the level of technical expertise in DOE. It made efforts to do that over the period I was there. But we did then—and they still do—encounter a massive resistance to the type of managerial force that is directed from any organization based on Rickover principles. In the first place, there are some fundamental objections to it. The national laboratories oppose it, to a laboratory. The contractors don't want it, and they use all sorts of artifices to go around to Congress and to bring down, to thwart, any effort to straighten up the program so constituted.

I'll say one final thing. After six years of trying this, I wrote a 110-page report entitled, "Technical Management Problems in the DOE." I would like to tell you that that report—having been circulated to all members of the Senate committee on the armed forces who are responsible, and to the Secretary of Energy, etc.—I would like to tell you

that gradually improvements are being made in the DOE's weapons defense program. They are not. Any sample you take, and I've taken a number, would convince one that the technical management capability in the nuclear weapons program of the DOE is retrogressive.

I've been asked to be brief. That's all I'm going to say.

DUNN: Okay. Thank you, Mr. Crawford…

Now for the penultimate feature of this session. We'd like to hear if any of our panelists have anything in particular to say. And we'll start with the active duty first.

FROM THE CLOSING COMMENTS OF THE PANELISTS:

BOWMAN: Bob, I think to sum, perhaps speaking for the other two just a little bit, we all three inherited an operation that is universally recognized as the best in what we do. It has been certainly my job to keep it there. So some questions that would come close to "What have you changed? What haven't you changed?" are pretty easy to answer. You don't change the success. You don't change the core value. But sometimes it's easy to commingle core value with culture and administration and process. So certainly, I think, all three of us changed the culture to a certain extent, the process, the administration, but not the core value. I think that if Admiral Rickover were to walk in today, into the brand new building, he'd have no trouble whatsoever recognizing his program…

DeMARS: I would like to illustrate one other aspect I think is very fundamental in the program. It jumped up at me when I read the recent bio by David McCullough of John Adams, which I'm sure you've read. Adams was a very feisty guy. He was a New Englander and he spoke up a lot, sometimes when he shouldn't have. Took on principled causes all the time. He was quoted some time, I think when he was Vice President or something, and he said, "I have long since learned that a man may give offense, and yet succeed." When I read that line I thought, "Rickover." Clearly. And I think at the heart of his business—and I think the three of us that succeeded him in this job sort of had the seeds of that in us, but I didn't realize it fully until I got into the organization—friction was encouraged inside, in the organization; friction was encouraged with the outside; and that was the crucible where truth got banged around and the right ideas came out. So I think that was a very important part of the program. I'm sure it's still alive today. Good, honest discourse, disagreement on things, and then once you make the decision, they get up and march off and do the right thing…

APPENDIX B—"THE DEMANDING CUSTOMER"

(Extract from "An Assessment Concerning Safety at Defense Nuclear Facilities: The DOE Technical Personnel Problem," (DNFSB/TECH-10) by John W. Crawford, Jr., March 1996)

It is a paradox that despite the power of management systems there is so much difficulty in carrying out large-scale, technically complex projects and programs. Such activities are normally conducted under contracts between the customer and one or more contractors engaged to carry out the associated functions. The customer will seldom have all the specialized technical capabilities in the depth and numbers required to accomplish these tasks, but it will certainly have large financial and technical interests in assuring effective management of the operations they entail.

Direction and guidance provided by the customer for contractor activities can take different forms. In many instances, the customer will arrange with contractor organizations to perform specific functions like research and development, design, procurement, construction, testing, and quality assurance, but will retain management of the total effort. In other instances, the customer will enter into arrangements where managing the total effort will be assigned to a selected lead contractor. The latter may still perform functions like those cited or have them provided by other organizations. Depending on the organizational arrangements involved, there will be one feature common to all—the need for the customer to exercise management across a customer-contractor interface. It is a difficult terrain. For one thing, customer management cannot use the direct measures and techniques available when the organization does the job with its own personnel. Few, if any, members of the customer's organization will have authority to direct the specific actions of contractor personnel. Management must be accomplished by other methods. Experience has shown the methods that are effective and those that are not.

The key principle is that management and other capabilities of the customer's organization should be used basically for one function: namely, to require and otherwise bring about effective management by the contractor organization or organizations to assure performance in accordance with the contract. The decisive test for any action

contemplated by the customer is whether it is conducive to this objective. *The principal pitfall is that the customer will use its capabilities to compensate for continuing weaknesses of the contractor*. Like other management principles, this one is logically compelling but difficult to apply. Departures from this principle are at the heart of countless management problems between customers and contractors. Many departures are deceptive in appearance; their very subtlety calls for managerial alertness to recognize them.

A second principle is that the customer should set forth technical requirements in sufficient breadth and depth to assure that the product will meet customer objectives, but not in such degree as will stifle contractor management, initiative, and innovative capabilities. A corollary is that the customer needs to be able to adjust requirements, as practicable, to accommodate difficulties being encountered.

The prerequisite need in applying these principles is that the customer has "in-house" capability as measured by technical competence among its own employees to shape, guide, direct, and assess the activities and operations of its contractors. No one would deny that the customer must have financial, legal, and administrative capability and that these should be competent enough to negotiate from a position of strength with their contractor counterparts. However, one does not find a comparably strong consensus on the need for customer organizations to have corresponding strength in technical management.

In carrying out complex technological programs the customer must make decisions over a broad spectrum of technical issues. Help in addressing such issues can often be obtained from third parties. Even so, it still takes technical competence to know what questions to ask and who can best provide answers. In the end, the responsibility for making technical decisions (many with large implications for cost, schedule, and performance) is a responsibility from which the customer can never escape.

Once contractors have been chosen, the need for a demanding customer capability, both technical and non-technical, will increase. The objective of intelligently applying the technical capabilities of a customer will be that the contractor perform at the standards required. As a result, there will be a need for contractors to match strength with strength. The converse is also true. If the customer organization lacks technical strength, the contractor will not feel the same pressure to achieve excellence. In this world of limited numbers of strong performers, even the best and most dedicated contractors will have difficulty manning all jobs with cadres equal in capability. Thus contractors will

tend to deploy their best talent consistent with incentives to perform which emanate from the customer. In this respect, a demanding customer capability is the best assurance that a project will be given priority by the contractor when it comes to the assignment of his most capable personnel.

Having cited the need for strong customer technical capability, it is important to caution against its misuse. *The general caution is that it should not be used to do work or perform functions for which the contractor is being paid*. This is a self-evident proposition, but it is regularly violated; for example, assume the customer has engaged a contractor to design a large technically advanced facility. As elements of the preliminary design are reviewed, system by system, customer personnel often find it necessary to urge redesign or reconsideration for what is poor, or marginally acceptable, work. The customer will often be able to reinforce these assessments by advancing better concepts and design features than those proposed by the contractor. Contractor personnel, anxious to please the customer and acknowledging the validity of his objections, will tend to adopt the revisions being urged. A situation can develop progressively in which customer technical personnel become, in effect, an adjunct of the contractor's design review organization.

Many customer personnel would not perceive this as happening; some would not find it objectionable if they did. Such individuals find professional satisfaction principally from making a contribution to the solution of problems and, not infrequently, from the appreciative remarks by the contractor about such contributions. It takes a firm hand to keep them from subverting the larger interests of their own organization.

There are major objections to allowing this pattern of inordinate reliance on the customer to develop. One is that the contractor will see no need to improve his deficient performance. The contractor will not be giving the customer that level of performance for which he is being paid. The irony is that customer personnel will have been aiding him in the process. The second is that the customer, by his intimate involvement, is giving up his position of full objective review. The pattern of activity described is likely to be most pronounced at middle levels of management. Customer *middle*-management is often reluctant to see that the problem is brought to the attention of contractor *top* management. Thus, the latter are shielded from the problem while the customer shoulders the task of solving the problems that arise.

It is the job of customer top management to stop the misapplication of technical talent which has this effect. An indifferent management may not be aware that behind the

rapport between customer and contractor is a design activity which reflects disproportionately more input by the customer than the contractor. The design also may be embodied more in the nature of compromise than customer top management would find acceptable if it knew the circumstances. The result is that the customer's capability has been used not to bring about strengthened contractor management but rather to help preserve it in a state of weakness.

A demanding customer will insist on developing clear, mutually agreed-upon understandings about relationships with the contractor. True responsiveness by the latter *always* obliges the contractor to use his own good judgment in questioning suggestions made by the customer staff if the contractor believes them to be ill-advised. Responsiveness is to be measured, not by the extent to which the customer responds automatically to guidance from customer representatives, but rather by the degree of responsibility exhibited in analyzing such guidance and then in acting on it or recommending reconsideration as appropriate. It is also to be emphasized that differences in important matters are not to be held unduly long at lower levels, where they foster animosity and weaken cooperation. Instead, they should be raised promptly to higher levels of management for resolution. The objective to be sought is open, constructive dialogue between the parties, giving the primacy to objective technical and other considerations and suppressing personal predilection and bias. The message to be conveyed is that the contractor has been engaged to use his best efforts and resources to provide a product or a service. He can be responsive only to the extent that he does this.

Circumstances may arise in which the customer, on the basis of its own experience and needs, will want to insist on courses of action that the contractor would not recommend as the preferred ones. Both parties should be clear about the matter when this is the case. They should also assure that the prerogative to make such decisions as are involved is not exercised on either side by individuals who are not authorized to make them.

The need for the demanding customer to have "in-house" capability emphatically should not be taken to imply that the numbers of personnel be large. A customer operating in a sound managerial relationship vis-à-vis a contractor should be able to provide the needed managerial oversight with far fewer numbers than the contractor is obliged to use. As problems arise, however, pressures often develop to increase numbers within the customer organization, to better cope with problems. As such demands arise, continuing vigilance is needed to avoid falling into the trap cited earlier of trying to

compensate for contractor weakness by doing the job for him. *The job of customer management is to convey assessments of contractor performance to contractor management, taking problems as high and as rapidly up the managerial ladder as is necessary to bring about corrective action and results*. The ability to do this depends more on competence than numbers. Thus, the objective should be to keep competence up and the numbers down. It is impossible to place too much emphasis on the role of customer top *management* in this process. They must have the competence to satisfy themselves that their key personnel are qualified to provide direction and guidance to the contractor, *but never doing his work for him.*

The difficulty which customer personnel often have in keeping the interests of their own organization in mind can be heightened when the site or sites at which the work is carried out are located at a distance from the place at which the customer's management, technical, and other capabilities are mainly located. Under these conditions, a field office will ordinarily be established at the work site. Here the customer's representatives interact with the more numerous contractor personnel. In proximity to the contractor's forces, field representatives easily lose the objectivity so essential to representing the customer and its interests effectively. Surrounded by contractor personnel, field representatives often acquire an outlook that more nearly represents the contractor's viewpoints than judgments consistent with the customer's own interests. When this happens, the representative needs to be replaced.

The matters cited thus far concern interactions between customer and contractor in line activities like design, construction, procurement, and testing. The avenues for assuring effective management during these activities are pretty much self-evident. It requires more managerial acumen to be aware of the full potential of the opportunities provided by the contractor's quality assurance program. A strong quality assurance program in the contractor's organization reinforces the efforts of the customer to assure strong line management. Such quality assurance is at its best when it anticipates the customer and operates to head off problems before the need arises for customer action. Operating inside the contractor's organization, the quality assurance organization is usually in a better position than the customer to discern developing problems and also to get a full understanding of the contributing causes. Yet managers in customer organizations often fail to appreciate these advantages and, thus, do not give sufficient attention to making sure that contractor quality assurance is strong.

Sometimes customer managers may resign themselves to the quality assurance function within the contractor being less than adequate. Again, they try to compensate for this contractor weakness by adding more quality assurance personnel in their own organization. The problem should be attacked where it is found—by insisting that the contractor's program be upgraded as needed until it is performing effectively. The customer just cannot afford to lose the advantages such a program provides. The demanding customer will not do so.

In closing, it may be well to recall that in coping with intractable problems, the temptation is to look for ever more elegant managerial solutions. Yet the answer is more likely to be found in a return to basic principles. In coping with the massive problems of building large-scale, technically oriented projects, there is the need to return to management fundamentals—those of the demanding customer. The greatest need will be to establish an ordered, disciplined, *well-documented* relationship between customer and contractor. This means a relationship in which the customer, fully endowed with the capability to manage, uses that capability in all its technical and other dimensions to insist that the contractor meet the standards of excellence agreed upon between them. *It also means not doing the contractor's job for him.* Accomplishing these very modest objectives of good management may not bring popularity; however, it will most surely go a long way toward bringing in projects within costs, on schedule, and meeting technical requirements.

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IX. POLARIS SUBMARINE-LAUNCHED BALLISTIC MISSILES: MANAGING SYNERGISTIC RISK

Andrew W. Hull

A. PROGRAM OVERVIEW

The Navy initiated its first solid-fuel rocket-propellant-development program in 1942 at the Naval Powder Factory. This program led to the fielding of solid-fuel air-to-ground rockets in 1943. From that time onward, Navy laboratories and contractors more or less continuously pushed solid-fuel development, especially to improve the specific impulse of solid energetic materials. This work included extruded double-base propellants (1945), internal-burning grain propellants (1945), polymerizable composite propellants (1949), and increased burning rated by using end-burning grains (1955).[1]

Navy interest in ballistic missiles began in late 1945 when the Bureau of Aeronautics put forward the idea of placing a satellite into Earth orbit. Dr. Harvey Hall (a civilian scientist working for the Navy) played a leading role in this project that envisioned using a liquid hydrogen-oxygen single-stage rocket (called Viking) to put the satellite into orbit. It soon became evident, however, that "full Navy support for an actual flight test vehicle program would not be forthcoming."[2] Nevertheless, the Chief of Naval Operations (CNO) was still willing to provide enough support in May 1946 to keep the program alive temporarily.

As part of early efforts to develop Viking, the Navy launched a captured German V–2 rocket from the deck of the aircraft carrier *Midway* on 6 September 1947. This proof-of-principle technology demonstration had major consequences, the most important of which was to prove that a missile could be successfully launched from the rolling deck of a ship at a time when skeptics doubted both the safety and technical feasibility of such

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1 Wyndham D. Miles, "The Polaris," in *The History of Rocket Technology*, ed. Eugene M. Emme, Wayne State University Press, Detroit, 1964, p. 165.

2 Vincent Davis, *The Politics of Innovation: Patterns in Navy Cases*, the Social Science Foundation and Graduate School of International Studies Monograph Series in World Affairs, Vol. 4, Monograph No. 3, 1966–1967, p. 31.

a launch. The Viking satellite launch vehicle program subsequently spawned a proposal in 1952 to build a military version capable of traveling 500 miles.[3]

From the late 1940s through the early 1950s, a tacit and informal alliance was built between younger naval officers and Navy civilian scientists to urge the Navy's leadership to become involved in a variety of missile, rocket, and Earth-satellite development projects.[4] During these early years, this alliance of young officers and scientists lobbied, presented supportive research papers, and used every opportunity and forum to keep the issue of a fleet ballistic missile (FBM) on the table.

Early interest in ship-launched ballistic missiles came to naught when the director of the Navy's guided missile program and the CNO vetoed the Viking project. The Navy's senior leadership was cool to the idea of ship-launched ballistic missiles for several of technical reasons. First, long-range cruise missile technology was far advanced over that of ballistic missiles in the early 1950s. (Indeed, some technology forecasters were then estimating it would take another 20 years to solve the numerous and complex technological problems associated with ballistic missiles.) Second, it was unclear whether ship navigation was sufficiently accurate to define the missile's launch point with precision. Third, the Navy conducted an experiment called Operation Pushover in 1949 to investigate the effects of an accident caused by a liquid-fueled V–2 on board a ship. The dangers of a liquid-fueled missile catching fire on ship left a lasting impression: "One look at the mess, and a shudder ran through every ship in the Navy."[5]

Early efforts to promote Navy development of ballistic missiles also met with bureaucratic and emotional objections. For one thing, it was not clear whether development should be the province of the Bureau of Ordnance or the Bureau of Aeronautics, both of which had quite different views of how to proceed if the Navy desired its own ballistic missile capability. There were also deep-seated fears in the Navy that developing ballistic missiles would be very expensive and that any resources would be siphoned off more mainline programs, such as shipbuilding. (Then Captain Hyman Rickover in the Bureau of Ships, for example, was one of the most vocal critics, fearing that work on ballistic missiles would siphon funds from the continued development and/or fielding of

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3 Ibid.

4 Ibid., p. 32.

5 Ibid., p. 33, as quoted in Graham Spinardi, *From Polaris to Trident: The Development of U.S. Fleet Ballistic Missile Technology*, Cambridge University Press, 1994, p. 20.

nuclear submarines.) Also, some naval officers did not believe that a strategic bombardment of cities was an appropriate Navy mission, nor did many believe that ballistic missiles would be much use against "targets of naval interest." Finally, some feared that the placement of ballistic missiles on ships (especially on submarines) would adversely affect career patterns and mission responsibilities.

Advocates of ship-launched ballistic missiles faced powerful obstacles from within the Navy, from other Services (especially the Air Force), and from the Eisenhower Administration's desire to hold down overall defense spending by limiting the number of ballistic missiles that could be developed. Beginning in 1954, the Head of the Surface-Launched Missile Branch of the Bureau of Aeronautics and the Chief Scientist of the Bureau's Research Division launched a campaign to establish a ship-launched missile program. They began by seeking support outside the Navy. In the summer of 1955, for example, they briefed the Technological Capabilities Panel of the President's Science Advisory Committee (the so-called "Killian Committee") on the advantages of ship-launched ballistic missiles. The Committee's final report included a statement that there was a national requirement for a sea-based ballistic missile.

Although the endorsement by the Killian Committee did not lead to a requirement endorsed by the CNO, it did help legitimize the concept of ballistic missiles within the Navy. More specifically, Killian Committee papers helped persuade influential figures within the Bureau of Aeronautics like Captain A.B. Metsger (director for the guided missile division) and Rear Admiral William Schoech (the Assistant Bureau Chief for R&D) to support the concept of developing an FBM.

Next, the Head of the Surface-Launched Missile Branch of the Bureau of Aeronautics (Rear Admiral James S. Russell) decided in July 1955 to act on his own authority to establish a sea-based ballistic missile program and moved with such speed that it proved bureaucratically impossible for the CNO to stop the program. Rear Admiral Russell then used his prerogative as Chief of a Bureau to bypass the CNO and appealed directly to the civilian Assistant Secretary of the Navy for Air, who agreed to support the program.

The program received another powerful boost when Admiral Arleigh Burke became CNO in August 1955. Burke became an immediate supporter of the fleet ballistic missile program and directed the entire Navy establishment to get behind it. Burke did more than just support the FBM program—he made it one of the highest priority Navy programs.

The CNO's decision was probably spurred by several factors. Among them was his desire to secure an important strategic mission (with all its attendant political and budgetary implications). Another was that the Navy had been successfully operating a nuclear-powered submarine for the first half of 1955 and thus had an apparently ideal platform for carrying out the strategic strike mission with ballistic missiles.

Unfortunately, the agreement about the need for the Navy to have a sea-based ballistic missile came too late for the creation of an independent Navy program. In September 1955, the President and senior officials of the DoD decided that the nation needed only four separate ballistic programs and so approved three Air Force projects and one Army program.

The Secretary of Defense did, however, allow the Navy to join the Army's liquid-fueled Jupiter program and, in response, the Secretary of the Navy created the Special Projects Office on 17 November 1955. The Navy's first step was to get the Army to modify some of the Jupiter's parameters to make it more suitable for basing on ships. After working with the Army for almost a year, the Navy requested permission from the OSD Ballistic Missile Committee to undertake an accelerated research, development, and feasibility study of solid-fuel ballistic missiles. In March 1956, the Navy received limited permission for a parallel solid-propellant program, with the understanding that the solid-propellant program would be only a variant of the basic Jupiter. The Navy used this opportunity, however, to act on its own initiative to begin the new FBM program, which was eventually accepted by the DoD in December 1956.

Senior Navy officials (including the CNO) realized, however, that the Navy had to get a fully functioning FBM into service quickly or face being shut out of the strategic bombardment mission by its rivals, the Air Force and Army. Thus, the Special Projects Offices was formed in 1955 with a charter to have a ship-launched ballistic missile ready for service by 1965. In the end, the Special Projects Office managed to introduce the Polaris A–1 missile (see Figure IX-1) into the fleet by 1960. The transition into the fleet was eased by the availability of nuclear submarines on the building ways at New London, one of which was cut apart so that the missile section could be inserted. This ship (the *George Washington*) was launched on 8 June 1959.

Figure IX-1. Polaris A-1 During Testing

B. BUREAUCRATIC STATUS OF THE SPECIAL PROJECTS OFFICE

The Special Projects Office was set up as a "special and unique in the organization of the Navy—a Manhattan District type of organization."[6] The Director, Rear Admiral William F. Raborn, reported directly to the Secretary of the Navy, and the FBM program was reviewed and approved as an entire package.[7] The Ballistic Missile Committee of the Navy, the OSD, the Joint Chiefs of Staff (JCS), and the National Security Council (NSC), in turn, reviewed this total package. Significantly, the very powerful technical bureaus of the Navy only played a supportive role and thus had no

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6 *Polaris Management: Fleet Ballistic Missile Program*, Special Projects Office, Superintendent of Documents, U.S. Government Printing Office, Washington, D.C., Revised September 1962, p. 3.

7 Ibid.

direct control over the course of development. This not only provided the Director of the Special Projects Office with the autonomy and freedom of action to set the scientific and engineering direction of the program, but also foreclosed the kind of infighting among the bureaus that had hampered earlier efforts to form an FBM program.

Dr. William Whitmore, who served as the Chief Scientist in the Special Projects Office from 1957 to 1959, later observed:

> The fleet ballistic missile system development represents an outstanding example of what can be done by an integration of a government-industry-academic team granted high priority and considerable administrative freedom.[8]

Whitmore goes on to say that much of what the Special Projects Office did (and how it went about doing it) is contrary to current government regulations and procedures.

Dr. Whitmore also recounted a meeting years later with Rear Admiral Charles Martell, who was then in the process of merging the Bureau of Ordnance and the Bureau of Aeronautics to form the Bureau of Weapons. According to Whitmore, Rear Admiral Martell remarked:

> Of course, Special Projects is the best thing the Navy has ever done organizationally—and, of course, we must never repeat it.[9]

C. SUBSTANTIVE S&T CHALLENGES

The Special Projects Office faced several difficult and potentially critical S&T challenges. Areas of major concern included[10]

- Determining the basic parameters (e.g., size, weight, etc.) of major sub-systems at the outset of the development process. (Accurately determining these parameters was critical given the concurrent nature of the development process and the tightness of the schedule.)
- Warhead design, including developing a small size and lightweight warhead, developing a high-yield device in a small package, and ensuring the survival of the warhead during atmospheric reentry.

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8 William F. Whitmore, "The Origin of Polaris," *Proceedings*, U.S. Naval Institute, Vol. 106/925, March 1980, p. 59.

9 Ibid.

10 For an in-depth discussion of how the Special Projects Office dealt with most of R&D issues, see Spinardi, *From Polaris to Trident*, pp. 39–56.

- Missile guidance and fire control.
- Precise navigation to determine the missile firing point with accuracy.
- The inadequacy of many existing metals and materials.
- How the missile was to be launched underwater, including issues like interaction of the missile with the sea during its ascent to the surface.
- Missile propulsion, including developing a high-impulse, large-diameter solid propellant; developing a strong but light casing for the rocket motors; thrust-vector propulsion controls for steering the missile; a means for terminating thrust; and curing the propellant after casting to avoid cracks developing.

D. S&T MANAGEMENT ISSUES

The Polaris missile project involved managing "enormous synergistic risk" to accomplish the interdependent development of dozens of different technologies.[11] The project was also forced to explore uncharted technological paths where even some of the basic phenomenology was not understood.

The S&T management task was complicated in several ways:[12]

- Because the technical problems of each subsystem had to be solved by narrowly focused specialists, there was a constant danger that the solution chosen to the subsystem problem would be detrimental to the large system.
- There was no certainty that agreed-upon projections of rate and direction of technological progress with each subsystem area were accurate. And even if the general projections were accurate, predicting how and when a particular technology advance would fit into the projection was still difficult. Thus, a constant danger was that a simple error in the selection of subsystem options could cause the whole system to fail.
- Technical branches of the Special Projects Office had considerable independence from centralized control.
- Developers operated under a tight (and frequently accelerated) schedule for completing the project.

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[11] "Management Book Review #1," *The Polaris System Development*, Cadmus, wysiwyg://154/http://wwwcadmus.ca/bookreviewpolaris.htm .

[12] Harvey M. Sapolsky, *The Polaris System Development: Bureaucratic and Programmatic Success in Government*, Harvard University Press, Cambridge, 1972, pp. 138–139.

E. S&T MANAGEMENT STRATEGY

Over the course of the Polaris program, a strategy evolved for dealing with S&T issues and for defining a role of scientists in what was essentially a weapons/systems development program. Some of this strategy was a matter of conscious choice, but some was dictated by necessity and/or evolving circumstances.

The program's managers emphasized developmental engineering first and foremost but made room for scientists because they could make a valuable contribution of the project's success. This was because all aspects of the project "involved pushing back the frontiers of science to a degree and scope which had never before been done."[13]

The issue of striking the right balance between engineering and science was one of the fundamental problems faced by program managers (PMs), but was never officially codified into roles and mission statements. Consequently, the relative impact of scientists waxed and waned during the project but never disappeared.

The program consistently enlisted the aid of scientists in

- Evaluating technological plans.
- Validating and encouraging engineers' approaches to problems and providing a sounding board for engineering proposals. (The Director of the Polaris program, for example, established a special advisory committee with the help of the Naval Ordnance Laboratory to review independently the program's important technical decisions and test results.[14])
- Identifying technology opportunities.
- Studying key phenomenological problems (e.g., the impact of wave-induced motion on a missile moving to the surface from various depths).
- Helping estimate the expected parameters for subsystems still to be built.
- Providing public validation of the scientific reasonableness of the Polaris program and its various concepts to the public, the administration, and Congress.

One of the major functions of the Chief Scientist and the Engineering Consultant was to maintain liaison with the scientific community. In part, this was to ensure that

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13 Vice Admiral (Ret.) William F. Raborn, "Interview No. 1 with Vice Admiral William F. Raborn," on September 15, 1972, Ed. John T. Mason, Jr. (Director of Oral History), *Series of Interviews on the Subject of Polaris*, U.S. Naval Institute, Annapolis, May 1982, p. 30.

14 Sapolsky, *Polaris System Development*, p. 155.

outside advice would be intelligently evaluated and interpreted. The Chief Scientist also acted as the Special Project Office's ambassador to the scientific community. Any scientist who had a question on technical issues was invited to attend a briefing on the entire program and was asked if he would be willing to contribute by working a research problem in a relevant technology area. The program attached so much importance to building trust (or silencing critics) that each technical branch in the Special Projects Office always had some money set aside to follow-up on suggestions of outside scientists. This was true no matter how relevant those suggestions were to the branch's established technical goals.[15]

Rear Admiral Raborn recognized the political importance of winning support from leading scientists from the very beginning. That was because scientists in the mid-to-late 1950s had become the final arbiters of whether a major defense project should be pursued. According to one account of the Polaris program,

> Faced with questions on the technical feasibility of the FBM system, Admiral Raborn apparently recognized that his own defense of the program would be severely discounted due to his lack of advanced technical training and to his deep involvement in the program's promotion. Thus, he sought whenever possible to buttress the program with the endorsements of top defense scientists. In congressional testimony and official briefings, it was always a statement from a noted weapons expert on a scientific advisory committee rather than one from Admiral Raborn that was used to defend the feasibility of a given technological goal.[16]

Rear Admiral Raborn had good reason for this policy. A report from the Committee on Undersea Warfare of the National Academy of Sciences (NAS) had gone a long way in establishing the national security requirement for a submarine-launched ballistic missile when it said that such a missile was both feasible and desirable. Subsequently, other scientists added their endorsement based upon the reputations of the scientists on the committee even though they themselves may not have been especially conversant with the subject technologies.

That policy notwithstanding, the Technical Director retained control over the formulation of the program's development effort. This was in direct contrast to the

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15 Ibid., pp. 49–50.

