

The Management of Research Institutions

N85-16665

(NASA-SP-481) THE MANAGEMENT OF RESEARCH
INSTITUTIONS: A LOOK AT GOVERNMENT
LABORATORIES (National Aeronautics and Space
Administration) 311 p HC A14/MF A01

Unclas
13535

CSCL 05A H1/81

a look at
government
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1983
10/10/83

ORIGINAL CONTENTS
COLOR ILLUSTRATIONS

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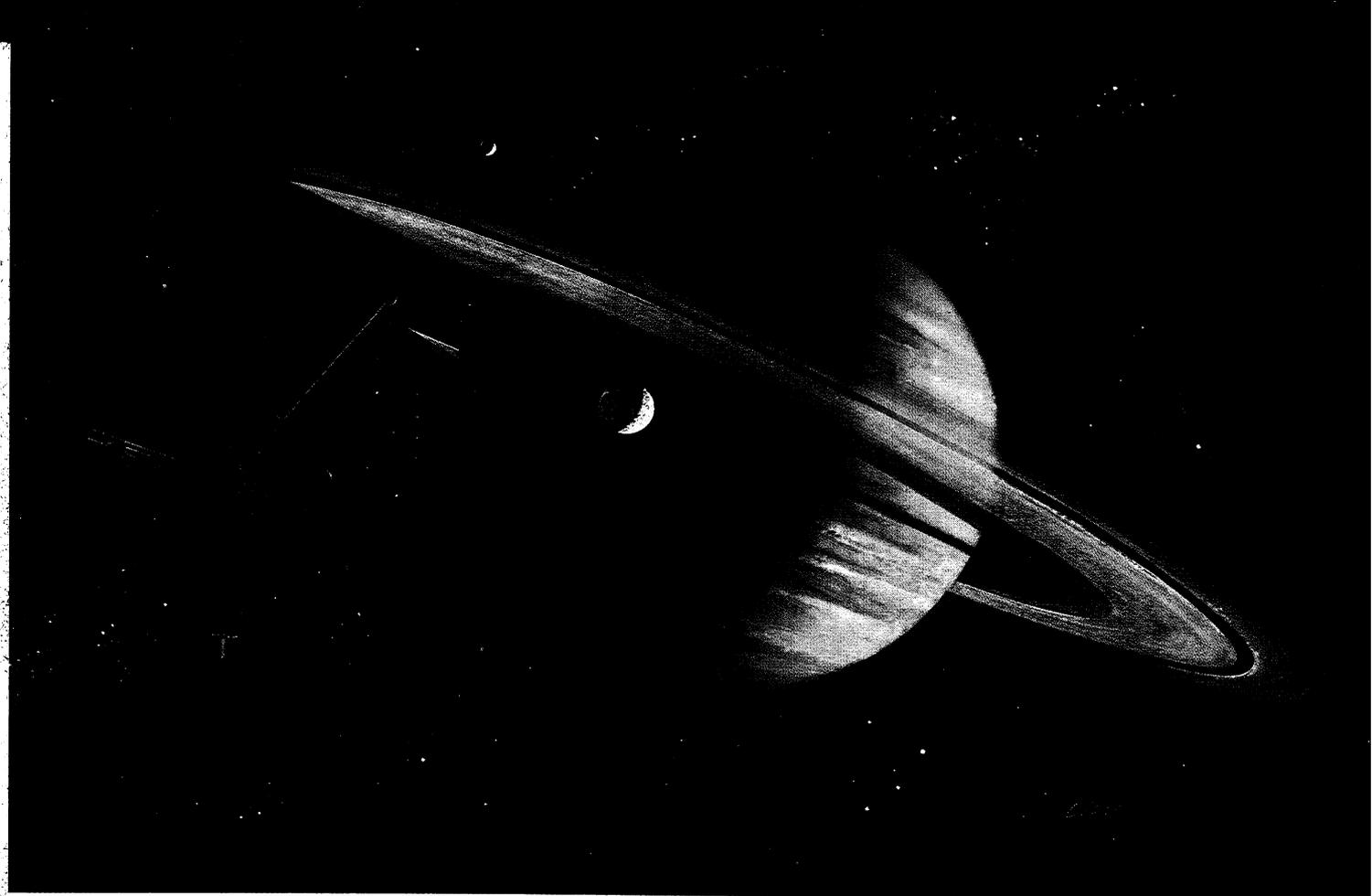
a look at
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laboratories

“Anxiety for the future time, disposeth men to enquire into the causes of things: because the knowledge of them, maketh men the better able to order the present to their best advantage.”

Thomas Hobbes

“What is now proved was once only imagin’d.”

William Blake



Frontispiece: Pioneer 10 deep space probe.

ORIGINAL PAGE
COLOR PHOTOGRAPH

Cover: Photograph of a memory chip.

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a look at
government
laboratories

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Library of Congress Cataloging in Publication Data

Mark, Hans Michael, 1929—

Management of research institutions.

(NASA SP ; 481)

Bibliography: p. 237.

Includes index.

1. Research institutes—United States—Management.
2. Technology—United States—Management. I. Levine, Arnold. II. Title. III. Series.

Q180.U5M325 1984

001.4'068

84-14869

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INTRODUCTION

Few persons would dispute the assertion that advances in technology have profoundly affected recent history. Yet the process by which technology is developed, refined, and brought to production is both complex and subtle, and it evades easy generalizations. But the very difficulty in grasping the nature of technology development should serve more as a spur than as a barrier to understanding. For one thing, the subject has a strong intellectual fascination. More important, technology affects each of us, whether we assist in developing it, consume it, invest in it, or pay taxes to finance it. This book looks at some of the institutions in which technology development occurs. For reasons explained below, we have selected for close examination primarily those laboratories operated by or managed for the Federal Government. And we want to answer one question: What do these institutions do and how well do they do it?

This might seem a fairly easy question to answer. The surprising thing is how few attempts have been made to answer it and how few of those have transcended the obvious. There are excellent studies of, for example, the functioning of large organizations, the formulation of science policy by the Federal Government and some large private enterprises, the genesis of scientific concepts, and the sociology of scientific disciplines. What we lack are accounts of the working of installations on the order of the National Bureau of Standards, the Naval Research Laboratory, or Oak Ridge National Laboratory, where much of the most advanced technology has been developed. No doubt, we could account for this lack in several ways: Scientists and engineers may not have found the right words to explain what they do; the compartmentalization of research and development makes it difficult for anyone to see the institution whole; and, in some of the larger laboratories, the best work often occurs entirely outside formal organizational channels. Whatever the reasons, we do not have a succinct account of how large technology development laboratories operate. This book is intended to provide such an account.

But that account is possible only within self-imposed limitations. Except in passing, we will have little to say about technology development in agriculture, medicine, geology, or the social sciences—say, the development of computers specifically designed to manipulate

large data bases. Instead, our focus will be on the systematic use of scientific knowledge to produce large, complex hardware systems: spacecraft, advanced weapons, nuclear reactors, aircraft, and electronic systems such as radar. This is the most visible and certainly the most expensive kind of technology development sponsored by the Federal Government and the very large private corporations, which is one reason for examining it. A second reason is that these institutions manifest the issues of "Big Science" in their most acute form: How do we translate basic scientific concepts into operating systems? How do we break down the compartmentalization between scientists and engineers? How do we permit discretionary research within the limits of a rulebound community? How do we redirect institutions as their larger programs are completed? And how do we maintain a certain necessary distance between the laboratory and its sponsoring agency or corporation?

A third reason for studying the large technology development laboratories is that the programs that serve to justify their existence are massive social facts. In one way, this only states the obvious, since laboratories on the order of Los Alamos National Laboratory or the Marshall Space Flight Center are vital to the economies of the regions in which they are located. We mean rather more. Quite simply, the aircraft and integrated-circuit electronics industries would have developed quite differently without the stimulus provided by federally-sponsored defense and space programs or the protocols and measurement tools developed at the National Bureau of Standards. The work of large Federal technology development laboratories and the large privately-sponsored ones served to set the direction that certain major industries — much of what is now fashionably called "high technology" — have taken.

In sum, the large technology development laboratory has been an important (though not easily quantifiable) element in American economic growth. Whether such laboratories can be directed by some central agency or the White House toward stimulating economic growth is still an open question. The notions that the laboratories can produce innovation on demand as part of some ill-defined "industrial policy" or that they constitute a republic of science whose members are accountable only to each other are also very questionable. The role of the large technology development laboratory is limited because it is important, important because it is limited. There is no sense in squandering national resources, and the roles of government and most commercial laboratories — or rather, laboratories whose missions are to produce commercial products — are not interchangeable. In the course of this book, we shall argue that most government laboratories exist to do work which commercial firms

have no incentive to do; to provide a portfolio of technical concepts, some of which may be taken up by the parent organization once current programs end; above all, to *define* those technology programs whose actual development will be mostly in the hands of industrial contractors. If our book is biased in favor of any thesis, it is that the greatest strength of the technology development laboratory is in basic and applied research and not (with rare exceptions) in product development.

This book, then, is intended as an introduction — for scientists, research administrators, students in technical areas, and the general public — to a subject that has not received the treatment it obviously deserves. Specifically, it is designed to accomplish three things deriving from our original question: to describe how technology development laboratories really operate; to identify conditions that militate in favor of or against the performance of a laboratory's mission; and to draw certain conclusions as to how such laboratories *should* be managed. Of course, the conclusions should follow logically from the analyses that preceded them. If our analyses are correct, we shall find that successful diversification is most likely to occur in areas closely related to the laboratory's core mission, or that the existence of a technical capability in a laboratory sometimes triggers a national or a corporate policy based on that capability.

Our work melds (or tries to meld) two viewpoints, two quite different kinds of experience. One of us is a physicist by training, a research administrator by profession, and a student of the history of science by avocation. The other is a social scientist specializing in the study of large technology-based organizations. We hope that our collaboration, based as it is on differing experience and perspectives, has been fruitful, though it has not always been easy.

The Management of Research Institutions originated as a course of lectures first delivered by one of the authors (Mark) as a Consulting Professor at Stanford University during the 1974-1975 semester. These lectures are the nucleus of this book. But in the process of trying to get our thoughts down, the book outgrew its original framework; we dropped several lectures, expanded others, and added much completely new material — some two-thirds of the text. We have tried, however, to retain the immediacy and spontaneity that mark a good lecture.

While we take full responsibility for everything in the text, we feel that, insofar as we accomplished what we set out to do, much credit is due to those persons who encouraged us, criticized our drafts, and eased the pains of bringing a book to press. John V. Foster, former director of Development at the NASA Ames Research Center, and Dr. Chester M. Van Atta, former associate director of

the Lawrence Livermore National Laboratory, participated in developing the material for the original lecture series. Jeff Struthers, of the Office of Management and Budget, Dr. Sylvia Fries, director of the NASA History Office, and Dr. Malcolm Currie, executive vice president of the Hughes Aircraft Company, read through our drafts and offered penetrating criticism. Many institutions patiently answered our requests for photographs and technical data, and we are pleased to acknowledge their assistance at appropriate points in the text. At NASA, Dorothy Kokoski cheerfully supervised our drafts as they went through typing, and Eleanor Burdette of the NASA Technical Library supplied research materials as and when we needed them.

Finally, we acknowledge the help of the late Frank "Red" Rowsome in bringing two very different authors together and making this work possible. This book is dedicated to his memory.

Hans Mark
Arnold Levine
Washington, D.C.
August 1984

CHAPTER I

What is Technology Development?

The Nature of Technology Development

There are certain assumptions in any era that are both widely held and at least partially true. One of these is the belief in a correlation between investment in scientific research and national productivity. Science, so the argument would run, generates technologies which alter and enrich the fabric of our lives. What was once slow and largely unconscious has now become a managed process. As Alfred North Whitehead wrote sixty years ago regarding the nineteenth century, "A new method entered into life. In order to understand our epoch, we can neglect all the details of change, such as railways, telegraphs, radios, spinning machines, synthetic dyes [or: transistors, communications satellites, computers, nylon, radar, microelectronics]. We must concentrate on the method itself; that is the real novelty . . ." (ref. 1.) What has happened since Whitehead wrote these words confirms their truth. Technology, conceived as a technique for mobilizing human energies and for making the most effective use of technical talent, is the dominant force in driving the economies of modern industrial societies.

But the success of modern product- and mission-oriented technological research is, paradoxically, an obstacle to understanding what has made its accomplishments possible. The inevitability with which research concepts appear to lead to operating systems is spurious. On the one hand, there are many urgent social problems for which the requisite research and development support does not exist; on the other, as Nathan Rosenberg observes, the rate of diffusion of new technologies "is intimately linked to the speed with which they come to offer distinct economic advantages over old technologies, which may continue to be improved, or to offer economic advantages for specific uses." (ref. 2.) The successful development of new technology is almost always difficult and uncertain, depending both on the customer's needs and on the speedy transfer of knowledge between disciplines. How the process of developing new technology occurs in one set of institutions — the mission-oriented Federal laboratory — is the subject of this book.

For our purposes, technology development can be defined thus: It is *the systematic use of the knowledge and understanding gained from*

scientific research directed toward the production of useful materials, devices, or methods, including design and construction of prototypes and demonstration of processes. (ref. 3.) This definition is broad enough to cover many of the categories used by other writers — and we mean it to be broad. Starting from a broad definition, we can avoid those subtle arguments which sprout like toadstools in the literature on science policy. For example: What are the differences among strategic research, product research, process research, and operations research? At what precise point does applied research become development? There would be nothing wrong with precise definitions, if they did not often lead to unproductive arguments about what an institution actually does — hardening of the categories, as it were. There have even been cases where a Federal laboratory would have reported no basic or applied research if it adhered strictly to definitions laid down by the National Science Foundation.* It is better to start with something comprehensive, refining our categories as we proceed.

Compared to the events that Whitehead discussed, the pace of technology development has accelerated immensely. (This is true both in absolute and relative terms, as any reader who owns a pocket calculator or personal computer or who plays video games can attest. Five years ago these items were either unavailable, or available only at prices beyond the reach of ordinary consumers.) In the case of major space and weapons systems, there must be simultaneous advances along a broad front: in electronics, materials, guidance and sensor systems, data processing, and the like. Of necessity, such research draws on many disciplines, since the problems to be solved are extremely complex; in some cases, new specialties combining several disciplines, like astrophysics and biochemistry, are created.

In this setting, the role of basic research becomes problematic. In

* There is something both amusing and depressing about arguments over what a research organization is doing. A study by the Congressional Research Service of the National Bureau of Standards (NBS) notes that different observers saw the Bureau doing different things. In 1970 and 1971, the Bureau responded to a survey by indicating that it was spending between \$13 and \$15 million annually on basic research. But at a 1971 congressional oversight hearing, the NBS Director “testified that only \$3 million was being dedicated to basic research. Later he explained the apparent discrepancy by suggesting that the first figures reflected the judgments of individual scientists about their work rather than the, presumably, more accurate estimates of top management. As he subsequently told the Visiting Committee, ‘if that’s how the NBS staff view their work from a motivational view, that is fine.’” Congressional Research Service, *The National Bureau of Standards: A Review of Its Organization and Operations, 1971-1980*. A study prepared for the Subcommittee on Science, Research, and Technology, U.S. House of Representatives, Committee on Science and Technology, 97th Congress, 1st Session (May 1981), p. 110.

basic scientific research the purpose is to find out why things happen as they do in nature. It depends on experiment and theory to devise a structure for some finite element of the natural world. The emphasis is always on the word "why." But for all the lip service paid to basic research, the proper relation between it and technology development is not susceptible to a once-for-all solution. The objectives of basic research are not always easy to define, and frequently the only quality control — whether a piece of basic research is good or not — is provided by the researcher's colleagues and collaborators. Moreover, research per se is seldom the objective of a product-oriented institution, or even of most publicly or privately sponsored research installations. The purpose of a space probe may be to expand our knowledge of the universe, but the goal of the installation managing its design is to produce an operating system within the time and budget allotted. Often, advances in basic research may be more a permissive than an active element in determining what kinds of technology development will be on the agenda. Indeed, the relation can and does run the other way: A technological breakthrough can be a stimulus to basic as well as product-oriented research, as producers and users try to understand, and thereby improve, the original innovation. A breakthrough, as Rosenberg has said, may signal "the *beginning* of a series of new developments of great importance, not their culmination . . . the development of the transistor or the explosion of the first nuclear device or the first achievement of heavier-than-air flight is really the announcement of a new set of possibilities far more than their attainment." (ref. 4.) There is nothing predetermined in deciding what kinds of development will be undertaken, especially where improvements in existing systems are to be more than incremental.

It can be argued that one of the most important changes in the way that technology is developed is that, over the past twenty years, we have come to understand the process so much better. If they are so inclined, research administrators are in a position to know that the lines between research and technology development run both ways; that the introduction of new technologies often marks only the beginning of a process of discovery; that the needs of government agencies have stimulated civilian industries, notably in electronics; and above all, that the decision to sponsor a major program of technology development always represents a political choice. Up to the eve of the Second World War, the rate of scientific advance was such that scientists, research managers, and government officials often perceived technology developments as flowing directly from scientific discoveries. Each new discovery was, in due course, developed into new technology and then into engineering projects; examples that come to mind include Roentgen's discovery of X-rays, James Clerk Maxwell's theory of electricity and magnetism, and John Dalton's atomic theory.

This is no longer true. Since technology development is generally very expensive compared to basic research, choices must be made. We simply do not have enough money to support all the possible developments that could be based on current knowledge. For example: Should we or should we not develop the technology to extract thermal energy from the oceans? Should we or should we not develop the technology of moving earth with nuclear explosives? Should we or should we not develop hypersonic passenger aircraft? All of these things probably could be done if the decisions to undertake the necessary technology developments were made, since the knowledge on which these developments would be based already exists. Yet for reasons of public policy, none of these programs has been undertaken. Our mechanisms for making choices regarding the initiation of new technology developments are still rudimentary. We have, as subsequent chapters will show, an established pattern, but it is not at all clear that this pattern is properly geared to the needs of our society.