16 Ibid., p. 49.

practice in the Air Force missile programs of the time, where outside scientific advisors could become deeply enmeshed in the direction of the development effort.[17]

A special Steering Task Group (chaired by the Technical Director of the Special Projects Office) was established in 1957 with the twin missions of recommending an optimum Polaris submarine system, including its parameters, and reviewing and advising on the technical progress during the program. This Steering Task Group met bimonthly and was composed of scientific representatives of Lockheed, Aerojet, General Electric, Westinghouse, Sperry, MIT, AEC, CNO, Bureau of Ships, and Naval Ordnance Laboratories.[18]

The Group, in turn, was divided into subcommittees corresponding to major functional areas of the FBM system (e.g., communications, missiles, submarine design). These subcommittees met monthly to review technical aspects of the project and ascertain the progress in assigned areas. These subcommittees were generally chaired by the technical branch chiefs of the Special Projects Office and prepared technical progress reports, which were forwarded to the parent Steering Task Group 2 weeks in advance of its bimonthly meeting. The minutes, subcommittee reports, and recommendations of the bimonthly meeting were then published. These became the basis for developing technical proposals and the eventual Technical Development Plan.[19]

Thus, the Technical Director, by virtue of his role as Chairman of the Steering Task Group and as the source of the Technical Development Plan, had a powerful influence over the course of R&D. Even though he could curtail the involvement of outside scientific and technical advisors, he could not eliminate that involvement entirely. For one thing, Rear Admiral Raborn invited the Chief Scientist and Engineering Consultant to attend weekly meetings at which the Technical Director briefed Rear Admiral Raborn on issues and progress. Raborn also relied on the Chief Scientist and Engineering Consultant to offer alternative views during those meetings.

Even though the Special Projects Office concentrated its efforts on actually building a system, it still sponsored a good bit of R&D. Sometimes the sponsorship was a matter of choice, and, at other times, it was dictated by necessity. R&D was, for example,

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17 Ibid., p. 50.

18 *Polaris Management: Fleet Ballistic Missile Program*, pp. 4–5.

19 *Polaris Management: Fleet Ballistic Missile Program*, p. 5.

carried out in areas where it was needed to achieve an initial (if only rudimentary) operating capability. It was considered more important to meet the original deployment schedule than to delay the schedule to take advantage of a technology that offered improved future performance. At the same time, PMs recognized that realizing the promise of immature technologies was essential if the FBM program was to reach its ultimate operational goals.

The twin priorities of getting a missile into the fleet at the earliest possible time and harnessing the potential of immature technologies to meet ultimate operational goals led the Special Projects Office to adopt a two-pronged strategy. The first part of that strategy was to incorporate only the most mature technologies into the A–1 model, even if that meant accepting lower performance and not meeting optimal operational goals. The second part was to start development of more capable next-generation systems, even before development of the initial model was completed. As depicted in Figure II-2, the Special Projects Office simultaneously began development on the A–2 model in 1958, 2 years before the A–1 version was fielded. Concurrent product development allowed the Special Projects Office to meet its tight original schedule using mature technologies while, at the same time, betting that the problems associated with higher performance (but immature) technologies could be overcome so that they could be incorporated in the A–2 missile. Likewise, development of model A–3 commenced before A–2 was finished.

The success of the concurrent development strategy rested in large part on a willingness to pursue R&D into immature technologies that promised to expand greatly the capabilities of later models. This was done to reduce risk and to speed the eventual fielding of a more robust operating capability that came closer to satisfying the original operational goals for the FBM concept.

At times, the Special Projects Office was forced by circumstances to support research into S&T issues, perhaps to an even greater extent than it wished. Sometimes theoretical and experimental research was authorized to rectify problems, such as base

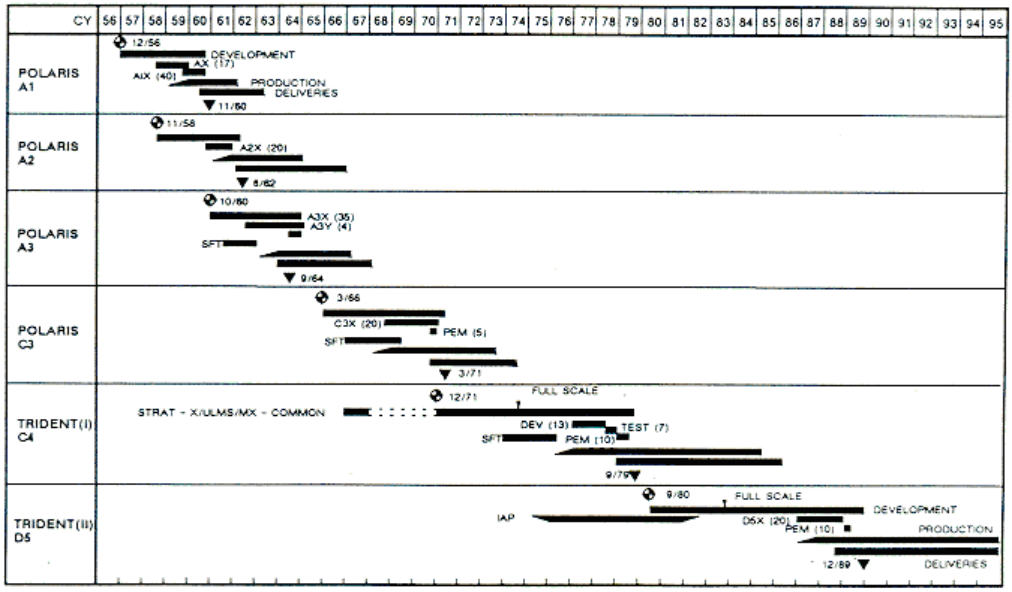

Figure IX-2. Missile Development Programs

overheating problems that were uncovered during testing.[20] In another case, a navigation concept that relied upon referencing a distinctive feature of the ocean floor required the collection of the necessary data by survey ships that actually mapped the ocean floor.

At other times, applied research was necessary to build equipment for proof-of-concept testing on key components, such as the underwater launching scheme.[21] In a similar vein, the developers built a console simulator for the Mark 84 Fire Control Subsystem to determine experimentally an operator's ability to perform a key experiment to investigate base heating functions with varying levels of automation (see Figure IX-3).[22]

The developers of Polaris missiles also benefited from the earlier investments of others in applied R&D. In the late 1940s (well before the advent of the Special Projects Office), members of the Metallurgy Branch of the Office of Naval Research (ONR), in

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20 Office of Public Affairs, Arnold Engineering Development Center, Arnold Air Force Base, http://www.arnold.af.mil/aedc/systems/62-1317.htm .

21 Miles, "The Polaris," p. 170.

22 Joseph G. Wohl, Ed., *Human Factors Design Standards for the Fleet Ballistic Missile Weapon System*, Vol. 1 ("Design of Systems"), NAVWEPS, OD 18413A, published by direction of the Director, Special Projects, May 1963, pp. 8–9.

Figure IX-3. Investigating Base Heating Problem

conjunction with materials experts in the Navy's Bureau of Weapons, recognized that a need *would eventually exist* for metals that would be strong at high temperatures. Based upon this perception of future need, the ONR initiated a "well coordinated program" into such materials in concert with industry and universities. Because of this program, which ran from 1948 through 1956, crucial problems associated with molybdenum and its alloys received attention. Eventually, the conditions and limitations for the use of this material and its alloys were established.

A retrospective Institute for Defense Analyses (IDA) study notes the following about the results of this forward-looking applied R&D into molybdenum and its alloys:

> The principal payoff occurred some years after ONR had reduced its support of research in this area. When the Polaris missile needed material for jetavators,[23] the necessary molybdenum technology was available. It must be emphasized, however, that at the beginning of the program the concept of Polaris was unknown to the materials monitors.[24]

Although histories and management case studies of the Polaris project indicate that a good bit of S&T was conducted, it is difficult to say with any precision how much of the project's resources went into that area. One of the few sources that breaks down Special Project Office budgets by type of activity notes that investments in "R&D" varied across time from a high of 100 percent in 1956 to a low of about 35 percent in 1960 when the first Polaris was field.[25] It is unclear, however, what was included under the "R&D" label. This could cover a widely divergent range of activities— e.g., design efforts, system testing and evaluation, basic and applied research—not all of which should be

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23 Jetavators were devices used to make in-flight course corrections.

24 Edward I. Salkovitz, Ronald W. Armstrong, and John P. Howe, *Case Studies of ONR-Supported Research*, IDA Paper P-645, October 1970, p. 9.

25 Sapolsky, *Polaris System Development,* p. 164.

considered "S&T" efforts. Nonetheless, these figures (coupled with the testimony of others that there was always some money available to pursue S&T projects) suggest that the Special Projects Office did indeed make a significant and sustained investment in S&T research, even though it is impossible to determine the amount precisely.

The Special Projects Office was also very receptive to innovative ideas and would sometimes undertake "idea safaris" to elicit them from private industry. In keeping with this policy, Rear Admiral Raborn would often visit contractors and listen to ideas from technical personnel. The Special Projects Office also exploited its ad hoc Steering Task Group of top scientists and engineers to conduct "brainstorming" sessions about proposed systems and concepts that were still just ideas.

Raborn's willingness to listen to outside technical experts had important consequences. Warhead size was one of the most significant technology issues, because small changes in weight had a multiplier effect on the amount of total propellant impulse required to reach a given range.[26] The availability of a small, high-yield warhead meant that the overall size and weight of the missile could be greatly reduced. A smaller, lighter missile, in turn, made it easier to install FBMs in submarines. In the summer of 1956, Edward Teller was attending a Navy-sponsored summer study at Nobska Point. At this meeting, Teller claimed that nuclear-armed torpedoes could be substituted for conventional high-explosive ones and could serve as a new antisubmarine weapon. At the time, the idea seemed farfetched given the current size of nuclear warheads. When challenged to support his assertion, Teller noted that the *trend* in warhead technology indicated reduced weight-to-yield ratios in each succeeding generation. When Teller was asked whether this same principle could apply to the Polaris program, he simply responded, "Why use a 1958 warhead in a 1965 weapon system?"[27]

Teller went on to predict that the 1 Mt weapon desired by the Special Projects Office could be made to fit the missile's envelope within the timeframe desired. Raborn's reaction was to take the unprecedented step of requesting written estimates from Los Alamos and Lawrence Livermore Laboratories regarding expected yield-to-weight ratios for the next several years.[28] The AEC then endorsed these requested estimates.[29] Armed

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26 Spinardi, *From Polaris to Trident*, p. 29.

27 Edward Teller as quoted by Spinardi, *From Polaris to Trident*, p. 30.

28 Whitmore, "The Origin of Polaris," p. 56.

29 Whitmore, "The Origin of Polaris," p. 56.

with these official estimates, the Special Projects Office then proceeded to develop overall missile designs based on *expectations* of having an as-yet-unavailable small, high-yield weapon by the time the missile development process was ready.

The decision taken with respect to estimating warhead capabilities had far-reaching effects. The Special Projects Office took the bold step of concentrating on "the emergence of technology trends" to replace "traditional reliance on technology events"[30] as in other programs. Consequently, Polaris PMs "always projected their thinking and planning well into the future—in almost every arena."[31] Doing so was both bold and risky since mistaken estimates on the timelines of available technologies could have disastrous downstream consequences.

The encounter between Edward Teller and representatives of the Special Projects Office at the Nobska Point meeting also had far-reaching consequences for Lawrence Livermore National Laboratory and the future of U.S. nuclear weapons design. According to Kent Johnson (Chief of Staff for the Laboratory's Nuclear Technologies Division), "Polaris was the turning point in nuclear weapon design."[32] More specifically, Livermore designers developed radical new designs for the primary and secondary initiators and novel ways to minimize the overall mass for the Polaris missile. According to one member of the Livermore design team, development of the Polaris warhead involved one major breakthrough and about four other major important ideas.[33] These design improvements, in turn, were "adopted in most subsequent U.S. strategic nuclear weapons…They set the tone and stage for the modern nuclear stockpile."[34]

The resultant warhead was a marvel. The combined weight of the warhead and the reentry body was less than 900 lb (in keeping with Edward Teller's earlier promise to deliver a warhead on the order of 600 lb that would weigh no more than 850 lb when combined into an integral reentry vehicle). To understand the significance of this accomplishment, one needs to understand the state of the art before the development of a warhead for Polaris. At the time, other Services were using warheads weighing over

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30 "Management Book Review #1."

31 "Management Book Review #1."

32 Kent Johnson, as quoted in "Fifty Years of Innovation Through Nuclear Weapon Design," *S&RT*, Lawrence Livermore National Laboratory, January/February 2002.

33 Interview with Carl Haussmann, as cited by Spinardi, *From Polaris to Trident,* p. 54.

34 Kent Johnson, as quoted in "Fifty Years of Innovation Through Nuclear Weapon Design," op. cit.

1,500 lb and combined warhead/reentry vehicle weights of 3,000 lb! The half a megaton yield also exceeded the program's self-imposed minimum requirements of 300 kt.[35]

The Special Projects Office tried to limit significant participation by Navy laboratories unless they possessed a technical competence not available in private organizations.[36] This was because they were seen as too sensitive to cutbacks in staff and facilities and/or to a shift in externally directed priorities. In addition, Navy laboratories held charters from the government. This allowed them, at times, to be unresponsive to clients, and thus their behavior could not be as easily controlled as a private contractor. Clearly, some Navy laboratories did participate in the Polaris program and made significant contributions. Nonetheless, their opportunity to make that contribution was usually the result of necessity (or, as we shall see next, of political expediency) rather than choice.

At the same time, the Special Projects Office recognized that it and the FBM program gained some much-needed legitimacy within the Navy by the "ostensible extensive use" of existing naval facilities, expertise, and command structures.[37] The Special Projects Office embarked upon a calculated policy of co-option, whereby naval facilities seemingly played a major role but, in fact, had their influence marginalized. The naval laboratories were, for example, invited to participate in the Polaris Ad Hoc Group for Long Range Research and Development, which was seemingly going to guide the future of the FBM program but which actually had little influence on program plans. Thus, over time, potential critics of the program and its developmental efforts were drawn into the program and implicated in its activities.[38]

Time was critical. Consequently, the Special Projects Office often pursued multiple approaches for developing satisfactory technologies and system concepts applicable to fulfilling ultimate requirements.[39]

- While the Special Projects Office was considering whether to abandon participation in the liquid-fueled Jupiter program, Lockheed drew up four

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35 Spinardi, *From Polaris to Trident,* pp. 54–55.

36 Sapolsky, *Polaris System Development,* p. 145.

37 Ibid., p. 48.

38 Ibid., p. 48.

39 Wohl, *Human Factors Design Standards,* p. 2.

basic preliminary plans for a small solid-fueled missile—each of approximately equal range and weight but differing in dimensions.[40]

- A particularly good example of this process was the approach for dealing with the atmospheric heating problem of the warhead during reentry. Initially, the Special Projects Office opted for the "heat sink" approach because the technology was more mature and thus could be used with confidence on the Polaris A–1. At the same time, the Office continued work on ablative materials for the warhead and eventually substituted ablative materials for "heat sinks" in subsequent generations of Polaris.
- The Special Projects Office pursued several schemes for ejecting the missile from the launch tube. These included using compressed air, a small rocket fastened to the base of the missile, the missile's natural buoyancy, and a balloon that would pull the missile to the surface.[41]
- Developers also considered two approaches for launching the missile under water. The "dry" approach involved encasing the missile in a shell, which would surround the missile all the way to the surface where the skin would peel off like the skin from a banana and the missile would ignite. The other concept envisioned a "wet" launch, where the bare missile would be launched unprotected through the water.[42]
- The issue of missile guidance also raised difficulties and so alternative technical paths were pursued. MIT's Instrumentation Laboratory investigated how inertial technology could be made small enough for use in a missile. The Instrumentation Laboratory also offered a "mathematical scheme" that became known as Q-guidance. This second approach involved using an onboard computer to make fairly simple adjustment calculations after the main elements of the Q matrix were programmed in advance of the flight at the Naval Ordnance Station in Dahlgren, Va.
- One of the critical aspects of the program was the navigation problem, in part because it was crucial for overall system accuracy and because the Air Force had repeatedly raised the issue while attempting to undermine the Navy's claim to the strategic missile mission. One as-yet-unproven approach in 1956 was reliance on a self-contained inertial navigation onboard the submarine. MIT's Instrumentation Laboratory believed in a system that relied upon letting gyroscopes remain in fixed orientation with respect to stars. As the stars moved and the submarine changed position, the gyroscopes were

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40 Miles, "The Polaris," p. 166.

41 Ibid., p. 171.

42 Ibid., p. 171, and Spinardi, *From Polaris to Trident*, pp. 40–41.

subjected to a varying gravity field. This led to imbalances of the rotors and hence to potentially significant errors. As an alternative, the Special Projects Office also investigated using the XN6 guidance system originally developed for the cancelled Navaho cruise missile program. The XN6 used a system whereby each of the three axes had two gyroscopes, which could be reversed, thereby averaging out the "drift" of the gyroscopes.[43]

- The Special Projects Office also investigated other less technologically advanced alternatives to the submarine navigation problem. One of these relied on surveying the ocean floor with sonar, identifying distinctive features, and using those as reference points for navigation. The program also considered a more accurate version of Loran (Long-Range Aid to Navigation), which navigated by calculating the time differences between the arrival of radio signals from widely spaced, land-based transmitters in fixed positions.
- For solid propellants, the Special Projects Office investigated both composite compounds and double-base cast propellants.
- The Office also explored alternate approaches to building rocket motor cases—low-alloy steel and fiberglass.

Pursuing multiple technical approaches simultaneously served several purposes. First (and foremost), it offered a way of finding at least a marginally satisfactory initial operating capability so that the design schedule could continue according to plan. Second, it served to identify, evaluate, and develop alternative technologies that offered better operational capabilities—ones that came nearer to meeting the ultimate operational goals of the Navy for an FBM. When such immature technologies showed promise, the Special Projects Office funded further R&D so that these technologies could be perfected in time for use on later models of Polaris, which were being concurrently developed along with the initial model.

Sometimes pursuing different technological paths also meant pitting different organizations and researchers against one another (e.g., MIT's Instrumentation Laboratory vs. the North American Aviation for the navigation technology.)[44] Rear Admiral Raborn strongly believed that such competition produced not only the best results, but also spurred rival organizations to work harder and more quickly less they lose out to a rival.

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43 Spinardi, *From Polaris to Trident*, p. 47.

44 Ibid., pp. 46–47.

The Special Projects Office sponsored more than just technically oriented research. For example, the Office made a major investment in pioneering human engineering research. In addition, the Director of the Program and Plans Division directed the development of a new project management tool that came to be called the program evaluation review technique (PERT). This was a simple technique for developing a flow chart that could tell PMs when a piece of equipment would be ready and how long it would take so that the PMs knew when to begin certain aspects of the project. PERT also allowed the senior managers to identify critical paths, key nodes, and overall program progress. This technique became so popular that the Special Projects Office set up briefings and seminars to teach others the technique.[45]

Rear Admiral Raborn also emphasized getting the highest quality people and then capturing them in such a way that they were totally dependent upon the Polaris project succeeding. At the beginning of the program, the CNO offered Raborn his choice of any 40 officers he wanted to assist in the FBM project.[46] In addition, the Civil Service Commission was persuaded to extend unusually high job ratings to positions in the Special Projects Office, thereby ensuring it access to just about any specialist it wished to bring onboard.[47] At the same time, Raborn made sure that each man knew that his personal success (and even continued employment) was directly tied to the success of the Special Projects Office. This ensured their dedication and willingness to put in long hours without complaint.

The same management philosophy applied equally to contractors. Rear Admiral Raborn "obtained from [contractors] promises of a completely dedicated group of people and separate buildings for our work."[48] The Special Project Office's commitment to having "captive" contractors even extended to constructing buildings as necessary "in

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45 For a detailed discussion of the PERT technique, its evolution, and use by the Special Projects Office see Sapolsky, *Polaris System Development*, pp. 94–129.

46 "Interview with Admiral Arleigh Burke, U.S. Navy (Retired)," ed. by Thomas T. Mason, Jr. (Director of Oral History), *Series of Interviews on the Subject of Polaris*, U.S. Naval Institute Annapolis, May 1982, p. 11.

47 There is a story (perhaps apocryphal) that when the local Civil Service Commission office slow-rolled Raborn's requests for upgrading civilian personnel slots, he persuaded the Head of the Civil Service Commission to intervene on behalf of the Special Projects Office. The Head of the Civil Service Commission then (according to the story) ordered his Deputy to go down to the local office, where he ordered local officials to process the necessary paperwork while he waited.

48 William F. Raborn, "Interview No. 1," p. 34.

order that our work could be absolutely segregated from the rest of the contractor's work and be given absolute top priority in their plants."[49]

To some members of the Special Projects Office, the selection of the right people was one of the most important factors in the success of the Polaris program. One member of the Office felt that Raborn's genius arose from his being a visionary, one with the ability to find the right people and to drive them unmercifully while making them enjoy being driven.

F. RELEVANCE FOR THE MISSILE DEFENSE AGENCY (MDA)

Both the FBM and ballistic missile defense (BMD) programs have operated at the confluence of emerging technologies and perceived national security needs. Consequently, both programs have been immediately enmeshed in technological, bureaucratic, and political controversies. Such controversies became even larger because of the significant investments required to reach fruition, often at the expense of other resource claimants.

Both programs have required incorporating dozens of immature technologies, charting unknown technological paths, and achieving technical breakthroughs at the time the projects were conceived—a problem compounded because multiple breakthroughs were required in multiple components (all of which had to be achieved) for the systems as a whole to succeed.

The two programs have strong organizational similarities. Both were created as special administrative entities and thus were outside the normal bureaucratic structures. The Strategic Defense Initiative Organization (SDIO), the Ballistic Missile Defense Organization (BMDO), and the MDA have all reported directly to the SECDEF. The Navy created the Special Projects Office outside the fiefdoms of the all-powerful bureau structure, which traditionally controlled all Navy design and development projects. This ensured special visibility to both programs. It has also given them a higher bureaucratic profile and greater access to high-level decision-makers.

There are also striking similarities in their internal organization. Both had jobs entitled "Chief Scientist" and "Technical Director." In addition, they were both organized into functional subdivisions. These organizational similarities were more than

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[49] Ibid., p. 34.

coincidence (e.g., an article in *Defense News* claims that the current Director of MDA deliberately modeled MDA after the Polaris Special Projects Office).

Finally, the development of Polaris ballistic missiles should be of interest because of its success. It is so highly regarded that a GAO report described the Navy's FBM program as "one of the few major weapon system acquisitions that, over the years, has consistently met or bettered its cost, schedule, and performance goals."[50]

There is, however, one major difference in the two programs. The FBM program had a single mission and unchanging requirements from its inception: produce a ballistic missile that could be launched from a submarine to deliver one or more nuclear warheads on target. Missile defense, by contrast, has had several different goals over the years. These have included developing (1) a space-based national missile defense against an all-out Soviet attack, (2) global protection against limited strikes, (3) ground-based theater defenses, and (4) ground-based national missile defense against rogue states and accidental launches.

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50 General Accounting Office, *Defense Acquisition: Fleet Ballistic Missile Program Offers Lessons for Successful Programs*, GAO/NSIAD-90-160, September 1990, p. 8.

X. SCIENCE AND TECHNOLOGY IN ATLAS MISSILE DEVELOPMENT

Richard H. White

This chapter addresses the need for investments in scientific and technological inquiries as part of the Atlas ballistic missile development program. Atlas (see Figure X-1) was selected because, as the first U.S. ICBM, it represented a watershed for U.S. strategic capabilities that had not been previously possible with then-existing engineering and technological know-how. Because of the growing prowess of Soviet missile capabilities in the 1950s, the development of Atlas could not wait for S&T to catch up to support the deployment of an operational U.S. ICBM. As a result, the paths of scientific and technological advance and of developmental and operational testing were intertwined in the program. What resulted was a project then referred to by the U.S. Air Force as "the greatest single research and development undertaking in the history of the United States, exceeding in scope even that of the Manhattan Project."[1]

Figure X-1. Atlas B/C

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[1] John L. Chapman, *Atlas: The Story of a Missile*, Harper and Brothers, New York, 1960, p. 8.

A. A BRIEF HISTORY OF ATLAS

The 25 February 1963 issue *of Aviation Week & Space Technology* contains a concise and insightful summary of the Atlas missile development program. It notes that over the 5-year period from 1957 to 1962, 87 Atlas missiles were tested. The first, launched on 11 June 1957 (Atlas 4–A (see Figure X-2), was a failure. The last, launched on 5 December 1962 (Atlas 21–F), was a success. "In between these two Atlases, which were similar in name and concept only, were 85 other missiles of varying configurations, varying objectives and varying degrees of success. Many problems had to be overcome involving guidance and control; high heat associated with atmospheric reentry; engine design, staging, and control; and alternate methods of airframe fabrication."[2] Subsequent to the test program, 126 Atlas missiles, each designed to be capable of "delivering nuclear warheads to within 2 naut. mi. of targets in the Soviet Union and Communist China," were deployed in the continental United States.[3, 4]

Figure X-2. Atlas A

In the civilian world, Atlas went on to serve as the primary launch vehicle for the NASA Mercury program.

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2 Jacob Neufeld, *The Development of Ballistic Missiles in the United States Air Force, 1945–1960*, Office of Air Force History, U.S. GPO, 1990, p. 73.

3 "Atlas Accuracy Improves as Test Program is Completed," *Aviation Week & Space Technology*, 25 February 1963, p. 54.

4 According to Hughes, the maximum number of operationally deployed Atlas missiles was 99 by 1964. Thomas P. Hughes, *Rescuing Prometheus*, Vintage Books: New York 1998, p. 137.

> [T]o launch a man into earth orbit—the Atlas missile had been selected as the launch vehicle from the very beginning. In reality when the program was approved in October 1958, there was no other rocket vehicle available for selection; that is, if the program objectives were to be reached at the earliest practicable date.[5]

Because of Atlas' strategic vulnerability as a cryogenically fueled (kerosene and liquid oxygen propelled) missile, the military rapidly moved on to deploy its "sister" missile, the Titan (a noncryogenic liquid-fueled missile), and Minuteman and Polaris (solid-fueled missiles). In March 1965, the last of the operational Atlas missile squadrons was deactivated.[6]

B. THREE TYPES OF S&T CHALLENGES

The developers of Atlas encountered three major types of technical challenges in pursuit of an operational ICBM: (1) those where phenomenology was not well understood; (2) those where additional technical investigation was necessary to support engineering solutions; and (3) those where testing was required to identify unknowns and perfect system integration. The dynamics of reentry vehicles (RVs), the need for greater precision in inertial guidance, and the development of a successful boost vehicle, respectively, are primary examples of such challenges within the overall Atlas development program.

1. Understanding the Phenomenology of Nose Cones and Reentry

In the early 1950s, the only available evidence of the dynamic loads and heating to be expected as an object reentered the atmosphere was from meteorites that survived the journey. The materials necessary to deal with such loads and heating and their behavior in such a hostile environment and the ability to engineer an RV (nose cone) that could adequately protect its payload (a nuclear device) so that it would still operate were all poorly understood.

Historically, the Convair company, an aircraft manufacturer, had played a key role in the thought going into the development of U.S. ballistic missiles. Well before the formal creation of the Atlas program, Convair had been working with the U.S. Air Force

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5 William M. Bland, "Project Mercury," in *The History of Rocket Technology: Essays on Research, Development, and Utility*, Eugene M. Emme, Ed., Wayne State University Press, Detroit, 1964, p. 229.