Thus technology development mediates between basic research and engineering — that is, the application of the mathematical and natural sciences to develop ways to utilize the forces of nature for human benefit. Where a particular field of study attracts a sufficiently large group of workers under an established name, they tend to form professional societies, start their own specialized publications, and organize departments within the university, leading to recognition by the scholarly community. Sometimes the origin of such a field lies in the recognition of a need (the splitting of engineering into electrical engineering, civil engineering, etc., and more recently, into systems engineering and biomedical engineering), and sometimes in combining two previously separate disciplines. In contrast to basic research, technology development *tends* to be a group activity rather than an individual enterprise.* In contrast to engineering, the time scale is longer and the costs — anywhere from ten million to billions of dollars — greater than for all but the largest engineering projects. The reasons for undertaking technology development have included, for example, a perceived crisis such as that leading to the development of radar and nuclear weapons in the Second World War, or some large perceived profit if the technology succeeds, as in the case of Polaroid-Land cameras and very large scale integrated circuits.

* The difference is much more one of degree than of kind. Basic research in such disciplines as high-energy physics and astrophysics is a group activity demanding access to sophisticated facilities and a large supporting staff. By comparison, companies such as Hewlett-Packard and 3M have deliberately kept their engineering and product development teams small. Yet there is still a difference in scale between a weapons development program and an experiment to detect the presence of neutrinos.

To summarize the argument to this point: Technology development is the process by which newly developed scientific principles are brought to the point where they can be applied in an engineering sense. Typically, the time between the beginning of a project and its completion (lead time) is on the order of five to ten years; a really large program, like the lunar landing mission, may cost billions of dollars. It has become increasingly clear since the 1960s that such programs — some of whose features will be discussed in the next section — cannot be managed successfully in terms of a classical hierarchical structure. What we are dealing with here is the “large-scale endeavor,” a concept applied to his agency by the former Administrator of the National Aeronautics and Space Administration (NASA), James Webb. The endeavor characteristically results from a new and urgent need or a new opportunity created by social, political, technological, or military changes in the environment. Most often, it requires “doing something for the first time and has a high degree of uncertainty as to precise results” and it will have second- and third-order consequences, often unintended, beyond the main objective (ref. 5). A large-scale endeavor is so complex that senior executives in the sponsoring organization cannot be expert in all facets of the operation. “They must delegate important responsibilities to lower echelons and then find ways to make sure the delegations accomplish their purposes without harmful compartmentation.” (ref. 6.) The organization must be adaptive; “no longer can you have a grand idea and then go to work and cut and fit and try.” (ref. 7.)

Webb’s description can, of course, apply to many endeavors besides the space program. Recent examples include the attempt to build and operate a national rail passenger network, to develop a strategic petroleum reserve, to build the Alaska pipeline, and many programs related to the national defense — all share many of the features Webb enumerates. But space and comparable programs have had certain advantages, stemming from the nature of their missions, in attaining their goals which most of the endeavors named above lacked.

Consider the American civilian space program of the 1960s. Goals could be stated in precise, operational terms. NASA would describe a goal within the broader mission: Put a communications satellite in synchronous Earth orbit; or, develop an unmanned spacecraft to soft-land on the Moon and a vehicle with a liquid-hydrogen upper stage to launch it. Such precision may be contrasted with those Federal agencies charged with improving the quality of education, fighting alcoholism and drug abuse, or finding permanent jobs for the hard-core unemployed. As Lindblom and Cohen have noted, “government agencies are again and again assigned . . . responsibilities beyond any person’s or organization’s known competence. They do not typically resist these assignments because they are funded and maintained for their efforts, not for their

results.” (ref. 8.) However this contrast developed, technology development managers have the tools and resources to deal with many technical unknowns and overcome enormous problems of time and budget. This book is about the logic of this process as it applies to Federally sponsored institutions.

The Federal Technology Development Laboratory

So far we have discussed technology development as a general category, without much regard to the sponsoring organization. At this point, it would be well to define those features which distinguish Federal from privately-sponsored research and technology development. However one defines technology development, the Federal Government is doing a lot of it. In 1981, Federal spending on research and technology development amounted to roughly \$40 billion, compared to \$34 billion in the private sector (ref. 9).^{*} While some two-thirds of Federal research and technology development obligations go to industrial firms or for basic research carried on by universities, the remainder is done under direct Federal supervision, whether through field installations run by government employees, non-profit contract research centers, or government-owned, contractor-operated facilities.^{**} The impact of these programs alone would be sufficient reason to discuss them; a really large development program like Apollo at its height employed over 400 000 persons and generated \$24 billion in expenditures, all of which — as NASA officials liked to point out — were spent on Earth. What makes Federal technology development distinctive?

First, while there is no “typical” Federal research installation, many of the larger ones combine basic research with engineering and technology development. NASA’s Ames Research Center, for example, has engaged in basic research, notably in the life sciences; it has been the systems manager for the Pioneer series of interplanetary probes; and its

^{*} But according to the National Science Foundation, 1983 was the first year in which corporate expenditures, at \$41.7 billion, exceeded government research spending. Mark Potts, “U.S. Companies Probe Technology’s Frontiers,” *Washington Post* (January 8, 1984), G14.

^{**} According to the National Science Foundation (NSF), about 24 percent of Federally-supported Research and Technology Development is carried on in some 700 laboratories directly operated by government personnel. Another 9 percent is carried on in so-called Federally-funded research and development centers, which normally work exclusively or mainly for a single sponsoring agency. For the purposes of this book, both kinds of organization are considered to be engaged in intramural research. See Chapters VII and VIII for a discussion of the role of the research and technology development laboratory working under contract to a single sponsor. On trends in Federal research and technology development obligations, see NSF, *Federal Funds for Research and Development, Fiscal Years 1981, 1982, and 1983* (NSF 83-320), Section 2.

wind tunnels and other test facilities have placed Ames in the forefront of aeronautical research. This combining of activities is not fortuitous. As recently as the early 1960s, it was still possible for engineers at the NASA Marshall Space Flight Center to build a large part of the lower stages of the Saturn rocket. Today, the role of centers like Ames and Marshall — as different as they are in other respects — is rather to draft the specifications, plan the program, select the prime contractors, evaluate contractor and system performance, and certify that all design criteria have been met. The ability to carry on some basic research is a necessary part of a mission-oriented center's activities. Basic research in life sciences or materials processing serves many purposes. It serves to attract many of the most capable scientists, particularly where Federal salaries are not competitive with those paid by industry for development work. It lays the groundwork for future programs; keeps professional staff abreast of state-of-the-art developments; and, where the agency has more than one research center, makes a cross-fertilization of ideas possible.

From this account, a second important feature of the Federal technology development laboratory, whether it is a NASA center, the National Bureau of Standards, or a research facility operated under contract to the Department of Energy, emerges. This is the need to draw on the support of industry and the universities to achieve its ends, or what one observer has called "participative responsibility." (ref. 10.) Because weapons systems, space, and many energy projects are truly national projects, the sponsoring agency must go outside the walls for the skills and expertise necessary to take the project from concept through proof of concept to the final operational phase. The agency does not stand in relation to its contractors simply as customer to vendor. In many cases, the "customer" cannot state very precisely in advance what kind of operating system is desired. It is this feature which accounts for the long lead times and cost overruns of certain space and weapons systems. The role of the technology development center becomes one of working with the contractor, making midcourse corrections where the program or project changes its scope, and in some cases dropping a particular approach once evidence accumulates that it is not an attractive one. Thus, much of the research carried out at these installations is done, less for its own sake, than to maintain the center's ability to deal on equal terms with its contractors.

Finally, a publicly-supported institution will have to make its case before a multitude of public bodies. The laboratory's budget, which embodies its operating plan, will be reviewed by the Washington headquarters of the sponsoring agency, the Office of Management and Budget, and congressional authorizing and appropriations committees. Compared to the secrecy enveloping most commercial Technology

Development, even defense-oriented Technology Development is carried out in a goldfish bowl. Yet in the absence of immediate payoffs, center directors and senior agency officials must solicit that public support without which no long-term mission can be sustained. For the laboratory and its sponsor, the political arena becomes the Federal substitute for the marketplace. They shop for constituencies as companies shop for customers.

Because the Federal technology development laboratory is not driven by the incentives of a market system, other incentives must be found to take its place. The record of Federal laboratories in commercial-type Technology Development is mixed at best. As one writer notes, "when government isn't trying to develop anything for the commercial market, it often produces commercial wonders; but when government sets out to foster commercial innovations, it usually falls on its face." (ref. 11.) Instead, the laboratory's mission serves as the driving force, and perhaps the severest test to which a mission-oriented installation can be put is how to react to the completion of the original mission. The center may convert its facilities to new uses, look for new sponsors, or sell a program to Headquarters in terms of existing capabilities — or sometimes all three. It is safe to say that at least once in the life of a large research installation, there will be an "agonizing reappraisal" of its roles and missions. In many laboratories this reappraisal is continuous.

While the primary focus of this book is the Federally-sponsored technology development institution, there are at least two other ways of "growing" technology. Some very large private organizations are capable of maintaining mission-oriented technology development laboratories similar to those sponsored by the Federal Government. The corporate laboratory of General Electric (GE) in Schenectady, New York is a good example. This institution was founded by Charles Proteus Steinmetz at the turn of the century and was led and developed by the great Irving Langmuir for two decades after the First World War. Both Steinmetz and Langmuir were scientists of the first rank, but they chose to apply their great talents to developing new technology rather than to doing pure disciplinary scientific research. The function of the Schenectady laboratory was, and is, to perform research and technology development relevant to the manufacturing divisions of GE. Each of these divisions also operates product development laboratories which focus on the specific requirements of the manufacturing divisions. The function of the corporate laboratory in Schenectady is to provide the broad technological base on which the product development is based.

The American Telephone and Telegraph Company had much the same arrangement before the Corporation was broken up in 1983 in the aftermath of an antitrust case. The corporate laboratories in Murray Hill,

New Jersey (the Bell Laboratories) provided basic technology development for the corporation's manufacturing arm, Western Electric. Western Electric did operate product development laboratories, but these depended on the Bell Laboratories for their technology. Similar arrangements can be found in other large corporations such as E.I. DuPont de Nemours and General Motors.

These institutions are like Federal laboratories in some ways, quite unlike them in others. Insofar as these arrangements are comparable to those of Federal agencies with technical responsibilities, many of the conclusions drawn in this book with respect to the latter apply as well to the former. The similarities extend to the number of disciplines under one roof, the need for expensive facilities and large support staffs, the role of the laboratories as sources of ideas for their parent organizations, and the mission orientation of both kinds of laboratory. The chief difference is that GE Schenectady and Bell Labs are oriented toward profits in a way not possible for the Goddard Space Flight Center or Los Alamos National Laboratory. At a laboratory like GE Schenectady, promising projects are sometimes dropped in favor of others which may yield a greater return on investment. To support funding for work on nuclear magnetic resonance, an advanced medical imaging technology, GE has phased down work on a successful project in ultrasonic medical diagnosis. Again, GE officials decided that the development of new circuit-board technology had to give way to a project in video bandwidth compression (ref. 12).

There is yet a third form of research and technology development, where the sponsoring and research organizations are separate. Under certain conditions, this "second party" technology development is quite common in industry: where the individual firm is too small to conduct its own research; where the industry is regulated, profits are guaranteed as a reasonable rate of return, and there is little or no incentive for the individual firm to conduct research in order to increase its market share; and where the technology is very advanced. In the second and third cases, even though individual firms may be quite large, it is still to their advantage to organize cooperative ventures, with each member having access to the research.

Thus the electrical utilities sponsor the Electric Power Research Institute in Palo Alto, California; the gas utilities sponsor the Gas Research Institute (Chicago); the property and casualty insurance industry sponsors the Insurance Institute for Highway Safety (Washington, D.C.); while fifteen computer and semiconductor companies recently organized the Microelectronics and Computer Technology Corporation (Austin, Texas) to do advanced research on supercomputers and artificial intelligence. As we shall see in Chapter X, in much of the work performed at the National Bureau of Standards, the Bureau acts as an "agent" for other agencies, trade associations, and small-batch

manufacturers. This way of developing new technology is as legitimate as doing it oneself or selecting, and then closely supervising, a contractor responding to an invitation to bid.

Having said all this, we would stress that some fraction of technology development in the United States is performed in organizations that will not be treated in this volume. We are speaking here of small, innovative, privately-financed corporations working in certain important fields, where large capital investments are not a precondition for producing new technology. The classical examples are, of course, computer development and genetic engineering. In both cases, it has proved possible to make critical contributions to new technology with a first class staff but only small capital investment in equipment. In contrast, this is not possible in aerospace, nuclear technology, materials development, and certain military technologies such as the design of warships. In all of these cases, large capital investments in equipment and facilities are required before any technology development can occur. It is in these areas that the Federal laboratories and those sponsored by the very large private corporations become important.

Plan of This Book

Our focus will be the research installation as a whole, rather than project management or systems engineering at one end or the sponsoring agency at the other. Our subject will be the management of applied research — its planning, organizing, performance, resource allocation, administration, coordination, and evaluation. Our timeframe will be the past twenty years, when many of the principal space and weapons systems first came “on line.” The process we will explore is how the gap between basic research and technology development, and between the acquisition of structured knowledge about the universe and the systematic use of that knowledge to create products or operating systems, has been bridged. Our assumptions will be: that few mission-oriented technical agencies are issued a blank check, and those few not for long; consequently, that even in basic research, some objective must be defined; that the task of the research administrator is to find some non-market incentive in lieu of profit considerations to drive the organization; that, in Federal installations, one of the problems of the administrator is to reconcile the annual funding cycle with medium- and long-range planning; and that another is to ensure that basic research and technology development can somehow be made compatible within the same installation. In sum, the theme of this book will be *the management of large research installations under conditions of normal financial limits*.

Rather than begin at once with an account of modern research

institutions, we propose briefly to trace their development along a wavering line running from the Institution of Prince Henry the Navigator in the fifteenth century through the founding of the British Royal Society in the mid-seventeenth century to the establishment of the great industrial laboratories of Germany and the United States early in this century. Chapter III will treat the period from the Second World War to the present, with emphasis on those features of government-sponsored technology development mentioned earlier: long lead times amid conditions of uncertainty; difficulty in specifying end products; the necessity of procuring services from outside the sponsoring organization; the creation of contractual instruments to handle very large programs; and the development of special management techniques for coordinating a network of suppliers. The reader will note that many of these features demand quite as much in entrepreneurial talent as in the skill needed to exploit advances in scientific knowledge.

For the remainder of the book, we will focus on the administration of the large technology development laboratories sponsored by NASA, the Department of Defense, and the Atomic Energy Commission and its successor agencies. In Chapters IV and V, we shall consider the identifying features of modern research institutions — those features that, whatever the difference in missions, are common to a contractor-operated facility like the Jet Propulsion Laboratory, an agency facility like the Ames Research Center, and a nuclear weapons development center like Lawrence Livermore National Laboratory. From the structure of institutions, Chapter VI turns to the management of projects, that combination of centralized planning and control with decentralized execution so characteristic of modern research institutions. Chapter VII focuses on the management of professional personnel, with emphasis on problems of career development and transition. Chapters VIII and IX will cover supporting functions and the techniques by which institution directors manage manpower and funds. In Chapter X, we shall consider the relation between technology development institutions and their sponsors, with special emphasis on congressional authorization and appropriations cycles. The final chapter will gather all these threads together by reviewing three major problems — or one problem with three aspects — within the organization: how to adjust to changing roles and missions, how to find or keep sponsors, and how to define the role of basic research in an engineering environment. In sum, the question to be answered (and the one being considered all along) is how innovation within the organization can best be fostered and maintained.

CHAPTER II

The Technology Development Laboratory From its Origins to the Second World War

The present technology development laboratory is a relatively young institution with roots deep in the past. While we could jump immediately into the problems of the contemporary research institution, to do so would omit some of the most interesting parts of the story. Few institutions wholly outgrow their origins, and today's mission-oriented laboratories are lineal descendants of the institutions established by the Royal Society of Great Britain, the German chemical laboratories of the early twentieth century, and the research bureaus sponsored by the Government of the United States. Neither systems engineering nor contract research nor the captive research facility with essentially one client emerged full-blown, as the result of some inexorable process. Nor are the technology development laboratories, on one hand, and the institutes devoted to theoretical research, on the other, absolutely distinct. Such things as the justification of research for utilitarian ends, the focusing of scientific activity in a group, and the need to justify continuously the organization's goals, are common to both. Because the origins of both kinds of institution are bound up with each other and because we believe that an inquiry into their common sources can provide a deeper understanding of today's Federally-sponsored research, we chose to begin there.

Origins of the Technology Development Laboratory

If an institution is to be judged by the extent and duration of its influence, the Lyceum of Aristotle was the most successful, as it was the earliest, of all research institutions (ref. 13). Founded by Aristotle during his last long residence in Athens (335-323 B.C.), the Lyceum was a combination of university, research center, and scientific academy. Like most research centers today, the Lyceum had a government sponsor, Alexander the Great, who had been Aristotle's pupil. The mission, as we would say, of the Lyceum embraced a vast research program; unlike Plato's Academy, whose work was purely theoretical, the Lyceum had a strongly practical bent, with important accomplishments in biology,

psychology, and anatomy. The work of the Lyceum included assembling a collection of maps and manuscripts, and the delivery of public lectures. In fact, almost all of Aristotle's surviving works consist of his lecture notes. The Lyceum long survived Aristotle, and its influence extended through the Middle Ages down to the seventeenth century, by which time Aristotelianism had become a byword for a dry, hairsplitting philosophy totally out of tune with the new system of the natural sciences. But in its origins, the Lyceum was precisely the opposite.