6 Neufeld, *Development of Ballistic Missiles*, pp. 235–237.

in investigating various aspects of missile design. Much of this work built upon German experience with the V–2. Convair was prompted to engage in this new activity when, in 1945, the U.S. Army Air Force solicited ideas from aviation contractors and requested proposals for "a ten-year program of research and development in four categories of missiles ranging from a low of twenty miles to a high of five thousand miles."[7] Convair proposed two designs: "one subsonic, winged, and, jet-powered; the other supersonic, ballistic, and rocket-powered."[8] The Air Force awarded the company $1.4 million to explore both designs, the ballistic missile variant of which was designated the MX–774. The goal was not to produce a working, fieldable system, but rather to explore the design and technical issues surrounding the proof of concept for such systems.[9]

The Atlas Program Office, because of its involvement with the MX–774, initially funded Convair to investigate the different, competing concepts of the day for designing an RV. According to Chapman, originally there were four different approaches to keeping the warhead cool upon reentry: heat sink, ablation, radiation, and transpiration.[10] "Much pioneer work on reentry was carried out by Convair in the early stages of Project Atlas, and at one time a transpiration-type lab specimen showed promise."[11] Nevertheless, many questions remained unanswered, and further investigation narrowed the choices of technology to heat sink and ablation. For the heat sink, "a quantity of metal such as copper to absorb the heat" would be employed. "[I]f enough mass is used in such a heat sink approach, it stores the heat, allowing an insulated interior to remain relatively cool." For ablation, the technique was "to get rid of heat by allowing some of the external surface of the body to burn or ablate away, carrying the heat from the body by dissipation."[12]

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7 John L. Chapman, *Atlas: The Story of a Missile*, New York: Harper and Brothers, 1960, p. 27.

8 Ibid., p. 28.

9 Ibid.

10 Ibid., p. 81. The heat sink, as the name implies, seeks to absorb the heat energy by conduction and thus keep temperatures below the melting point of the nose-cone material. The ablation type comes down like a meteorite: the heat erodes its surface as it falls. Radiation types seek, with the aid of insulation, to radiate the heat away from the nose-cone surface. Transpiration types use liquids to protect metal surfaces.

11 Ibid.

12 G. Harry Stine, *ICBM: The Making of the Weapon that Changed the World*, Orion Books, New York 1991, p. 98.

Recognizing that there were two potential paths to solving the reentry-heating problem and without prior knowledge of which approach could be successful, the Atlas program management funded scientific and technical inquiries for both. "Solving the reentry problem consumed as much as 11 percent of the Atlas development budget."[13]

As related by Hughes in *Rescuing Prometheus*, basic research into the phenomenology of the "gas dynamics" of reentry, as well as the types of materials and their properties to withstand the project loads, was required.[14] "Not only did choice of material affect heat transfer, but the aerodynamical shape of the heat sink and the nose cone did as well."[15]

> Background studies led engineers and scientists, in conjunction with their academic consultants, to conclude that sufficient theoretical knowledge of copper heat-sink behavior existed to permit design of a nose cone for full-scale test missiles. On the other hand, they concluded, theoretical knowledge of ablation was insufficient to support the design of a nose cone dependent on ablation. In the case of ablation, experiment—or testing—had to precede design. In other words, experience had to come before theory.[16]

As part of the effort to understand the atmospheric effects of reentry on the nose cone, Lockheed Aircraft Corporation was awarded a contract to develop a small research rocket to launch quarter, half, and full-scale models of the Atlas RV. The primary aim of effort was gathering phenomenological data on the atmosphere's effects on the vehicle's skin temperature, pressure, acceleration, radiation, dislocation and ion density. Between May 1955 and March 1957, Lockheed conducted 25 operational research flights employing the X–17 (a 41-foot long, three-stage rocket—see Figure X-3) to boost the multiple examples of each type of model to ultrasonic speeds. These flights also were pivotal in demonstrating that blunt nose cones were the best shape for use on both Atlas and the later Titan missile.[17] After that, one more model was launched on 22 August 1957 to gather more data on nose-cone vibration.[18]

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13 Hughes, *Rescuing Prometheus*, p. 126.

14 Ibid.

15 Ibid., p. 127.

16 Ibid.

17 Cliff Lethbridge, "X-17 Fact Sheet," Spaceline.

18 *The 655th: Missile and Space Launches Through 1970,* 45th Space Wing Office of History, Patrick Air Force Base.

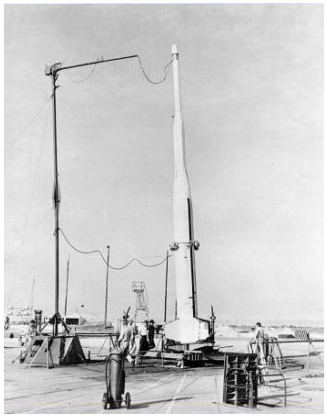

Figure X-3. X–17 Research Rocket

Two separate contracts for the development of the Atlas nose cone were awarded: one to General Electric and one to AVCO, who worked independently on both technologies. To promote competition among the contractors, little information was shared between them.[19] Support for testing and research activities were provided by "existing government, university, and industrial test installations," including facilities at the Jet Propulsion Laboratory (JPL), and the Naval Ordnance Laboratory (NOL). In addition, the Atlas program funded specific needs such as "shock tubes at AVCO's research laboratories and the X–17 reentry test vehicle at the missile systems division of the Lockheed Corporation."[20]

To keep nose-cone R&D on track, the systems engineer, Ramo-Wooldridge, used panels of consultants "from academia and government research institutes to advise periodically on the nose cone problem."[21] A "fresh look" panel, headed by Robert Bacher of Cal Tech, assisted the Western Development Division (WDD) by providing a general review of "high velocity aerodynamics in general," as well as providing insights on the design of different types of nose cones.[22]

Nevertheless, not all went smoothly with the management of the nose cone development activity. For instance, in the view of the Atlas PM and Ramo-Wooldridge, General Electric failed to create a "streamlined organization" that was "dedicated solely

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19 Hughes, *Rescuing Prometheus*, p. 127.

20 Ibid., p. 128.

21 Ibid.

22 Ibid., p. 129.

to the nose cone project."[23] In addition, "[o]n the basis of its computer studies, engineers found the initial design of the GE engineers for a nose cone 'completely technically unsound.'"[24] Over time, General Electric's performance improved, and flight testing proceeded apace.

> By June 1958, seventeen heat-sink nose cones had been tested with wind tunnels, the X–17, and test missile flights. These tests verified nose heat transfer, dynamic stability, pressure distribution, drag calculations, and other parameters affecting design of both the heat-sink and ablation nose cones. Confident about the test data, engineers recommended moving from design and production of heat-sink nose cones to design and production of the theoretically superior ablation for subsequent use on both the Atlas and Titan missiles.[25]

2. Perfecting Inertial Guidance

The maturation of electronics and photonics has today yielded extremely precise inertial navigation systems (INSs) in the form of the ring laser gyro, a device that literally uses the affect that motion has on light propagating through fiber optics to detect acceleration and calculate displacement. In the early 1950s, physical gyroscopes did the same task, albeit with far less accuracy. Moreover, in the initial phases of Atlas development, the most precise way to guide a missile was through the use of radio navigation systems, which relied upon ground-based instrumentation to keep a missile on course. And herein lay a conundrum for the designers and, ultimately, a compromise solution.

As originally stated by the Air Force, the challenge for the Atlas program was to be able to achieve a circular error probable (CEP) of 1,500 ft at intercontinental ranges (although this was later relaxed considerably). Based upon accuracy alone, radio guidance would have been the preferred solution. However, because radio guidance is susceptible to jamming and the electromagnetic effects from nuclear detonations, self-contained inertial guidance was expected to be more robust and reliable in the projected threat environment.

At the beginning of the 1950s, the relative maturity of radio guidance over inertial guidance was considerable. While INSs were being made available for air travel at that

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23 Ibid.

24 Ibid., p. 130.

25 Ibid., p. 131.

time, their accuracy was not yet sufficient to guide a ballistic missile accurately at a range of more than 5,000 miles.[26] "In 1953, available inertial and radio guidance systems were complicated, unreliable, and too inaccurate for us at ranges of 5,500 miles or more if an acceptable CEP was to be attained."[27] Moreover, there existed two technical "camps," one, dubbed the "elektronikers," which saw radio guidance as the most expeditious and robust approach, the other, the "inertial mafia," which favored inertial guidance. As MacKenzie points out, due to the positive press that radio and radar received during the Second World War, radio guidance was initially favored for Atlas.

> In the United States of the 1950s, "probably no technique is more generally associated with guided missiles in the public mind than is radar," and the prestige of radar, following its great success in World War II, was high. Radio, radar, and electronic components had had a great deal of effort lavished on them, much more than inertial components. The "electronikers," as they were sometimes called by proponents of inertial guidance, were indeed well entrenched.[28]

As noted, while radio guidance was attractive because of its maturity, it also had the drawback of being more vulnerable than inertial systems—it could be jammed and its ground tracking stations were vulnerable to attack. The competition between conceptual approaches, as with nose cones, again led the management of Atlas to use a two-track strategy for developing an operational guidance system, and "American Bosch Arma Corporation and AC Spark Plug Division of GM, both working in conjunction with Dr. Draper's guidance laboratory at MIT, were contracted to develop a parallel, self-contained all-inertial guidance system."[29]

Despite the willingness of WDD to fund R&D of inertial systems, this did not mean that the die was cast. First, there was a need to prove that inertial could achieve acceptable levels of performance. Recognizing that an operational test bed would be required, Atlas management decided to use inertial guidance on the shorter range (1,500 mi) Thor missile concurrently under development with Atlas. As a result, the first several series of Atlas missiles (A through D) were to be guided by radio, while, at the

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26 G. Harry Stine, *ICBM: The Making of the Weapon that Changed the World*, Orion Books, New York, 1991, p. 78.

27 Ibid., p. 92.

28 Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance*, Cambridge, The MIT Press, 1990, p. 118.

29 Stine, *ICBM*, p. 98.

same time, inertial guidance systems designed by Draper's research activity would be operationally investigated on Thor. "It was an important contract for Draper's Instrumentation Laboratory. Finally, their accumulated expertise could be put to work in a major national inertial guidance program."[30] As a result, "Thor went ahead with inertial as the primary system and a radar system as back-up."[31]

> MIT-designed gyroscopes and accelerometers were produced for Thor by AC Spark Plug, though not without considerable difficulties and an initially low yield of satisfactory instruments. The onboard computer was analog, but an ingenious mathematical scheme called Q-guidance, developed at the Instrumentation Laboratory by Richard H. Battin and J. Halcombe Laning, shifted the bulk of the computational requirements out of the missile."[32]

Effectively, the government funded an alliance between a university research organization and a major commercial contractor in which the former designed and developed the technology and the latter produced the product. Moreover, this was done in the context of providing an environment in which ongoing research into inertial guidance could be improved, tested, and deployed for a less ambitious system. The ultimate goal was that this would eventually lead to much more robust capabilities suited for Atlas and follow-on ICBM's such as Titan and Minuteman.

Despite the significant efforts put into Atlas inertial guidance, it is ironic that it never did outperform the radio-guided systems of the day.

> The radio-guided Atlas D had performed far better than its five-mile specification.… By 1963, Atlas D test firings were landing within a nautical mile of the target 80 percent of the time and were significantly more accurate than the inertially guided Atlas E and F, which could manage only 1.5 nautical miles 80 percent of the time.[33]

Nonetheless, because of the growing vulnerability of radio systems and the desire to place all the land-based ballistic missile systems in below-ground silos, inertial guidance reigned. As pointed out by MacKenzie, "Radio guidance supporters would have had difficulty in credibly disputing that inertial was less vulnerable: the 1960s move of

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30 MacKenzie, *Inventing Accuracy,* p. 121.

31 Ibid., p. 120.

32 Ibid.

33 MacKenzie, *Inventing Accuracy*, p. 122.

putting missiles into silos increased pressure to get rid of any remaining installations above ground."[34] And in the long run, "[r]adio guidance was never seriously considered for an American ICBM after Atlas and Titan I."[35]

3. Boosting System Design, Integration, Testing, and Synergies

The first use of vertical-launch, liquid-fueled rockets as weapons is attributed to the Germans in World War II with their V–2 system. However, the V–2, a 200-mile range missile operating at Mach 2, was not scalable for use at ranges of 5,000 miles or more. Fortunately, for Atlas, as Stine points out, the development of liquid-fueled boosters of requisite thrust had been pioneered as part of the Navaho cruise missile program. "The available ingredients included a series of unproven booster rockets developed for the Navaho missile program and an airframe design projected for the 1950-concept Atlas ICBM program."

According to Neufeld, other events also conspired to improve the chances of Atlas' success. Most notably, Convair, the prime contractor on the earlier 1950-concept Atlas program, MX–774, "invested its own funds in missile research, including the solution of problems related to pressurized tanks, separation of the warhead (or reentry vehicle) from the missile airframe prior to reentering the atmosphere, and thrust vector steering."[36] "Nevertheless, only the most limited information was available on such crucial items as high-rate-of-flow fuel and oxygen pumps, vibration effects, and the feasibility of igniting a rocket engine at altitude."[37] In particular, "[n]o one knew very much about the vibration effects on the missile's structure and subsystems.… If the combustion frequency goes into resonance with the natural frequency of the missile airframe and its subsystems, it can shake a large rocket vehicle apart."[38]

As an R&D program, the role of the MX–774 was to prove concepts necessary to the development of an operational ICBM. Beginning with the German V–2 as the point of departure, Chapman relates that four major areas were investigated. First, the weight of

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34 Ibid., p. 123.

35 Ibid., p. 122.

36 Neufeld, *Development of Ballistic Missiles*, pp. 69–70.

37 Robert L. Perry, "The Atlas, Thor, Titan, and Minuteman," in *The History of Rocket Technology: Essays on Research, Development, and Utility*, Wayne State University Press, Detroit, 1964, p. 145.

38 Stine, *ICBM*, p. 93.

the missile was reduced by using the structure of the vehicle itself as the fuel tank, which eliminated the need for a separate internal tank. Second, the new design reflected the need for only the payload to reenter the atmosphere, which eliminated the need for heat shielding for the missile body. Third, the stiffeners and stringers traditionally used in aircraft construction were eliminated, and the internal pressure of the fuel and nitrogen used for pressurization were used to provide the necessary structural support. Fourth, the MX–774 used swiveling engines for steering.[39]

As with the MX–774, the Atlas faced a weight constraint to be able to deliver its nuclear payload efficiently and cost effectively. Two issues were involved. The first, partially addressed by the MX–774, was the need to minimize the structural weight of the lifting body. The second involved the weight of the payload (the nuclear device).

To minimize the weight of the lifting body, the Atlas design team decided to build upon the MX–774 experience by using a pressurized-tank concept. At first, the engineers attempted to use a riveted aluminum structure, but found that "no sealant would hold the riveted seams.… They finally settled on stainless steel because it had the same strength and weight advantages of aluminum and yet was weldable."[40] This led to the noteworthy concept of "a stainless steel balloon."

> The skin of the Atlas was as thin as a dime, varying in thickness from 0.020 to 0.040 inch, depending upon the loads the skin had to support. The liquid oxygen and hydrocarbon fuel (RP–1) tanks were the airframe, and a single insulated bulkhead separated the two tanks. The Atlas airframe had no internal structures such as formers and stringers to support the thin skin. The missile had to be kept under an internal pressure of 10 pounds per square inch—a little less than half that required by a radial tire. Otherwise it would have collapsed under its own weight.[41]

At the time of the Atlas lifting body's initial design, a five-engine configuration was anticipated because of the size of the expected nuclear payload. (The subsequent Atlas design required three engines because of the reduction in the payload weight, as discussed below.)

> The first complete tank, based on the five-engine missile and therefore larger than present Atlas tanks, was constructed by Solar Aircraft, in

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39 Chapman, *Atlas*, pp. 32–33.

40 Ibid., p. 88.

41 Stine, *ICBM*, p. 94.

> San Diego, under subcontract, in 1954. Convair trucked its gigantic balloon to a new outdoor laboratory at Point Loma, pressurized it and began putting it through a series of jolts, strains, bends, jabs, and all-around hard knocks to see what it could take. It passed with flying colors.[42]

The weight and size of the payload was what determined the ultimate configuration of the Atlas, the necessary thrust, and number of engines to place a nuclear payload on target 5,000 mi away. While the mass of thermonuclear warheads was rapidly shrinking at the time that Atlas was conceived, they were still quite heavy. As noted, the original Convair conception of the Atlas was a five-engine configuration since that represented the amount of thrust necessary to loft the nuclear weapons of the day. In fact, doubts about the ultimate feasibility of producing an operational ICBM were directly related to the "nature and size of prospective warheads."[43] All this was to change radically in a short span of time.

> By early 1953 the United States had exploded a theormonuclear device. The Atomic Energy Commission was predicting that thermonuclear weapons could be much lighter than their atomic predecessors. The "Operation Castle" tests during the spring of 1953 gave further support to this expectation.
>
> In a meeting of the Scientific Advisory Board of the Air Force held at Patrick Air Force Base in early 1953, Dr. Edward Teller and Dr. J. von Neumann stated that it would soon be possible to build a thermonuclear warhead weighing not over 1,500 pounds with a yield of one megaton.[44]

With this information in hand, the Atlas program management revised its requirement to a three-engine lifting body and, in 1954, began R&D activities to achieve the goal of an operational system by 1960.

C. PROGRAM MANAGEMENT

Periodically in the history of the United States, there has been a need to defy convention to achieve a critical national goal. During World War II, the Manhattan

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42 Chapman, *Atlas*, p. 88.

43 Edmund Beard, *Developing the ICBM: A Study in Bureaucratic Politics*, New York, 1976, Columbia University Press, p. 184.

44 Ibid.

Project was the prime example of such an approach to system development. It was highly classified, set apart from other activities, bureaucratically unfettered, and robustly funded.

In the beginning, Atlas was not seen as special. The reason was two-fold: because it challenged the extant role of the strategic bomber community as the primary deliverer of strategic weapons and because of significant skepticism on behalf of senior scientists of the day, including Vanevaar Bush, that an ICBM was technically feasible in the near future. As a result, in the early 1950s, Atlas was accorded only a small budget and low priority by the Air Force.

Intense lobbying by various administration officials and intelligence reports citing growing Soviet missile and thermonuclear capabilities eventually forced the DoD to recognize the need for the development and deployment of a land-based U.S. ICBM. The Air Force recognized that it might lose control of its strategic nuclear bombardment role if it did not reverse its position and embrace the development of the system. As a result,

> [O]n June 21, General Putt issued a basic Project Atlas directive to ARDC [Air Research and Development Command], officially authorizing establishment of a West Coast field office. Furthermore, ARDC was notified that the Atlas program had been given the highest priority in the Air Force. All major commands had been directed to support the program accordingly. The West Coast office was to have authority to develop the complete weapon system including ground support, and operational, logistic, and personnel concepts. By this directive direct communications between the West Coast office and the Air Staff were authorized.[45]

> [B]y late fall of 1954…the ICBM organizational arrangement was set and the project had been designated the highest priority in the Air Force. The Western Development Division [WDD] was established as a branch of Headquarters ARDC. General Bernard A. Schriever was both Commander, WDD, and Deputy Commander, ARDC, thus ensuring immediate coordination of WDD and the parent command. The Ramo-Wooldridge Company [RW] was assigned systems engineering and technical direction of the entire effort. RW and the WDD occupied the same headquarters building in Inglewood, California.[46]

The use of the Ramo-Wooldridge Company as the systems engineer and technical director for the effort was very controversial. The accepted Air Force approach of the day was to hire a prime contractor—usually one of the major aircraft manufacturers—to

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45 Ibid., p. 171.

46 Ibid., p. 178.

perform the design function, oversee all the subcontractors, undertake integration, and ultimately produce the product. However, the Atlas PMs evaluation of suitable prime contractors suggested that even Convair, the developer of the original Atlas concept, was not sufficiently well versed in the full range of scientific, technical, and systems engineering domains to develop the first ICBM successfully.

> In raising substantial doubts about the ability of Convair and other large airframe manufacturers to manage an ICBM program, the committee and Gardner stirred up a hornet's nest. The entrenched prime-contractor approach relied upon an airframe manufacturer to design an airplane that would meet Air Force specifications, to supervise the design of subsystems by subcontractors, to take responsibility for keeping them on schedule, then to assemble the sub-systems and flight-test the weapon. In the opinion of committee members, the manufacturers did not have the personnel and test facilities to fulfill such functions in designing and developing an ICBM; nor did they have a finely honed systems engineering approach comparable to the one take, for instance, by AT&T or by Ramo's division at Hughes for building tightly knit electroinic communications systems.[47]

What emerged was a management approach that relied heavily upon attracting the best and brightest scientific and engineering talent to work along multiple paths toward a set of interdependent objectives. Ramo-Wooldridge was able to offer salaries at levels attractive to the most talented individuals as well as opportunities to engage in leading-edge research. To ensure success, General Schriever was convinced of the need to maintain interest and participation from the scientific community at large.

> I strongly believe that the program needs the continued support of the scientist; therefore, the organization cannot be anathema to him. In a negative way, he would probably withdraw his active support; and, in a positive way, he might argue very convincingly at the highest levels the need for a special organization outside the military to carry out the program. If successful, such a move could permanently endanger the philosophy of military research and development.[48]

All this was overseen, and to a large degree micro-managed, by the Atlas program management staff and Ramo-Wooldridge. "Clear lines of authority and responsibility were established. Closely integrated into the WDD management organization were

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47 Hughes, *Rescuing Prometheus*, p. 90.

48 Beard, *Developing the ICBM*, p. 175.

people from the Air Materiel Command, the Strategic Air Command, and the Air Training Command; the first one had to procure the Atlas, the second had to operate it, and the third had to train the people to use it." To achieve the necessary oversight, black Saturday was implemented—"an all-day monthly meeting during which Schriever and his staff would probe every nook and cranny of the program, looking for problems or potential difficulties."[49] Management of these challenges was similar to the Manhattan Project where multiple, concurrent approaches tried to ensure that single points of failure were avoidable.[50]

> To ensure that no single program element would prove faulty or unworkable and thereby paralyze the entire program, a decision was made to contract for alternatives to each of the major subsystems. Then when the refined Atlas design was proven sound, it became possible to employ the backup systems in a second ICBM. The decision to proceed with this weapon, which became Titan, was made in May 1955. Titan differed from Atlas in airframe, engine, guidance system, and in being two-stage. The Atlas was "one and one-half stage," meaning everything was burning at liftoff. Convair favored this design because of the uncertainty of rocket ignition at very high altitudes. Titan was specifically planned for emplacement in underground, hardened silos. The original Atlas was to be above ground.[51]

Reflecting the degree to which the program was path breaking new scientific and engineering principles, the overall ballistic missile development effort (Atlas, Titan, and Thor) had a large R&D component. In particular, the budget estimates (see Table X-1) are for the full range of research, development, science, and technology activities undertaken during the design of Atlas. Note that procurement in this budget refers to the procurement of the test vehicles and not the deployed ICBMs.

George Alexander concisely summarized the Atlas testing program in his 1963 *Aviation Week and Space Technology* article.[52] Most instructive for our purposes is his description of the way in which the Atlas program evolved the design of the missile over time.

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49 Stine, *ICBM*, p. 190.

50 Ibid., p. 189.

51 Beard, *Developing the ICBM,* p. 184.

52 George Alexander, "Atlas Accuracy Improves as Test Program is Completed," *Aviation Week & Space Technology*, 25 February 1963, pp. 54–78.

Table X-1. ICBM Development Budget Estimate ($ millions)[53]

Fiscal Year	1955	1956	1957	1958
Research and Development	20.0	20.0	20.0	20.0
Procurement	87.7	233.0	450.0	500.0
Facilities	46.2	50.0	13.0	---
Public Works	7.1	26.3	8.5	---
Total	**161.0**	**329.3**	**491.5**	**520.0**

> [The e]ntire program was to run much like a conveyor belt, with a steady supply of basic vehicles coming down the line at GD/A's San Diego, Calif., plant. Modifications dictated by test flights at the Cape were to be incorporated in missiles on the line.
>
> Rather than wait for all major elements of the weapon system to reach developmental maturity before starting flight tests—and possibly miss the 1959 date—Air Force decided to begin launches in April 1957, with the hardware then at hand and to add elements as they became available.[54]

The Atlas approach to system integration and testing was thus also one of experimentation. Despite attempts to account for overall systemic interactions and synergies, no one, based upon the knowledge base of the day, could predict how the different parts of the overall system function together.

D. WHAT IS THE RELEVANCE OF THE ATLAS MISSILE PROGRAM TO MDA?

Atlas was developed in response to a projected threat from the Soviet Union, namely, the likely deployment of land-based, nuclear-tipped ICBMs in the heartland of the USSR capable of reaching the U.S. mainland in a matter of minutes. While this threat had not yet become a reality, information reaching the leadership of the United States regarding the successes of Russian missile development programs indicated that this capability could materialize as soon as 1960.[55] The decision to rush into the development of an ICBM before all the supporting technologies were fully mature was a high-risk, high-payoff endeavor in which "the best was the enemy of the good." That is, the U.S.

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53 Neufeld, *The Development of Ballistic Missiles*, pp. 131.

54 Ibid., p. 60.

55 At a Congressional hearing on 25 May 1955 before the Military Applications Subcommittee of the Joint Atomic Energy Commission, Gardner predicted that the Soviet Union would test full-scale ICBMs some time between 1960 and 1963.

leadership was willing to live with a less than "perfect" system as long as it provided the necessary minimum capability to respond to a Russian ICBM attack and thus furnish a credible deterrent to such an event.

Today, MDA faces a similar challenge. While so-called "rogue" states do not yet possess land-based, nuclear-tipped ICBMs capable of reaching the United States, information regarding the potentials of their missile-development programs suggests that such a capability could materialize within a decade. As with the Atlas program, U.S. leaders are faced with uncertainty about the ultimate nature of the threat, its timing, and whether to deploy a less-than-perfect system.

There are some differences between the Atlas experience and that of missile defense efforts. Even when deployed, Atlas was considered capable of only delivering 80 percent of its payloads to its targets[56]—the remaining 20 percent would presumably either stray or be launch or reentry failures. Such a low probability of success might not be acceptable in the missile defense arena. The way in which the Atlas program was conducted is also different from current approaches to system acquisition. Launching 87 test vehicles in an evolving configuration was not economically efficient, but it was technically expedient. Today's oversight organizations would likely pale if the launch-fail-instrument-launch-succeed approach were applied to current missile defense development.

The development of the Atlas ICBM was unique in many respects. While it might be seen as an extension of prior work in the area of ballistic missiles, pioneered largely by the Germans in World War II, it was much more. The scientific and technical work necessary to make Atlas a reality required extension of the knowledge base in a host of areas including basic research into the phenomenology of gasses at hypersonic speeds, the behavior of materials at superheated temperatures, and understanding the behavior of complex structures under extreme stress. Because of the fundamental nature of such investigations and the significance of each to the successful development of Atlas, the entire endeavor was high risk.

While ARDC was established with "specific responsibilities for research and development as distinct from production,"[57] the program largely relied upon contractors

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56 Alexander, "Atlas Accuracy Improves," p. 54.

57 Beard, *Developing the ICBM*, p. 129.

to perform the work. The retention of Ramo-Wooldridge was very important because of their ability to attract "the best and brightest" to work on the project and because of their unique experience with addressing the complexities of large systems. Their role as an "overseer" for ballistic missile development (a role transitioned to the Aerospace Corporation in 1960,[58]) cannot be overstated.

Overall, the Atlas program should be considered as one giant R&D activity in which the pace of progress could be measured not only by engineering advancement, but also as a result of scientific progress.

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58 Hughes, *Rescuing Prometheus*, p. 138.

XI. DEFENDER—SCIENCE AND TECHNOLOGY FOR BALLISTIC MISSILE DEFENSE

Richard Van Atta and Jack Nunn

A defense against ballistic missiles was the logical response to the developing threat of the 1950s and early 1960s. Latent concern about this threat had developed even earlier when German V–2 rockets (with a range of about 200 mi) rained down on England. These early missile defense developments were Service driven, but there was little coordination between the Army, Air Force, and Navy. This condition appears to have remained the same until the late 1950s.

In May 1946, the Army's Stillwell Board, looking at future defense needs, recommended to the Army the development of an antimissile defense. However, because of lack of an immediate threat and severe budget restrictions, missile defense developments remain limited until the 1950s. To be sure, all three Services made an effort to develop some capabilities. The Navy, for example, pursued its project "Bumblebee." Bumblebee's objective was to conduct research on and develop guided missile technology and to deliver a Navy surface-to-air missile (SAM) system—first against aircraft, but ultimately including antimissile capabilities in the Standard Missile.[1]

The Air Force Wizard program was established while the Air Force was still a part of the Army. It continued on. The Air Force chose to discontinue R&D on short-range SAMs after 1949 and concentrated on the combination of the Semi-Automatic Ground Environment (SAGE) and the Bomarc. In 1953, the Air Force R&D Command directed a group from Lincoln Laboratory and the Air Force Cambridge Research Center to study defense against an intercontinental missile attack. Project Wizard 3 followed. While this study did not result in a useful Air Force missile interceptor, it apparently did

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1 Evan Nau, *The Bumblebee Project*. www-personal.umich.edu/%7Ebuznau/bumblbee.html.

result in the development of the concept of Ballistic Missile Early Warning System (BMEWS). Missile competition between the Services grew.[2]

The Army's entry into the antimissile competition was based on its antiaircraft defense effort. It turned to its major missile contractor in the air defense area, the Bell Telephone Laboratories—Western Electric team (BTL) for initial studies on antimissile defense. Nike-Hercules, a more capable air defense missile with a range of over 75 miles, had begun development in 1953. A formal feasibility study was initiated at BTL in February 1955 and completed in September 1956. The study concluded that hitting a missile with a missile was indeed feasible, and a missile development program was initiated. The new missile, the Nike-Zeus (see Figure XI-1), was a continuation of the older antiaircraft systems, Nike-Ajax and Nike-Hercules, developed and produced for the Army by Bell Laboratories. Like the older antiaircraft missiles, the Zeus was a "terminal defense" system in which a relatively small area, say one city, was defended by (1) a radar system, to acquire and track the incoming target, (2) a SAM equipped with a nuclear warhead to destroy it, and (3) the associated guidance and computer equipment. In November 1956, the Army, with Assistant Secretary of Defense (R&D) concurrence, directed the full system development of Zeus by BTL.

Figure XI-1. Nike-Zeus

Between 1953 and 1958, competition among the Services in all things dealing with missiles continued to grow. Fights over roles and missions flourished. The disputes were focused on "(1) tactical air support, (2) continental air defense, (3) intermediate and

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2 Robert Frank Futrell, *Ideas, Concepts and Doctrine: A History of Basic Thinking in the United States Air Force 1907–1964*, Air University, p. 251.

ICBM development, and (4) military airlift."[3] Despite attempts by Secretary of Defense Wilson to rationalize the Services' missile activities, the fights continued.

Finally, after the shock of Sputnik, Secretary of Defense McElroy brought some order to the process. When he established the Advanced Research Projects Agency (ARPA), he gave it responsibility for providing unified direction and management of the antimissile programs and outer-space projects, as well as nuclear test detection.[4] At the same time, he directed the Air Force to continue that portion of the Wizard program related to early warning radars, tracking and acquisition radars, communications links between early warning radars, and the active defense and the data-processing components needed for an integrated system.[5] He directed the Army to continue its development of the Nike-Zeus, concentrating on system development that would demonstrate the feasibility of achieving an effective, active antimissile system that could discriminate against electronic countermeasures and decoys.[6] At least in part, the tasking of the Army and Air Force seems to have been driven by the fact that the Nike-Zeus missile was superior to the Air Force's air-breathing Bomarc. Similarly, it seemed that the Air Force's Wizard program held more promise as a BMD radar system than Army alternatives.[7] Thus, starting in 1958, the Army limited itself to work on the missile and launch system and on the development of the associated acquisition, tracking and control equipment necessary for an integrated ground-based ballistic missile interceptor. The Air Force concentrated on creating an effective missile detection system.[8] As will be elaborated below, *ARPA was assigned the basic research responsibility for both areas.*

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3 Benson Adams, *Ballistic Missile Defense*. American Elsevier Publishing Co., Inc., 1971, p. 22.

4 When ARPA was established it was assigned three Presidential Initiatives: space, nuclear test detection, and missile defense. For discussion of DARPA's origins, see Richard H. Van Atta et al., *An Overall Perspective and Assessment of the Technical Accomplishments of the Defense Advanced Research Projects Agency: 1958–1990*, Alexandria, Va.: Institute for Defense Analyses, DARPA Technical Accomplishments Volume III, IDA Paper P-2538, July 1991. Detailed history is provided in Richard J. Barber and Associates, *The Advanced Research Project Agency, 1958–1974,* Washington, D.C., December 1975. For a detailed recounting of the events leading to the establishment of ARPA and its initial assignments, see Herbert F. York, *Making Weapons, Talking Peace*, New York: Basic Books, 1987, pp 134–171.

5 Adams, *Ballistic Missile Defense*, p. 252.

6 Ibid.

7 Ernest Yanarella, *The Missile Defense Controversy*, The University Press of Kentucky, Lexington, Ky.: 1977, pp. 40–41.

8 Ibid.

A. MISSILE DEFENSE STUDIES THAT DEFINED THE THREAT AND FRAMED THE NATION'S TECHNICAL RESPONSE

Special studies played an important role in shaping the missile defense S&T effort and the development of its forces. The Army Air Forces, precursor of the U.S. Air Force, initiated two long-term studies, Projects Thumper and Wizard, that were to explore the feasibility of developing interceptor missiles that could destroy missiles moving as fast as 4,000 mph at an altitude as high as 500,000 ft.[9] The Thumper project was conducted by General Electric Corporation. It concluded that the then currently available technology could not defend against a V–2 type rocket.[10] The Wizard project, on the other hand, was a longer term study that continued to be funded by a Air Force once it became a separate Service. The Project Wizard-3 study outlined by a Lincoln Laboratory/Cambridge Research team had ultimately involved three study contracts to three aircraft-electronic industry teams to identify the means needed to detect/identify and intercept/ destroy hostile ballistic missiles. All three contracts were cancelled when Secretary McElroy made his decision on missile defense development.

However, as noted earlier, these Air Force studies led to the development of high-powered radar with a range of up to 3,000 mi (BMEWS) (see Figure XI-2), combined with computers that permitted a quick determination of a ballistic missile's trajectory.[11] These radars could not only provide warning, but also provided information on U.S. missiles and led to better understanding of the all-important reentry body problem.

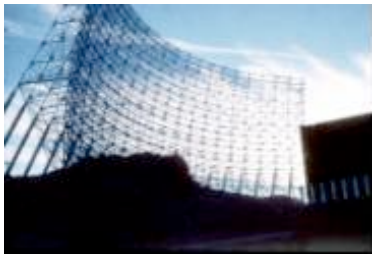

Figure XI-2. BMEWS Detection Radar

The Technology Capabilities Panel (TCP) report, published in 1955, a seminal study on the defense of the United States, concentrated on air-defense issues but

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9 www.acq.osd.mil/bmdo/bmdolink/html/milstone.html .

10 Ibid.

11 Futrell, *Ideas*, p. 270.

contained a short and very worthwhile discussion of missile defense.[12] The TCP anticipated that if an ICBM threat developed, it would do so in the 1960–1970 timeframe and that with a 1-Mt warhead such a weapon could be effective against cities and aircraft on the field. Meeting this threat required the same basic steps as in air defense—detection, tracking, interception, and destruction; however, with missile defense, the components were acknowledged to be "far more advanced than those presently in use or being developed for use against aircraft."[13]

Missile detection and its associated warning was clearly essential and, the Panel believed, possible. The TCP suggested several possibilities, including over-the-horizon (OTH) radar, which might have "some chance of detecting an ICBM at take-off, [but] has many practical limitations" (p. 117).

Radar developments were absolutely essential. The Panel concluded that:

> There is a need for a *large-scale program* for development of components and techniques broadly suitable for these and other types of high power radars applicable for the detection and tracking of ICBMs. The program must not stop with laboratory development but must include environmental testing, manufacturing processes and experience in operation of complete radar systems. The building-up of such a program is technically sound and is clearly a *must*. Such a program will not evolve naturally from present radar work because the requirements are so different. [p. 117]

The TCP stated that it was confident that a radar warning system for ICBMs could be developed and installed within 5 years—if it were given a high priority. Its conclusion was that the technology development was extensive but well within the state of the art. If sites were chosen carefully, only a few stations "would cover all probable missile approach paths and provide approximately 15 minutes warning for targets in the United States." Moreover, the "target" for each of the missiles could be localized "to an area approximately 50 miles by 100 miles" (p. 117). Although the Soviet ICBM threat did not yet exist, the TCP recommended "work be started immediately on the development of equipment and planning for the installation of this radar warning system…" (p. 117).

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12 Executive Office of the President, Office of Defense Mobilization, *The Report to the President by the Technological Capabilities Panel of the Science Advisory Committee,* 14 February 1955, p. v. (TCP Report)

13 Ibid., p. 116. Page numbers for later quotes appear in the text.

The TCP expressed less optimism about the ability to destroy the reentry body that contained a weapon. While it did not view the destruction as impossible, destroying the ICBM-delivered weapon was orders of magnitude more difficult than destroying the bomber-delivered weapon. The TCP noted that the interception problem divided into two categories determined by the altitude of interception. Above 100,000 ft (exoatmospheric), the incoming missile was expected to follow an essentially straight line. The intercepting missile would also probably have less maneuverability but would have time to reach the target, even if relatively slowly. Therefore, even an existing missile might serve as a booster (e.g., the Nike). The TCP stated that programming an interceptor and guiding it to such high-altitude intercepts using rocket thrust would present "many new development problems" (p. 123). Below that altitude (endoatmospheric), faster missiles would be required. The Army was working on short-range missiles (Project Plato), as was the Navy for sea applications.

Although the TCP recommended beginning a broad R&D effort in radar and continued interceptor R&D, it stated that it "could not now justify a recommendation for a 'crash' program in an effort to develop a defense system along a specific technological line. There simply is not at this time a sufficient body of knowledge upon which to base the design of a defense system in which we could place confidence" (p. 121). Instead, the TCP recommended "an expansion and stepping up of the theoretical and experimental work on a broad front" (p. 121). It also recommended "the establishment of a full-time technical group to carry out a rapid but thorough examination of the entire [missile defense] problem with the objective of laying the framework for the expanded program" (p. 121). Many of these recommendations appear to have influenced the decision to establish ARPA 2 years later.

In the short term, studies appear to have continued to be Service specific. The Air Force had Wizard, and the Army had Nike and Plato. However, as noted earlier, that changed dramatically in 1958 with the rationalization of missile work and the establishment of ARPA, which became the chief research organization for missile defense.

Shortly after the Sputnik launch, DoD studies began to focus on the reentry vehicle, both for understanding offensive forces and defensive forces, with specific considerations of measures to ensure that ICBMs could penetrate possible Soviet ballistic

missile defenses.[14] ARPA's Project DEFENDER and PENAIDs were closely linked—ARPA Director Dr. Jack Ruina and DDR&E Harold Brown tasked ARPA with analyzing the ability of the United States to penetrate Soviet defenses and conducting research to ensure that the United States could.[15] With the emergence of the multiple independently targeted reentry vehicle (MIRV), the DoD commissioned IDA to conduct the large-scale PEN X Study in 1965. This study involved many of the relevant agencies in the defense community.[16] By then, the offensive S&T activities considering penetration aids for U.S. offensive weapons were closely tied to the defense S&T efforts aimed at discriminating the real warheads for interception and destruction.[17]

B. DEFENDER—MISSILE DEFENSE PROGRAM

As mentioned previously, the Services were engaged in a hard-fought struggle to maintain control of the development of the anti-ballistic missile (ABM) systems. Missile defense research had been underway for over a decade, and the competition between the Services had been evident from the very beginning. This competition led to redundant research efforts and an inefficient use of resources. On 26 November 1956, Secretary of Defense Charles Wilson distributed a new policy directive to the Services with the intent of clarifying their missions. The *Clarification of Roles and Missions To Improve the Effectiveness of Operation of the Department of Defense* assigned the roles associated with point defense to the Army and with area defense to the Air Force.[18]

> Unfortunately, this directive did not specify the threat—i.e., air or missile threat—to which each type of defense was to be addressed. Moreover, until the systems were ready for development and testing, it was not known whether area or point defenses would be more effective. The directive did not indicate which Service was to have operational control over a defense system, once it was developed and deployed. Despite the directive, the conflict over the air and missile defense systems of the Army

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14 Richard H. Van Atta, Sidney Reed, and Seymour Deitchman, *DARPA Technical Accomplishments, Volume IV,* "Chapter IV, PENAIDS," Alexandria, Va.: Institute for Defense Analyses, IDA Paper P-2429, April 1991.

15 Interview with Dr. Charles Herzfeld, June 21, 2002.

16 Institute for Defense Analyses, *The Pen X Study (U),* IDA R-112 (Summary), August 1, 1965, (CLASSIFIED).

17 Charles Herzfeld, interview.

18 Adams, *Ballistic Missile Defense,* p. 22.

> (Nike-Hercules and Zeus) and those of the Air Force (Bomarc and Wizard) continued.[19]

When ARPA was established as part of DDR&E, it was "in response to the urgent need for the centralized management of selected high-priority military research projects."[20] One of the organization's major tasks was to conduct basic research in the BMD area. According to Congressional testimony of Dr. Charles Herzfeld, then Director of ARPA, Project Defender was tasked

> with the responsibility for research and exploratory development to provide the basis for the ballistic missile defense of the future. Because defense and offense are opposite sides of the same problem, Project Defender's mission and activities also contribute in an important way to the development and improvement of our strategic offensive weapons. Thus, Project Defender fulfills its function as a source of new ideas and technological advances which assists the services in both their assured destruction and damage limiting roles. To achieve the objectives of the Defender program, we carry out field and laboratory management programs, as well as various technology development programs.[21]

In earlier testimony by Dr. Jack Ruina, a preceding Director of ARPA from 1961–1963, the Project was described as:

> oriented toward understanding fundamental phenomena, the interpretation of data derived from experiments, the formulation of new systems concepts, and the uncovering and developing of new techniques and ideas for advanced defense capabilities. In addition, Defender has been assigned this past year a study of the effectiveness of the present and future U.S. program in penetration aids for strategic weapons. Our ballistic missile defense program is not charged with the development or refinement of presently authorized defense systems (i.e., BMEWS or Nike-Zeus). Defender must be looked on to continue the pursuit of relevant science and technology.[22]

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19 Ibid., pp. 22–23.

20 Dr. Charles Herzfeld, Director of ARPA, testifying before the Subcommittee of the Committee on Appropriations in the House of Representatives on Department of Defense Appropriations for 1968. pp. 138–139. Herzfeld headed the Defender project before becoming Director of ARPA.

21 Ibid., p. 139.

22 Testimony of Dr. Jack Ruina, Director of ARPA, before the Subcommittee of the Committee on Appropriations in the House of Representatives, in 1963, pp. 108–109.

Project Defender spanned a decade—from the creation of ARPA to its transfer to the Army in 1967. The project was divided into several different components that focused on three distinct aspects of the missile defense problem: missile phenomenology, detection and tracking, and interception and kill. In the early 1960s, the project consisted of research into a wide variety of techniques and devices required for effective BMD. This research included "advanced sensor technology, interception technology, kill mechanisms, and an area which includes research in infrared, ultraviolet, and visible techniques of detection of space objects, as well as laser devices research."[23] The kill mechanisms portion of the ARPA work focused on hypervelocity impacts to destroy ICBMs and explored the most effective fragment (material, size, and shape) required to defeat an ICBM. Defender established "centers of excellence" in several technology areas where there was insufficient technical capability or focus—e.g., the Joint Instrumentation Laboratory for Astrophysics at Boulder, Colorado. Not enough was known about radiation in space, and this was needed to address reentry.

The largest portion of the project was devoted to missile phenomenology (the flight of a ballistic missile from launch to reentry), which accounted for about 40 percent of the effort. ARPA efforts covered all aspects of the missile's flight, including the reentry phase of missile flight and additional efforts on detecting and targeting at the launch phase. Central to the phenomenology work undertaken by ARPA's Defender project was Project PRESS (Pacific Range Electromagnetic Systems Studies), the major field measurement element of ARPA's research on ICBMs' reentry into the Earth's atmosphere.[24] Lincoln Laboratory was brought in to manage the PRESS operation. The project supported the development and use of the TRADEX tracking and detection L-band and ultrahigh frequency (UHF) radar with an array of ground- and air-based optical and IR sensors located on the Kwajalein atoll chain. (The TRADEX radar was a $38.5 million program.) An aircraft was specially outfitted for optical and IR measurements. Such measurements were valuable for investigating emissions associated with reentry chemical phenomena.

PRESS was explicitly set up to provide high-quality scientific information on RV reentry equally to the offensive and defensive sides of the missile-penetration problem. PRESS was a measurement facility for reentry experiments for both defensive and

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23 Ibid., p. 109.

24 Van Atta, et al., *DARPA Technical Accomplishments, Vol. II*, "Chapter I, PRESS," April 1991.

offensive capabilities. ICBMs would be flown into the test range, and measurements would be taken using the assembled sensor systems. The data would be air delivered to Lincoln Laboratory for analysis.

In 1965, a dual very high frequency/ultrahigh frequency (VHF/UHF) radar named ALTAIR (ARPA long-range tracking and instrumentation radar) was constructed as part of PRESS to simulate Soviet BMD radar capabilities against U.S. reentry vehicles. Another radar system, ALCOR, a high-resolution C-band radar, was developed and employed around 1970. Radar systems were upgraded over the life of Project PRESS. The PRESS facilities at Kwajalein were transitioned to the Army when the Defender project was transferred and renamed the Kiernan Reentry Measurements (KREMS) facility. This facility became a major part of the national R&D facility operated by the Army's Strategic Defense command, serving both Service and strategic defense initiative (SDI) measurement needs.

By the early 1970s, considerable confidence was expressed in the ability to model reentry phenomena successfully, based on PRESS and related data. Over time, the PRESS activity and the Bell Telephone Lab work on Nike X became more coordinated through an ARPA-Army agreement on RV measurements. Throughout the 1960s, a large number of experiments involving different types of RVs and penetration aids were conducted. Data from the PRESS facility were important in assessing the difficulties of the discrimination problem but also indicated capabilities of a NIKE-X system would be sufficient against a presumed "unsophisticated" threat. This apparently had some impact on the decision to deploy the SENTINEL BMD system in 1967.

1. Detection and Tracking

a. Radar

Project Defender had a major focus on radar technology. It consisted of four major radar programs:[25]

- **Electronically steerable array radar (ESAR)**

  In 1957, the President's Science Advisory Committee panel, as well as other experts, had pointed out the need for ballistic missile surveillance capable of

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25 Discussion of the radar technology was taken from Dr. Herzfeld's testimony before Congress, pp. 140–141, and Dr. Ruina's testimony before Congress, pp. 110.

tracking a large number moving at very high speeds. Existing dish radars had severe limitations in doing this, and electronically steered phased arrays were seen as an alternative. ARPA developed the ESAR as the first two-dimensional (2–D) array entirely steered by phase control. This was followed by the more advanced FPS-85 multifunction phased array, which became part of the Air Force SPACETRACK system. ESAR is an example of an outside organization (ARPA) developing and exploiting a "disruptive technology" that the "product-line" organization (Army) and some experts—including Bell Labs and Lincoln Labs—would not consider.

- **Advanced design array radar (ADAR)**

  The major radar development was ADAR, which was expected to provide the basis for an actual defense system of the future. The ADAR was to provide the Army's Nike-X system with radar technology that would be available with minimum delay and maximum efficiency.

- **ARPA-Lincoln coherent observable radar (ALCOR)**

  ALCOR became part of Project Press for high-resolution observation of RVs.

- **ARPA long-range tracking and insrumentation radar (ALTAIR)**

  ALTAIR was to provide better spatial resolution than the existing TRADEX radar. ALTAIR was a "powerful, coherent, dual frequency radar." It was incorporated into the PRESS/KREMS facility.

The radar systems developed under Project Defender were the basis for later early-warning radars and large, ground-based space-surveillance radars, including large phased-array radars (e.g., ESAR). Impacted systems include the Navy's AEGIS and the Air Force's PAVE PAWS, and COBRA DANE.[26] The technologies also contributed to the development of later OTH radars.

b. IR Sensing

From its outset, ARPA's Project Defender began studies of sensors across the spectrum from radar, IR, and visible to improve understanding of the phenomenology of ballistic missiles from launch to reentry.[27] A major question that confronted those dealing

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26 *DARPA Technology Transition*, pp. 37, 81, and 122.

27 Sidney G. Reed, Richard H. Van Atta and Seymour J. Deitchman, *DARPA Technical Accomplishments*, Chapter VII, "TABSTONE Infrared Measurements," Volume I, Alexandria, Va.: Institute for Defense Analyses, IDA Paper P-2192, February 1990, p 7-2.

with missile defense in 1960 was the utility of IR satellite early-warning systems against ICBMs. Under the Defender Project, ARPA initiated project TABSTONE, a comprehensive program of IR field and laboratory measurements, analysis, and technology development.[28] Over an intensive 18-month period of research, "TABSTONE had progressed far enough for ARPA to give a positive answer which raised the level of confidence in DoD and enabled development of the technology of the current U.S. IR satellite early warning system (SEWS)."[29] This scientific assessment was crucial since there were concerns that IR sensors could mistake IR radiation from reflected sunlight for rocket-engine plumes— especially noted in the early MIDAS satellites that had been orbited by the Air Force.

Then DDR&E Harold Brown assigned ARPA the question of whether a MIDAS-type system could be feasible. To do so, TABSTONE went back to the fundamentals, including a broad range of field measurements, and drew upon a broad segment of industry, academia, and military service labs, the National Bureau of Standards, and even participation of Canadian and United Kingdom groups. A follow-on to TABSTONE was an ARPA Plume Physics program, and, subsequently, DARPA supported studies of the use of new IR technologies for detecting and tracking plumes that transitioned to the SDI.

2. Interceptor Technologies

Project Defender's focus on interceptor technology was an outgrowth of the concern for reduced time to intercept posed by the Nike X concept of hard point missile defense.[30] The investigation of a high-acceleration missile was undertaken as an element of the HIBEX (High Boost Experiment) Project that commenced in 1963.[31] HIBEX was explicitly conceived to provide a competitive alternative to "the Nike SPRINT follow-on to the Nike Zeus." Secretary of Defense Robert McNamara and DDR&E Harold Brown were concerned that the Army needed competition to push them to reach for advanced capabilities and execute the mission.[32] While the Army SPRINT was constrained by the

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28 Ibid., pp. 7-4–7-5.

29 Ibid., p 7-1.

30 Van Atta, et al., *DARPA Technical Accomplishments*, Vol. II, Chapter III, "HIBEX—Upstage," p.□-1–3-2.

31 HIBEX was conducted in parallel with the investigation of a low-cost phased-array radar needed to provide the detection and tracking for hard point defense.

32 Charles Herzfeld, interview.

limitations of a production-oriented engineering program, HIBEX, as a research project, could explore the boundaries of performance with risk of failure.[33]

Some of the research challenges of HIBEX were

- Developing a high-burn-rate propellant capable of standing several hundred "g's" and
- Understanding the aerodynamic characteristics of vehicles in hypersonic flight.

As a research project, the ARPA program pushed the thrust capabilities well beyond the Army's original target for SPRINT, and, when HIBEX demonstrated that 400 gs were achievable (10 what Army SPRINT was aimed at), this pushed the Army to adopt the technology, albeit at a "more practical" 120 gs. ARPA had an imperative to "beat SPRINT" before it got too far down the road.[34]

The HIBEX was successfully concluded in 1966. It was a high axial acceleration interceptor. The program was focused on hard point defense against a sophisticated threat, which differentiated it from the interceptor technology being developed by the Army.[35] The interceptor program also explored laser angular rate sensors (LARSs).

A final portion of Project Defender was the GILPAR program. GILPAR was assigned the task of investigating and testing all new missile defense concepts. A report from the GILPAR program set back the Zeus program in early 1960. The report indicated that it was unlikely that the technologies in existence in the 1960s were capable of responding effectively to the missile threat. Furthermore, the GILPAR report found that the anticipated technologies of the 1970s and 1980s were also unlikely to provide effective missile defense capabilities.[36]

3. Observations on DEFENDER's S&T Strategy

- DEFENDER was a *threat-based program*—there was strong connection between the threat assessment and the technical work. S&T had to have a product in mind to be useful to the organization. S&T also provided an

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33 Van Atta, op. cit., p. 3–5.

34 Herzfeld, interview.

35 Indeed, following HIBEX was ARPA's UPSTAGE program, a second-stage vehicle capable of maneuvering to engage maneuverable RVs—this was well beyond the near-term focus of the Army program.

36 Adams, *Ballistic Missile Defense,* p. 38.

understanding of how systems would be able to respond to the threats they are supposed to meet.

- ARPA DEFENDER had a national program focus—clear mission and responsibility.
- DEFENDER started out as a top-down national program focused on a broad imperative and mission, but it had under it a collection of more specific technical elements. (PENAIDs was additional effort that provided important input regarding the nature of the threat—PENAIDs provided the first net technical assessment of both sides' capabilities.)
- For promulgating and providing an archive of research, Herzfeld started the *Journal of Missile Defense Research* (which became the *Journal of Defense Research*). This was a SECRET refereed journal designed to be a mechanism to improve the quality of the reporting on what the DEFENDER program was doing.
- DEFENDER needed a roadmap. Initially it was "a collection of interesting technology programs." Scientific/technical meetings (classified and unclassified), including the JASONS, addressed "complicated scientific problems" such as the prospect of particle accelerators for missile defense. ARPA Director Ruina played an important role in establishing the organizational style and in setting the challenges and attracting the people to address them. The "architecture" was done largely in the context of preparation for budget hearings and in meetings with SECDEF Robert McNamara and DDR&E Harold Brown. The focus was concept, content, and management approach. The hearings on the Hill were important— DEFENDER was half of ARPA's budget. The congressional hearings were closed and classified. Crucial in this was that Brown and Ruina understood and supported what DEFENDER was doing.
- DEFENDER, and later SDI, had systems architectures, based on models and analyses, that were operationally oriented against conducting projected engagements—who decides, what information needed, etc. (Herzfeld noted that these models were not good enough, but they used the best they had available.) This required data and projections on what the system was intended to defeat. This then drove some of the S&T to address those aspects of the problem for which we really did not know the answers (driving lab and range experiments).
- DEFENDER was a "best effort" approach vs. a hard timeline but with an underlying credo: "be hugely ambitious." Risk-tradeoffs were laid on the competitors. If a PM was not sufficiently ambitious, the DARPA Director would reject his program. ARPA was an organization that had an

organizational focus on high-risk/high-payoff research and thus provided a mechanism to push the state of art beyond the "normal" development programs of the services (as was specifically the case of HIBEX).

- DEFENDER, while built on some legacy work, was clearly focused on the motif to "shape R&D to look ahead." The program took a longer term perspective. For example, Herzfeld knew that the laser as a weapon would not get anywhere near a deployable capability during his term at ARPA. It was at least 10 years out, but he supported it.
- The program balanced the higher risk laser against more near-term hit-to-kill. DEFENDER sought to balance its risk portfolio by spreading high- and medium-risk projects and even did lower risk ones (but these were largely for measurement technologies to build instrumentation to evaluate the higher risk programs). Challenges and focus were based on analyses.

XII. TURBINE ENGINE CASE STUDY

William Hong and Paul Collopy

This case study examines gas turbine-engine development from the 1960s through the establishment of IHPTET (Integrated High Performance Turbine Engine Technology) program in the late 1980s and its role in enabling continued U.S. leadership in air-breathing propulsion capabilities.

IDA researched documented history of aircraft engine development through the 1980s and interviewed several current and retired personnel from the government and the large engine companies. From their observations, we observe that the S&T management structure during that time had the following general characteristics:

- Joint inter-Service programs [e.g., the Air Force and Navy in the Joint Technology Demonstrator Engine (JTDE) and the three Services plus NASA in IHPTET] allowed work toward common problems but with separate, defendable budgets within each organization.
- Technically competent government personnel in program management challenged field personnel (government and industry) to work outside of their "comfort zone."
- Senior management personnel stability in the laboratories and the Pentagon, in some cases for more than 20 years, enabled a deep understanding of the issues associated with particular technologies and consistent direction and support of technologies from concept to fielding.
- Stable, multiyear funding allowed the establishment of long-term research teams in government and industry that could become deeply acquainted with technologies and challenges.
- As management changed, the succession of leadership was closely attended so that the basic approach and philosophy remained stable.
- Small, focused teams with minimal levels of management and strong leadership skills worked directly on the technologies in government and industry laboratories.