Although the results of Aristotle's research proved immensely significant, his method of organized scientific research lapsed after his death and had to be rediscovered some eighteen centuries later. The first tentative revival of goal-oriented research probably occurred in the early fifteenth century, when the growth of commerce made improvements in navigation (especially the determination of longitude), improved ship design, and improved artillery imperative. Perhaps the earliest institution with the earmarks of a modern technology development laboratory was the organization set up by Prince Henry of Portugal, or Henry the Navigator (1394-1460) (fig. 1), near Sagres at Cape St. Vincent in southwestern Portugal. Opinions differ about the nature of Henry's "laboratory." One historian (J.H. Parry) states flatly that "the story of a school of astronomy and mathematics at Sagres is pure invention," while another (Marie Boas) says that he set up "a veritable research institute" at Sagres (ref. 14). From the little that we do know, certain conclusions follow:

- Henry's institution was multidisciplinary. We would not go so far as Parry, since it appears that mathematics, astronomy, cartography, navigation, and certain things connected with the preservation of food and water were represented. In conducting the affairs of his "laboratory," Henry recognized the importance of establishing relations with the creators of new knowledge. Thus he founded the chair of mathematics at the University of Lisbon.
- The "laboratory" was mission oriented, since its purpose was to master the art and science of navigation. According to Parry, "Prince Henry placed gentlemen of his own household in command of the ships, and set them definite geographical objects to be reached and passed. Thus from the habit of making fishing and casual trading voyages along a relatively short stretch of coast, there developed a programme of progressive, though intermittent, exploration much further south." (ref. 15.)

There was, then, a stress on applications and practical results and — what is less certain — an interest in scholarship and research, so far as these made the former possible. Henry wanted to open profitable new trade routes, to convert pagans, and to make contact with any



National Museum of Ancient Art, Lisbon, Portugal

FIGURE 1.—*Prince Henry of Portugal (1395-1460). Prince Henry, also known as Henry the Navigator, established what was probably the world's first technology development center in 1420.*

Christian rulers who might be found. To achieve these ends, he encouraged improvements in cartography and navigational instruments. Partly through his efforts, Lisbon, by the late fifteenth century, was the most important center in Europe for practical astronomy.

The institution founded by Prince Henry in 1420 did not survive, although it remained a center for the study and promotion of navigational enterprises for some years after Henry's death in 1460. We know, for example, that Christopher Columbus spent several years at Sagres before his epoch-making journey in 1492. However, it is probable that the Portuguese Government did not realize the importance of what Henry had started in Sagres. Thus, Henry's institution was more of a short-lived experiment than an enterprise with the base of support necessary to become a permanent feature of Europe's technological landscape. Henry's institute was finally completely destroyed by Sir Francis Drake in his famous preemptive strike against Spain in 1587 — the "singeing of King Philip's beard" of our high school history textbooks — the year before the Duke of Medina Sidonia led the Spanish Armada on its abortive expedition against England. Did Drake realize how important Henry's institution was and thus make it a special target? It is interesting to speculate, but we do not know.

There were, however, two concepts that Henry's organization showed in embryonic form — the yoking together of scientific investigation to practical ends, and the concept of research as a cooperative enterprise — with a promising future. But that future lay beyond the Iberian Peninsula. First in Italy, followed by France and England, groups of scholars met for discussions, to exchange ideas with foreign correspondents, and, where funds permitted, to publish their proceedings. The oldest such society devoted to scientific investigation was the Academie des Lincei, founded in Rome in 1600, of which Galileo was an active member. More significant was the informal society founded by the French priest Mersenne in 1635, which brought together scientists and philosophers like Descartes, Hobbes, and Pascal. This society became the nucleus of the Academie des Sciences (1666) and, through the activities of its founder, the nexus for scientific communications throughout much of northern Europe.

By the mid-seventeenth century many of the preconditions for technology development institutions existed. As we have seen, a network for the exchange of ideas covered much of Europe; it became fashionable among the clergy, the nobility, and public officials to dabble in scientific experiments; while the new philosophy represented an attempt to incorporate a scientific world view. The great philosopher Baruch Spinoza, for instance, earned his living as a lens-grinder, and his philosophy can be considered as an attempt to give a completely naturalistic view of the world. But the most influential voice on behalf of

organized scientific inquiry was that of Sir Francis Bacon (1561-1626), politician, essayist, and propagandist for the scientific method. Although his own scientific work was insignificant, Bacon saw very clearly the importance of corporate scientific activity. In his *Advancement of Learning* (1605) and especially in *The New Atlantis* (1627), Bacon set forth the program adopted by the Royal Society forty years later. As Hall puts it, "The object of Bacon's model organization was not merely to bring men together, but to set them to work in common on the tasks most important for science, so that it resembled a scientific institute more than a modern scientific society. The vast realm of natural knowledge, he felt, was too vast for one man to tackle single-handed . . . To the efforts of individual pioneers, as Sprat put it later in speaking of the Royal Society, 'we prefer the joint Force of many Men.' " (ref. 16.)

It was out of this soil that the Royal Society of London for Improving Natural Knowledge grew. Like its counterparts in France and Italy, it originated in meetings of private persons — in this case, meetings that began during the English Civil Wars of the 1640s and continued under Oliver Cromwell's Commonwealth. The Society was founded in 1660, only becoming "Royal" when it received a charter in 1662. In the beginning it was nothing but an association of gentlemen who were friends of the newly restored Charles II. It had no laboratories, its members received no government stipends, and it never had the funds to sponsor the Baconian research program in which many of its members believed. But absence of government funding also signified absence of government control. The founders of the Royal Society were free to pursue their interests, although from time to time they were called upon by the government to provide scientific advice.

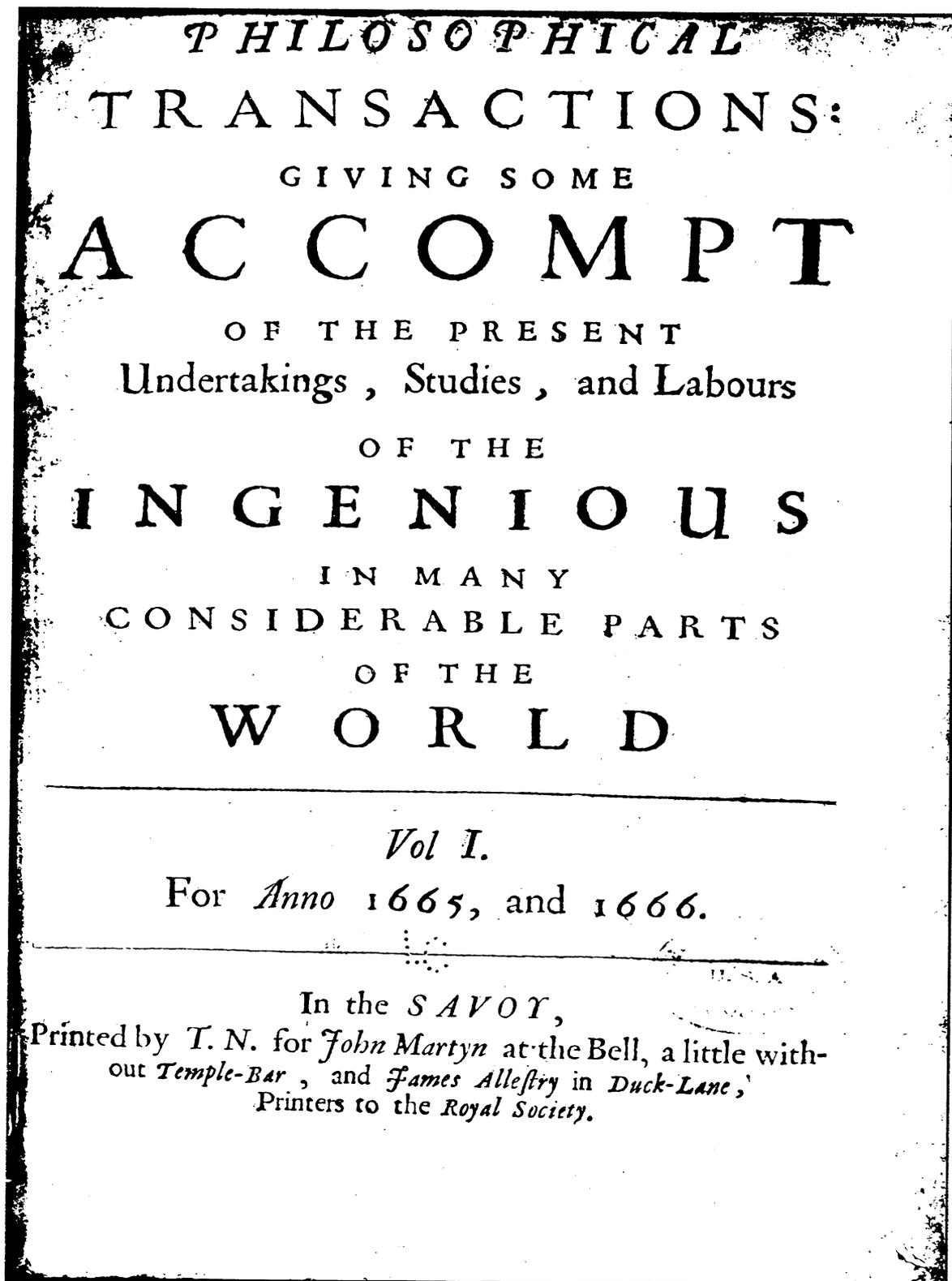
There were several features of the Royal Society which serve to explain how it set the pattern for the establishment of learned academies throughout Europe and elsewhere. While the Society benefited from a cultural climate in which science was fashionable, it also did something to create that climate. In its origins the Royal Society's purpose was strongly utilitarian, and a large portion of its early research was devoted to socio-economic needs: to methods of determining a ship's position, especially its longitude; to studies of times of tides; to experiments in ship construction and ship accessories; to studies of methods of mine ventilation, metallurgy, and general mining techniques; to experiments with gunpowder, measuring the velocity of bullets, and relating the length of a gun barrel to the range of a bullet (ref. 17). The research sponsored by the Royal Society could be justified on both practical and religious grounds. Science was a means of increasing the nation's wealth, whether by increasing the depths to which mines could be worked or by determining nautical longitudes; it could serve the interests of the state by improving military technology; it was a mental discipline, better than the

outworn Aristotelianism at which much of the Society's propaganda was directed; and in a country still under the cultural influence of Puritanism, it was a means by which the glory of God's handiwork could be revealed (ref. 18).

Thus the Royal Society, in its origins, had a strongly practical bent, which was reflected in what Bishop Sprat, the Society's first historian, called the "plain and naked" style of the papers delivered by its members. For the Society was an agency, not only for producing new knowledge, but for disseminating it. Beginning in 1665, the Royal Society published its *Philosophical Transactions*, the first journal to print original scientific communications regularly (fig. 2). Given a culture favorable to the growth of science, the influence of such members as Christopher Wren, Robert Hooke, and Isaac Newton (President from 1703 to 1727) and the Society's emphasis on applied research, the Royal Society did more than any other institution of its day to promote the advancement of research applied to social needs.

Much of the Royal Society's work was carried out through institutions with which it had working relations. The most important of these was the Royal Observatory which was founded in 1675 as a separate institution, although the Society came to exercise a "vague surveillance" over it (ref. 19). The Royal Observatory (fig. 3) was located in Greenwich, several kilometers downstream from London, at the site of one of the royal dockyards. Once again, a mastery of navigation was the motive behind much technology development. National power and prestige depended on the ability to conquer, colonize, and then trade and exploit, and this could only be done by securing the best possible ships and the best trained crews. The first Astronomer Royal, John Flamsteed, was an independent character, and the government recognized that if the Observatory was to be useful in providing navigational data, it would have to exercise control. To this end a Visiting Committee consisting of Isaac Newton, Christopher Wren, and several others — all members of the Royal Society — was appointed in 1703. The Visiting Committee managed to gain control of the Observatory, thus establishing a pattern that persists to this day, by which governments exercise control over their technology development institutions through the various academies of sciences. This implies programmatic, not administrative, control. The administration of the Observatory then and of laboratories today goes through different channels. But the program content of the laboratory, then as now, tended to be determined by scientific and technical committees representing the academy of sciences of the nation involved.

When Edmund Halley, who joined the Board of Visitors of the Observatory in 1710, succeeded Flamsteed as Astronomer Royal ten years later, the Observatory became more and more a prototype of a technology development laboratory. Two examples will bear this out:



Library of Congress, Rare Book Division, Washington, D.C.

FIGURE 2. — Title page of the first volume of the Royal Society's "Philosophical Transactions."

The accurate measurement of longitude, and the voyages of Captain Cook. While the major function of the Observatory from the beginning had been astronomical observations and the production of accurate charts, other problems began to attract the attention of the scientists and instrument makers working there. The most famous of these problems (and one that preoccupied the Royal Society) was the vexing one of determining longitude.



Royal Observatory, Greenwich, England

FIGURE 3. — *The main building of the Royal Observatory at Greenwich. Designed by Sir Christopher Wren and erected, 1675-80, it still stands and is still in use.*

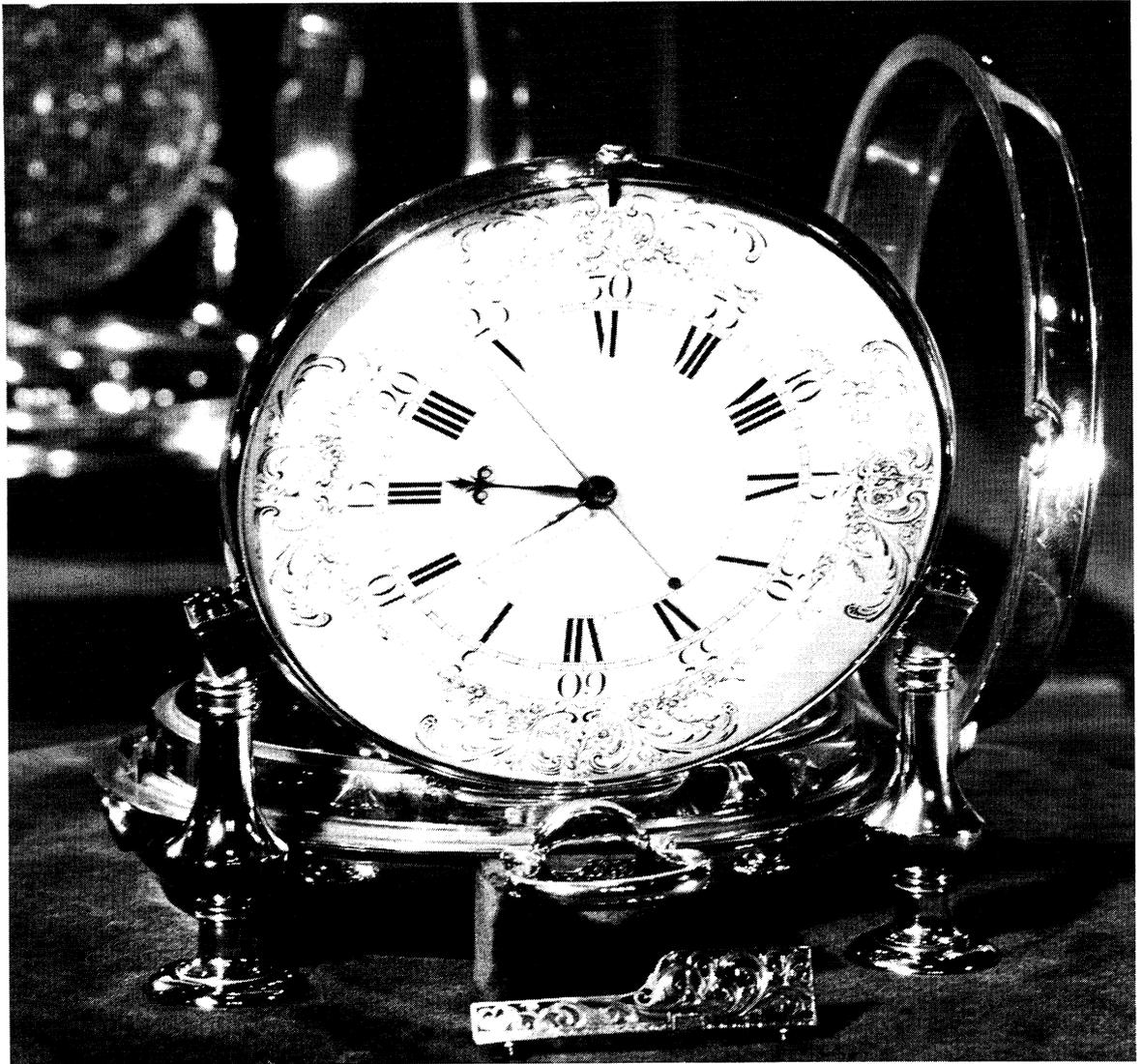
Latitude is easy to determine; all one has to do is to measure the azimuth of the pole star and one can determine one's latitude. Determining longitude is much more difficult. There are astronomical methods for determining longitude, but these tend to be difficult and require a skill not often available to the average mariner, especially if the measurements have to be made from the heaving deck of a ship. The easiest way to determine longitude is to determine the local time by measuring the Sun at its azimuth at noon and comparing it with "Greenwich" time, i.e., the time at the Greenwich Observatory. Since the rate of the Earth's rotation is known, the longitude can be calculated from this measurement. Obviously, this method requires an accurate clock or "chronometer." This problem was considered so important that a Board of Longitude was appointed in 1714 under the Royal Society and a prize

of £20,000 offered for solving the problem of producing an accurate chronometer. The prize was finally awarded in 1773 to John Harrison who, over a span of forty years, developed five models (ref. 20). The principal problem was to develop a spring clock with a constant tension spring independent of the environmental conditions of temperature and humidity. To do this, Harrison invented bimetallic strips, many of which were tested in what today would be called "environmental test chambers" located at the Observatory.*

The chronometer thus produced (fig. 4) was tested by Captain James Cook on his three famous voyages (1768-1771, 1772-1775, 1778-1779). These voyages were carried out under the joint sponsorship of the Royal Society and the Royal Navy, and the Greenwich Observatory played an important role in their development. They mark the high point of scientific exploration in the eighteenth century. A Yorkshire native like Harrison, Cook (1728-1779) was a remarkable character (fig. 5). He was a farmer's son, was completely self-taught, and rose in the ranks of the Royal Navy through sheer ability. His three voyages served the interests of science in several ways. On his first voyage he measured the transit of Venus across the disk of the Sun from the island of Tahiti in 1769. This measurement was necessary to establish precisely the parameters of the solar system and was of great practical importance in establishing an accurate calendar. On his second voyage Cook — who had failed to find the "Terra Incognita Australis" the Admiralty thought might exist — searched again for a southern continent. This voyage accomplished three things: It marked the first important tests of Harrison's chronometer; Cook developed a cure for scurvy after assuming (correctly) that it was caused by the lack of fresh fruits and vegetables in the sailors' diet; and it marked the beginning of the first thorough investigation of the large-scale ocean currents and weather patterns in the Pacific. More than any other person, Cook laid the foundations for the systematic study of Pacific geography.