- Open communications and high levels of trust between personnel in the government and in the companies ensured the companies that the government would safeguard their competitive advantages.
- On common problems, the government encouraged competitive development by the engine companies, even when not all the companies were selected for particular contracts.
- Development (6.2 and 6.3) programs were tied into transition plans for systems applications with buy-in by the user communities, particularly the engine companies.
- A sufficient number of development opportunities were available for technology transfer, including military acquisition programs and commercial products, to provide a path forward to anticipate technology needs.
- Anticipated technology needs were prioritized so that planning and execution could be brought to fruition at the correct time. Development program delays waiting for the "appropriate miracle" to occur were, for the most part, avoided.

A. LESSONS FROM TURBINE ENGINE S&T EXPERIENCE

Beyond these management characteristics, several recurring themes were found in this case study's examination of the history of aircraft engine development in the United States between 1960 and 1985:

1. *The primary value of basic research is to provide models, methods, and tools to predict the performance of a design configuration. Such tools allow designs to be refined before they are implemented in hardware.*

Examples are compressor aerodynamic models (leading to better stall-tolerant engines), advanced high-cycle-fatigue analyses for compressor and turbine blades, heat-transfer models for hot section and cooled turbine blade designs, and computational fluid dynamic (CFD) models leading to more efficient compressor blade designs. Many of these basic advances and models came out of government laboratories in the Air Force and NASA. These advances did not draw the same attention as "technologies" that could be embodied in a piece of hardware (such as a variable compressor vane) passed around a conference table. Thus, they often do not get the credit they deserve as critical steps in the creation of the today's aircraft engine. However, in our interviews with technology managers, analytical models were consistently at or near the top of the list of critical developments during the 1960 to 1985 period.

Basic research is often credited with discovering and harnessing basic phenomena, and thereby leading to technologies that apply the new phenomena to enhance products and systems (the Linear Model of technology development). However, we did not find technologies arising from basic research in this way during the period we investigated.

2. *Full-scale demonstrators matured technologies, prototyped component designs, and explored system integration issues. They also vetted technologies, sometimes showing that investment in a once-promising technology should be ended. These demonstrators were immensely valuable to technology development and transition.*

One example is the GE–1 core engine demonstrator in the early 1960s. The GE–1 developed several critical technologies for contemporary engines and fathered two derivative demonstrators whose impact can be felt to the present day in military and commercial aircraft engines. Almost all the profit stream of GE Aircraft Engines since 1980 has resulted from engines derived from these three demonstrators. The tests were funded by Contributing Engineering, an early form of IR&D, and the Air Force Advanced Turbine Engine Gas Generator (ATEGG) program. Additional demonstrator programs at NASA in the 1970s also contributed technologies used on commercial engines. However, NASA full-scale demonstration programs began to be curtailed in the late 1970s, and component technology has almost ceased flowing from NASA into production aircraft engines.

3. *Tight-knit teams with a vision, long-term commitment, minimal hierarchy, and minimal oversight can discover and deliver major technical advances.*

Numerous examples of skunkworks-type programs exist in industry and government and are credited with advances such as the first variable geometry turbojet (J79) and the Mach 3 engines of the early 1960s (J58 and J93).

More structured research programs, such as the IR&D program of the 1980s and IHPTET of the late 1980s and 1990s, have featured bureaucracies on the industry and government sides that conduct systematic layered reviews of technology programs. These programs have notably not produced the radical innovations that have transitioned to products in service. They have, however, succeeded in introducing many incremental technological improvements, particularly in raising cycle-temperature limits.

On the other hand, IR&D, IHPTET, and other government-funded programs have invested decades of research into programs such as ceramic-matrix composites, metal-

matrix composites, analytical sensor redundancy, and performance-seeking controls, many of which have so far not transitioned into production in a meaningful way—or at least to anticipated levels. Each of these technologies has survived dozens of annual reviews at many layers in industry and government.

A 1970s study of corporate research programs that used disciplined annual reviews observed that programs that failed to meet their objectives were much more likely to receive funding to continue than programs that succeeded. This "reward for failure" rests on a sunk-cost argument: since a great deal of money has been invested in a program, continuing the program is imperative until positive results are achieved. Research in turbine engines appeared to follow this pattern during the 1980s and 1990s.

Why do small, independent teams succeed at radical innovations? We speculate that it may be because they have the freedom to persist in developing high-risk technologies that hierarchical organizations would abandon. Early on, the promise of radical technologies is not clear. Support of them is often a matter of faith as much as reason. This distinguishes radical from incremental advances. Thus, radical technologies cannot survive layers of reviews. They depend on champions and trust. When an agency funds a tight-knit team and depends on trust rather than reviews and tollgates, a significant risk is that there will be little to show for the investment at the end of the day. On the other hand, when layers of oversight and reviews are used, there is almost no chance of successful radical innovation. Too many participants hold veto power. In addition, small, tight-knit teams can develop technology rapidly and inexpensively so that a much higher failure rate becomes tolerable.

4. *A great deal of gas-turbine technology development in the 1960s and 1970s came about to correct problems for engines that were already in service and was conducted as part of the product management of the engine rather than through offline technology-development programs.*

Examples such as the compressor stall and durability problems in the early TF30 and F100 turbofan engines for the F–111 and F–15 are classic cases. Component Improvement Programs (CIPs) funded most of the work to correct the deficiencies in these engines. One of the key technologies of this period, the ability to maintain the aerodynamic stability of a high-pressure compressor, particularly in a turbofan configuration, was developed primarily under a CIP for these two engines.

The TF30 and F100 experiences provide positive and negative lessons for spiral development. On the positive side, the engines were fielded with minimal capabilities and

were successively improved. For the TF30, the improvements never brought the engine to a satisfactory level of performance. The premature retirement of the EF–111 Raven was largely caused by the unreliable TF30. On the other hand, the current F100–229 is an excellent, high-performance fighter engine.

On the negative side, the development of these engines in the field was very expensive and painful. The pain was caused by performance that fell short of promises, which could be avoided in the best-managed spiral development programs. The cost impact is more intractable. Substantial time is required to develop and demonstrate technology improvements to aircraft engines. Every significant change to an engine requires lengthy requalification to ensure safety. Without major change to the product qualification process, the frequent product releases envisioned in the spiral development process cannot be as frequent or inexpensive in the aircraft engine domain as those that other types of products have experienced.

B. S&T PROCESSES AND PRODUCT MATURITY

When considering the applicability of turbine engine S&T processes to other domains, an important point to consider is that the turbine engine is a system whose basic architecture has not changed since the 1960s. Nevertheless, in this period, turbomachinery used in Brayton cycle engines has undergone refinements in aerodynamics, materials, controls, and numerous other areas that have translated into countless performance and economic gains.

In very fluid domains, such as missile defense or net-centric warfare, where basic concept architecture is not yet fixed, the incremental methods used in IHPTET and other engine programs may not achieve the success experienced in the turbine-engine S&T programs. IHPTET methodology is more applicable to systems-acquisition programs in the spiral development phase. Such methods include the Goals, Objectives, Technical Challenges, and Approaches (GOTChA) process that develops quantitative, phased goals for technological advancement. The GOTChA process works well for incremental programs where goals and objectives can be applied to development of technologies for existing systems architectures. It is less clear whether “blue sky,” first principles’ basic research will lend itself readily to this more structured approach or whether it will support architecture design and systems-of-systems concepts. In other words, the less well defined the concept, the less the IHPTET model applies.

The extent to which the GOTChA process from IHPTET can apply to less well-defined S&T programs depends on the ability to define *quantifiable goals* as part of the exercise. The IHPTET planning process and model applies to the *highest* mission-level goals that can be quantified and thus verified. In novel system-design areas, such as network-centric warfare or missile defense, goals may be hard to quantify at the system-architecture level. Still, IHPTET methods may work well on subsystems or components such as interceptor-missile-guidance systems.

The IHPTET model has been successfully applied to other domains. The Integrated High Payoff Rocket Propulsion Technology (IHPRPT) program was reorganized in the 1990s along parallel lines to the IHPTET program, including the creation of a Steering Committee structure. NASA is exploring the application of the IHPTET/GOTChA process to portions of its own program structure. Such discipline in technology planning carries many benefits. Even where the wholesale import of the IHPTET program is not the best answer, many process elements may be useful in other domains. Experience has tested and improved the IHPTET methodology, making it an attractive alternative to starting from scratch.

During the period studied, the turbine-engine domain benefited from the number of opportunities for system development. A large number of new engines were developed for a wide variety of aircraft applications. A dozen new centerline large turbojets and turbofans entered production in the United States in the 1960s. In comparison, only one such engine entered production in the 1990s. Thus, during the 1960s and 1970s, there were frequent opportunities to transition all varieties of technologies into production. Few military domains offer such opportunities at present.

C. MANAGEMENT OF TURBINE ENGINE S&T PROGRAMS FROM THE 1960s TO THE 1980s—THE DEMONSTRATOR CONCEPT

Before the 1960s, research into engine phenomena was generally carried out in the context of engine procurement programs. Requirements for the engine were established, and technology development was part of the process of designing a new engine. The time leading up to the 1960s produced significant advances in turbine engines because of the sheer number of different aircraft being developed and the attendant empirical findings that came with that experience. Every program provided opportunities to develop new components, explore new material temperature capabilities, and work in new aerodynamic regimes.

Bernard L. Koff, former engineering manager at both General Electric Aircraft Engines and Pratt & Whitney, notes that

> Engine programs were defined setting goals for performance, weight, reliability, cost and schedule. Contracts were let to industry upon evaluation of a proposal and based on perceived capability in meeting requirements. The Air Force and Navy set up Program Offices at WPAFB and the Naval Air Station in Trenton, NJ, to coordinate monitor and evaluate progress on engine development contracts. Both the Naval Air Station in Trenton and WPAFB had laboratories to develop specific technologies to support engine components. The Trenton facility also tested engines and Navy personnel, using limited resources, worked with industry on improved technologies for fleet engines.
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> Key engine development programs often fell short of meeting requirements in terms of performance, weight, cost and schedule. The compressors encountered blade fatigue failures and low stall margin causing engine instability in flight maneuvers, the combustors would burn out, flame out and send hot streaks to the turbine while turbine blades would suffer oxidation, over temperature and premature failure. Maintenance and lack of durability of the engine cores at Air Force bases was a major issue to be addressed.[1]

Nevertheless, the sheer number of engines developed in the 1950s and 1960s indicates that opportunities for technology advancements were plentiful.[2] These procurements addressed a full spectrum of sizes and classes but had a tendency to be very "scattered" in terms of any technology developments.[3]

The advancements were significant as a whole. For example, turbine engines became economically and technologically viable for use in commercial passenger aircraft at this time. However, in this period, engines were frequently designed without specific applications in mind; instead, aircraft tended to be designed around available engines and the propulsion capabilities they represented.[4]

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1 Bernard L. Koff, "Jet Engine Case Study for MDA," Institute for Defense Analyses working paper, 2002.

2 J. Richard Nelson, Institute for Defense Analyses, "An Approach to the Life-Cycle Analysis of Aircraft Turbine Engines," presentation at George Mason University, 18 April 2002.

3 Ray Standahar (retired DoD Staff Specialist for Propulsion), personal interview, October 2002.

4 James Nelson (retired AF Colonel, AF Staff, GE Aircraft Engines), personal interview, October 2002.

The modern history of more formal S&T for engine development started around 1960, when the Aero Propulsion Laboratory at Wright-Patterson Air Force Base (AFB) found itself with zero budget resources for the development of new technology for turbine engines, partly based on the argument (which has periodically recurred since then) that "turbine engines are a mature technology—nothing more needs to be done." At that point, the Propulsion Laboratory management conceived the concept of a gas generator platform to develop a future engine technology base. A gas generator refers to the central components of a turbofan engine: the high-pressure compressor, combustor, and high-pressure turbine. Part of the rationale was that engine development time is much longer than airframe development time, so engine components and technologies needed to be developed ahead of time so that they would be available off the shelf when an aircraft system development began. The first nascent effort at a technology demonstrator was called the Lightweight Gas Generator program, which formed the basis for the Advanced Turbine Engine Gas Generator, or ATEGG, in the mid-1960s. ATEGG was set up to test components in a realistic full-scale core engine environment, since it is the gas generator that sets the basis for overall system performance. The purpose of ATEGG was to use a proven existing platform to test new technologically advanced components developed by industry.[5]

> It is a program which can be made successful by a contractor and success can be permitted by the Government but success cannot be assured by the Government. It is a program where the output thrust or airflow is only in appropriate "class," it is not an engine and its purpose is to permit testing as cheaply and correctly in as close to engine environment as possible…The contractor must establish that all component work which he does is accomplished to fit in the same airflow unit. This means that all agencies or organizations which pay for component work provide ultimately the hardware of the selected airflow so that the benefits and knowledge obtained from all sources benefit all.[6]

The core demonstrator concept required that engine components used to test new technologies in designs and materials had to be made to a common scale to fit the demonstrator system. However, when the component technologies were transitioned to production engines, they merely had to be scaled up or down to the production application. The

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5 Ernest C. Simpson, *The Last Great Act of Defiance—The Memoirs of Ernest C. Simpson, Aero Propulsion Pioneer,* James St. Peter (Ed.), Wright-Patterson AFB: Air Force Wright Aeronautical Laboratories, 1987.

6 Ibid.

time scale for these transitions was on the order of 2 or 3 years. While ATEGG was not an official inter-Service program, Navy-supported technologies were included. Among the participating companies were General Electric, Pratt & Whitney, Curtiss-Wright, and Allison. Later, Continental and Lycoming were added to supply components for turbo-prop/turboshaft engines.[7]

Part of the reason ATEGG was created was to provide for industry and government laboratories a means of technology development that would be of sufficient interest not only to the companies' military engines, but also to commercial applications. In the early 1980s, ATEGG was expanded to include durability/life testing to create a more systematic approach to addressing engine failures, in contrast to "point" solution approaches common before.[8]

The JTDE program was set up in the mid-1970s as the first Air Force/Navy joint engine demonstrator effort. In contrast to ATEGG, the JTDE program demonstrated advances to the entire engine, not just the gas generator components. The two Services worked to a common set of requirements, but each did its own contracting from its own budgets. JTDE fulfilled a desire to expand engine S&T efforts beyond the core components to include fans, low-pressure turbines, and mechanical systems and accessories. However, like ATEGG, JTDE was driven by the same overall desire to advance capabilities, to demonstrate new technology approaches, and to provide experience to the personnel involved. The result of these types of demonstrator programs was "real test data applicable to real engines,"[9] rather than performance estimates from analytical models. These were carried out with 6.3 level funding, in contrast to basic 6.1 scientific research or 6.2 programs that concentrated on development of individual engine components.

The idea for JTDE came from two PMs, Tom Sims in the Air Force and Jim Petty in the Navy, who were friends since their college days. This ongoing relationship enabled the elements of trust necessary to get the program started, not only within the Services but also at the OSD level. The "Joint" aspect reflects the more formal Navy participation. An important aspect of JTDE was that this program worked toward common problems but with separately defendable budgets within each Service. Later, this model was

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7 Standahar, personal interview.

8 Richard Hill (Director, AFRL Propulsion Directorate), personal interview, September 2002.

9 Nelson, personal interview.

followed by IHPTET, which also included Army and NASA participation. The advantage in this arrangement was that each Service could feel that it had control over its own budget, while allowing for efforts that addressed common problems. In addition, the engine managers within each Service could point to their commitment to the programs as a way to defend them against budgetary raids from within their own Service commands. JTDE was a precursor to the IHPTET management structure. The ATEGG cores and JTDE engines fed significant technology into the Advanced Tactical Fighter demonstrators that led to today's F119, F135, and F136 engines.

Brief mention should also be made of the Aircraft Propulsion Subsystems Integration (APSI) program, which arose out of the problems encountered with the Pratt & Whitney TF30 turbofan for the F–111. APSI was devised to determine differences in performance of engines in their installed vs. uninstalled states, with primary emphasis on thrust losses. As such, it enabled better understanding of phenomena such as inlet distortion, incompatibility between inlets and fans and nozzle drag. APSI continues to this day, recognizing the importance of fully integrating engine and airframe designs for best matched performance.

Lee Coons of Pratt & Whitney comments that the significance of these demonstrator programs was that they

> were an integration of existing component demonstrators. One of the great advances I believe in the mid to late 70s was the focus on demonstrators that resulted from system studies that looked at future weapon systems. These system studies looked at advances in technology at the component level and resulted in engine configurations from which advanced technology component programs were formulated and executed. At the conclusion of the component demonstrations, the components were easily integrated into a demonstrator engine that could easily be tied back to an advanced weapon system capability. One such system study that was conducted at Pratt & Whitney in 1976 was a company sponsored system study looking at the potential replacement of the F–15. The new capability postulated for this weapon system was sustained supercruise. Individual technologies were assessed as to their payoff in this weapon system and the key technologies selected to be pursued under company IR&D and government 6.2 and 6.3 programs. What emerged from this technology planning and execution was a joint technology development effort

between industry and the government that provided the technology base for the F119 and F120 engines.[10]

The S&T advances made in gas turbine aircraft engine programs had to be demonstrated physically by some means, whether by specialized demonstrator programs or by field improvements and upgrades. The original performance demonstrator concept was later expanded to encompass elements of structural durability when field problems became dominant. The companies embraced this approach because they could test ideas that had relevance not only to military applications, but also to civilian aircraft and hence to their commercial business plans.[11] A major source of S&T funding from the 1960s through the 1980s was IR&D, which was nominally industry funding. However, government procurements allowed a certain amount of IR&D (the IR&D "cap") to be charged to contracts as allowable overhead expense, which meant that government funded a fraction of IR&D, up to the ratio of military revenue to total revenue. In the 1960s, IR&D research was loosely managed and highly innovative, resulting in military turbofans, high-bypass commercial turbofans, film-cooled turbine blades, and a wide range of titanium components and manufacturing processes. By the 1980s, the government tied the IR&D cap to an annual review of IR&D programs. The government developed a bureaucracy to conduct the annual reviews, and industry developed corresponding bureaucracies to prepare reviews of each technology program and internally regulate IR&D research. These reviews eventually incorporated technology roadmaps that showed where technologies would be inserted into fielded products. These roadmaps evolved, under IHPTET, into Advanced Turbo-Propulsion Plans (ATPPs). Possibly because of the oversight of these multilayered reviews, the level of innovation declined in IR&D-funded research. In the 1990s, the government disbanded IR&D reviews, which led to industry abandoning long-range research in favor of quick payoffs.[12] The review process was doomed, in any case, by shrinkage of military revenues relative to commercial business in the engine companies. By the mid-1990s, government was funding a minor fraction of the IR&D budget, and so it had no stick to wield in determining how the research money would be spent.

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10 Leland Coons (retired IHPTET PM at Pratt & Whitney), personal communication, September 2002.

11 William Heiser (retired Propulsion Laboratory Chief Scientist and Professor at the AF Academy), personal interview, October 2002.

12 Koff, "Jet Engine Case Study for MDA."

D. TRANSITIONS FROM S&T TO FIELDED ENGINES

Among the engines developed during the period before IHPTET, the Pratt F100 engine program was a famous example of extremely aggressive technology advancement and the problems that could arise. Ray Standahar wrote the requirements document for this engine and noted that prior to the F100, engine capabilities (or lack thereof) tended to limit aircraft designs.[13] The F100 program tried build engines to serve the most advanced airframes, capabilities, and mission attributes, but F100 "teething problems" (compressor stalls and especially durability, with failures common at 100 hours) led to extensive efforts to improve serviceability and durability.[14] A major cause of problems was that initial engine development programs did not always have sufficient resources to work out these technical problems before the engines were fielded.[15] Examples such as the compressor stall and durability problems in the early TF30 and F100 turbofan engines for the F–111 and F–15 are classic cases of engine designs outpacing the capabilities of the materials and integration execution. Much of the S&T efforts in the 1970s onwards addressed engine durability problems, carried out under efforts such as the Component Improvement Programs (CIPs) that were also originally intended to introduce field changes to enhance performance.

CIPs were started in the 1950s, as part of the Continuing Engineering efforts, which were considered and funded as a production line item. Their role in S&T has similarities to spiral development processes. CIPs were the result of visionary leadership in the Air Force, which realized that field use of engines did not often follow original design intentions.[16] This was a major contributing factor to problems in the F100, especially thermal loadings caused by new throttle transients performed by pilots who were learning to exploit the full capabilities of the F–15 airframe.[17] However, the CIP concept was redirected in the 1980s toward safety and durability issues, although

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13 Standahar, personal interview.

14 James St. Peter, *The History of Aircraft Gas Turbine Engine Development in the United States…A Tradition of Excellence*, Chapter 15, Atlanta: International Gas Turbine Institute of the American Society of Mechanical Engineers, 1999.

15 Hill, personal interview, and Standahar, personal interview.

16 Nelson, personal interview.

17 St. Peter, *History of Aircraft Gas Turbine Engine Development*.

technologies that addressed durability and which also enhanced performance were acceptable.[18]

The F100 case also led to the creation of the Engine Model Derivative Program (EMDP), which served as the vehicle by which the Air Force and Pratt & Whitney could qualify a new low-pressure turbine and other core components created under the CIP to enhance durability. The introduction of the F100–220 under the EMDP reflected this type of upgrade and appears to serve as a model for such improvements.

The most dramatic application of EMDP funding was the creation of GE's F110 engine, originally designated the F101 Derivative Fighter Engine. The F101 engine powered the B–1 bomber and was itself derived from the demonstrator engine that lost the F–15 competition to the F100. EMDP developed the F110 from the F101 to replace the TF30 in the F–14 and provide an alternative powerplant to the F100 in the F–16. The F110 was also qualified for the F–15, and a dry version (i.e., with no afterburner) of the F110, the F118, was used to power the B–2 bomber and re-engine the U–2 reconnaissance platform.

In the 1970s through the mid-1980s (leading up to the formal creation of IHPTET), the technological emphasis in turbine engines for the military was in durability enhancement, rather than in other performance-related factors such as thrust-to-weight:

> To a significant extent, the lack of increase in thrust-to-weight ratio in the 1970–1985 period was due to the desire for an engine life substantially greater than that achieved by the F100–100 in 1973. There was a great emphasis on durability during this 15-year period, which culminated with the introduction of the F100–220 and the competing F110–100 in 1985. The most tangible result of this effort was an increase in the mean-time-between overhauls of the latter engines by a factor of about 2 over the fully developed F100 (and an even larger factor over the F100 as originally produced). More specifically, data obtained in 1995 show the depot interval for the F100–100 as 450 engine flight hours (EFH), the F100–220 interval as 675 EFH, and the F110–100 interval as 950 EFH. It can also be speculated that with the introduction of production competition between the F100 and F110, the so-called "Great Engine War," emphasis by the manufacturers was devoted to cost reduction—particularly through life improvement—as opposed to performance improvement. Since the mid-1980s, the emphasis has returned to performance improvement while maintaining a long life. For example, the depot intervals of the engines

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18 Dean Gissendanner (DoD Staff Specialist for Propulsion), personal interview, October 2002.

introduced in 1989, again according to 1995 data, are 1725 EFH for the F100–229 and 1435 hours for the F110–129.[19]

Most of these improvements, however, did not result from advanced S&T programs. Initial work to address low reliability of the F100–100 was funded as a CIP, which became a tool to fix production problems rather than perform research or discover new technologies. The durability increases in the F110 and F110–129 came mostly from commercial development work because the F110 shared a common core with the CFM56, which was being produced by the thousands in this period. The funding for this work is identified as IR&D in the government accounting structure but is better understood as post production engineering improvements, like military Continuing Engineering funding. Durability improvements on the CFM56 carried over to the F110. The F110–129 and F100–229 engine developments from the original F110–100 and F100–200, which specifically emphasized durability improvements, were funded as EMDPs, not S&T. Government direction to address durability problems in military aircraft through ATEGG[20] certainly played a role, but direct contributions from government S&T toward durability enhancement are not widely acknowledged within the industry.

E. WHERE DID THE INNOVATIONS COME FROM?

Gas turbine innovations in the 1960 through 1985 time period, came from industry development programs, industry research, military labs, NASA research, and joint military/industry work to correct problems with engines in the field. Even individual innovations seldom trace back to a single facility but, instead, arose from complex interactions among these teams. Because of this complexity, the reported sources of innovation vary substantially depending on point of view. However, some elements of the picture are widely recognized. Air Force laboratories introduced several basic advances such as such as compressor aerodynamic models (leading to better stall-tolerant engines), heat-transfer models for hot sections and cooled turbine blade designs, CFD models leading to more efficient compressor blade designs, vibratory analysis capability, and high work fans. NASA also contributed major research to develop component performance models, CFD mathematics and software, and analysis methods to predict engine noise and emissions. Progress in analytical capability was synergistically tied to the

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19 Donald Dix, "Technology Trends in U.S. Aircraft Engines 1970–2000," Unpublished Draft IDA paper, 2000.

20 Hill, personal interview.

development of computational capabilities. Not only did better computers enable more powerful analyses, but major analytical tasks such as CFD also provided a significant market for the most powerful supercomputers. Computer advances were key to improved stress analyses and, consequently, the first credible predictions of engine part lives.

However, the engine-manufacturing companies actually put many of these and other advances into practice, and the implementation entailed a substantial portion of the basic research. Industry used a combination of research contracts (funded by the government labs) and internal funding (IR&D) to support introduction of new technologies. However, because of the close working relationships between government and industrial technologists, personally and intellectually, the partnership aspect of advancing turbine engine capabilities has been a significant factor. Also, the frequent migration of technology personnel between different engine companies led to cross-pollination of innovations and transfers of technologies. According to one industrial technology leader, the interests of the government labs and industry were complementary:

> Because the interest of the companies was more near term than the government it brought a healthy tension. This tension was nicely resolved though the government and industry partnership commitment. This resulted in the companies funding nearer term and more conservative technologies and the government funding higher risk higher payoff technologies. If the more aggressive technologies fell short or missed achieving the goal on schedule, the more conservative (nearer term) technologies were used to keep the program moving forward.[21]

F. IHPTET

In a very general sense, the creation of IHPTET in 1987 was a consolidation of then ongoing demonstration programs, rather than the start of something completely new. However, IHPTET was distinguished from its predecessors by its focus on a measurable leap in performance (doubling thrust-to-weight ratio) and its success in maintaining funding stability. The Background section of the IHPTET Technology Development Approach (TDA) document states:

> The IHPTET initiative was formally initiated on 1 October 1987, but its roots can be traced back to 1982.

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[21] Coons, personal communication.

> The High Performance Turbine Engine Technologies (HPTET) effort began in 1982 as an advanced technology development study in the Air Force Wright Aeronautical Laboratories, Aero Propulsion Laboratory (APL). The APL initiated the "Integrated Technology Plan for the 90s" (ITP–90). Realizing that advanced materials development was a pacing item, the Materials Laboratory (ML) joined the initiative as a partner in 1984 with an increased emphasis being placed on advanced materials and structures. The assessment by the Materials Laboratory regarding the optimistic development time necessary for the critical materials was very influential in setting the technology demonstration dates.
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> In 1985, in compliance with the direction of the Commander, Air Force Systems Command, to increase the gas turbine engine industry involvement in the HPTET, major planning reviews were held with the following seven aircraft engine companies: Allison Gas Turbine Division; Garrett Turbine Engine Company; General Electric; Lycoming; Pratt & Whitney; Teledyne CAE; and Williams International. The Navy and NASA also participated in the development of corporate long-range plans to accomplish the ambitious goals of the HPTET initiative by the turn of the century. The seven engine companies also made substantial commitments of company resources to their long-range plans which included company efforts that complemented the HPTET goals.
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> At the urging of the Deputy Under Secretary of Defense for Research and Advanced Technology (DUSDR&E/R&AT), the Army, Navy, Defense Advanced Research Projects Agency (DARPA) and National Aeronautics and Space Administration (NASA) joined the Air Force in developing a coordinated long range plan embracing the goals of the HPTET initiative. The resulting technology development and demonstration plan represented a fully integrated Government/Industry activity, and thus the Integrated High Performance Turbine Engine Technology (IHPTET) program was born.[22]

When IHPTET was formed, it consolidated several existing demonstrator programs, including ATEGG, JTDE, and APSI. Additional participation by the Army, NASA, and DARPA has also contributed significant programs and efforts within IHPTET, though these were often already existing core activities in those agencies and Services. IHPTET, like its predecessor demonstrator programs, was resisted at first within some parts of the laboratory structure because it took 6.2 level "sandbox"

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22 Robert Henderson et al., *IHPTET Technology Development Approach (TDA),* 1998.

programs and forced them to consider transition paths.[23] But once it was realized that these paths could result in the fruition of new ideas and concepts, resistance to technology transition planning dropped away.