From the late eighteenth century, the British Government, either directly or through autonomous bodies, increased its sponsorship of scientific research. The government maintained observatories at Greenwich and the Cape of Good Hope and research laboratories at Army and Navy installations; subsidized occasional expeditions, like the voyage of HMS Challenger (1873-1876), one of the pioneer events in the history of oceanography; and, beginning in 1849, made an annual grant of £1,000 to the Royal Society, raised to £4,000 in 1882.

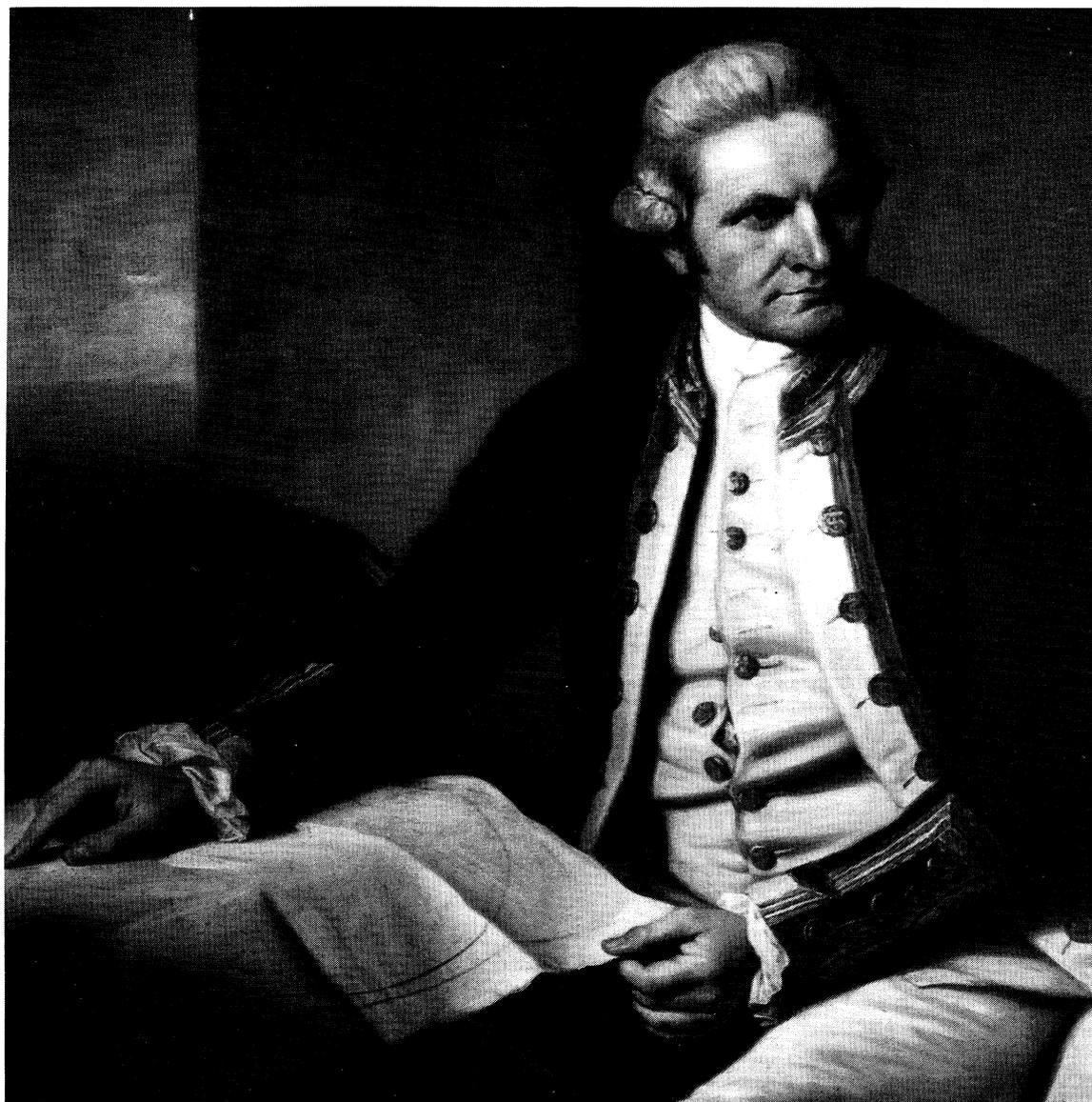
* The prize was awarded only after George III intervened personally, owing to the reluctance of the Royal Society and the Board of Longitude to award the prize to a Yorkshire clockmaker.



U.S. Naval Observatory, Washington, D.C.

FIGURE 4.— *The Harrison Chronometer, the first clock with accuracy sufficiently good for the precise determination of longitude at sea.*

By mid-century, the government had created a pattern of aid to science which was to have a lasting influence on science-government relations in Britain and the United States. Where possible, the government preferred to work through small committees of scientists empowered to provide stipends for researchers and to consider grant applications. And the scientists were ready to oblige. By this time a well-organized scientific community had come into being with official spokesmen, professional societies like the British Association, publications like *Nature* and a political program with a “lobby” to back it. This program had three goals: financial support from the government, more science in the university curriculum and, to make science policy more



British National Maritime Museum, London, England

FIGURE 5.—*Captain James Cook, Royal Navy, the most accomplished navigator and explorer of the eighteenth century.*

uniform, creation of a Ministry of Science with Cabinet rank (ref. 21). But scientists had to contend with the reluctance of officials to extend their spheres of influence or to initiate any policy that might mean some increase in expenditure or staff, no matter how trivial. The government's conservatism extended to providing scientific and technical education. Before 1870, there was no provision even for universal elementary education, although there was nothing in it inconsistent with a philosophy of economic liberalism. More important, no system of secondary education (let alone advanced scientific instruction) was possible until elementary education had been provided for.

Put differently, in Britain (unlike Germany) the industrial revolution preceded the educational revolution. To the extent that Britain did train scientists for industry, it was emphatically not because of pressure from manufacturers. Few industrialists understood either the value of planned research or the relevance of a scientific discovery to industrial production. The facts were public and notorious: the discovery of aniline by an Englishman, Perkin, in 1856 and the transfer of the industry to Germany within a dozen years; the total dependence of British industry on Germany for scientific instruments; the fact that the crucial inventions in the mass-production of steel were made by Bessemer, an independent inventor, Siemens, a German resident in England, and Gilchrist Thomas, a police-court clerk. The difference between British and German industry was between one based on planned innovation and another based on rule of thumb and non-standardized production. It is hardly surprising that few university graduates chose scientific careers. Jobs were few, salaries miserable, advancement unlikely. In 1900, there were only 200 scientists in government service, rising to 300 on the eve of the First World War.

We have examined the origins of applied research in Britain because the British approach to the organization of scientific research has been extremely influential. We can go even further: The technology development laboratory, at least in the United States, is the offspring of the German research laboratory and quasi-public scientific associations modeled on the Royal Society. On the eve of industrialization, the states comprising the German Empire (after 1871) had an educational system superior to Britain's. Education tended to last longer, to cover a much higher percentage of children of school age, and to link elementary classes with the middle and secondary classes where technical education began. And while in Germany rigorous scientific research began in the universities (with Liebig's laboratory at Giessen in the 1820s), by the 1860s industrialists had begun to perceive that progress in the sciences opened a variety of alternative paths for economic development. As Ben-David notes, "An original idea with practical implications could now be explored and exploited within a short period of time by a group working in concentrated fashion." The most striking examples of such applied work were the development of aniline dyes — building on Perkin's discoveries — and immunizing vaccines. Both "led to the establishment of nonteaching research laboratories employing professional researchers who were not professors." (ref. 22.) In Germany, but not in Britain, it was possible, not only for a university graduate to pursue a scientific career, but to move into the ranks of the managers and directors of the giant enterprises (BASF, Bayer, Hoechst, AEG, etc.) made possible by research.

Thus by 1900, most of the elements of the technology development laboratory were in place. These were, first, the existence of a pool of

university-trained chemists, physicists, and engineers; second, an understanding of the process by which research could be transferred from the laboratory to the factory; third, a system by which government could draw on quasi-official learned societies for unbiased advice; and fourth, an educational system which produced the technicians and administrators who supported the scientific enterprise. How these processes worked themselves out in the United States up to the eve of the Second World War comprises the rest of this chapter.

Research and Development Institutions in the United States

The United States did not have a formal research establishment supported by the Federal Government until well into the country's history. While there were learned societies established before and during the Revolutionary War, like the American Philosophical Society (founded by Benjamin Franklin in 1743) and the American Academy of Arts and Sciences (1780), they had very limited funds and supported no development institutions. In fact, the first academy supported by the government, the National Academy of Sciences, was not chartered by Congress until 1863.

In the United States most, if not all, of the research institutions supported by Federal funds originated as the result of war or a crisis perceived by the public as major. The first of these institutions was the U.S. Naval Shipyard in Washington (1798). Here as in England, maritime technology set the pace for publicly-funded applied research, owing to the importance of warships and fleets in keeping ocean trade routes open. During the Revolutionary War the U.S. Navy had no ships designed from the keel up as warships; all the warships used were converted merchant transport ships mostly procured from foreign shipyards. When the Navy was disbanded after the war, U.S. flag vessels were at the mercy of Barbary Coast pirates, as well as England and France, both of whom took American ships almost at will during the Napoleonic Wars.

These conditions drove the United States to establish and develop a Navy. The Navy Department was established in 1798 as the first "regular" service. From then until the Civil War, the Navy sponsored many important technical developments. Beginning with the construction of the Washington Naval Shipyard, the Navy undertook to build warships that no private shipyard would build, because there was no profit in building them for a nearly bankrupt government. Under Joshua Humphreys, who was appointed "Naval Constructor" in 1799, the Navy designed and built the famous heavy frigates that dominated the War of 1812. These ships—the "Constitution" (fig. 6), "Constellation," "President," and "United States"—were the most advanced of their time.

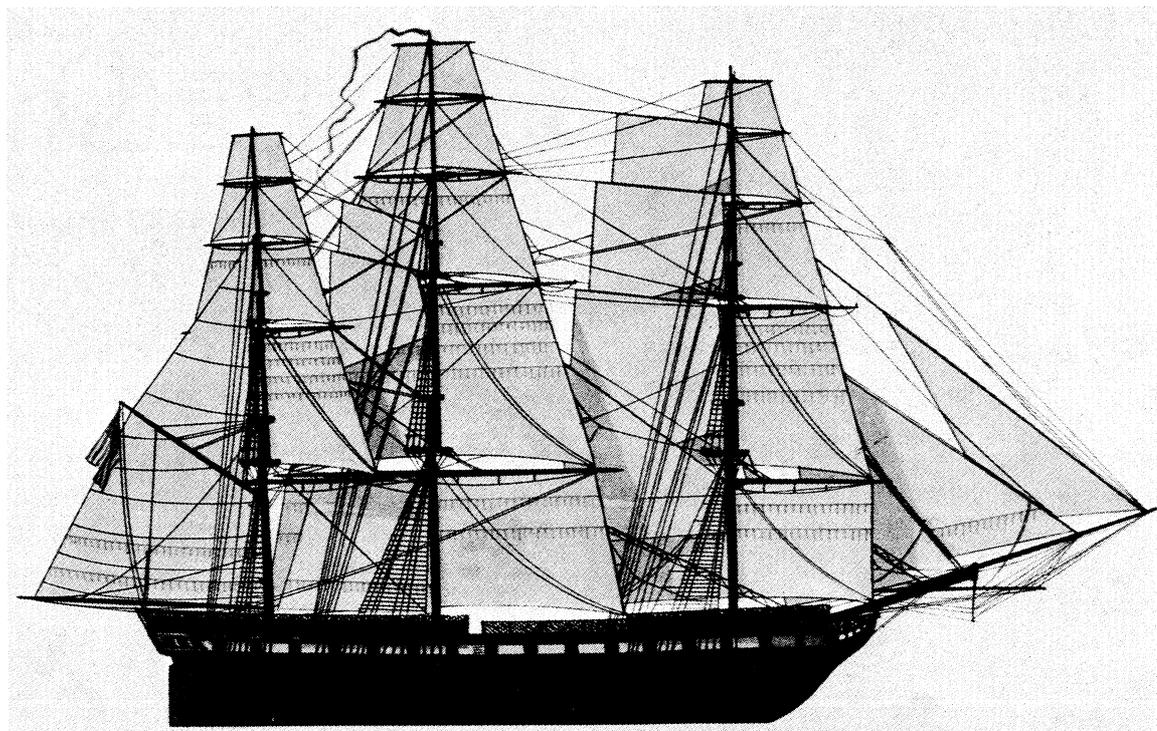


FIGURE 6. — *The U.S.S. “Constitution,” one of the class of heavy frigates designed and built by Joshua Humphreys that were so technologically superior that they dominated single ship actions with the Royal Navy during the War of 1812.*

Because they were built of pine rather than oak, they were much faster than similar ships and carried more guns. During the War of 1812 they proved superior to any ships the Royal Navy could muster against them.

The War of 1812 confirmed the Navy’s importance. Between the Treaty of Ghent, which ended the war, and the outbreak of the Mexican War in 1846, the Navy sponsored several important developments: the construction of the first “slide” or “ways” (1821), so that ships could be hauled out of the water for scraping, painting, and general maintenance; the building of the first steam engine intended for a ship (1826); and, most important, the use of steam for the propulsion of ocean-going ships and the replacement of wood by iron in their construction. The first steam warships were built at the Washington Yard in 1842. These ships were sidewheelers and, as such, were involved in the lengthy controversy over the best means of using steam propulsion. At the time, the Navy was experimenting with steam propulsion and especially with propellers for large ships—experiments which led to the steamships employed in the Mexican War (1846-1848). This war first saw the large-scale use of steamships by the U.S. Navy and, with it, the problems of keeping steamships at sea. Among the latter were:

- Boilers. It is well known that boiler scale is deposited inside hot water boilers and pipes. The maintenance of boilers and pipes at sea was a major problem for the Navy.
- Metal fatigue. Little was known as to why metals became brittle with use and broke.
- Gaskets and glands. No one knew what were the best materials for sealing rotating shafts to the glands in the hull through which the shafts passed.

These problems were not solved during the war, but had to await the more leisurely approach possible during peacetime. The attack on these problems was made by Commodore Matthew C. Perry who, after serving in the Mexican War, became commandant of the Brooklyn Navy Yard. He persuaded the Navy to subsidize construction of six iron steamships, which were built for commercial services but could be converted to commerce raiders in the event of war. The ships were leased to a private shipping line, which proceeded to use them as packets in the North Atlantic service. In this way, many of the problems Perry encountered during the war were studied and solved. This represents one of the earliest examples of the transfer of technology from military to civilian applications.

The other major consequence of the Mexican War was that something was done about the dismal state of the ordnance. Guns were both inaccurate and dangerous. In 1844, for example, a gun burst and killed the Secretary of State and the Secretary of the Navy as they were inspecting the frigate "Princeton." During the war, in 1847, a young lieutenant named John Dahlgren joined the staff of the Washington Naval Shipyard. Dahlgren was one of the most innovative of American engineers. He was the first to apply systematically important new scientific principles to the construction of guns. He successfully developed and constructed rifled cannons and built first-class foundries, laboratories, and test facilities. Without question, his work as Chief of the Ordnance Department of the Washington Naval Shipyard contributed greatly to the favorable position of the Union Navy during the Civil War. When he died in 1870, he had turned the Naval Shipyard from an institution that was primarily a shipbuilding establishment into a technology development center. As Dupree notes: "Thus the Navy in the Civil War came to terms with every important phase of the technological revolution that affected it. Under constant criticism from outside and riven by internal controversy, the department nevertheless managed to find officers well qualified to handle the new research technology and put them in positions where they were able to act. In no important way did they further the naval revolution, but to keep pace with it was a major accomplishment which hinted at government's potential ability to apply scientific procedures to technological problems." (ref. 23.)

The Civil War spawned three other important developments of a technological nature. The first was the encouragement of railroad technology, particularly the standardization of the gauge of American railroads at four feet eight inches (or 1.42 meters) and the devising of new methods for the rapid laying of tracks. The second was a concurrent improvement in civil engineering techniques. At the end of the Civil War, there were several institutions for technical education loosely modeled on the example of France's Ecole Polytechnique founded in 1794: the U.S. Military Academy at West Point (1802), which was also the nation's first engineering school; Rensselaer Polytechnic Institute in Troy, New York (1824); the Brooklyn Polytechnic Institute (1854); and the Massachusetts Institute of Technology (1865).

Yet the two most important structures of the immediate postwar period — the Eads (1867-1874) and Brooklyn Bridges (1869-1883) — were designed by men who received their training elsewhere. James Buchanan Eads made his fortune by developing a method of salvaging boats that had gone to the bottom of the Mississippi; during the war he built a fleet of armor-plated boats to defend the waterways for the Union. His great bridge (fig. 7) over the Mississippi at St. Louis was unique in the number of innovations it embodied: It was the first large structure anywhere to use steel for the structural members; the first in America to use pneumatic caissons to found the piers; the first arch bridge to use cables to cantilever the arches out from the masonry in order to keep the channel open while the bridge was under construction; and finally, it was one of the first bridges in America where each part was manufactured and tested to the most rigorous specifications. John Roebling, on the other hand, had studied in Berlin under Hegel and after immigrating to the United States for political reasons, farmed before turning to engineering. The Brooklyn Bridge, which Roebling designed but did not live to build, embodied all of the basic elements of the modern suspension bridge. It was also, when it was completed, half again as long as the next longest span — Roebling's bridge over the Ohio at Cincinnati.

But the single most significant event of the period, so far as it affected American technology, was the enactment of the Land Grant Act of 1862. This farsighted legislation, introduced by Congressman (later Senator) Justin Smith Morrill of Vermont, provided for Federal subsidies for the support "of at least one college (in each state) where the leading object shall be, without excluding other scientific and classical studies and including Military tactics, to teach such branches of learning as are related to agriculture and the mechanic arts in such manner as the Legislature of the states may prescribe . . ." The Morrill Act, as it is often known, accompanied the Homestead Act of 1862, which made it possible for many Civil War veterans to migrate westward and farm what had been public land. The institutions of learning constructed under the

Morrill Act concentrated on agriculture and engineering, those fields vital to the rapid development of the new lands. In 1890, Morrill secured an act which appropriated for each land-grant college an annual sum gradually increasing to \$25 000; in 1900, this support became permanent.



Smithsonian Institute, Museum of American History, Washington, D.C.

FIGURE 7.— *The bridge over the Mississippi River at St. Louis built by James B. Eads, 1867-74. An example of “high technology” in the nineteenth century.*

Great as the impact of the two Morrill Acts has been on American education, their impact on science policy has been greater still. The 1862 act, as well as the creation of a Department of Agriculture the same year, marked the first time that Congress implicitly recognized that its constitutional duty to provide for the general welfare included sponsoring some scientific research. When the Hatch Act was passed in 1887 as an addition to the Land Grant Act, it required the establishment by each of the land-grant colleges of agricultural and engineering experiment stations which were to “acquire and diffuse useful and practical information on subjects connected with agriculture.” At a stroke, the Hatch Act (in Dupree’s words) changed the Department of Agriculture “from a single central agency into a nexus of a system of semiautonomous research institutions permanently established in every state.” (ref. 24.) This system, supported since 1934 by the Agricultural Research Center at

Beltsville, Maryland, has done much to give the United States the preeminent position in agriculture it enjoys.

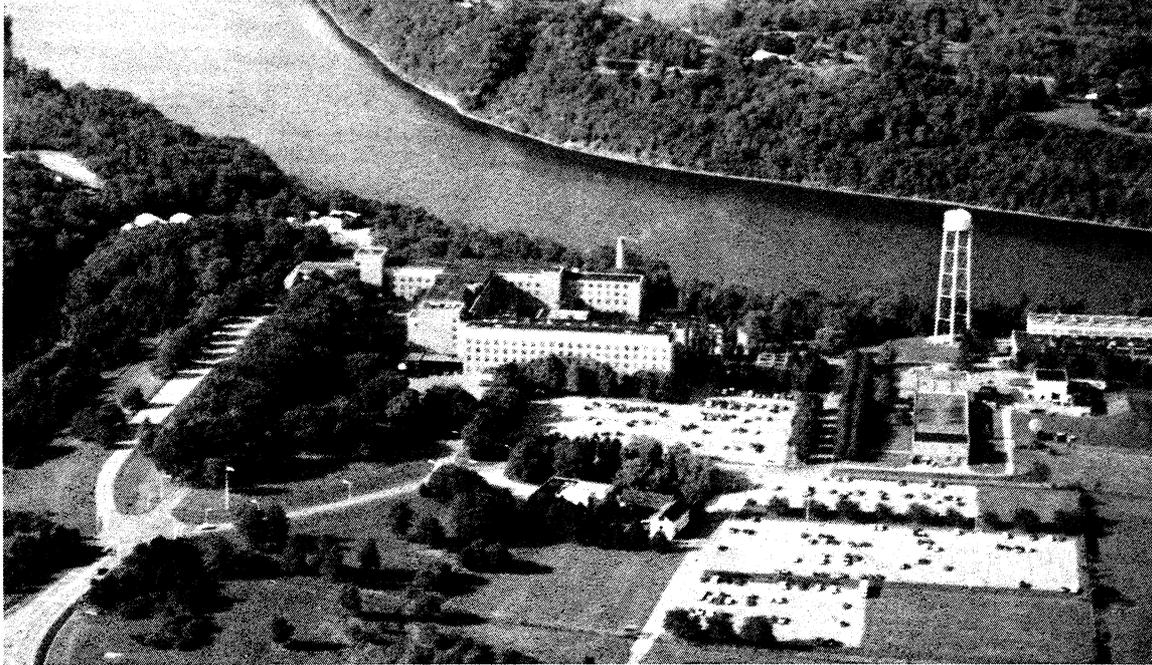
Yet it must be conceded that until the Second World War, scientific research was a rather peripheral activity of the Federal Government. In addition to its arsenals and shipyards, the government had several bureaus engaged in scientific research. Beginning with the Coast Survey, founded in 1807, the most important included the Public Health Service (1818),* the Naval Observatory (1842), the Geological Survey (1879), the National Bureau of Standards (1901), and the National Advisory Committee for Aeronautics (1915), as well as the Smithsonian Institution, chartered by Congress in 1846 as an independent establishment which, nevertheless, received congressional appropriations. When we consider that such early Presidents as Jefferson, Madison, and John Quincy Adams followed the progress of science with the keenest interest, it seems surprising that they and their successors did so little to promote scientific research. Don Price, in his *Scientific Estate*, has provided a clue: "One half of Jefferson's theory defeated the other half. Jacksonian democrats were quite willing to follow Jefferson in opposing establishments and class privilege, and relying on applied rather than theoretical science. But they were not interested . . . in building up . . . scientific institutions that would bring America up among the leaders of science." (ref. 25.)

There was, then, no possibility of a centralized scientific establishment, no Department of Science, such as the National Academy of Sciences advocated. Indeed, until the First World War the Academy's role as science advisor to the government was negligible. Not until 1916, when the National Research Council (NRC) was created to serve as the Academy's operating arm (the NRC was made permanent by executive order of the President in 1918) did the Academy have the mechanism to stimulate research contributing to the national welfare.

In the post-Civil War period, there was a notable growth of "private" research institutions — those sponsored by industry, universities (especially those, like Johns Hopkins, modeled on the German graduate school), and the great foundations. It is important here to distinguish technology development from product development. The point is that private investment without government sponsorship (directly or through subsidies and tax credits) had insignificant impact on technology development before 1900. In product development almost all investment has been private, and there it has been exceedingly important. The great

* Although the Public Health Service was not formally established until 1912, 1818 marks the establishment of the Surgeon General's Office and the Army Medical Department, with authority to prevent and treat disease. See Appendix II for details.

research establishments of E.I. DuPont de Nemours in the Brandywine Valley of Delaware, those of General Electric in Schenectady, New York (fig. 8), and the Bell Telephone Laboratories, Murray Hill, New Jersey (founded in 1925) dominated applied product-oriented research in the United States well beyond the Second World War.



General Electric Company, Schenectady, New York

FIGURE 8. — *The Corporate Research Laboratory of the General Electric Company at Schenectady, New York.*

Consider the development of electrical technology in the United States. American entry into electrical technology came relatively late. The Americans — Morse, Bell, Edison, Westinghouse, and others — tended to be brilliant amateurs, while the Europeans (for example, the Siemens brothers) were professionals. A generation later, rationalization, sustained by a supply of newly-minted engineers and Ph.D.s (most of whom got their degrees in Germany), set in. Charles P. Steinmetz, founder of GE's Laboratory, is shown in figure 9. GE played a prominent role in advancing radio technology, with Alexanderson's work on the alternator and Langmuir's (fig. 10) on the vacuum tube and the feedback circuit. At Bell Labs there was Davison's work demonstrating the wave nature of electron beams, which led to L.H. Germer's method of studying the crystal structure of surface films, Harold Black's principle of negative feedback as applied to amplifiers and, in 1947, the work of Bardeen, Brattain, and Shockley in developing the transistor. Yet neither GE's Schenectady laboratory nor Bell Labs were by any means centers for



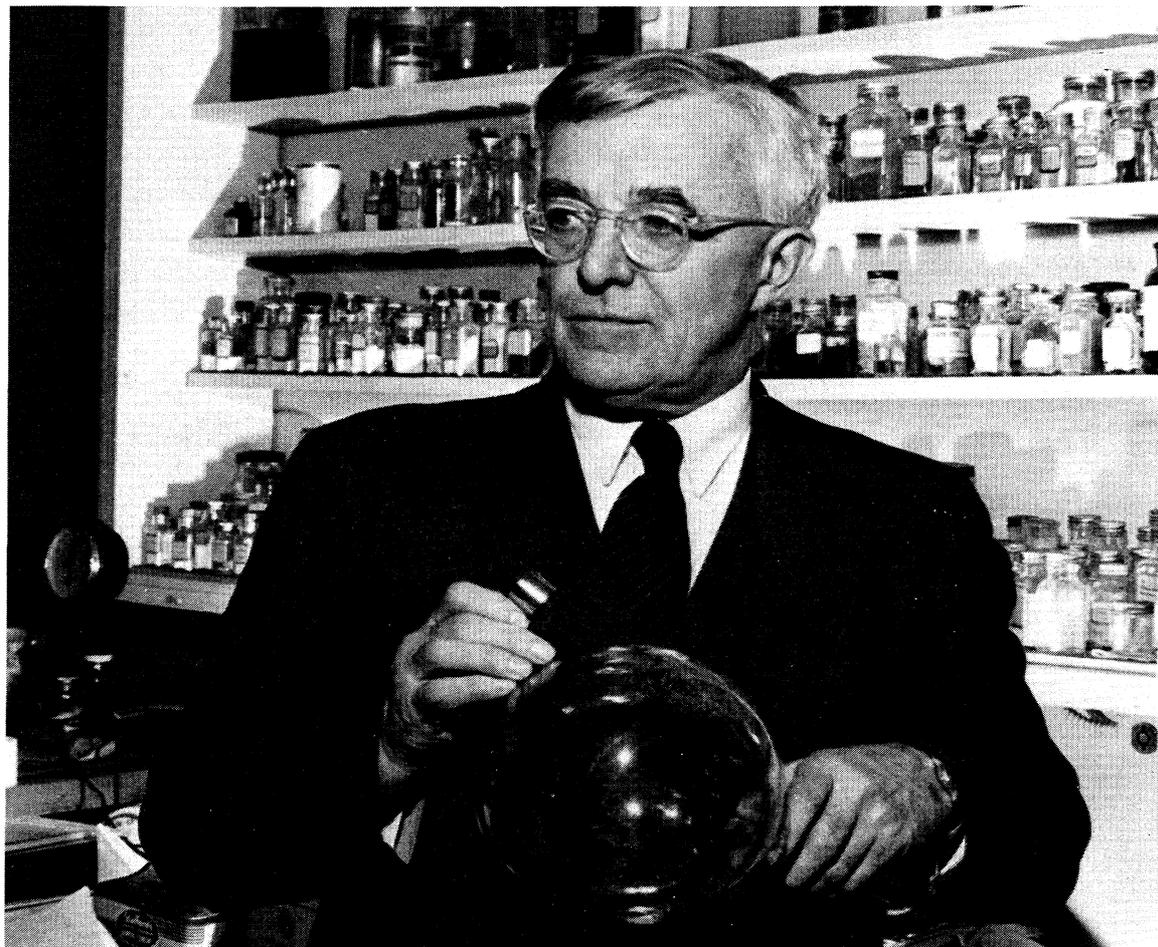
General Electric Company, Schenectady, New York

FIGURE 9.—*Charles P. Steinmetz, founder of the General Electric Company's Laboratory and the man responsible for much of the technology development in the early days of the electrical industry.*

theoretical research. Team-work dominated Bell Labs, most projects required prior approval by the laboratory director, and very few scientists enjoyed Davison's liberty to follow his research wherever it led him (ref. 26). The industrial laboratory was oriented, overwhelmingly, toward goals set by corporate management.

Where government collaborated with industry, the former tended to become a service agency responding to specific problems set by the latter. A classic example of this relationship was the history of the National Advisory Committee for Aeronautics. In contrast to electrical technology, aviation was something that began in the United States. The U.S. military saw advantages in aviation and in 1908 awarded the Wright brothers their first military contract. However, this did not represent continuing sponsorship of aeronautical research. The United States did not create an aeronautical technical organization until the First World War, when the major European powers quickly developed a number of sophisticated combat aircraft. This event did not go unnoticed in the United States, and in the 1915 Naval Appropriations Act a rider was attached establishing the National Advisory Committee for Aeronautics (NACA).

The NACA was empowered to conduct research and development in aviation, as well as to advise the President on how best to develop



General Electric Company, Schenectady, New York

FIGURE 10.—*Irving Langmuir, one of the distinguished scientists who worked at General Electric Laboratory. He received a Nobel Prize in 1932.*

aviation technology. The Langley Memorial Laboratory was established in 1917, followed by the Ames and Lewis Laboratories in 1939 and 1940. After dropping its advisory role in 1926, NACA concentrated on conducting aeronautical research, with emphasis on the needs of industry and the services. Yet the relationships engendered made it difficult, if not impossible, for NACA to do the kind of long-range research implied by its charter. In certain critical areas, like boundary layer research, NACA took no official interest until research had been underway in Europe for almost twenty years. In effect, NACA ended by becoming captive to the interests of its sponsors. "Pressed by the need to get the next generation of fighter aircraft into operation or the next prototype into production, both the services and industry tended to focus . . . on immediate problems, on incremental advances in the state of the art, on refinement of the equipment at hand." (ref. 27.)

Conclusions

By 1939, the organization of Federally-sponsored research and development had taken on many of the features it still retains. The system was strongly pluralistic and decentralized, with no central department for science confronting some nonprofit organization representing a united scientific community. The government operated through a network of research bureaus, laboratories, and research stations, down to the level of the county extension agent. In contrast to what was soon to follow, the role of the government contractor was mostly limited to supplying specific kinds of equipment; the Federal Government had not reached the point where it would delegate to industry the management of entire installations, supervision of huge R&D projects, and responsibility for monitoring thousands of subcontractors. The work of the pre-1939 research bureaus, whether it involved setting product standards, testing airplane models in wind tunnels, or mapping the United States, was either repetitive or so long-term in effect that it never would come to a definite end.

Yet the system, such as it was, was exceedingly flexible. From the modest beginnings of the Washington Naval Shipyard and the Coast Survey, the Federal Government had gradually assumed responsibility in many other areas impinging on the general welfare. What was more, there was no hard and fast division between basic and applied research, between the university and industry, or between the scientist employed by the Federal Government and one whose research was subsidized by a land-grant college. Competition was the order of the day: state universities competing with private universities, and Federal research bureaus competing with each other for funds. Finally, within the constraints of a Federal bureaucracy, most of the requirements for research in government service — conditions formulated by Ferdinand Hassler (the first director of the Coast Survey) as far back as 1842 — were partially met: need for long-term support, need for flexibility in objectives, freedom to publish, access to the international scientific community (ref. 28), and improvements in the position of the professional scientist. How the system was transformed under the stress of war into the nodes of mission-oriented scientific agencies, depending heavily on the private sector for contract services, is the theme of our next chapter.

CHAPTER III

The Technology Development Laboratory From the Second World War to the Early 1970s

The United States' entry into war after Pearl Harbor did much to shape the organization of science and technology development. Put simply, because of the exigencies of war, the government was now prepared to spend almost unlimited amounts to achieve a single technological objective. Where scientists in and out of government had had little political influence, their chief spokesmen now had direct access to the President; and where the government contract had been at best a clumsy device for procuring research and technology development, it now became a flexible instrument, once freed from the restraints of competitive bidding. Much depended on the timeliness of the principal administrative decisions. The entry into war was preceded by more than a year-and-a-half of careful planning, based on the knowledge that: The United States was being drawn into war on the side of Britain; that, lacking some mechanism to coordinate relations between government and scientists, the United States would be ill-equipped to use the most advanced military technology; and that Germany had the potential to develop a nuclear bomb far more powerful than any conventional weapon.

The prime mover in the creation of a wartime scientific organization was Dr. Vannevar Bush (fig. 11). In 1940, he was President of the Carnegie Institution of Washington, Chairman of the National Advisory Committee for Aeronautics, and a former Vice President of the Massachusetts Institute of Technology (MIT). Bush numbered among his friends some of the most influential scientists in the country, including Frank Jewett, President of the National Academy of Sciences, Karl Compton, President of MIT, and James B. Conant, President of Harvard. All of them were disturbed at the United States' lack of military preparedness, especially since they believed that the next war would be highly technological (ref. 29). They also believed that to mobilize science and technology a new Federal agency, rather than a reconstituted National Research Council, was needed. Bush eventually met with President Roosevelt, who (on June 27, 1940) approved the establishment



FIGURE 11.—*Vannevar Bush. An early pioneer in computer research, he later became one of the great organizers of technology development during the Second World War when he headed the Office of Scientific Research and Development (OSRD).*

of the National Defense Research Committee, with Bush as chairman. One year later this was expanded by executive order into the Office of Scientific Research and Development (OSRD), again with Bush as head.

The establishment of the OSRD marked a radical break with earlier science-based agencies. It operated no laboratories and did not take over projects already underway. Rather, it sponsored whatever research and development — from theoretical work to development of weapon systems — was deemed necessary to the war. The contract was the OSRD's favorite instrument. Between 1940 and 1941, most of the obstacles to procuring research and technology development were removed. The National Defense Expediting Act of 1940 authorized the

services to buy through negotiated contracts involving either fixed price or cost-plus-fixed-fee, while the War Powers Act of 1941 freed the services "of most legal restraints and restrictions in the way of speedy procurement; the sole consideration was whether the action proposed would facilitate the prosecution of the war." (ref. 30.) The OSRD broke down the compartmentalization between public and private universities, and "for the first time in the nation's history, substantial federal funds were going to university laboratories" (ref. 31) outside the field of agriculture. The OSRD also began to emphasize functional distinctions in its own staff which were to become important in post-war sponsorship of science and technology development: between the contracting officer, who was responsible for the fiscal aspects of the project, and the scientific officer, who was responsible for the substantive aspects.