Whether IHPTET represents an incremental or a more radical example of technology change in turbine engines is a matter of perspective. The aggressiveness of the performance goal (doubling thrust per pound of engine weight) suggests radical innovation. However, the individual technology innovations are incremental: higher specific strength materials applied in particular components, increased material temperature capabilities, and improved cooling systems. The state of gas turbine product evolution may preclude radical change—the basic components and the thermodynamic cycle is set. Fundamental architecture has not changed since the TF30 design in the 1960s. IHPTET's most eloquent proponent states the case for radical improvement as a sum of incremental advances:

> It is clear that significant progress has been made in the last three decades; in terms of performance measures, this progress has been most noticeable in the last 15 years. In general terms, the mechanisms for such progress are well known. Higher maximum cycle temperatures and lighter weight components and structures increase the output-to-weight ratio—higher temperatures by increasing the output per unit airflow, and lighter weight components and structures decreasing the weight per unit airflow. Higher combustion-initiation temperatures (higher pressure ratios in simple-cycle engines) and improved component efficiencies decrease the specific fuel consumption—the higher temperatures by increasing the theoretical efficiency and component efficiencies by achieving actual performance closer to the theoretical maximum.[24]

However, some saw the benefit of IHPTET primarily in the novel way it organized S&T research:

> The IHPTET program brought several major things to the table. They include a broad set of agreed upon revolutionary propulsion system goals, a highly integrated and disciplined government and industry technology planning and review process, and a very integrated and disciplined resource commitment. The IHPTET goals brought unification of a vision for the future. In fact, IHPTET became so engrained at PW [Pratt and

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23 Robert Henderson (former AFRL PM and current IHPTET Director), personal interview, October 2002.

24 Dix, "Technology Trends."

> Whitney], it was accepted as a core part of the overall technology development plan for all of PW. The program was actively supported by the executive team managing PW. Long range IR&D commitments were made and in general were more firm than in the past as the management team was familiar with how the money was being invested as opposed to the old days where there was a feeling of the money going into a black hole of sorts. The recognition of IHPTET by the management team allowed changes in personnel at several levels without the plan being put in jeopardy.[25]

IHPTET requires technologies to have a transition plan before receiving support, with a user, such as an Air Force Systems Program Office, signing off to use the particular technology once it has been demonstrated to IHPTET requirements. The user connection protects the technology development effort, while buy-in from the field activity and the engine contractor ensures eventual application. The F119 relies on turbine cooling technologies originating under ATEGG and JTDE.[26]

The 15-year commitment to IHPTET was a major step for both the government and the engine companies with respect to programs and funding stability.[27] The financially stable, multiyear nature of the program was essential to its success.[28] Another critical element was the early definition of key technologies, with a division of responsibility for the participants to allocate the resources to carry out the program. For many years, industry contributed roughly half of IHPTET's overall budget.

> A well planned program has the advantage of knowing where it will go even in times of budgetary constraint; whereas other programs which repeatedly redo their plans to fit changing (usually shrinking) budgets will be reactive in nature, and hence at a disadvantage.[29]

G. MANAGEMENT CULTURE IN THE TURBINE ENGINE COMMUNITY

The success of turbine engine S&T programs derived in large part from the cooperative interaction between various technical and management personnel in the government and industry, which allowed problems and approaches to their solution to be

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25 Coons, personal communication.

26 Henderson, personal interview.

27 Further details on the origins of IHPTET and the personalities involved are in Chapter 19 of St. Peter. See note 18.

28 Belcan Corporation, "Transcript of Workshop on Aircraft Engine S&T Programs," November 2002.

29 Hill, personal interview.

communicated readily. These relationships tended to be different from the type seen in more conventional acquisition programs. The distinctive characteristic was partly caused by the longevity of government and industry technical personnel who could interact with each other. In many cases, careers were measured in decades, not years.[30]

One benefit of this long retention was the foundation for long-term working partnerships/relationships (even friendships) with counterparts in the other community. Relationships based on trust between the government managers and the industrial personnel enabled them to keep each other informed of progress, to work out problems, and to encourage competitive solutions and activities, while the companies could feel that their trade secrets would be kept safe by government S&T managers. Trust enabled informal ground rules that both sides would not pursue avenues of inquiry or actions that were legally open to them if this might undermine the relationships that had been built up. The long-term relationships between government managers in the Services also helped to ensure that the government had a united position when negotiating matters with industry, which was considered a key toward resolving disputes that did arise.

Another key attribute of the IHPTET organization was the high technical competence within government management. Often, this resulted from managers having prior experience as working scientists or engineers. Air Force program management tended to have more laboratory orientation, whereas the Navy emphasized experience in the fleet and experience dealing with field problems.[31] Both organizations agreed that familiarity and training in engines was needed to manage S&T programs successfully. A level of technical competence was also required at upper levels in the Pentagon. There, a strong advocate of the programs could not only provide cover for the S&T programs at the laboratory level, but could also challenge the field management personnel to work outside of their comfort zones.[32]

One of the possible downsides to the depth of relationships described in the turbine-engine community is the possibility that new ideas or concepts perceived as generated "outside" a community may not get a full hearing or an impartial evaluation regarding their technical validity (the "Not Invented Here, or NIH, Syndrome"). Another

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30 Belcan Corporation, "Transcript of Workshop."

31 Albert Martino (retired Naval Air Systems Command and Office of Naval Technology PM), personal interview, October 2002.

32 Gissendanner, personal interview.

potential pitfall is that the relationships could prevent levels of technical oversight and skepticism that a more "adversarial" culture might generate. These potential problems, however, are seen even in obviously dysfunctional communities. In this case study, no mention was made of the rejection of new propulsion concepts because of the NIH Syndrome, and the closeness of the community has not precluded vigorous technical exchange and disputes among its members. Nevertheless, the possibilities for such less desirable characteristics arising from close relationships do exist and need to be acknowledged in any S&T community.

Steering committees, with government and industry participation and input, are essential elements of the IHPTET model. The IHPTET steering committee concept draws on successful experiences in the Joint Army-Navy-NASA-Air Force (JANNAF) Interagency Propulsion Committee.[33] Although industry members cannot have veto or other powers over Federal Acquisition procedures, these steering committees, on a purely technical basis, provide an informational role in the decision-making. A side effect is that the participating Services must act in concert on policy matters to avoid being "played against each other" by the companies.[34] As long as the steering group encourages ongoing technical exchange without getting bogged down in ways that hamper the S&T work through endless bureaucratic box-checking, they can provide a useful mechanism for building the types of relationships mentioned elsewhere. Such a system should allow sufficient freedom to support S&T managers in their extensive, ongoing planning processes. The IHPTET program incorporates continuous planning activities, which can be tedious and time consuming, but, if the contractors are doing the bulk of the heavy lifting, it behooves the government managers to use their skills in making sure the work is responsive to the strategic plans. In the technology planning process, it is impossible to overestimate the work involved, but it is also impossible to overestimate the benefits that come with it.[35]

It is not clear that these stable relationships and the resulting levels of trust and teamwork can exist in a more typical present-day government organization that institutes short-term management assignments and suffers from unstable funding. The consequences of present realities are that "complex systems will continue to require

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33 Richard Weiss (retired Director, AF Rocket Laboratory), personal interview, November 2002.

34 Belcan Corporation, "Transcript of Workshop."

35 Heiser, personal interview.

intuitive rather than analytical judgment; thus not having a sense of institutional memory for development of such systems puts the programs at risk."[36]

H. RADICAL INNOVATION IN THE ENGINE COMMUNITY

The 1960s were glory days of aircraft engine development. The decade opened with the variable-geometry J79 and high-altitude J75 entering service. The two most ambitious gas turbine engines to date, the Mach 3 J58 and J93, were developed early in the decade. Pratt & Whitney produced the first military turbofan, the TF30, and GE built the first (and to date, highest bypass) subsonic turbofan, the TF39. Three GE–1 demonstrators in the 1960s developed the core component designs for the F101, F110, F118, F404, CFM56, and by way of the TF39, the CF6 engines, essentially GE's entire large engine product line 30 years later.

Major technology accomplishments delivered to production in the period from 1960 to 1975 include[37]

- Titanium compressor airfoils and disks;
- Nickel alloy disks, beginning with Inconel 718 and progressing to powdered metallurgy turbine disks;
- Investment cast nickel alloy turbine blades, with cast-in cooling passages, resulting in production engine turbine rotor inlet temperatures of 2,450 °F;[38]
- Turbofan architecture;
- Thermodynamic cycle modeling, to a level that was useful for performance prediction and control design; and
- Control strategies that deal with interacting components and manage compressor stall margin.

Despite billions of dollars expended on aircraft engine development, the only comparable advances since 1975 have been digital controls (an adaptation of technology from another industry) and higher turbine temperatures. At the same time, engine

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36 Jim Williams (Dean of Engineering, The Ohio State University, and former manager at GE Aircraft Engines), personal interview, October 2002.

37 Thomas F. Donohue (retired General Manager, GE Aircraft Engines), personal interview, October 2002.

38 For example, the CF6 achieved 1,345 °C in 1970. Twenty-four years later, the state-of-the-art GE90 exceeded this by only 80 °C (Williams, 2002).

development has slowed from two engines per year in the 1960s[39] to a the point where, in the 1990s, Pratt & Whitney produced no all new large engines and GE only produced one. Explaining this precipitous drop-off in research productivity is the key to understanding aircraft engine S&T in the period. Three forces can be observed as contributing to the slowdown, without speculating on their relative importance:

- The aircraft engine is a "mature product"[40] or a "commodity."[41] Maturity refers to the idea that technologies can be ranked by the ratio of payoff to development cost. The high-payoff, low-development cost technologies were developed first. By 1975, the only technologies left to discover were low payoff for high-development cost. Technology investment has reached a point of diminishing returns. The commodity argument is similar: the performance of competing product lines has converged and airframes have matured so that competition is focused on price and reliability. Therefore, product improvement is concentrated on making engines more rugged and less expensive. These improvements are best achieved by modifications to production engines rather than new models, so they are seldom tracked as research or development. Both arguments presume that the array of potential technology improvements to engines is more or less fixed and has been known since 1960. The really good ideas for performance improvement are already taken, so the opportunities are exhausted.
- Complexity theory offers a more sophisticated view of the same phenomenon.[42] Early in the development of a complex product family, various architectures are explored. Design effort focuses on the most promising architectures, and they are refined into higher value products. Exploration of new architectures is reduced because when a new architecture is compared with the current, refined architecture, it invariably comes up short, if only because it lacks the decades of incremental improvements that benefit the status quo. Furthermore, adopting a radical change entails a substantial investment and costs to the infrastructure that has been built up around the status quo design. Thus, to adopt a radical design change, the new concept in its unrefined state must be significantly superior to the refined status quo. This is unlikely to happen even if the new architecture is very superior to status quo architecture when compensation is made for the refinements.

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39 Donohue, personal interview.

40 Dave Edmunds, Chief Engineer, F119 engine, F–22 Systems Program Office, personal interview, October 2002.

41 Belcan Corporation, "Transcript of Workshop."

42 Stuart A. Kauffman, *The Origins of Order: Self-Organization and Selection in Evolution,* New York, Oxford University Press, 1993.

Thus, complex products tend to lock into particular architectures over time, and radical technologies that entail major changes to the product become less and less attractive.

- The third theory is organizational. Like Lockheed Skunkworks projects, the engines of the 1960s were developed by small, highly motivated teams of engineers and machinists with flat organizational structures, operating with a minimum of reviews and oversight.[43] A great deal of responsibility was entrusted to a small number of people, so that if an engineer wanted to incorporate a radical technology, he often had the latitude to do so without obtaining consent from many others. This could be an essential element of achieving radical improvements. Early on, the promise of radical technologies is not clear and seldom quantifiable. Support of them is often a matter of faith as much as reason. Such designs cannot survive multiple layers of top-level reviews but instead depend on champions and trust. When management funds a tight-knit team and depends on trust rather than reviews and tollgates, there is a significant risk that there will be little to show for the investment at the end of the day. However, when layers of oversight and reviews are used, there is almost no chance of successful radical innovation. In addition, small tight-knit teams can develop technology rapidly and inexpensively so that a much higher failure rate becomes tolerable.

Numerous examples of skunkworks-type programs exist in both industry and government. What is more interesting is that more structured research programs, such as the IR&D program of the 1980s and IHPTET of the late 1980s and 1990s, have featured bureaucracies on the industry and government sides who conduct systematic layered reviews of technology programs. These programs have notably failed to produce radical innovations that have transitioned to products in service as promised. They have succeeded in introducing many incremental technological improvements, particularly in raising cycle temperature limits. On the other hand, IR&D, IHPTET and other government-funded programs have invested decades of research into programs such as ceramic-matrix composites, metal-matrix composites, analytical sensor redundancy, and performance-seeking controls, many of which have so far failed to transition into full-scale production in a meaningful way—or at least to anticipated levels.

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43 See, for example, the description of the development of the GOL1590, the demonstrator that led to the J79, in Neumann (1984).

Many veterans of the period reinforced these points:

> Past experience dictates that the planning process be balanced, so that meticulous planning does not become incompatible with more radical innovative potential, and may require that some separation between the two activities could be warranted.[44]

> Bureaucracies grew up in the lab and in the industry to manage IRAD in meticulous detail, which may have detracted from its effectiveness. Although IRAD was supposedly contractor controlled, there was still Air Force oversight and review. IRAD was, in many cases, not effective. Research programs under IRAD were not often transitioned into products.[45]

> In the 1960s–1970s the Pentagon Propulsion S&T staff had considerably more discretionary power to make decisions on what S&T programs to fund.[46] This discretion went away by the mid-1980s.[47]

> The government has spent considerable funds and resources to introduce first low and then high-temperature advanced composite materials to replace metals. The gas turbine engineers were not successful in applying these materials into many engine components due to their inherent lack of ductility….The government WPAFB materials personnel persisted in expending resources for high temperature composites for aircraft and ramjet engines at the expense of exploring and developing improvements in monolithic alloys.[48]

Thus, one shortfall in propulsion S&T during this period was the inability to abandon lines of research that did not deliver results to products or to associated demonstrator programs. One suggestion:

> An overarching group can be established to sort out which research projects go forward and which should be stopped. Component technology groups may not be able to do this effectively on their own. An overarching group may be assigned to each component, perhaps not a permanent group, but ad hoc. That is, a permanent management structure, but ad hoc

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44 Weiss, personal interview.

45 Donohue, personal interview.

46 Standahar, personal interview.

47 Gissendanner, personal interview.

48 Koff, "Jet Engine Case Study for MDA."

technical review group. The key role of the review group is vetting technologies so that money can be focused on worthwhile ideas.[49]

A perspective on the materials side was given by Jim Williams, currently Dean of Engineering at Ohio State University, who managed the Materials Laboratory at GE Aircraft Engines for several years. Dr. Williams noted that in many S&T programs, continuous, incremental improvement needs to be pursued rather than counting only on radical innovation. This also relies on prioritizing the technologies that are critical to achieving a program success and pursuing those—in other words, start worrying sooner rather than later about transitioning and aspects of manufacturing those components needed to build the desired systems. Designs should not get ahead of the necessary technologies or the ability to use them to carry out the designs. Otherwise, it will be easy to lose momentum and group cohesion if disruptions occur because of a lack of planning to address the pacing technologies. One of the failures of the IHPTET program from Dr. Williams' perspective, in sharp contrast to the opinions cited previously, has been the delayed investment in the materials needed to bring the goals to final realization (caused by several reasons, such as lack of resources or inadequate time to address unforeseen problems). Current business practices, which are moving toward engine leasing rather than purchases, especially in the commercial sector, which is now larger than the military sector, make the question of durability even more important to the companies. This tends to suppress further any drive toward using innovative materials that have not undergone rigorous testing and manufacturing certification.[50]

I. GENERAL APPLICABILITY OF IHPTET S&T MANAGEMENT METHODS

Gas turbine engines rank among the most useful and most technically impressive artifacts of our age. The S&T programs of the last half century have been remarkably successful, both for the revolutionary advances before 1975 and the steady stream of incremental improvements in cycle temperatures and thrust-to-weight ratio after 1975. The sustained flow of technology improvements since 1975 is largely because of the methods used by ATEGG and JTDE, culminating in the disciplined IHPTET process, plus NASA activities, such as the Electrical, Electronic, and Electromechanical (EEE) program. Can the IHPTET management processes be effective in other product domains?

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49 Edmunds, personal interview.

50 Williams, personal interview.

One industry executive who managed technology programs under the IHPTET system for many years answers as follows:

> The propulsion technology focus has probably not changed much over the years. The quest has typically been for improved performance and that usually means higher turbine temperature (for higher specific thrust) and better materials for lower specific weight. Aerodynamic technology was oriented towards higher efficiency and reduced number of stages (for reduced weight and cost). This was true for fans, compressors, and high and low pressure turbines. Combustors and turbines had to manage the higher temperatures with reduced cooling air. What IHPTET drove was an accelerated pace in achieving the higher levels of performance. It also brought an integrated government and industry team to attack the aggressive goals and a disciplined process for planning and program accountability. IHPTET, I believe created a new culture for effective development of propulsion technology at a pace that provides propulsion system capability that has helped the US develop and deploy superior weapon systems.
>
> I believe that IHPTET is a benchmark in best practices for S&T planning and execution and could serve as a model for other S&T efforts within the government. As in all successful efforts, it needs a high level champion with a passion to drive the process.[51]

The IHPTET structure has been consciously applied to research in rocket propulsion. The IHPRPT program was reorganized in the 1990s along parallel lines to the IHPTET organization, including the creation of a Steering Committee structure. The quantitative, goal-oriented approach that marks the GOTChA and IHPTET management models, when applied to the rocket programs under IHPRPT, came up with mixed results.[52] There are several reasons, which contrast rocket propulsion against the air-breathing propulsion industry:

- Lack of a truly commercial industry for rocket propulsion;
- Less settled technologies options available until the systems-development stage; and
- Less overall government support at a steady funding rate, whether to the military or civilian (NASA) agencies.

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51 Coons, personal communication.

52 Weiss, personal interview.

In addition, the rocket community requires a conscious effort on the part of the government to set aside funds to support more fundamental, radical ideas—industry will not do this because of a lack of IR&D funds and any sort of commercial market from which to recoup research spending. The retired director of the Phillips Laboratory recommends that some fixed percentage of the S&T budget, such as 10 percent, be set aside for this purpose.[53]

Currently, the Vehicle Systems Office at NASA Headquarters is exploring the application of the IHPTET/GOTChA process to its own program structure.

It is not clear that the structured IHPTET approach can be effective in blue sky, first principles' basic research or research at the architecture and large platforms level of planning, where radical designs are the essence of S&T.

One consumer of IHPTET technology comments:

> The period from 1940 to 1970 or 1975 was ripe for new technologies in aircraft engine development. After 1975, the products have approached maturity and there have been correspondingly fewer inventions. IHPTET began in the early 1980s and is a program perhaps geared best to incremental technology development for a mature product… One reason S&T in this period was so successful is that there was an architecture established for the engines (essentially the architecture of the TF30 and TF39). This allowed science research at the component level, where phenomena could be understood at a detailed level. In the 1940s and somewhat in the 1950s, a variety of architectures was investigated, so that it was hard to focus much attention on one component of one configuration.[54]

The extent that the GOTChA process from IHPTET applies to less well-defined S&T programs depends, in part, on the ability to define quantifiable goals as part of the exercise. The IHPTET planning process and model applies to the highest mission level at which goals can be quantified, and thus verified. For research programs that investigate issues of basic feasibility, quantified goals are often not established. The question is "Will it work?" rather than "How much?"

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53 Weiss, personal interview.

54 Edmunds, personal interview.

XIII. S&T ORGANIZATION AND MANAGEMENT IN THE ARMY NIGHT VISION LABORATORY, 1954–1990: MANAGING SYNERGISTIC RISK

Ivars Gutmanis and Michael Lippitz[1]

EXECUTIVE SUMMARY

Since the 1950s, the U.S. Army has supported research, development, test, and engineering (RDT&E) work in the area of night vision. This case study focuses on the efforts of an evolving government S&T organization known as the U.S. Army Night Vision Laboratory (NVL). NVL's efforts are widely recognized as highly successful, having resulted in strikingly superior warfighting capabilities for U.S. forces. NVL also contributed to the formation of a dynamic commercial market that DoD has been able to leverage to improve the quality and cost-effectiveness of a wide variety of night-vision systems.

NVL's leadership, management culture, and technical capabilities played crucial roles in the development of superior U.S. night-vision capabilities. In addition to inventing new night-vision technologies, NVL consolidated and channeled the skills and resources of technologists, industry, and DoD acquisition and warfighting communities. This case study highlights contributions of and synergies among NVL disciplines, organizational structures, and management practices:

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1 Several individuals rendered significant assistance in the preparation of this report. Their contributions are acknowledged with appreciation.

Lucien Biberman, IDA; Lester MacKay, former Deputy for Development, NVL; Dr. Martin Nisenoff, ONR; Ronald Petrie, NVL; Prof. Albert P. Pisano, FANUC Chair, U. of Calif., Berkeley; Robert I. Scase, former Deputy Director Semiconductor Bench, NIST; Robert Scola, P.E., U.S. Army Picatinny Arsenal; Ed Sheehan, former Chief or Development and former Director, NVL; John Stoner, MG USA, Retired, and former Commanding General, ECOM; Dr. Mark L. Swinson, Sandia National Laboratories; Jerry B. Warner, Col., U.S.A. Ret.; Dr. Robert S. Wiseman, former Director, NVL.

- Establishment of reputation for technical skill and effectiveness in the field through close S&T cooperation with Army staff, system developers, and field commands;
- Development and use of technology-management practices—some of them pioneering—well suited to addressing both long-term innovation and immediate system needs, with an emphasis on leveraging industry's S&T efforts; and
- Building and maintaining internal technical capabilities and application expertise.

Establishment of Reputation for Technical Skill and Effectiveness in the Field

Internal politics is part and parcel of government organizations. However, rather than become bogged down with bureaucratic priorities, NVL managed them astutely. Robert Wiseman—who either directed or oversaw NVL from 1954–1968 and served in Army headquarters until 1981—took pains to build and maintain relationships with NVLs overseers in the Army staff, with the broader technology community, and among civilian authorities in OSD, military warfighter leadership, and Congress.

The Army staff and NVL worked together to provide superior equipment to troops in the field, encompassing the whole of the systems-development process, from underlying scientific principles to real problems faced by soldiers in combat. Large numbers of NVL personnel spend long periods in the field, sometimes even accompanying deployed troops onto actual battlefields. This close involvement has built respect and trust between the soldiers and the NVL personnel who work with them.

NVL systematically used well-planned field tests to gain data for their modeling activities. They employed these models as advanced management tools for translating an understanding of user needs and strategic problems into mathematical systems analysis tools that captured quantitatively the relationship of physical parameters in equipment to final system performance in the field. They then applied these tools comprehensively to the design, product engineering, testing, and long-term research portfolio decisions.

In the 1950s, NVL's attention to organizational issues helped them defend their "turf" against other Army organizations that believed they owned the NVL mission. In 1961, a commission on "limited warfare" chaired by the Nobel Laureate Luis Alvarez highlighted the importance of being able to fight at night. Having succeeded in establishing itself during the turf battles of the 1950s, NVL was well positioned to be

recognized for its potential to contribute to this new defense policy. Its connections and respect within the Army staff led to a four-fold increase in resources.

With this increase in support, Director Robert Wiseman and his deputy John Johnson assembled a staff devoted to developing an understanding of (1) the basic physics and engineering issues of photo-emissive technology and image intensification and (2) the relationship between the physical parameters of imagery and the effectiveness of such imagery to a human user in detecting and identifying observed objects. This dual approach led to a synergism between the research, development, and engineering that lasted well past NVL's early years.

By 1965, NVL consolidated its control of most aspects of night-vision technology work in the Army. In the years to come, they would expand their scope not only into new technologies relevant to the night-vision mission, but also into new missions supported by their broadening technology expertise. However, as the laboratory matured, it lost certain aspects of its early creative leadership. Over time, the Army increasingly distributed NVL's management and mission to other organizations. These moves disrupted the close working relationships between the NVL basic research staff and night-vision systems developers, leading to a loss of the morale that had made NVL one of the most effective and productive government laboratories in the United States.

Development and Use of Pioneering Technology-Management Practices

The success of NVL's assertive, user-oriented S&T approach and bureaucratic prominence were based, in part, on the development and use of pioneering technology-management practices suited to its comprehensive vision for night-vision capabilities. NVL was a pioneer in the use of concurrent engineering, modular design techniques, and rapid prototyping. NVL routinely invested in multiple technical approaches (short and long term) for addressing particular night-vision functions, moving resources around as one approach or the other advanced.

Perhaps the most important part of making this management approach work was careful coordination with PMs and contractors. This was not always an easy task because modular design approaches are often not perceived as being in the interest of private firms or of individual programs. Maintaining a competitive environment and performing "due diligence" for the government without discouraging private innovation require careful handling of business arrangements and intellectual property. NVL pursued this

task vigorously, often exchanging technical personnel with contractors, but they did so delicately to avoid charges of favoritism.

Building and Maintaining Internal Technical Capabilities and Application Expertise

Managing short-, medium-, and long-term S&T in a multidisciplinary environment requires technical expertise in a variety of areas. NVL concentrated on filling gaps in contractor capability, but also developed capabilities across the technical spectrum. This approach permitted NVL to manage contractors aggressively since they could often complete a prototyping job in-house if a contractor failed. Wiseman and Johnson realized the greater the in-house technical capability, the better NVL could manage complex technical systems. This in-house capability produced a team that could and did act as "smart buyers."

NVL identified the requirement for communication, coordination, and bridging among different technical communities and invented several formal and informal mechanisms for effecting this coordination, akin to today's "integrated product and process teams." This early organizational management philosophy gave all participants a coherent view of the entire development and deployment process and helped teams "do it right the first time."

NVL found that maintaining a whole-systems perspective was motivational for its personnel because people could understand the relevance of their work to ultimate organizational goals. These organizational goals were stated in terms of the user, as opposed to being abstract technical accomplishments. NVL management's earnest belief in these goals infused itself throughout the organization and was a source of inspiration for the people working there. NVL sought to maintain continuity in this management philosophy by developing and promoting from within.

A. THE HISTORY OF NIGHT-VISION TECHNOLOGY AND THE U.S. ARMY NIGHT VISION LABORATORY

Since the 1950s, the U.S. Army has supported robust RDT&E work in the area of night vision along a broad spectrum of technologies. The principle actor in these efforts was an evolving government S&T entity that will be referred to here as the Night Vision

Laboratory or NVL.[2] As with any history of technology, the role of individual people or groups can be hard to discern. It is not possible to separate cleanly the actions of people or groups from the broader context, an environment that shaped them as they simultaneously shaped it. However, it is clear that NVL's technical capabilities, management culture, and leadership played a crucial role in consolidating and channeling the skills and resources of technical, business, acquisition, and warfighting communities.

NVL's performance in night-vision technology advancement took the form of two sequential but different technologies. From its inception in 1954 to approximately the mid-1970s, NVL managed advancements in near-IR image-intensification systems. From the mid-1970s onward, NVL directed much of the development of thermal IR systems using a technology known as FLIR (forward-looking infrared). A summary appears in Table XIII-1.

NVL's efforts are widely recognized as highly successful, having resulted in strikingly superior warfighting capabilities for U.S. forces. NVL also contributed to the formation of a dynamic commercial market that DoD has been able to leverage to improve the quality and cost effectiveness of a wide variety of NV systems. Biberman argues that NVL "became one of the most successful Department of Defense research and development laboratories, producing a family of very effective night vision devices that are used today for applications not even imagined in the late 1950s."[3] The following paragraphs summarize some important elements of night-vision history.