In essence, the OSRD established the framework within which mission-oriented research and technology development could be carried on. And of all the missions originally sponsored by the OSRD, the most far-reaching was the program to build a nuclear weapon. It was the largest Federally-sponsored technology development program to that time; it led directly to the post-war programs in weapons development and the peaceful uses of atomic energy; and it created institutions like the Los Alamos National Laboratory and the Argonne National Laboratory which are still among the nation's foremost technology development laboratories today. Because the nuclear weapons program is *the* prototype of one kind of Federally-sponsored technology development, the facts deserve to be retold.

The Manhattan Project

During the 1930s, major advances were made in the study of the atomic nucleus. The neutron was discovered by James Chadwick in 1932. In 1938, two German chemists, Otto Hahn and Fritz Strassmann, in order to explain the results of experiments involving the bombardment of uranium with neutrons, advanced the radical hypothesis that after capturing a neutron, the uranium nucleus may break up into two or more large fragments — each fragment being the nucleus of an atom of intermediate mass. When these results were published in 1939, the German theoretical physicists Lise Meitner and Otto Frisch speculated on the fracture of the uranium nucleus, which they called "fission." From the known dependence of the binding energy of nucleons within the nucleus as a function of the nuclear mass, they predicted that a large quantity of energy would be released by each fission event and that neutrons, perhaps more than one for each fission event, would be released. Thus a "chain reaction," in which each fission event emitted neutrons which in turn induced fission events in other uranium nuclei, might be possible.

By 1940, the established scientific facts justified assuming that a nuclear chain reaction could be achieved. The Second World War began with Germany's invasion of Poland in September 1939. The scientific developments mentioned above had been carried out in Germany at a time when the possibility of creating a chain reaction — possibly one of violent, explosive force — was freely discussed between scientists of many nations. It could only be assumed therefore that Germany would attempt to be first in producing such a device — a nuclear bomb.

During the 1930s, many of the most outstanding European scientists came to America to escape dictatorial regimes. More than most American scientists, they were aware of the dangers of National Socialism and undertook to awaken American officials at the highest levels to that danger. In July 1939, the emigre scientists, Eugene Wigner, Edward Teller, and Leo Szilard persuaded Albert Einstein to write a letter to President Roosevelt, alerting him to the danger of Germany developing a nuclear bomb of far greater explosive power than any other conventional bomb. As a result, the Advisory Committee on Uranium was established and held its first meeting in October 1939, with Lyman Briggs as chairman. The committee reported to the President's military aide, General E.M. Watson. The first funding for the committee was \$6 000 for the purchase of enough uranium to investigate the feasibility of designing a nuclear explosive.

The task became more urgent when, early in 1940, it became known through intelligence channels that the Germans were indeed working on the problem of the fission bomb (table 1). After June 1940, the American uranium program expanded rapidly. The Uranium Committee became a section of the National Defense Research Committee, and also established a working relationship with the British, who had independently started work on the uranium problem.

During 1940 and 1941, under the guidance of the Uranium Committee, several projects were begun, primarily in university laboratories. The most important results of these projects were the following:

- Uranium fission process. Experiments conducted at Columbia and Princeton Universities confirmed the model of nuclear structure developed by Niels Bohr and John Wheeler in Copenhagen (ref. 32). In particular, it was demonstrated that slow-neutron fission of uranium-235 produces, on the average, nearly three neutrons per fission.

- Chain reactions. In principle there are two types of chain reaction — the fast or explosive type — and the moderate type, in which a “moderator” is used to slow down the neutrons to speeds more likely to cause fission reactions in uranium-235. Enrico Fermi proposed the use of low-atomic-number materials with low neutron absorption properties as

Table 1. U.S. and German Research on Building a Nuclear Explosive, 1939 to 1943

<u>Time</u>	<u>United States</u>	<u>Germany</u>
1939	Einstein Letter (July) Uranium Committee (October)	Discovery of uranium fission German program established (August)
September 1, 1939 War starts		
1940	Section I of NDRC organized Plutonium experiment in Berkeley Isotope separation research Theoretical work	Distinguished German scientists brought into Program Feasibility report <i>positive</i> . Either carbon or D ₂ O moderations. No isotope separation Pu ²³⁹ selected as best potential fuel. D ₂ O selected as moderator.
1941	National Academy Study. Positive Feasibility (November) OSRD organized to take over the work	Many small-scale reactor experiments. Technical difficulties and administrative problems.
December 7, 1941 Pearl Harbor		
1942	Manhattan District organized (September) First reactor (December)	Heisenberg successfully makes subcritical assembly (October)
1943	Oak Ridge, Argonne, Los Alamos, and Hanford organized	British destroy Norwegian D ₂ O Plant (February) Plant returned to full production (June) American bombers attack D ₂ O facility (November). German military officers discontinue production, causing Germany to abandon effort to build nuclear weapon.

moderators. Heavy water (water containing only the heavy isotope of hydrogen, deuterium), beryllium metal, and graphite were regarded as prime candidates. But the time needed to build a separation plant and produce enough heavy water for a pilot chain-reacting "uranium pile" was too great; and beryllium was too scarce and required a level of purification beyond what existing technology could provide. Graphite was soon identified as the material of choice; fairly pure graphite was already in production for other purposes, so that improvements in existing industrial processes were believed feasible (ref. 33).

In 1939 and 1940, experiments on the slowing down of neutrons in graphite were carried out at Columbia University (ref. 34). Concurrent theoretical studies based on diffusion theory made it possible to predict the results to be expected in various arrangements of uranium and graphite moderator, including the number of neutrons that would escape from lattices of various designs and dimensions. At about the same time, Enrico Fermi and his colleagues at Columbia University hit on the concept of arranging separated chunks of uranium in a matrix of graphite moderator. Their idea was that, by separating the uranium into small chunks, the neutrons would escape into the surrounding moderator and be slowed down below the uranium-238 resonance capture range (i.e., discrete velocity ranges at which atomic nuclei absorb neutrons) before entering another chunk of uranium. The result would be to set the proportion of neutrons lost by capture in uranium-238 and that available for fission of uranium-235. The essential features of the chain-reaction "uranium pile" were thus produced and the first nuclear chain reaction was carried out in December 1942 (ref. 35).

● **Plutonium.** In 1940, Edwin McMillan, Philip Abelson, and Glen Seaborg, working at the University of California in Berkeley, bombarded uranium with neutrons and high-energy deuterons using Ernest Lawrence's newly constructed cyclotron. In so doing, they produced the first two transuranic elements, neptunium and plutonium (ref. 36). Both plutonium-238 and plutonium-239 were important discoveries. Plutonium-239 had slow-neutron fission properties similar to uranium-235 and could be used to produce a chain reaction. In May 1941, Ernest Lawrence suggested to the Uranium Committee that if a controlled chain reaction could be achieved with natural uranium, it might be possible to produce plutonium-239 in large amounts by neutron capture in uranium-238. The plutonium could be separated from the uranium by chemical means — a far simpler process than that of separating the uranium isotopes. The product would be a highly concentrated form of fissionable material with which a violently explosive device with less total weight could be built than with imperfectly separated uranium-235. The committee immediately incorporated Lawrence's suggestion into the program.

● Isotope separation. The separation of the uranium isotopes to obtain weapons-grade uranium-235 was one of the key technical constraints in the uranium program. In 1940 to 1941, three techniques were considered: the gaseous diffusion of uranium hexafluoride through porous barriers; the separation of isotopes by the centrifuge method; and the electromagnetic separation of isotopes. Under the emergency conditions of wartime, all of these methods were pursued and enormous investments were made in the construction of pilot plants. As things turned out, only the gaseous diffusion method was practical at the time.

By late 1941, work on the uranium project was far advanced. In its report of November 6, 1941, the National Academy of Sciences, which reviewed the project periodically, concluded that a bomb of superlatively destructive power could be made by bringing a sufficient mass of uranium-235 together quickly; that the mass required for explosive fission was between 2 and 100 kg; that the energy released by such a device would be equivalent to 300 tons of TNT; that the separation of uranium isotopes in sufficient quantity to devastate Germany's military capability could be achieved; and finally, that if an all-out effort were undertaken, fission bombs could be produced within three to four years.

This report to the President led to the complete reorganization of the uranium program. With the attack on Pearl Harbor and American entry into the war a month later, the urgency of the program greatly increased.

Until the end of 1941, the uranium program was carried out under the OSRD. In March 1942, Bush predicted that the fission bomb could be completed in 1944, and recommended to the President that the Army Corps of Engineers be brought in for the construction of full-scale plants. A new district of the Corps of Engineers was formed in August 1942 to carry out the "DSM Project" (Development of Substitute Materials), as the fission bomb project was designated. In September 1942, Brigadier General Leslie R. Groves was appointed head of the new "Manhattan Engineering District" (so named because its head office was then in New York) and given full responsibility for DSM, under the guidance of a Military Policy Committee chaired by Bush. The OSRD Uranium Committee continued to act in an advisory capacity, but responsibility for all research activities and production plant design, construction, and operation was rapidly transferred to General Groves and his Manhattan District staff. The intensified research and development effort was concentrated in several major organizations, in part by expanding existing laboratories, in part by setting up new facilities.

While a detailed account of the Manhattan Project is beyond the scope of this chapter, its efforts on the nature of American science and technology development were profound. The Manhattan Project created (or assigned new responsibilities to) research and development facilities which survived the project and played a critical role in post-war science

and technology development. The most important of them were the Metallurgical Laboratory of the University of Chicago, where Enrico Fermi and his group produced the first controlled chain reaction in December 1942; the Argonne National Laboratory, which became the center for the study of reactor designs of all types; Ernest Lawrence's Laboratory at Berkeley, which was assigned responsibility for developing the electromagnetic method of separating uranium isotopes and designing a production plant for the large-scale separation of uranium-235; the Clinton (Tennessee) Engineering Works, originally built for the production of uranium-235 by the gaseous diffusion method, which became the Oak Ridge National Laboratory and played a major role in the development of atomic power reactors; the Hanford (Washington) Engineering Works, at which the first great reactors to produce plutonium were located, and most importantly, the Los Alamos (New Mexico) Scientific Laboratory. Located at the top of an isolated mesa some 50 kilometers from Santa Fe, the laboratory was established in March 1943, with J. Robert Oppenheimer as director. At Berkeley, Oppenheimer had been carrying forward the theoretical work on fast-neutron chain reactions, and his job now was to achieve the ultimate goal of the Manhattan Project. (The key personnel in the Manhattan Project are shown in figures 12 through 14.)

The goal was to produce fission bombs. As other tasks of plutonium and isotope separation were transferred from scientific to plant engineering personnel, top-flight scientists from the various segments of the program were brought to Los Alamos to solve the ultimate problems of designing the bomb: in particular, determining the optimal method for detonating the critical mass and figuring out how the critical mass of uranium or plutonium would behave in the interval between the chain reaction and the explosion. By the spring of 1945 there were well over two thousand scientific and technical personnel at Los Alamos (ref. 37). Their efforts culminated in the test explosion of a plutonium implosion bomb in the New Mexico desert near Alamogordo, on July 16, 1945, followed by the dropping of nuclear bombs on Hiroshima and Nagasaki a few weeks later.

The Manhattan Project created an operating philosophy — a set of standard operating procedures — that was to be adopted for a variety of purposes, including the development of space and weapon systems during the post-war years. The project had shown, in Richard Nelson's words, what could be done where there was "a willingness to make large early bets on particular technological options and force these through, or engage in parallel efforts at very high cost." (ref. 38.) The nuclear bomb development program followed several paths simultaneously: plutonium or the separation of the uranium isotopes; electromagnetic or gaseous-diffusion separation of the uranium isotopes; alternative designs of



FIGURE 12. — *Three leaders of the effort to build nuclear weapons during the Second World War. From left to right, J. Robert Oppenheimer, who headed the Los Alamos Laboratory where the first atomic bomb was built; Enrico Fermi, who supervised the achievement of the first self-sustaining nuclear chain reaction; and Ernest O. Lawrence, who invented the cyclotron. (Both Fermi and Lawrence won Nobel Prizes.)*

nuclear bombs. It was the concurrent approach, as much as anything, that enabled the United States to produce a nuclear weapon before Germany did.

The crucial error the Germans made in their effort to develop a uranium bomb was to reject graphite as a neutron moderator in favor of heavy water. It turned out that the graphite samples used by Walter Bothe and his group to determine the neutronic properties of the material had impurities that resulted in larger than acceptable values of the neutron capture cross sections. Thus, the Germans would have to employ the only other available moderator, heavy water, in order to build their plutonium producing “uranium piles.” Once the German effort came to depend entirely on heavy water, it was probably doomed. There was only one plant, in Norway, that made heavy water, and it was vulnerable to raids by the British, who destroyed the plant in February 1943, and by the Americans, who put it out of commission for good the following November.



FIGURE 13. — *Major General Leslie R. Groves, U.S. Army. Groves headed the “Manhattan District” during the Second World War. This was the organization established to produce the first nuclear weapons.*

A second feature of the Manhattan Project which was to influence post-war American science and technology development was the tendency to locate government-sponsored research in the private sector rather than in government arsenals. Bush and the other OSRD members quite deliberately decided to circumvent the problems of working through civil-service establishments with little experience in large-scale development projects. Once the decision was made to build production facilities, their operation was assigned to some of the largest firms in the country. Thus for the Clinton Engineering Works, Westinghouse and General Electric were selected to manufacture the mechanical and electrical components and Tennessee Eastman to manage the facility; DuPont operated the Hanford Works; while until recently Union Carbide operated Oak Ridge National Laboratory.



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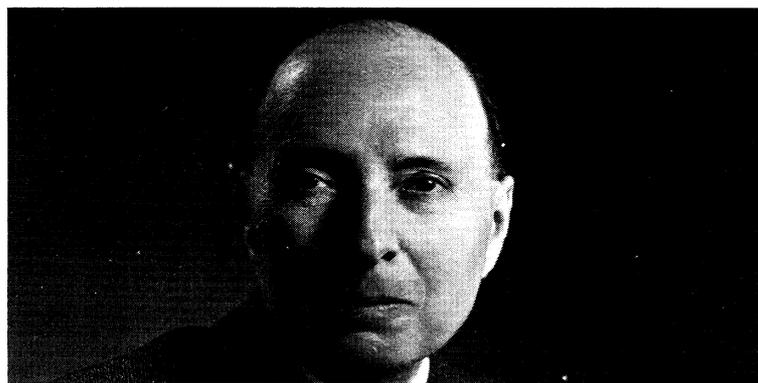
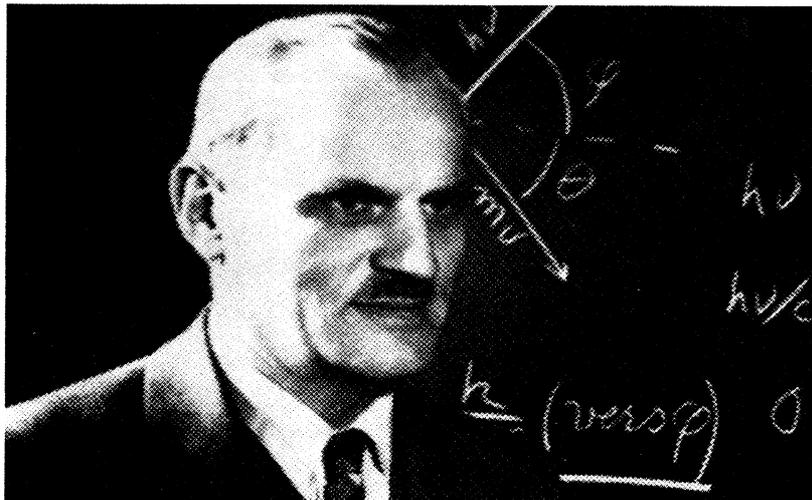


FIGURE 14. — *Three physicists who have had major influence on technology development and on public policy. Edward Teller provided the ideas for the first thermonuclear weapons, Arthur Holly Compton did the design calculations for the first successful nuclear reactor, and Eugene P. Wigner made important contributions to nuclear theory. (Wigner and Compton both won Nobel Prizes.)*

This emphasis on research and development conducted by the private sector had important repercussions over the next three decades. For many large weapons programs, project managers tended to avoid “in-house” arsenals and laboratories except where no qualified commercial sources were available. In the 1960s, the National Aeronautics and Space Administration and the Department of Defense let enormous base operation contracts, by which companies provided support services for entire installations — everything from trash collection to computer programming to mission control. The rationale was that this was the only way to assemble quickly the manpower needed to accomplish goals of national importance and (in theory) to disperse it when those goals were accomplished.

The Post-War Period: Origins of Government by Contract (1946 to 1957)

In 1945, very few people expected that American science and technology would return to its pre-war state. The genie had been let out of the bottle, and there was little inclination, even had it been possible, to put it back in. In his July 1945 report to the President, *Science — The Endless Frontier* (published in the same month as the Alamogordo test explosion, which it did not mention), Bush sketched an ambitious program of Federal support for basic research. For our purposes, the post-war period — from 1946 to the launching of Sputnik in October 1957 — can be taken as the period in which the basic institutional arrangements of American science came into being, some by act of Congress, some by executive order, some by agency regulations, and some by informal agreement between the sponsoring agencies and what, for lack of a better word, may be called their clients. Important long-term changes occurred in: Federal policies toward the support of basic research; Federal procurement policy; use of captive research organizations; policy regarding the uses of atomic energy; and philosophies of project management, especially in the larger weapons programs.

First, the Federal Government would continue to support basic research, and would do this through several agencies. Although the National Science Foundation was chartered by Congress in 1950 with the mission of supporting basic research, it was clearly understood (and affirmed by executive order in 1954) that this in no way preempted the research sponsored by other agencies. In 1946, the Navy had taken the initiative in sponsoring research when Congress created the Office of Naval Research, with the aim of sponsoring free, non-directed research, almost none of which was classified. In the same year General Dwight Eisenhower, as Chief of Staff of the United States Army, drafted a memorandum which was a blueprint for a continuing relation between the

services, civilian scientists, industry, and the university. The principles set forth in this document have dominated Federal research policies to this day:

- “(1) The Army must have civilian assistance in military planning as well as for the production of weapons . . .
- (2) Scientists and industrialists must be given the greatest possible freedom to carry out their research . . .
- (3) The possibility of utilizing some of our industrial and technological resources as organic parts of our military structure in time of emergency should be carefully examined . . . There appears little reason for duplicating within the Army an outside organization which by its experience is better qualified than we are to carry out some of our tasks . . .
- (4) Within the Army we must separate responsibility for research and development from the functions of procurement, purchase, storage and distribution . . . The inevitable gap between the scientists or technologist and the user can be bridged, as during the last war, by field experimentation with equipment still in the development stage . . .
- (5) Officers of all arms and services must become fully aware of the advantage which the Army can derive from the close integration of civilian talent with military plans and developments . . . In general, the more we can achieve the objectives indicated above with respect to the cultivation, support and direct use of outside resources, the more energy will we have left to devote to strictly military problems for which there are no outside facilities or which for special security reasons can only be handled by the military.” (ref. 39.)