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2 The term Night Vision Laboratory, "NVL," refers to R&D, design, assembly, manufacture, testing (laboratory and field), and various procurement activities (R&D, parts, components, systems) by several sequential U.S. Army entities. Today, NVL has been incorporated into the U.S. Army's Communications Electronics Command (CECOM) Night Vision and Electronic Sensors Directorate (NVESD).

3 L.M. Biberman, ed., *Electro-optical Imaging: System Performance and Modeling,* Bellingham, Washington: SPIE Press, 2000.

Table XIII-1. Summary of Technology Advances in Night-Vision Systems 1954–2001

1950–1960 (Commencement of Generation 0—Night-Vision Devices)

Photocathode
Commencement of Image Intensifier Technology
Modeling of Night-Vision Imaging Systems
Visionics: Target Detection, Orientation, Recognition, and Identification
Development of the First Generation of Passive Night-Vision Technology

1960–1970 (Commencement of Generation 1—Night-Vision Devices)

Commencement of Fiber-Optics Technology
Advancements in Visionics Fiber-Optic Coupled Image Intensifier
Advancements in Far IR Technology
Advancements in Light Source Analyses and Adaptation
Thin Film Development
Sensor Technology (Sensor Fusion)
Commencement of Laser Technology: Development of Initial Monolithic Diode Arrays

1970–1980 (Commencement of Generation 2—FLIR Systems Development)

Linear Scanning Technology
Advancements in Multiple-Element Detection Arrays
Lasers, Laser Designators, Diode Pumped Lasers, Tunable Lasers
Commencement of Uncooled IR Sensors Technologies
Nonlinear Frequency Conversion
Microchannel Image Intensifier

1980–1990 (Commencement of Generation 3—Night-Vision Devices)

Higher Sensitivity Microchannel Image Intensifier
Advancements in FLIR Systems
Advancements in Laser Technologies
Target Recognition Technologies
Commencement in Uncooled IR Sensor Technologies
Advancements in Cooled IR Sensor
Multispectral Imaging: Improved Sensitivity and Resolution

The DoD's night-vision technology advancements after World War II were, for the most part, based on domestic S&T efforts, reaching back to the 1930s and intensified during World War II.[4] The early night-vision R&D activities, such as a low-powered IR illuminator used for the "Infrared Sniper Scope," were managed by the National Defense Research Committee (NDRC) of the Office of Scientific Research and Development (OSRD), within a section called Infrared Devices. In the early 1940s, NDRC was reorganized and Section of Division 16 (Optics and Camouflage) was given

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4 C.G. Suits, G.R. Harrison, and L. Jordon, eds., *Science in World War II: The Signal Corps,* Office of the Chief of Military History, USDA, Washington, D.C., 1956; U.S. DoD, FC 90-1 Night Operations, Fort Leavenworth, Kan., 1985.

responsibility for applying IR techniques to the military activities.[5] Among the U.S. industry establishments during World War II, the Radio Corporation of America (RCA) was the principal commercial entity engaged in night-vision research, with a focus on the electrostatically focused image tubes.

During World War II, the IR technology effort was also conducted by the Soviet Union, the United Kingdom, Poland, and Germany.[6] The activities in the United Kingdom focused on the development of proximity tubes. In Germany, the development of the IR image converter tube on the eve of World War II made it possible for Germany to be the first country to deploy IR equipment in the field.[7]

The limited but valuable night-vision experience that the military gained in combat during World War II sparked interest in night-vision technology advancement in the immediate post World War II period, albeit on a modest scale. In the 1950s, efforts focused on several night-vision technology approaches: near-IR image tubes for sniper scopes, binoculars for soldiers, and periscopes for tanks; improved carbon searchlights as a near-IR light source; advances in chemical compounds for conventional flares; and cryogenics for cooled detectors.[8] NVL pursued these improvements with research programs in the areas of IR photocathodes, electro-optical components (to improve gain), carbon arc and tungsten sources, and weight reduction. RCA developed improved photocathodes under contract to NVL.

In 1956, the government established a DoD-wide classified organization called the Infrared Information Symposium (IRIS), with NVL as a principal member. The purpose was to exchange technical information on night-vision S&T. In the early 1960s, U.S. industry increased their activities in night-vision devices. For example, at Texas Instruments a small group of scientists engaged in night-vision R&D for the NVL as part

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5 Ibid.; D. Terrett, *The United States Army in World War II: The Signal Corps,* Office of the Chief of Military History, USDA, Washington, D.C., 1956.

6 V.Y. Jrenov and B.W. Tyapkin, *Infrared Technology in Military Science,* Voyeinizdat: Moscow, 1963; T. Buralewski and A. Sala, *Night Vision,* Ministry of Defense: Warsaw, 1965; V.Y. Kichka, *Infrared Page in Warfare,* Voyeinizdat: Moscow, 1958.

7 L.W. Nichols et al., "Military Applications of Internal Techniques," *Proceedings Institute of Radio Engineers,* Vol. 47, No. 3, 1959, pp. 1611–1648.

8 U.S. DoD, *Summary Technical Report of Division 16, NDRC: Image Forming Infrared,* Office of Scientific R&D, Washington, D.C., 1946; Nichols et al., *Military Applications;* Biberman, *Electro-Optical Imaging*.

of the THERMOGRAPH program. NVL contracted for R&D with RCA—a continuation of RCA efforts conducted during World War II.[9] (Biberman, 2002).

Later in the decade, NVL developed a management tool called "visionics," which linked the physical properties of night-vision devices to their effectiveness in the field. NVL used the results of visionics analyses to adjust and expand its R&D activities in passive night-vision technology. A series of other R&D projects were conducted or directed by NVL to investigate technologies of passive image intensification and to improve the performance of multistage cascade tubes. These included magnetic focusing, use of thin films, fiber-optic interfaces, and other technology advancements. In particular, the use of fiber-optic interface in the night-vision systems applied in the First-Generation Fiber-Optics Cascade Intensifier was an important initial night-vision technology advancement directed by NVL.

U.S. defense concerns became acute with the onset of the Cold War in the late 1950s. The increasing Cold War tensions with the Soviet Union forced the United States to consider possible "limited war" with perceived Soviet "client states." Under President Kennedy's overall mandate to improve the nation's military capabilities, the U.S. Army undertook a study for the conduct of limited war. Dr. Luis Alvarez, a distinguished scientist from University of California, chaired this study. Alvarez's committee cited night-vision capability as critical component for limited war.

Because of this study, NVL's budget was almost quadrupled. The value of this investment was quickly made apparent in Vietnam. The utility of the first-generation Image Intensifiers in the field, as reported by the U.S. military personnel, clearly established the exceptional value of the night-vision capability to the U.S. military. U.S. military operations in Vietnam and the successful use of the NVL-developed night-vision devices, which "took the night away from Charlie," established the basis for further night-vision device R&D, design, fabrication, and deployment. In the subsequent years, the important utility of the night-vision capability in the U.S. armed forces was established in the Panama military operations, the Persian Gulf, the Balkans, and Afghanistan. The considerable advantages of the night-vision uses in military conflicts were readily recognized by other nations and were purchased by numerous foreign nations, including Germany, France, the United Kingdom, Syria, Japan, and Egypt.

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9 Biberman, *Electro-Optical Imaging*.

The NVL activities were conducted both in-house and under contract with commercial establishments. Among such activities in the 1960s, NVL directed a real-time IR imaging research; with Hughes, jointly developed integrated thermal night sight for the tube-launched optically tracked wire-guided (TOW) missile; conducted critical appraisal and review that compared the use of a laser image intensifier with that of thermal devices; and undertook direction of research activities that combined the parallel-scan and long-wavelength detectors conducted with Texas Instruments (TI).

Concurrently, with the increasing acceptance and recognition of night-vision's utility in the military operations, NVL received recognition and funding for R&D, design, product engineering, and testing night-vision devices. In 1973, George Heilmeier, Assistant Director (Electronics and Physical Science) of the Office of the Director of Defense Research and Engineering (ODDR&E), prepared a memorandum of the status and concerns of night-vision system programs "the most rapidly expanding areas of electronic technology…" and citing NVL's superior performance. For example, discussing the need to consult expert opinion in the design of FLIR and the unfortunate practice by various PMs to delegate this requirement and the design review process to various prime weapons systems contractors, Heilmeier stated:

> An exception is the review procedure delegated to the Army Night Vision Laboratory. However, the program managers are circumventing the procedure (as in the case of SCOUT, NICV, and AAH) which leaves the laboratory minimum authority. Throughout DoD, since they have little real authority, the laboratories assume little responsibility in selecting the best course.[10]

Heilmeier continues and lists the common problems in the management of the night-vision device design by various PMs who do not have the NVL's expertise.

- Program Office independence often leads to unnecessary waste and duplication;
- Requirements are often unrealistic, and specifications often do not reflect system objectives;
- The impact of small redesigns is often ignored or underestimated;
- FLIR programs to date have been job shop operations; and
- Paperwork costs are excessive.

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[10] Heilmeier, Memorandum to the Director, Defense Research and Engineering, 1973.

In the 1970s, 1980s, and early 1990s, the NVL continued to direct the advancement of the night-vision devices. In the 1980s, these included development of the Thermal Weapon Sight, which was compatible with limited size and cooling requirements, and the high-performance scanning HdCdTe FPA program development of large scanning hybrid arrays.

The NVL's very important contribution to the advancement of the night-vision technology and devices in 1980s and 1990s was the initiation and direction of standardization of night-vision system components and the introduction of the modular concept in night-vision devices. As reported by Biberman:

> The Army, NVL, once again seeing a meaningful and maturing technology, stepped up to controlling the industry by standardizing the system components. This time the program was designed around upgrading the existing common module systems with an improved system that could be used across the battlefield. That is, rather than upgrading a program at a time, the technology would be upgraded across the programs. One of the advantages would be that various systems in the battlefield would all have similar performance. The Program became known as Horizontal Technology Integration (HTI). The key component for this development became a Standard Army Dewar Assembly (SADA). A family of FPAs and Dewars were defined, but the principal assembly was designed around a 480 by 4 scanning array for generating noninterlaced, TV-compatible imagery. The development has continued with all the new army armor systems using this technology.[11]

NVL either conducted in-house, cooperated, or directed most of these activities. Other U.S. DoD entities engaged in the development in night-vision devices were the

- U.S. Air Force Avionics Laboratory at WPAFB,
- Naval Weapons Center (now the Naval Air Warfare Center Weapons Division) at China Lake,
- Naval Research Laboratory (NRL), and
- Naval Air Development Center (NADC).

DARPA and IDA made significant contributions to the NVL activities. Among more important commercial contractors that performed R&D services and facilitate night-

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[11] Biberman, *Electro-Optical Imaging*.

vision devices were Martin Marietta, Rockwell International, Hughes, Texas Instruments, Honeywell, and others.[12]

The increased maturity of the night-vision devices and the utility of such devices in military operations became understood by foreign nations. For example, the vision technology advancement and device development have been conducted by France (SOFRADIR produced complete in-Dewar-detector assemblies), Germany, the Netherlands, Russia, the United Kingdom, and Israel. Table XIII-2 presents the principal foreign countries engaged in night-vision technology development.

Table XIII-2. Principal Foreign Entities Engaged in Defense Night-Vision System Developments

France	*Russia*
SOFRADIR	Federal Scientific-Production Center
DGA/SPART	
Eurocenter (Tiger helicopter)	
Germany	*The United Kingdom*
Eurocoptor Deutschland GmbH	Thales Optrenics, Ltd.
Zeiss Optranik GmbH	BAE Systems, Ltd.
Delft Electronic Products BV	Oxley Developments Company
The Netherlands	*Israel*
Delft Electronic Products BV	Elbit Systems, Ltd

Several critical technical appraisals of the NVL activities case studies and U.S. DoD operations manuals of the night-vision device operations in the field testify to the achievements of the night-vision technology advancements by the NVL since 1954.[13] The following sections highlight three particular aspects of NVL's S&T management success:

- Establishment of reputation for technical skill and effectiveness in the field through close S&T cooperation with Army staff, system developers, and field commands;
- Development and use of technology-management practices—some of them pioneering—well suited to addressing both long-term innovation and immediate system needs, with an emphasis on leveraging S&T efforts by industry; and

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12 Ibid.

13 Ibid.; P.J. Klass, "Exclusive Report on Infrared," *Aviation Week,* 4 March 1957, pp. 13–24; E.L. Dereniak and G.G. Boreman, *Infrared Detectors and Systems,* John Wiley and Sons: New York, 1996; D.P. Bolger, *The Battle for Hunger Hill,* Novato, Calif.: Presidio Press, 1997.

- Building and maintaining internal technical capabilities and application expertise.

B. ESTABLISHMENT OF REPUTATION FOR TECHNICAL SKILL AND EFFECTIVENESS IN THE FIELD

Internal politics is part and parcel of government organizations. However, rather than become bogged down with bureaucratic priorities, NVL managed them astutely. Robert Wiseman—who either directed or oversaw NVL from 1954–1968 and served in Army headquarters until 1981—took pains to build and maintain relationships with NVL's overseers in the Army staff and with the broader technology community. In addition to keeping his direct line of management apprised of and involved with NVL's activities, Wiseman built support among civilian authorities in OSD, military warfighter leadership, and Congress through regular demonstrations of night-vision capabilities. He encouraged NVL personnel to be active participants and leaders in pertinent meetings and conferences that were attended by all of the Services. These activities helped build NVL's technical reputation as well as making external part of the night-vision "team."

Wiseman also worked closely with the Army staff to provide superior equipment to troops in the field. To some extent, because of the lack of well-developed night-vision capabilities or the lack of interest in industry, NVL took under their purview the whole of the systems-development process, from underlying scientific principles to real problems faced by soldiers in combat. Large numbers of NVL personnel spent long periods in the field, sometimes even accompanying deployed troops onto actual battlefields. This first-hand experience with real problems helped NVL personnel contribute to and buy into S&T priorities that might differ from their main interests. Wiseman established relationships with key program officers responsible for generating military requirements to determine realistic near- and far-term goals. These efforts helped establish NVL as an organization that could benefit program rather than hinder their progress. To reinforce this, Wiseman assigned personnel to each major system PM who had a night-vision requirement. These contacts helped NVL shape its S&T program to meet the needs and schedule of its "customers."

For the most part, these relationships avoided the type of complex and time-consuming communications effort in the chain of command of many of DoD's technology programs. These efforts also considerably enlarged the defense "market" for night-vision systems, expanding night-vision applications from individual soldiers to

increasingly expanding weapons systems such as the M60 ABRAMS Tank, M1 Main Battle Tank, APACHE helicopter, TOW missile, and others. This close involvement has built a relationship of respect and trust between the soldiers and the NVL personnel who work with them.

In the 1950s, NVL's attention to organizational issues helped them defend their "turf" against other Army organizations that believed they owned the night vision mission. Then, in 1961, a commission on "limited warfare" chaired by the Nobel Laureate Luis Alvarez highlighted the importance of being able to fight at night. When their draft report reached the desk of Colonel Shraeder, NVL overseer on the Army Staff, he alerted Alvarez that the Army had many ideas as to how to deal with the night-fighting problem. Alvarez visited NVL and pushed for greater support. Wiseman briefed LTG Trudeau and received more than a four-fold increase in budget.

With this increase in support, Director Robert Wiseman and his deputy John Johnson assembled a staff devoted to developing an understanding of (1) the basic physics and engineering issues of photo-emissive technology and image intensification and (2) the relationship between the physical parameters of imagery and the effectiveness of such imagery to a human user in detecting and identifying observed objects. This dual approach led to a synergism between the research, development, and engineering that lasted well past the NVL's early years. In particular, NVL systematically used well-planned field tests to gain data for their modeling activities. Their systematic correlation of field performance with laboratory performance grew into a sub-branch of night-vision technology now called visionics. Visionics was critical to develop relationships between hardware characteristics [resolution (line pairs/mm), contrast, brightness, and motion] and the ability of human visual performance (detection, classification, recognition, and identification).

NVL employed visionics comprehensively to the design, product engineering, testing, and long-term research portfolio decisions. Visionics became a powerful tool for PMs and contracting officers in evaluating contractor proposals, devising field tests, and planning operational engagements. "Visionics was the backbone that guided the selection and funding of research programs for maximum payoff, provided optimization of

equipment design, and established necessary testing techniques for both laboratory and field measurement."[14]

In particular, NVL and its overseers in the Army staff never lost sight of the fact that they had responsibility for not only advancing the state of the art in night vision, but also for addressing immediate applications and equipment needs. They understood and took seriously that total value for the warfighter was a combination of quality and quantity. This meant that is was important to balance technical capabilities with cost. For instance, for night-vision goggles with S25 cathodes (which were relatively low cost) vs. goggles with GaAs cathodes (which were much costlier but had much better and longer life) the choice went to the more expensive goggle that proved to offer a lower lifetime cost.

All was not smooth sailing, however. In 1963, DARPA had expressed an interest in the FLIR technology but concluded that there was no requirement for infrared sensors of the type. Then, in 1967 cancellation of the SEAMORE program (aimed at multisensor integration into single operation) was also a setback. The cancellation was caused by the shifted emphasis on the role of U.S. Army's helicopters from surveillance to active combat. This cancellation allegedly resulted in a delay of radar and FLIR integration for 25 years.[15]

By 1965, NVL consolidated its control of most aspects of night-vision technology work in the Army. In the years to come, they would expand their scope not only into new technologies relevant to the NV mission but also into new missions supported by their broadening technology expertise. However, as the laboratory matured, its oversight was transferred from ERDL at Fort Belvoir to CECOM. CECOM tightly controlled the number and level of technical staff, driving many key innovators out to industry. A few years later, the NVL's research mission was transferred to the Harry Diamond Laboratory. Similarly, production engineering organizations were separated from their underlying S&T, which reduced researchers' visibility into near-term problems and other important feedback. It also led to inefficient production decisions, such as product improvement programs on equipment that was about to be overtaken by new technolo-

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14 J.A. Walkenstein, "Color Night Vision, a Critical Informataive Multiplier," University of Miami: Miami, Fla., 2000.

15 D. Lacy, *Development of the Modular FLIR Business and Technology at Texas Instruments* (draft), Dallas, 1985.

gies. Within defense programs, the separation of 6.3 and 6.4 work from its underlying 6.1 and 6.2 base meant that PMs were subject to industry influences to move into development areas where there was insufficient technology base to support them.[16]

Taken together, these moves disrupted the close working relationships between the NVL basic research staff and night-vision systems developers. Researchers lost their connection to intimate knowledge of how their equipment was performing and how it was saving lives in the field and giving U.S. forces an advantage. Over time, this disconnection lead to a loss of the morale that had made NVL into what was probably one of the most effective and productive government laboratories in the United States.

C. DEVELOPMENT AND USE OF PIONEERING TECHNOLOGY MANAGEMENT PRACTICES

The success of NVL's assertive, user-oriented S&T approach and bureaucratic prominence were based, in part, on the development and use of pioneering technology management practices suited to its comprehensive vision for night-vision capabilities. In addition to the visionics work described earlier, NVL was a pioneer in the use of concurrent engineering, modular design techniques, and rapid prototyping. (The emphasis on prototyping was particularly important. NVL personnel learned a lot from prototypes taken into the field.) NVL routinely invested in multiple technical approaches (short and long term) for addressing particular night-vision functions, moving resources around as one approach or the other advanced.

These practices were consistent with NVL's user focus. NVL management recognized early that success in night-vision equipment would depend on the invention and integration of numerous rapidly changing technologies. By necessity at first (being a small lab with very limited funding), they adopted a cooperative approach in their relationships with contractors. They cooperated with and used night-vision technologies developed by other government laboratories. However, they also maintained these practices even after NVL had accumulated significant sway over all DoD night-vision technology because these practices were key to continuing the rapid development of night-vision technology. Such practices were also consistent with taking life-cycle cost and upgrade seriously.

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[16] These numbers refer to research categories: 6.1, basic research; 6.2, applied research; 6.3 demonstration and validation; and 6.4, engineering manufacturing development. Source: http://www.cnsr.org/dodsntfaq.php, 20 November 2003.

1. Integration of Near-, Mid-, and Long-Term S&T

NVL conducts R&D and subsequent activities for near-term, mid-term, and long-term objectives. In the 1960s and 1970s under the "three-generation plan," NVL simultaneously conducted technology advancement for the improvement of the fielded night-vision devices and for the next so-called two generations. Table XIII-3 identifies these NVL activities in the mid-1960s through the mid-1970s. Simultaneous R&D activities for several technology generations significantly benefited the advancement of night-vision devices because of feasibility of designing some common night-vision system component technologies to be used in several types of night-vision devices. For example, the development of a smaller second-generation intensifier tube allowed the development of small, lighter, and better head-mounted night-vision goggles.

Table XIII-3. NVL Coextensive Activities in 1960s and 1970s

First Generation Image Intensifier
Small Starlight Scope
Night Observation Device, Medium Range
Night Observation Device, Long Range
Hand-held Starlight Binoculars
Night-Vision Goggles

NVL was an early adopter and practitioner of concurrent engineering, which helped speed technology adoption. Before the practice was popular, they spent a lot of time making what today would be called "PERT" charts of projects arranged in order of importance and urgency. NVL also emphasized realistic testing:

> Another key activity was frequent night demonstrations and field tests in which all participated. This gave the researchers and developers practical experience on what it was like to operate in darkness and experience field problems from the soldiers' point of view. It motivated us all, and the vertical integration structure enabled the Branch to focus needed talent on critical problems.[17]

2. Development of Common Technical Standards

Commencing in the late 1970s and continuing in the next decade, NVL undertook activities to design and develop Thermal Imaging System (TIS) or the FLIR devices, a

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[17] R.S. Wiseman, *Conquest of Darkness by Management of Stars,* Expansion of Dowd Memorial Lecture, National IRIS Symposium, 18 June 1991.

significant advancement over the previous and, then, existing night vision devices. As summarized by Chapman,

> The Vietnam conflict surfaced an important fact; the ability to see at night as effectively as during the day in indispensable. As the Imaging Intensification technology was maturing and its application base spread to the combat vehicle and airborne communities, an emerging technology began to show promise of providing long-range detection and identification capabilities without the need for ambient illumination. Technological achievements in such diversified areas of infrared transmitting optics, solid state electronics, miniature cryogenic refrigerators, high density semiconductors, and quantum detectors were married to create an Infrared Imager, a system which displays a visible analog image of the infrared radiation emitted from a scene. One of the early systems to be built was integrated into an airframe and aimed in the direction of flight to allow pilotage at night. It was given a name; FLIR, Forward-Looking Infrared.
>
> Four years of development brought the technology to a reality for several Government customers. Aggressive competition was creating industrial areas of expertise of complete systems down to miniature solid state emitting arrays and was also creating products so expensive that customers could not purchase the quantity they desired.[18]

To overcome the cost issue, a technical baseline for FLIR was devised. This baseline consisted of 11 components that would meet the essential needs of most DoD's night-vision requirements. These were conceived as common modules for thermal, cryogenic, optical, and electronic components. The goal was to achieve economies of scale, establish production facilities, maintain configuration control over the proposed systems, and reduce the overall life-cycle cost. Table XIII-4 identifies the different "customers" for the common modules.

Table XIII-4. Principal U.S. Army Entities Engaged in Common Module Evaluation and Applications, in the 1980s

Army Tank-Automotive Command	Army Armament, Munitions, and Chemical Command	Fighting Vehicle System
Army Communications-Electronics Command	Army Project Managers	Advanced Scout Helicopter
Army Aviation Systems Command	TOW	COBRA
Army Missile Command	CHAPARRAL	Advanced Attack Helicopter
Army Armor Center	Tank Systems	

[18] S. Chapman, "Keeping Pilots in Cockpits," *Air Force Magazine,* July 1997, pp. 66–69.

The formal beginning of cooperative FLIR development among the Services was 19 June 1973, when a charter signed by the Joint Logistics Commanders (JLC) formally established a Joint Technical Coordinating Group (JTCG) for TISs. JTCG was to examine interservice coordination on TIS R&D, procurement, and logistics. As a result of the group's subsequent study, the JCL agreed upon a Tri-Service policy to use Common Modules for FLIR systems where possible, and assigned control of configuration management to the NVL, effective 15 July 1974. The Army Missile Command implemented the policy on 6 September 1974 and designated the Director, NVL, as the Army Missile Command Configuration Manager for TIS Common Modules. The JLC policy required mandatory coordination between the Services for the TIS development programs and continued to oversee the policy. The JTCG prepared a Joint Services Development Plan for TIS, which documented all known Service requirements, to include both ongoing and scheduled FLIR development programs.

However, NVL's authority was not complete, and it faced an uphill battle because modular design approaches are often not perceived as being in the interest of private firms or of individual programs. Private firms will generally prefer to maintain proprietary solutions that play to their technology strengths and serve to "lock in" customers. Programs are typically not interested in paying the initial performance and cost penalties associated with adopting an open system. For instance, for the Army's main battle tank (ABRAMS M1–A1) development program,

> the AMC leadership always sided with NVL, that is, until the new main battle tank. The XM-1 PM was chartered to procure under "Prime Contractor Responsible" policies, i.e., the contractor is fully responsible for all onboard sensors and items, such as the TIS, are the responsibilities of the contractor. This created a difficult configuration management problem for NVL and the Army. The M1 TIS resulted in loss of commonality of the Detector, LED, Visible Collimator, Bias Board and Postamic modules.[19]

Organizationally, NVL authority as configuration manager was also limited. The configuration control process was conducted by the Configuration Control Boards (CCBs), which conducted a review of proposed changes/advancements in the Common Module. The NVL was one of the principal members of CCB. However, as reported by

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[19] L.L. Berman, *Independent Assessment of Army Night Vision Common Modules,* U.S. Army Research Office, 31 May 1986.

Contros,[20] the results of the reviews were not always satisfactory because of poor coordination among the CCB members and NVL.

In 1977, efforts were made to establish formal Tri-Service procedures for configuration control of Common Module systems utilizing the JLC Standard Integrated Support Management System (SISMS). A Joint Operating Agreement (JOA) and Joint Operating Procedures (JOPs) were drafted and designated the Army as Chairman of a Tri-Service CCB and, within the Army, the Director, NVL, as the Thermal Imaging System Common Module (TISCM) PM. Each Service was to have its own internal CCB and establish an internal position before submittal/approval of any configuration change. PMs were to initiate the development of JOPs for configuration maanagement in cooperation with each participating Service and provide for adequate representation for the multiservice CCB.

However, this agreement and associated procedures were never formalized, mainly because of Air Force and Navy reluctance to provide funding to the Army to perform the configuration management function. The Army thus continued to operate as it had done since Army Missile Command's directive in 1974 (i.e., with the Director, NVL as Configuration Manager; the CCB being an Army, rather than Tri-Service, approval authority; and with the Army providing the Navy and Air Force technical data and guidance to those Services' PMs).

Dedicated NVL personnel were assigned to each major system PM and worked with them to facilitate adoption of Common Modules, often taking on responsibilities that were not typical for a government laboratory. By maintaining close contact with PMs and contractors and working with them to solve problems, NVL came to be perceived as supportive and responsive to program needs, while simultaneously being attentive to NVL's longer term interest in night-vision development. However, there were limits to what NVL could accomplish because the commercial prime contractor for the weapons system was effectively independent from NVL direction, despite AMC's (DARCOM) attempts to enforce NVL's authority:

> the interaction and coordination involved generates an approval/disapproval cycle of considerable length on the most simple and routine change documents. Secondly, delays are often experienced when CCDs

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20 W.S. Contros et al., *AMC Implementation Study of Common Module Program Management,* U.S. Army Research Office, 31 October 1986.

> are submitted with inadequate substantiating data (test results, design analysis, cost analysis, etc.). Often CCDs are generated when production problems are experienced and contractor delivery is affected. The PMs are pressured to resolve the issue so as not to affect fielding or delivery to major system production lines. This often leads to inadequate communication prior to submittal of the CCD to ensure proper substantiating data is identified, and subsequently inadequate time to accumulate and evaluate the data. Finally, the absence of an institutionalized P^3I program has contributed to non-compliance. Advances in technology by-passed the technical level of the Common Modules, and prompted system managers to seek deviations from the AMC Common Module policy.[21]

One of the problems challenging NVL was absence of separate funding for the Common Modules. Hence, NVL was confronted by the funding decisions of each Project Manager regarding the benefits of additional funding for the Common Module development. This led to insufficient funding by individual PMs for the Common Module Project Manger. The fact that most PMs for most weapons programs, such as the APACHE helicopter and the ABRAMS tank, were senior military officers as compared with the NVLs Project Manager for Common Module did not facilitate additional funding for NVL activities.[22] The manager had limited options in obtaining additional development funds.

The original implementing policy of July 1974 allowed for "continuing technological developments," but the Army's implementation activities of that policy did not set up a system to exploit it effectively. Thus, NVL had limited or no control. A structured technology insertion program was not pre-planned or administered. A configuration manager was appointed and procedures were developed for configuration control, but the organization and process to back him up were not institutionalized above the NVL level. The major system schedules, costs, roles, and missions controversies overrode the process.

Furthermore,

> Proliferation was advanced by contractors such as advancing system performance requirements and expansion in system applications, and by management decisions which made execution of configuration control practices difficult. These included having prime contractor responsibility,

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21 Ibid.

22 Ibid.

> such as in the M1 Tank, and in expanding the industrial base to achieve competition.[23]

Nonetheless, NVL direction of the Common Module development was judged to be successful:

> The concept of commonality is one in which the entrance optics and display methods are unique to each system, but the remaining components are standardized and used within all FLIRs. Applications range from small lightweight manportable sights to air-to-ground fire control subsystems. The Army's Night Vision and Electro-Optics Laboratory awarded a contract under which the design effort in developing module configurations considered system performance, packaging constraints, and maintenance requirements as the basis for success. These factors drove such things as the number of video channels per board, partitioning of the video function, variable scan rate and scan angle of the scanner, 90° "elbow" design in the optics, and the ultimate selection of 180 channels in the detector. At the conclusion of this interactive design process, a full hardware qualification program was conducted…. These tests included design verification tests of individual modules, environmental testing to evaluate operational suitability, and demonstration of module interface compatibility in various test bed systems. All design goals were met and specifications were updated to reflect actual design parameters.[24]

Table XIII-5 summarizes the modular FLIR development process. Contros summarizes the NVL performance as follows:

- Although the Army failed to execute its own policy on common modules—particularly because of inadequate P3I—FLIR capabilities have been widely exploited in the Army (demand for Common Modules have exceeded forecasts).
- Proliferation of systems and Army cost of ownership has been controlled, if not reduced.
- An industrial base has been established in both the United States and the North Atlantic Treaty Organization (NATO).
- Substantial benefits were achieved through the Common Module approach to Thermal Imaging technology advancements directed by NVL.[25]

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23 Ibid.

24 Chapman, "Keeping Pilots in Cockpits."

25 Contros et al., *AMC Implementation Study*.

Table XIII-5. Sequence of Modular FLIR Development, 1960–1980

Period	NVL Activities
1960–1970	Early FLIR development of state-of-the-art thermal imaging techniques by NVL.
1971–1972	NVL organizes ad hoc study group on commonality of thermal imaging systems.
1972	NVL conducts evaluation of FLIR system data.
1972–1973	NVL awards contract to Texas Instrument to build a Common Module system.
1973–1975	In parallel with the Common Module development program, NVL awards development contracts to Texas Instruments and Hughes Aircraft for TOW night sights. Texas Instruments uses Common Module technology, and Hughes Aircraft uses serial scan technology. Based on field performance tests, NVL selects Texas Instruments for follow-on development and production.
1974	NVL designates Tri-Service Executive Manager for Common Modules by Joint Logistics Command.
1974–1976	NVL conducts competitive development program for the M60A3 tank thermal sight. Texas Instruments and Hughes Aircraft competes their respective FLIRs. NVL determines Texas Instruments' Common Module FLIR is superior in field performance and selects Texas Instruments for follow-on production. U.S. DoD policy issues a directive to require all future FLIR programs to use Common Modules.
1975–1978	Because of Texas Instruments' decision to only sell completed FLIR systems and not sell modules to other companies for future competitive FLIR contracts, NVL establishes second sources for critical modules. NVL also initiates a major program to establish alternative sources in accordance with the Texas Instruments-generated data package. The success of these second source module programs ensures a competitive environment for future FLIR development and production programs.
1977–1979	NVL awards initial production facility contracts to establish Common Module production capability and a mobilization base at alternative sources for six contractors to achieve production rate of 200 module sets per month. This allows effective competition on future FLIR procurements and ensures adequate industrial production base for critical technology components.
1977–1980	NVL awards Hughes Aircraft a development contract to provide modular FLIRs for the XM–1 main battle tank and contracts to Martin Marietta and Northrop for the development to supply modular FLIR, Target Acquisition Designation Sight/Pilot Night Vision systems (TADS/PNVS) for the Army's Advanced Attack Helicopter (AAH).
1978–1980	NVL awards a second source contract to Kollsman to produce approximately 2,000 TOW man-portable FLIR systems using Common Modules from alternative qualified or facilitated sources.

3. Coordination of Contractor Efforts

As suggested by Table XIII-6, NVL's coordination with contractors was a key element in its management approach. This was not always an easy task. Maintaining a competitive environment and performing "due diligence" for the government without discouraging private innovation require careful handling of business arrangements and intellectual property. NVL pursued this task vigorously, often exchanging technical personnel with contractors, but they did so delicately to avoid charges of favoritism.

Table XIII-6. Commercial Contractors Engaged by NVL, 1954–1970

Aerojet	EMR Phot-E1	Itek	Perkins Elmer	Varian
Avion	EOS	ITT	Phillips	Varo
B&H	Erie Tech	Kollsman	Polan	Velonex
Bendix	Farrand	L-K Elect	Rauland	Venus Sci
CBS	Garret	Machlett	Raytheon	Victor
Chicago Aerial	GE	Magnavox	RCA	Westinghouse
Chrysler	Gilfillan	Metavac	Strong Elec	Wollensa
Corning	Hanovia	Mosaic Fabrications	Texas Instruments	
Crouse Hinds	Honeywell	Optics	Trin	
Dumont	HP	Tech	TRW	
Duro Test	Hughes			

In the mid 1950s, NVL was compelled to seek assistance from knowledgeable scientists and engineers in commercial firms. In NVL's early years, they visited laboratories around the world to become aware of existing efforts. NVL abilities to secure assistance from private firms were hampered by the perception that commercial markets for night-vision devices were limited. For instance, the Dutch at Delft were developing X-ray image intensifiers for use in the medical market as a diagnostic tool for radiologists, and RCA was motivated to develop their image orthicon camera tube for the emerging television market.

Furthermore, many private sector firms lacked the expertise and experience to engage in night-vision development activities effectively. No commercial firm had the expertise to fulfill all the R&D requirements associated with night vision:

> Contractors were made part of the team and understood their role in the success of the overall program. There was no company with total systems understanding or capability. RCA and ITT were the early image tube developers but they did not have interest in the high voltage power supply or optics or the system integration. The optical houses knew their optics, but they were not involved in making image tubes or power supplies. The NVL had the knowledge of the needs of the user and the capability to translate the field requirements into technical specification, which incorporated understanding the limits of technology, user capability and environment, and initial cost and logistical support problems. It required a Government-Contractor team to succeed. This required mutual under-

standing of common goals and trust. The NVL was a hands-on system integrator.[26]

NVL management practices with commercial contractors were designed to enlarge the industrial base for night-vision technology advancement and manufacture of devices. NVL elected to negotiate multiple contracts, where possible and feasible. For example, in the 1961–1965 period, NVL negotiated a total of five initial contracts for the development of an image intensifier tube and two contracts for advancement of fiber optics. Subsequent contracts were awarded to contractors that had the best results from the initial effort. NVL operations emphasized competitive awards for most of the support activities. Only because of the unwillingness by American Optical to conduct development of fiber-optic technologies, a sole source contract for the required effort was awarded to Mosaic Fabrication, Inc.

Despite this lack of interest, NVL identified contractors that had some knowledge, if not expertise, in technologies that could add to the in-house knowledge of night-vision technologies. NVL engaged RCA as one of its initial contractors in the 1950s. RCA Laboratories had some interest in the area because of its research and manufacturing activities in the area of sniper scopes and image converters during World War II and was interested—albeit to a limited extent—in continuing these activities. Much later, NVL was also able to establish contracting agreements with Texas Instruments, Hughes Aircraft, Martin Marietta, and Northrop. Other contractors also assisted NVL with their expertise in IR technology.

The improved capability of the night-vision technology, directed by the NVL in 1960s, resulted in the increasing acceptance and applications of various night-vision devices in the 1970s in weapons systems such as the TOW night sight, the gunner/commanders sight for the M60A3 tank, and the Tank Thermal Sight (TTS). "This major milestone was significant because the combined production quantities of TOW night sights and M603A3 sights would be on the order of 20,000 systems."[27] Through this increasing demand for the night-vision devices, NVL maintained two principal objectives in its procurement activities with the U.S. industrial entities: the assurance of competition among contractors and the expansion and maintenance of commercial industrial base for

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26 R.S. Wiseman, "Conquest of Darkness by Management of Stars," an expansion of the Dowd Memorial Lecture, National IRIS Symposium, 18 June 1991.

27 Biberman, *Electro-Optical Imaging*.

night-vision devices in terms of R&D activities, manufacturing processes, and capability to fabricate devices.

To accomplish these objectives, NVL conducted a competition among its contractors. For example, Texas Instruments, Honeywell Corporation, Hughes Aircraft Company, and other firms attracted to the increasing demand for night-vision devices competed for a contract to develop night-vision device modules. To increase the industrial base for night devices, NVL often established the second and third source for night-vision equipment. The production runs for these sources were limited, but these NVL activities significantly enlarged the U.S. industrial capability in night-vision-systems fabrication:

> As the night vision technology matured and funding for R&D and manufacturer rapidly increased, NVL engaged in efforts to enlarge the industrial base for night vision technology and manufacture. At the same time, NVL undertook measures to maintain competitive structure among the rapidly increasing number of commercial contractors willing to participate in the night vision technology advancement and device fabrication.[28]

The performance of night-vision devices (e.g., the FLIR) in 1979, the resulting funding for R&D, and the increasing procurement levels resulted in a significant increase in commercial contractor activities for the development and production of night-vision devices. From the 1980s onward, private industry recognized the benefits of membership to the nation's defense industrial base, and several major industrial firms established exclusive organizations to conduct night-vision-focused actions. Equally important, the advances that had been made in night-vision technologies clearly established the role of night vision in the nation's defense posture, and the relatively large procurements of night-vision systems stimulated further private sectors enterprises to become NVL contractors. Table XIII-7 presents the major activities of the contractors on behalf of NVL between 1954 and 2001.

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[28] Ibid.

Table XIII-7. Major Activities by NVL's Contractors, 1954–2001

Period	Contractor	Activities
1950s	RCA	Initial contractor support for NVL
1959 and early 1960s	Texas Instruments	THERMOGRAPH project
1960s	HRB	Advancement of IR technologies
Mid 1960s	Texas Instruments	Color CRT Sun structure
1965–1967	Aerojet General	Southeast Asia, Nite Ops Program
1954	Hughes	Real-time IR imaging device integrated TOW thermal night sight
Late 1960s	Hughes	B–52 aircraft program
Late 1960s	Honeywell	Parallel scan system
1970s	Hughes	Experimental imaging systems
1970s	Texas Instruments	Night-vision module development FLIR
Late 1980s–early 1990s	Hughes	High-performance staring FLIR program
Late 1980s–early 1990s	Honeywell	Night-vision devices based on various thermal detective principles

Before NVL's lead in the development and adoption of its common components, various independent PMs of the night-vision system diverted their own night-vision device design and fabrication, which resulted in "unnecessary waste and duplication."[29] For example, at least five night-vision devices were under development in the early 1970s (TOW, Dragon, Chaparell/Vulcan, HELLFIRE, M–60, and Universal Viewer), and all these types and others could be satisfied by common components. NVL activities to ensure competition among the "second source" suppliers of night-vision modules, with respect to Texas Instruments and other potential commercial FLIR suppliers, is an example of such activities by NVL. Texas Instruments, under a contract to NVL, designed a system configuration for night-vision devices and standardized the modules (rather than a single sensor unit), which allowed the development of critical modules "as discrete modular components." The design in the module configuration "considered systems performance and packaging requirements as a fundamental basic for success." At the conclusion of this interactive design process, Texas Instruments conducted a full hardware-qualification program.

> These tests included design verification tests of individual modules, environmental testing to evaluate operational suitability, and also demonstration of module interface compatibility in a test bed system. All design goals were met and specifications updated to reflect actual design parameters. At the conclusion of the design and qualification effort, the common module technical data package was submitted in draft form. This

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29 Heilmeier, Memorandum to Director.

technical baseline consisted of those documents, specifications and drawings.[30]

However, NVL argued against the sole-source procurements for advanced night-vision technology development and device manufacture to Texas Instruments, in the 1980s and 1990s. NVL did not want night-vision technology and device fabrication to become the exclusive domain of one company. NVL procedures to encourage competitive contracting in its procurement were also manifested by its avoidance of sole-source contracts and preference to compete various "second-source" contracts for night-vision parts, components, and systems. The outcome of the second-source program has been that companies with no previous FLIR technology are now able to compete in developmental and production contracts. Because the high-technology items are the Common Modules, which are now available from their industry sources, companies such as Martin Marietta, Kollsman, and Northrup have begun fabrication of intensifier systems, even though they had no related experience.

In summary, the history of NVL's management of its contractors demonstrates flexibility but, at the same time, a clear recognition of the principal management requirements within NVL contractor engagements:

- Various S&T issues required to advance night-vision technology are complex, and NVL required assistance from contractors.
- The complexity of the R&D, design, and fabrication issues required close coordination between NVL and contractors to identify the contractor actions.
- The complexity of the required assistance dictated selection of several (competing) contractors to render assistance to NVL.
- Comprehensive and rigorous review by NVL of the contractor actions was needed.

In all contracts with private contractors, procurements were conducted after determination of the best among several competing technologies. For example, NVL advanced both the cascade image-intensifier tube and thin-film applications (secondary emission, demagnification, time integration) at the same time to determine the optimum technology for further advancement. On the basis of progress reviews, contractors may have been terminated, actions by several contractors combined, and/or new contractors

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30 Lacy, *Development of Modular FLIR*.

selected. NVL broke most of its development acquisitions with private-sector firms into multiple solicitations to facilitate such flexibility.

NVL contracting procedures assigned U.S. Government ownership of the results developed by contractors. Where required, NVL allowed for rights the intellectual property to contractors. In these cases, NVL conducted additional or substitute efforts to ensure availability of all technical data in full competition to all contractors. To coordinate information sharing, NVL required periodic visits by the Contracting Officer's Technical Representative (COTR) to NVL to coordinate activities. The COTR was expected to coordinate formally and continuously with the NVL project team leader, project engineer, and other NVL personnel.

Consistent with the need to have lowest cost for greatest use, NVL's goal in its contract arrangements was to achieve competition, and contracts were awarded to develop such competition. This was most evident in the NVL development of the Far Infrared Common Module program, where NVL not only conceived the basic design, but also paid one contractor to develop the prototype and paid another contractor to prove out the design package developed by the first contractor.

Table XIII-8 summarizes specific NVL's contracting policies and practices.

Table XIII-8. NVL's Contracting Policies and Procedures

Selection of commercial enterprises for R&D activities on the basis of lowest bidder.
Changes and modification of contractors on the basis of performance and results.
Collaboration and cooperation between contractors with in-house R&D activities.
Procedures, which require the design and use of system of systems, designed and applied to the contractor base with the objective to focus various contractor activities (an early, if not initial, application of system of systems).
Practice by NVL to commence the contractor relationships by awarding relatively small-value contracts to establish the "credibility" of contractors.
Emphasis on policies, which result in competitive structure among the potential contractors.
Procedures, which allow appropriate interaction and data, exchange among contractors for (a) advancement of existing products, (b) second source procurement, and (c) achievement of modularity in night-vision apparatus.

D. BUILDING AND MAINTAINING INTERNAL TECHNICAL CAPABILITIES AND APPLICATION EXPERTISE

As described in the previous section, managing short-, medium-, and long-term S&T in a multidisciplinary environment requires technical expertise in a variety of areas.

The relative absence of scientific and technical information and data on night-vision technology in the 1950s and early 1960s required NVL to assign personnel across the entire spectrum of night-vision technology conversion into night-vision device application and systems. As stated by Robert Wiseman "the tube contractors did not know anything about the fiber optics requirement" and "in-house programs were developed to fill gaps in industry…."[31] This permitted NVL to manage contractors aggressively, since they could often complete a job in-house if a contractor failed. (This would not be a practical approach for systems much larger than night-vision equipment because these systems would require thousands of engineers.) Maintaining an in-house capability for independent test and evaluation was particularly important for NVL. NVL also maintained intimate knowledge of drawings and specifications, which served to build its expertise. Combined with the user value modeling tools described earlier, this approach kept NVL focused on the ultimate goals of the organization rather than the narrow interests of particular groups.

Making in-house expertise useful in a multidimensional environment requires communication, coordination, and bridging among different technical communities in order to maintain a cohesive working environment. NVL identified this requirement early on as a primary management challenge and invented several formal and informal mechanisms for effecting this coordination. NVL organized personnel into a matrix management structure and distributed its personnel to various tasks using multiplex assignment structure. Akin to today's "integrated product and process teams," NVL's "commodity management" approach gave whole product ownership to integrated teams representing product functions: S&T, production, maintenance, etc. This gave all participants a coherent view of the entire development and deployment process and helped teams "do it right the first time." Wiseman describes the principles of the Night Vision Team:

> The Branch learned to work as a team. The individual stars came together by recognizing and relying on each other's talents. They developed mutual respect and interdependence, which made for an unbeatable team. Each participated and contributed to the overall goal. This is now called "high performance work environment."
>
> There were a lot of informal strategy sessions. The needs were still considerably bigger than the new budget. Planning was done by those responsible for the work, and plans were drawn on the blackboard and flip

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31 Wiseman, *Conquest of Darkness*.

> charts. There were no formally documented plans, but all knew where we were going and how their efforts fit into the big picture. All were dedicated and true believers in our product being of key benefit to the soldier.
>
> The Night Vision team grew as a team. There was continuity in management philosophy. It was vertically integrated with everyone dedicated to the goal of providing the soldier with the equipment needed in time at a reasonable cost. There was synergism between the research, development, and engineering. [32]

The matrix personnel assignment project teams assigned a team leader to be in charge of a specific technology advancement project. The team leader for a project, in close cooperation with a PM, represented communications linkage with other entities for technology advancement, product R&D, product engineering, prototype development, and testing. NVL personnel practices also facilitated extensive technical personnel interchange with corresponding personnel in other entities engaged in night-vision system development. These include personnel from contractors and night-vision device project members, including project managers from night-vision system programs and projects in other U.S. Army and other Service entities. Such interchanges allowed the formation of inter-entity technical teams that determined and conducted appropriate design, technical, engineering activities within the NVL technology advancement program. Unlike many government labs, NVL also took advantage of the industry COTRs to establish relationships with contractors.

NVL found that this interdisciplinary, cross-function organization, which detailed equipment knowledge throughout the life cycle, served as a font of innovation for NVL by increasing cognizance of how various subproblems are interrelated and how they affect overall system performance for the user. In particular, NVL found that 6.1a and 6.1b work needed to be connected to 6.2 and higher work in order to remain relevant. NVL resisted various efforts to break apart the NVL practices for the sake of Army functional reorganization. Eventually, NVL lost the battle and was broken into two parts. Development stayed at NVL, while research activities went to the Army Research Laboratory.

NVL found that maintaining a whole systems perspective was motivational for its personnel because people could understand the relevance of their work to ultimate

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[32] Ibid.

organizational goals. These organizational goals were stated in terms of the user, as opposed to being abstract technical accomplishments. NVL management's earnest belief in these goals infused itself throughout the organization and was a source of inspiration for the people working there. This included paying attention to contracting officers and personnel offices to gain their support for NVL's needs. This helped them in future years when they were forced to fight various bureaucratic forces that sought to impose arbitrary restrictions on how personnel and tasks were managed. Thanks to management's consistent attention to broader organizational issues and relationships, they were often successful in mitigating the impact of such restrictions. They did not so much "break the rules" as much as they took advantage of existing authorities and top-level support to create an effective, mission-oriented organization. For instance, NVL's team structures were not defined by Civil Service rules and, hence, were beyond the purview of personnel offices. This conferred greater flexibility in reassigning personnel.

From 1971–1978, NVL took advantage of an OSD initiative called Project Reflex, which was originated by Deputy Secretary of Defense David Packard to give R&D organizations greater management flexibility. Freed from restrictions on personnel levels and salary or on the balance between in-house and contract research, NVL could optimize its staff for the tasks as hand. Greater numbers of lower level people could be hired to support the most productive higher level people. The number of expensive higher level people could be reduced without fear or losing the billets. Funds could be moved from salaries to equipment or from travel to overhead at will. This created incentives to keep track of previously "free" resources such as computer time, photocopying, and so forth, and demand for these overhead services fell.

NVL sought to maintain continuity in its management philosophy by developing and promoting from within. NVL personnel were brought into briefings with high-level overseers to inform them of organizational priorities and to provide training for the time when they might take on managerial responsibilities. Until 1996, top management at NVL had about 20 years of experience in NVL management philosophies and techniques, but NVL did not depend on individuals to have all the qualifications necessary. The emphasis was on complementary management teams that could cover the following management areas:

- Expertise in both R&D and military field experience;
- View of the big picture and the ability to deal with details;
- Technical knowledge and administrative/management skill; and

- Communication skills with the technical and nontechnical communities.

NVL also conducted an extensive technical recruitment program aimed at other U.S. DoD entities, academic and commercial entities. Early on, there was no pool of talent with night-vision experience; therefore, NVL selected personnel who had basic talents, showed evidence of hard work, and had the right personality for working on teams. They were more concerned with achievements than with patents or publications. In later years, established personnel were encouraged to recruit from their colleges, and formal joint technical programs were established to facilitate recruitment. NVL's recruitment was aided by being able to offer prospective personnel hands-on experience, opportunities for immediate major participation in acute ongoing projects, and opportunities to engage in relatively underdeveloped science domains.

GLOSSARY

2–D	two-dimensional
AAH	Advanced Attack Helicopter
ABM	antiballistic missile
ADAR	advanced design array radar
AEC	Atomic Energy Commission
AFS	Advanced Fiber Systems
ALCOR	ARPA-Lincoln coherent observable radar
ALTAIR	ARPA Long Range Tracking and Instrumentation Radar
AMC	Army Materiel Command
APSI	Aircraft Propulsion Subsystems Integration
ARDC	Air Research and Development Command
ARPA	Advanced Research Projects Agency
ATEGG	Advanced Turbine Engine Gas Generator
ATPP	Advanced Turbo-Propulsion Plan
BDU	battle dress uniform
BMD	Ballistic Missile Defense
BMDO	Ballistic Missile Defense Organization
BMEWS	Ballistic Missile Early Warning System
BPA	Bisphenol A
BTL	Bell Telephone Laboratories—Western Electric team
CCB	Configuration Control Board
CEC	Corporate Executive Council
CECOM	Communications and Electronics Command
CEO	chief executive officer
CEP	circular error probable

CFD	computational fluid dynamics
CIP	Component Improvement Program
CNO	Chief of Naval Operations
COTR	Contracting Officer's Technical Representative
CR&D	central R&D
CRD	Corporate Research and Development
CS	Chief Scientist
CT	computerized tomography
CTO	Chief Technology Officer
DARCOM	Department of the Army Material Development and Readiness Command
DARPA	Defense Advanced Research Projects Agency
DoD	Department of Defense
DOE	Department of Energy
ECOM	United States Army Electronics Command
EEE	Electrical, Electronic, and Electromechanical
EMDP	Engine Model Derivative Program
ERDA	Energy Research and Development Administration
ERDL	Engineering Research and Development Laboratory
ESAR	electronically steerable array radar
FBM	fleet ballistic missile
FFE	fuzzy front end
FLIR	forward-looking infrared
FPA	focal plane array
FY	fiscal year
GAO	General Accounting Office
GEMS	GE Medical Systems
GOCO	government-owned/contractor operated
GOTChA	Goals, Objectives, Technical Challenges, and Approaches
GPO	Government Printing Office

HEU	highly enriched uranium
HHS	Department of Health and Human Services
HIBEX	High Boost Experiment
HQ	headquarters
HTI	Horizontal Technology Integration
HTS	high-temperature superconducting
ICBM	intercontinental ballistic missile
IDA	Institute for Defense Analyses
IDWA	Interdivisional Work Authorization
IHPRPT	Integrated High Payoff Rocket Propulsion Technology
IHPTET	Integrated High Performance Turbine Engine Technology
INS	inertial navigation system
IR	infrared
IR&D	independent R&D
IRIS	Infrared Information Symposium
ITT	International Telephone and Telegraph Company
JANNAF	Joint Army-Navy-NASA-Air Force
JCS	Joint Chiefs of Staff
JFWTC	John F. Welch Technology Center
JLC	Joint Logistics Commander
JOA	Joint Operating Agreement
JOP	Joint Operating Procedure
JPL	Jet Propulsion Laboratory
JTCG	Joint Technical Coordinating Group
JTDE	Joint Technology Demonstrator Engine
KAPL	Knolls Atomic Power Laboratory
KREMS	Kiernan Reentry Measurements
LARS	laser angular rate sensor
LMFBR	liquid metal fast breeder
Loran	Long-Range Aid to Navigation

Loran	long-range aid to navigation
MDA	Missile Defense Agency
MIRV	multiple independently targeted reentry vehicle
MIT	Massachusetts Institute of Technology
mph	miles per hour
MR	magnetic resonance
MRI	magnetic resonance imaging
NAA	North American Aviation
NADC	Naval Air Development Center
NAS	National Academy of Sciences
NATO	North Atlantic Treaty Organization
NDRC	National Defense Research Committee
NIJ	National Institute of Justice
NOL	Naval Ordnance Laboratory
NPD	new product development
NR	Naval Reactors
NRL	Naval Research Laboratory
NSC	National Security Council
NVESD	Night Vision and Electronic Sensors Directorate
NVL	Night Vision Laboratory (U.S. Army)
ODDR&E	Office of the Director of Defense Research and Engineering
ONR	Office of Naval Research
OSD	Office of the Secretary of Defense
OSRD	Office of Scientific Research and Development
OTH	over the horizon
P&IP	packaging and industrial polymers
P3I	Preplanned Product Improvement
PAC	Program Approval Committee
PACE	Product and Cycle Time Excellence
PERT	program evaluation review technique

PET	positron emission tomography
PI	principal investigator
PM	Program Manager
PNVS	Pilot Night Vision System
PRESS	Pacific Range Electromagnetic Systems Studies
PWR	pressurized water reactor
R&D	research and development
RCA	Radio Corporation of America
RDT&E	research, development, test and engineering
RFP	Request for Proposal
RISC	reduced instruction set computing
ROI	return on investment
RW	Ramo-Wooldridge Company
S&T	science and technology
SADA	Standard Army Dewar Assembly
SAGE	Semi-Automated Ground Environment
SAM	surface-to-air missile
SBU	strategic business unit
SDI	Strategic Defense Initiative
SDIO	Strategic Defense Initiative Organization
SECDEF	Secretary of Defense
SECNAV	Secretary of the Navy
SEWS	satellite early warning system
SISMS	Standard Integrated Support Management System
SSN	an attack submarine (nuclear propulsion)
STAC	Strategic Technologies Advisory Committee
TADS	Target Acquisition Designation Sight
TCP	Technology Capabilities Panel
TDA	Technology Development Approach
TEP	Technical Effectiveness Process

TI	Texas Instruments
TIS	Thermal Imaging System
TISCM	Thermal Imaging System Common Module
TOW	tube-launched optically tracked wire-guided missile
TST	Technology Stage-Gate
TTS	tank thermal sight
UHF	ultrahigh frequency
VHF	very high frequency
VLSI	very large-scale integration
WDD	Western Development Division
WPAFB	Wright-Patterson Air Force Base

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14. ABSTRACT

The Missile Defense Agency tasked IDA to study and assess management methods and organizational structures that have proven successful in the development of technologies, with an emphasis on the roles of longer term science and technology work and radical innovation. This report provides detailed cases studies of large private companies that have been consistently technically innovative and defense programs that undertook large-scale systems developments.

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