Implied in Eisenhower’s memorandum was the distinction between basic theoretical research and development which was at the heart of the Manhattan Project. Without the theoretical research on the structure of the atomic nucleus and the applied research devoted to isotope separation and the creation of transuranic elements, the production of weapons-grade material would have been impossible.

Science, then, was to be more closely integrated with national technology development goals than at any previous time. But this meant that the legal framework within which research and development was pursued would have to be overhauled. In 1947, Congress passed the Armed Services Procurement Act which, while affirming the principle that contracts for services and supplies were to be let by advertising for bids, listed seventeen exceptions; the most important of these were

services purchased from educational institutions and services for experimental or developmental work (ref. 40). In 1948, Congress further authorized long-term research and development contracts, and provided for indemnifying contractors for losses incurred in certain kinds of developmental work. In 1949, many of these powers were delegated to civilian agencies. In the same period Congress did something to enable Federal agencies to compete with industry for the best engineers. In 1947, Congress authorized the Secretary of Defense (and subsequently, certain civilian agencies) to fill forty-five scientific and professional positions at salaries from \$10 000 to \$13 000, a range then equivalent to that of the highest ranking government officials. Congress intended these "Public Law 313" positions to be used for recruitment rather than retention, and each agency head was empowered to determine the appropriate salary within the bounds set by legislation.

Yet to officials in the new Department of Defense (DOD) and the Atomic Energy Commission (AEC), even these institutional arrangements did not go far enough in giving the agencies the expertise they needed. Particularly in weapon system development, where one firm might design the system and then bid on the hardware, there were serious conflict-of-interest problems. What DOD and the AEC attempted was, for quite different reasons, to create "captive" non-profit contract research organizations working for one sponsor. The best known of the defense-oriented centers are the RAND Corporation (which began as a contract between the Army Air Corps and Douglas Aircraft in 1945) and the Institute for Defense Analyses. The idea underlying these organizations was that they could provide disinterested advice to their sponsors; that individual researchers, freed from routine administrative tasks, could conduct research well in advance of the sponsoring agency's current needs; and that, by their existence, they would serve as catalysts for innovation in the client agency (ref. 41). Although practice did not always conform to theory, an organization like RAND could play a significant role in shaping agency programs at the earliest, the conceptual, stage.

The AEC, on the other hand, created a different kind of captive organization. Established by Congress in 1946, the AEC was charged with three different, and not entirely compatible, tasks: to produce weapons-grade nuclear materials for the Department of Defense; to develop and then transfer reactor technology to the private sector; and to regulate commercial reactors, once they came on line. The AEC deliberately chose not to operate its own laboratories, although the Atomic Energy Act of 1946 specifically provided for Federally-conducted research and development. Instead, the AEC contracted with private organizations — universities and for-profit firms — to operate its laboratories: Los Alamos, Oak Ridge, Argonne, Hanford, Lawrence's

Radiation Laboratory, and the rest. The universities received almost two-fifths of the funding for R&D contracts (ref. 42). AEC managers were aware of the role played by university-managed laboratories in the Manhattan Project; and they assigned to the universities functions performed by other agencies in their own laboratories.

The first success of the new Atomic Energy Commission was the creation of the first thermonuclear weapons. Under Edward Teller's leadership much of the basic conceptual work had begun during the war. When the war ended, there was great pressure to dismantle the nuclear weapons complex, and many people involved in nuclear weapons work returned to their pre-war pursuits. There were a few, however, among them Norris Bradbury who succeeded J. Robert Oppenheimer as Director of Los Alamos, and a group of people around Ernest Lawrence in Berkeley, who recognized that the nuclear weapons complex created by the Manhattan Project would have to be maintained and expanded. In their post-war work on thermonuclear explosives, Teller and his collaborators drew on the people and the facilities of Los Alamos and the other institutions of the old Manhattan Project that became part of the new Atomic Energy Commission. The Commission retained the institutional arrangements under which the Manhattan Project operated, and the University of California stayed on as the contractor that operated the Los Alamos Laboratory.

Because the nuclear weapons complex remained more-or-less in existence at the end of the war, it was possible to verify Teller's brilliant theoretical insight quickly and show that it would indeed be possible to create thermonuclear explosives. However, the political controversy surrounding the decision to develop these weapons also had another consequence relevant to the management of technology development. Teller and some of his collaborators felt that in the effort to spawn new technologies that were highly classified and politically controversial, it would be important to introduce an element of competition within the government-contractor community. Accordingly, they proposed that another nuclear weapons development laboratory be established with roughly the same functions as those carried out by Los Alamos. As a result of their proposal, a branch of Ernest Lawrence's Berkeley-based Radiation Laboratory was established at Livermore, California (about 65 kilometers east of Berkeley) in 1952. In due course, the new laboratory became independent of its parent (it is now called the Lawrence Livermore National Laboratory) and it has, along with the Los Alamos Laboratory, made vital contributions to the development of nuclear weapons (fig. 15). The competition that Teller felt was necessary has proved to be very beneficial and indeed, other agencies have found it worthwhile to build "internal" competition of this kind into their programs.



University of California, Lawrence Berkeley Laboratory

FIGURE 15. — *An aerial view of the Lawrence Livermore National Laboratory which was established in 1952 to develop thermonuclear weapons technology. The Laboratory's administrative building is in the corner near the bottom of the photograph.*

The policy of contracting out technology development was compatible with as much or as little technical direction as the AEC considered desirable. At one end, the basic research carried on at the national laboratories was almost entirely free of technical control, except where the quality of the scientist's work was being evaluated. At the other end, the AEC was heavily involved in project-type work, notably in the development under Captain (later Admiral) Hyman G. Rickover (fig. 16) of reactors for the propulsion of submarines and other naval vessels. The important point, as far as Rickover was concerned, was that although he was a military man, he was forced to work through a civilian agency to achieve his ends. Rickover succeeded by inventing a unique organizational method. He had himself appointed to two jobs: one as head of the AEC's Naval Reactors Program, and the other as Director of Nuclear Propulsion in the Navy's Bureau of Ships. Rickover saw that by occupying similar positions in both agencies, he could cut the usual red tape, allowing him to justify the program for military reasons in his capacity as a naval officer and then using his position of authority in the AEC to organize the development laboratories needed to create the reactors. Once the reactors were developed by the AEC, the process of transferring the technology back to the Navy had to occur. This arrangement proved to be highly effective and it remains in force to this day. Rickover also adopted Teller's idea of competitive technology



Official U.S. Navy Photo

FIGURE 16.—*Admiral Hyman G. Rickover. Rickover led the development of nuclear powered submarines.*

development centers by using both of the major contractors with whom he worked to establish such institutions. He provided the resources to build the Knolls Atomic Power Laboratory (General Electric) and the Bettis Laboratory (Westinghouse), and both have been extremely important in providing basic technology for nuclear reactors for military as well as civilian purposes.

From the very beginning, the joint Navy/AEC nuclear program was oriented toward transferring the technology of nuclear reactors to the civilian sector. The president of Westinghouse, Gwilym Price, had to overcome the attitude of company officials that "Westinghouse made its profits on conventional products and that the company should give them first priority . . . Price realized that such an attitude would never give the company competence in radically new technology like atomic energy. Price and others . . . saw that Rickover was offering an opportunity that the company dare not miss. Westinghouse not only needed Navy contracts but also had to be in a position to enter a future civilian market for power reactors." (ref. 43.) It is no accident that all of the power reactors currently in use in the United States are either pressurized water

reactors or boiling water reactors of the type developed by Westinghouse and General Electric for the Navy. It may be that other technologies, such as the high temperature gas cooled reactor, might have been further developed but for the technical decisions reached early in the nuclear propulsion program. The point is that both contractors were strongly encouraged to think about and develop ideas for commercial power reactors, and that the present nuclear power industry is a product of the nuclear submarine program.

This account of the nuclear propulsion program raises a broader question: To what extent did the major development programs of the 1950s and early 1960s represent a change, if not an improvement, over the techniques used in the Manhattan Project? As Sapolsky notes, the atomic bomb dropped on Hiroshima was the first of the modern weapons and the last of the old. For all its technical sophistication the bomb was developed "apart from the ancillary equipment upon which its effectiveness depended . . . The physical size of the bomb was determined not by the limits of technology, but by the dimensions of the bomb bay doors of a B-29, an aircraft designed several years before the bomb." (ref. 44.) As weapons came to be seen as complex systems of interrelated components — for instance, the airframe, guidance system, warhead, engines, and so on of a cruise (air-breathing) missile — two systems development philosophies evolved: that of the Air Force's Western Development Division, which managed the Air Force Intercontinental Ballistic Missile (ICBM) Program, and the Navy Special Projects Office, which oversaw the development of the submarine-launched Polaris missile.

Specifically, by 1953, RAND Corporation scientists and an Air Force Strategic Weapons Evaluation Committee chaired by John von Neumann had concluded independently that an ICBM was technically feasible. In early 1954, the von Neumann Committee recommended that the United States undertake an ICBM program on a highest-priority basis. On the basis of this recommendation, the Air Force established the Western Development Division under then Brigadier General Bernard Schriever to direct and coordinate its ballistic missile programs. The Western Development Division was a blank-check outfit set up to run a crash program to close the missile gap. Under Schriever's philosophy of concurrency, production and operations were telescoped together, even while research and development were proceeding. Concurrency meant "simultaneous work on basic and applied research, vehicle design, component design, test facility design and construction, component and system testing, the creation of production facilities, and the design, proof, and test of launch site facilities without which the missile would be impotent." (ref. 45.) In this sense, Schriever perfected the methods first pioneered by the Manhattan Project.

Schriever's approach to program management was equally radical. The case for hiring systems contractors to manufacture and integrate the components was not open and shut. The Army, at its Huntsville Arsenal, was capable of developing weapon systems (for example, the Jupiter intermediate-range missile) as complex as the Air Force's Thor intermediate-range missile. In essence, the Air Force turned to private contractors because it had neither the depth of competence found in Army laboratories nor the time to recruit engineers. In addition, the Air Force preferred to foster a civilian aerospace industry in peacetime. The research and development capabilities were there, ready to be exploited. Moreover, many Air Force weapons managers believed that, in contrast to the Army's arsenal system, their relations with industry significantly shortened the time period necessary for weapons systems development.

While retaining ultimate responsibility for its programs, the Air Force delegated to civilians every aspect of the research and development cycle. In several cases the Air Force selected a prime contractor for technical integration, testing, assembly, subcontracting, and the like; this was how the Bomarc missile and the B-58 bomber were developed. In its ballistic missile programs, the Air Force worked through several associate contractors for components and subsystems and hired a separate organization, the Ramo-Wooldridge Corporation (subsequently TRW, Inc.), to serve as technical director of the program. Excluded from hardware production, Ramo-Wooldridge was both line and staff; the former, insofar as it did systems engineering and provided technical direction for the Western Development Division, and the latter, inasmuch as it also did long-range planning studies for the Air Force. There is no doubt that Schriever's brilliant and inventive approach to pioneering new organizational arrangements has profoundly affected the management of technology development in the United States.

The Navy's Special Projects Office was like the Air Force's Western Development Division in that it was at the farthest remove from the pre-war arsenal system. But to a much greater extent than the Air Force in its ICBM program, the Navy provided the technical direction for Polaris. This did not mean that the Special Projects Office had any special design capability. Rather, it was the design preferences of the Special Projects Office, and not those of its contractors, that dominated Polaris. Unlike the Air Force's missile programs, "the technical alternatives were not the product of a single organization, nor were they filtered through a single organization. The Special Projects Office dominated because it was dependent technically on many contractors, not one . . . Always in the boundary areas between subsystems, but often also within subsystems, the Special Projects Office branches and, most importantly, the Technical Director, had the opportunity to compare competing proposals." (ref. 46.)

The National Aeronautics and Space Administration (1958 to 1970)

Despite the sophistication of the research and development we have described, the United States was unprepared for the Russian announcement, on October 4, 1957, that a satellite — Sputnik — had been placed in near-earth orbit. Sputnik, which was Russia's contribution to the 1957-1958 International Geophysical Year, seemed to represent the greatest threat to national security since the German nuclear weapon program became known. So far-reaching an event was perceived and acted on in different ways. It led President Eisenhower to appoint James Killian of MIT as Special Assistant to the President and to transfer the Science Advisory Committee from the Office of Defense Mobilization to the Executive Office of the President. Reconstituted as the President's Science Advisory Committee (PSAC), it gave the scientific community greater access to the White House than at any time since the OSRD. With the Special Assistant as Chairman of PSAC and, in 1959, of the newly created Federal Council for Science and Technology, Eisenhower hoped to obtain a body of politically neutral technical experts to provide disinterested advice at all levels of the government.

But the most important consequence of Sputnik was the decision by the President and Congress to consolidate and make more effective an American space program. This decision led to the 1958 Space Act and the creation of a new agency, the National Aeronautics and Space Administration. NASA would be a civilian agency, with the National Advisory Committee for Aeronautics as its nucleus; it would be headed by a strong administrator, rather than a committee; it would have authority to let large development contracts; in a vaguely-defined way, it would coordinate its programs, especially those having military value or significance, with those of DOD; and it would acquire certain installations (in addition to those inherited from the National Advisory Committee for Aeronautics) and projects needed to carry on its work. In late 1958, by executive order NASA acquired the Jet Propulsion Laboratory, a government-owned facility operated under an Army contract by the California Institute of Technology which ultimately acquired the role of developing unmanned spacecraft to explore the solar system. In 1960, NASA acquired from the Army the rocket team headed by Wernher von Braun located at the U.S. Army's Redstone Arsenal in Huntsville, Alabama. Von Braun and his colleagues were already working on the Saturn rocket and, after President Kennedy's May 1961 decision committing the United States to land a man on the Moon and return him safely before the end of the decade, the Marshall Space Flight Center (of which von Braun became the director) was placed in charge of large launch vehicle development. Additionally, NASA established the Goddard Space Flight Center in Greenbelt, Maryland, in 1959 to direct

the agency's unmanned earth orbiting satellite programs. Prominent NACA and NASA leaders are shown in figures 17 through 21.

The lunar-landing decision was the real turning point for NASA. Under T. Keith Glennan, the first NASA Administrator (1958 to 1961), the agency had grown in a steady, undramatic way. Under his successor, James E. Webb (1961 to 1968), NASA exploded, with a budget of \$5.5 billion (1965 dollars) and 36 000 government employees supervising 400 000 contractor employees by 1965 to 1966. In considering NASA's success in accomplishing the lunar landing, we must ask what, in organizational terms, allowed NASA to get the job done. Five features seem noteworthy (ref. 47):

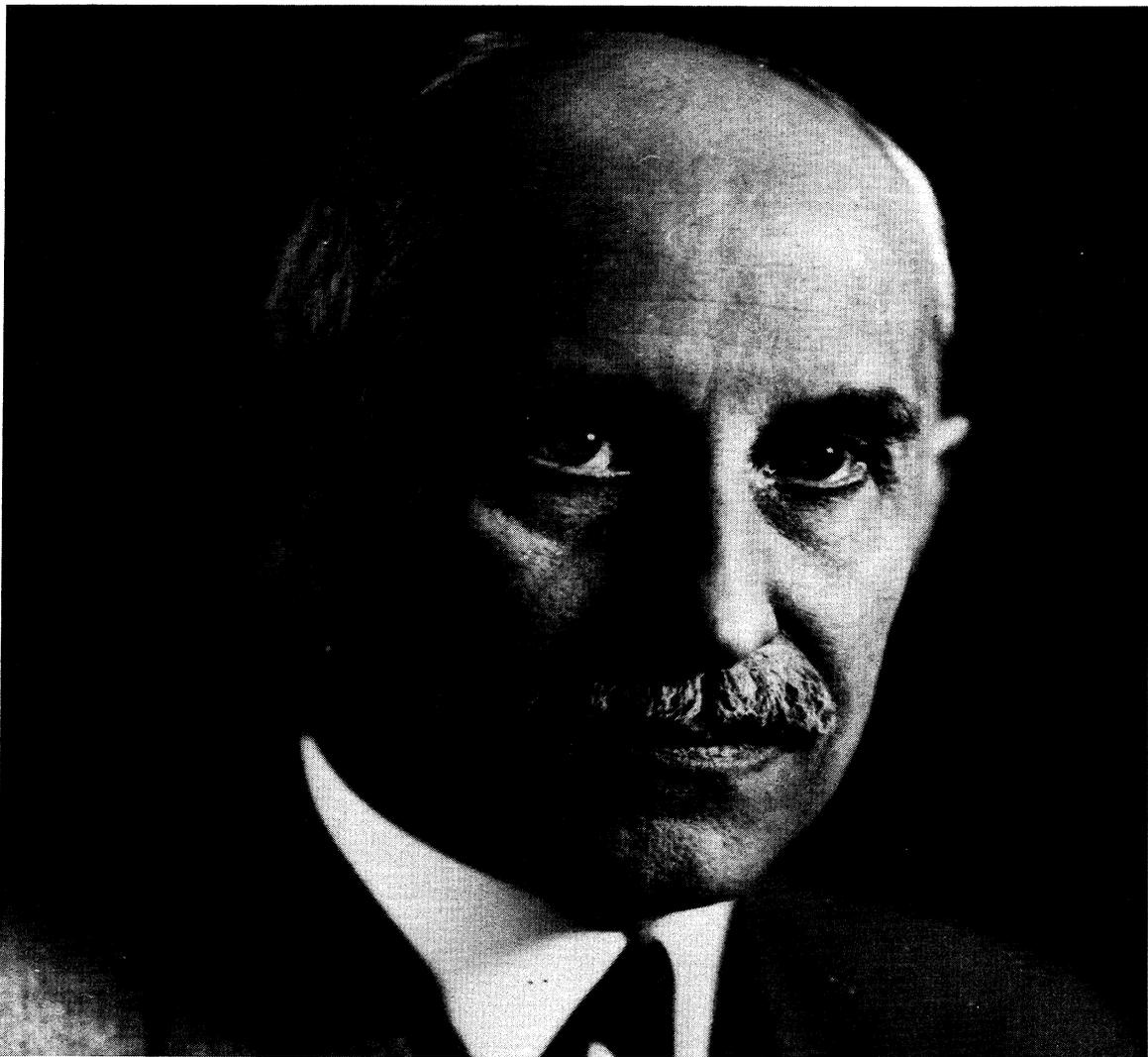


FIGURE 17. — *Orville Wright who, with his brother Wilbur, achieved the first sustained powered flight in 1903. Wright later served as a member of the government's National Advisory Committee for Aeronautics.*



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FIGURE 18.— *Three leaders of the National Advisory Committee for Aeronautics: Jerome Hunsaker, a member (and chairman) of the Committee; James H. Doolittle, the last Chairman of the Committee; and Hugh Dryden, for many years the Staff Director of the Committee.*



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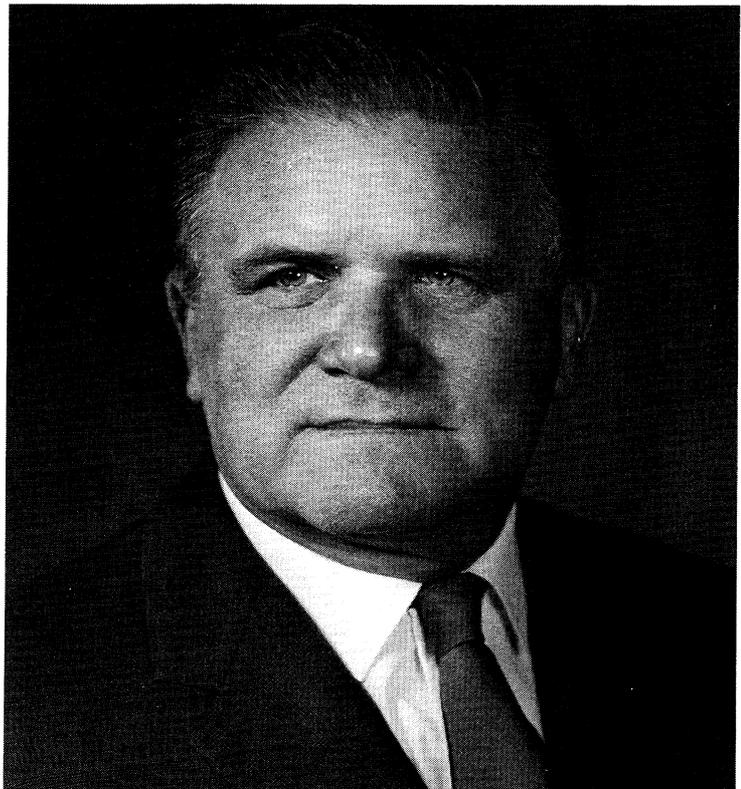


FIGURE 19. — *The first two Administrators of the National Aeronautics and Space Administration, T. Keith Glennan and James E. Webb.*

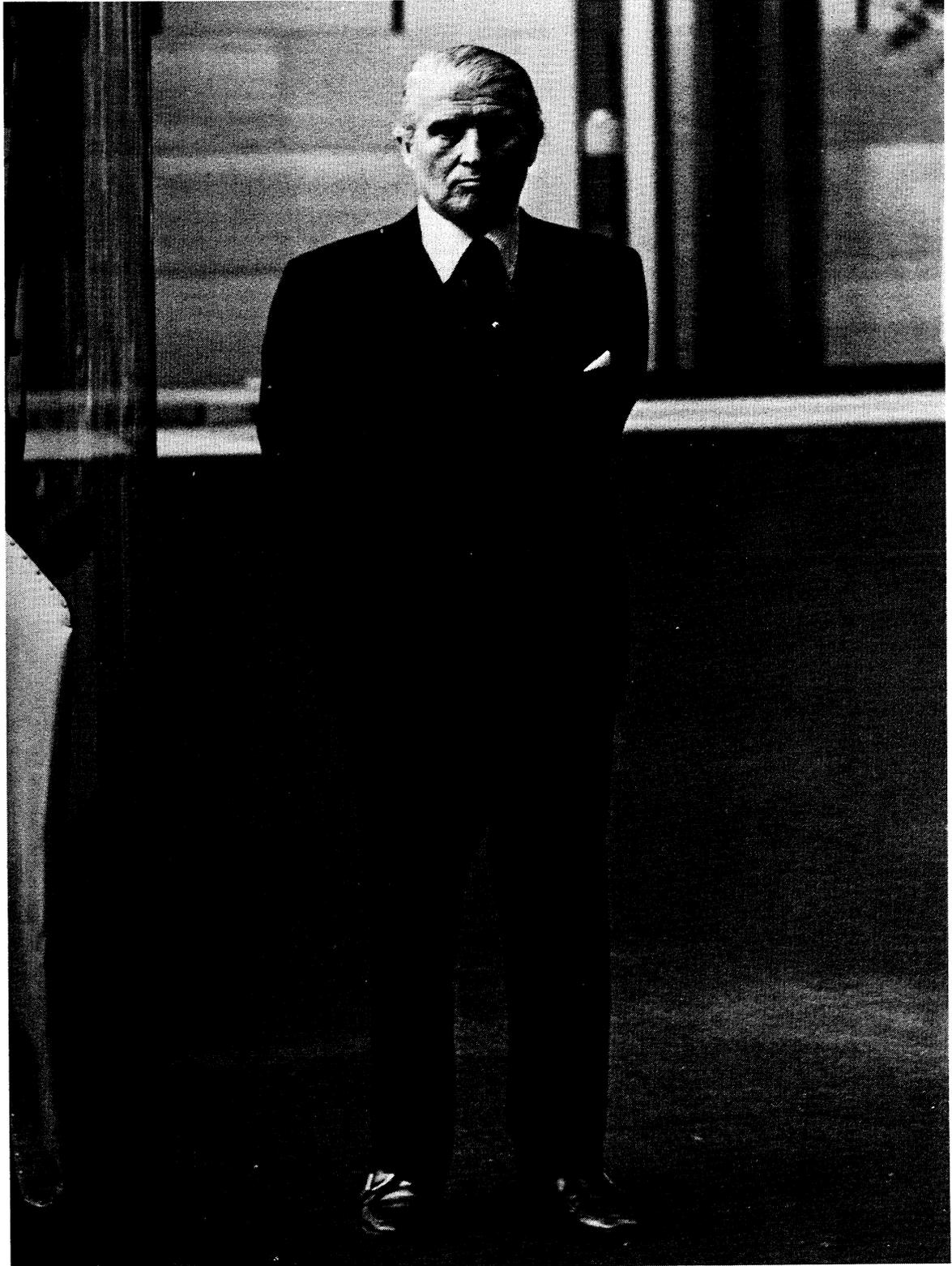


FIGURE 20. — *Wernher von Braun, whose team led the development of the Saturn rocket that put the Apollo astronauts on the Moon, was also one of the early pioneers of rocket technology in the 1930's.*

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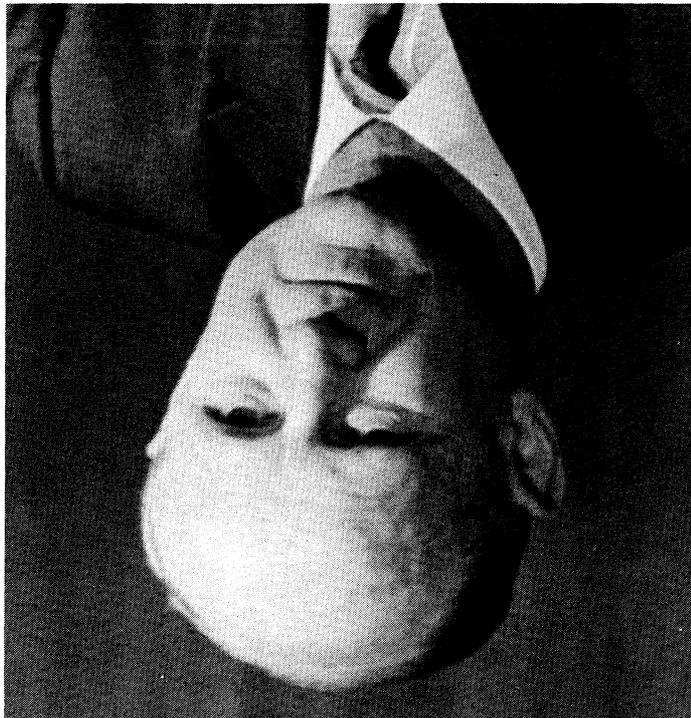


FIGURE 21.—Two important leaders of NASA, William Pickering and George Low. Pickering was, for many years, the Director of the Jet Propulsion Laboratory where unmanned planetary exploration is the primary mission. Low was a leader in the manned space flight effort first as Deputy Director of the Manned Spacecraft Center, then as Apollo Spacecraft Project Manager, and finally as Deputy Administrator of NASA.

1. NASA in the early 1960s had an organizational flexibility unmatched by any agency of comparable size. In this period NASA had no formal agencywide long-range plan; no general advisory committee of outside scientists, such as those established for the AEC and DOD; no centralized range structure for tracking, data acquisition, and mission control; no inspector-general, chief scientist, or chief engineer; no central planning staff attached to the Office of the Administrator. These functions were handled in other, much more decentralized ways. Moreover, the absence of a plan or general advisory committee rescued NASA from becoming captive to policies which might cease to be relevant. To maintain this flexibility and to adapt the agency to change, there were frequent reorganizations, notably in 1961, 1963, 1965, and 1967. But they were not ends in themselves. They were designed, rather, to turn the agency from one set of programs to those of a quite different sort. For NASA was quite vulnerable. It had to stake a claim to territory of its own, rather than become a supporting arm of the military services or, like the AEC, a supervisory agency with a small in-house staff and contractor-operated facilities.

2. Another element in the success of the NASA organization was the unusual power of the Administrator. The Administrator could appoint people to "excepted" positions in the civil service, award contracts without competitive bidding, reprogram funds within appropriation accounts and transfer between them, and devise and administer a custom-tailored entrance examination for new employees, among many other things. Examples such as these represent influence within the system, not a departure from it; variances from the norm were allowed by Congress, the Bureau of the Budget, and the Civil Service Commission. Without the authority, for example, to negotiate major contracts, it is unlikely that the lunar landing would have occurred on schedule. It is hard to imagine Apollo (the lunar-landing program) or a major unmanned program like the Orbiting Observatories becoming operational, had the agency been rigorously bound by competitive bidding or other rules that would have constrained its ability to choose its sources for acquiring hardware. The power available to the Administrator depended on congressional willingness to tolerate practices that the legislature might have challenged elsewhere. And when that toleration ceased, particularly after the January 1967 fire that killed three astronauts, NASA also fell victim to red tape. Thus by 1969, it took an average of 420 days to process a contract involving a procurement plan, 3 months for Headquarters to review the plan, and 47 days for Headquarters to approve a negotiated contract.

3. One of the most important aspects of Apollo was the speed with which the crucial administrative and program decisions were made and the prime contracts awarded. Except for the decision to go to all-up

testing (the testing of all the major Apollo components together), the major Apollo program decisions were made between August 1961 and July 1962. Had they been stretched out over a longer period, it seems unlikely that they would have received the support that they did. In contrast, it took almost four years to reach the decision to build the Shuttle and to decide on the final configuration of the system. The first serious conversations about the Space Shuttle were conducted in 1968 in connection with then-Administrator Paine's call to start thinking about a "post-Apollo" program for NASA. The Shuttle program itself was not finally approved by President Nixon until January 1972.

4. The NASA leadership has, for the most part, recognized the political importance of the space program. There is no doubt that the space program that NASA operates has strong political content and popular appeal. The planetary exploration program and the spectacular pictures it has provided of new worlds in the solar system, the Apollo program, and, of course, the Space Shuttle have all been used as demonstrations of American national competence in technology. Generally, the nation's political leadership has been sensitive to the political advantages of a strong space program and has provided the necessary support to carry it through.

5. As mentioned before, NASA was and is remarkably decentralized for so large an agency. Authority was, and is, delegated to the centers to negotiate contracts up to a specified amount, to transfer funds between programs, to start new research tasks without seeking specific authorization, and to shift manpower from one division to another. The strategy of senior management was and is, to give the centers what they need to get the job done, but not so much that their work would lose its relevance to the agency's mission. During the 1960s, the "research" and "development" centers* tended to become more like each other; centers with a mixture of projects weathered the budget and manpower cuts at the end of the decade better than those with one or two large development programs that were phasing down.

By 1969, most of the centers, particularly Marshall, were in the early phases of a "withdrawal process" brought on by cuts which began in 1967 and were to continue uninterrupted to the late 1970s. The Electronics Research Center (ERC) was transferred from NASA to the Department of Transportation in 1970 and one or two other centers narrowly avoided closure. The problem of new roles and missions could be alleviated by the centers, but only in part. NASA officials conceded in principle that a

* The "research" centers are Ames, Langley, and Lewis Research Centers. The "development" centers are the Johnson Space Center and the Marshall Space Flight Center.

less-than-best laboratory might be closed: if it had served its initial purpose; if there was no likelihood that a new role for the laboratory could be found; and if the closing down of the laboratory would not leave a significant gap in the national capability to do technology development work. But most of the centers were adaptable and many had gone through at least one reorganization in the late 1950s, moving from aeronautics to launch-vehicle development, or from development work on guided missiles to lunar and planetary probes, as with the Jet Propulsion Laboratory. By 1969, another cycle of reorganization was underway. Yet the more subtle changes in a center's mission could only occur very gradually.

To summarize, NASA prospered during the early 1960s, because of its administrative flexibility, the political skills of its senior officials, the delegation of program management to the field, and the timeliness with which the important decisions were made. But the same elements were not enough to enable NASA to weather the severest test to which any mission-oriented agency can be put: namely, how to react to the completion of the mission. And this, as we shall see, is not NASA's problem alone.

Epilogue: The 1970s and After

In retrospect, the early 1960s were the golden age of Federally-sponsored, mission-oriented technology development. Beginning in about 1967, the budgets of the three largest mission-oriented agencies — DOD, AEC, NASA — accounting for 90 percent of all Federal research and technology development outlays, began to decline (ref. 48).

Part of this decline was due to specific developments such as the Vietnam War, skepticism over the future of commercial nuclear power (confirmed, for many, by the Three Mile Island incident in 1979), and the phasing down of Apollo. Yet the decline was general, part of systemic changes in the American economy which continue to the present. Where spending on all kinds of research and technology development from all sources amounted to 2.9 percent of the gross national product in the mid-1960s, it had fallen to 2.2 percent by 1978. During that period, Federal spending, especially on applied research, prototype, and demonstration programs fell from 1.9 percent to 1.1 percent of the gross national product (ref. 49). Clearly, such a continued trend has major implications for American economic growth or lack of it. We cannot enter into so enormously complex a subject here, but we would like to examine briefly the efforts of the technology-based agencies to adjust to cutbacks.

The fate of the AEC was the most dramatic. It disappeared. In 1974 to 1975, its regulatory functions were transferred to the newly-created

Nuclear Regulatory Commission and its other activities were given to the newly-created Energy Research and Development Administration. In 1977, the Department of Energy was created, assuming overall management of all Federal programs related to energy. The AEC laboratories tried to adjust. As early as 1964, the AEC had issued guidelines for laboratories wishing to perform non-nuclear work for other customers: The proposed work should not lead to an increase in the size of AEC or contractor staff, should not require new facilities, should not be subcontracted, and should be done only if the other sponsoring government agency could not get the work done as conveniently by private industry (ref. 50). Within these guidelines, the laboratories worked for a variety of clients; Oak Ridge has worked for DOD, NASA, the Office of Saline Water in the Interior Department, and the Public Health Service; while the Lawrence Livermore National Laboratory has carried out non-classified work, most recently in the area of superconducting materials. Whether such work is a holding action or will turn into a new mission remains to be seen.

Defense technology development was hit particularly hard in the late 1960s. Basic academic research — a precondition for certain kinds of defense technology — fell from \$137 million in 1965 to \$108 million in 1974 (ref. 51). Only 4 percent of the total was classified, as universities began to withdraw from certain defense projects. Nor were matters improved by the “Mansfield Amendment” attached to the 1970 defense authorization act, which forbade the use of DOD funds to finance any research unless, in the opinion of the Secretary of Defense, it had “a potential relationship to a military function or operation.” (ref. 52.) The case of NASA is particularly interesting. Apollo and its successor, Skylab, were completed by 1974. Long before then, it was apparent to agency officials that to maintain a manned spaceflight program, NASA would have to confront the problem of deciding upon the next major thrust in space technology. The answer to that problem was the Space Shuttle. The main lines of the Shuttle program were drawn up by NASA task forces in 1968 and 1969, modified in 1971, and approved by President Nixon in January 1972. Briefly, the Shuttle is a partially reusable launch vehicle, consisting of an orbiter, two recoverable solid-fuel, strap-on boosters, and an external fuel tank, which is jettisoned shortly after the main engines have used all the fuel. The orbiter is designed to be launched like a rocket, operate in near-earth orbit for up to several days and, returning to Earth, land like a glider (fig. 22). It is designed for a variety of functions: to conduct experiments in zero gravity, place communications or weather satellites in orbit, and eventually, to supply an orbiting space station.

The Space Shuttle system, then, is radically different from Apollo. For all its sophistication, Apollo was simple and its principles were well

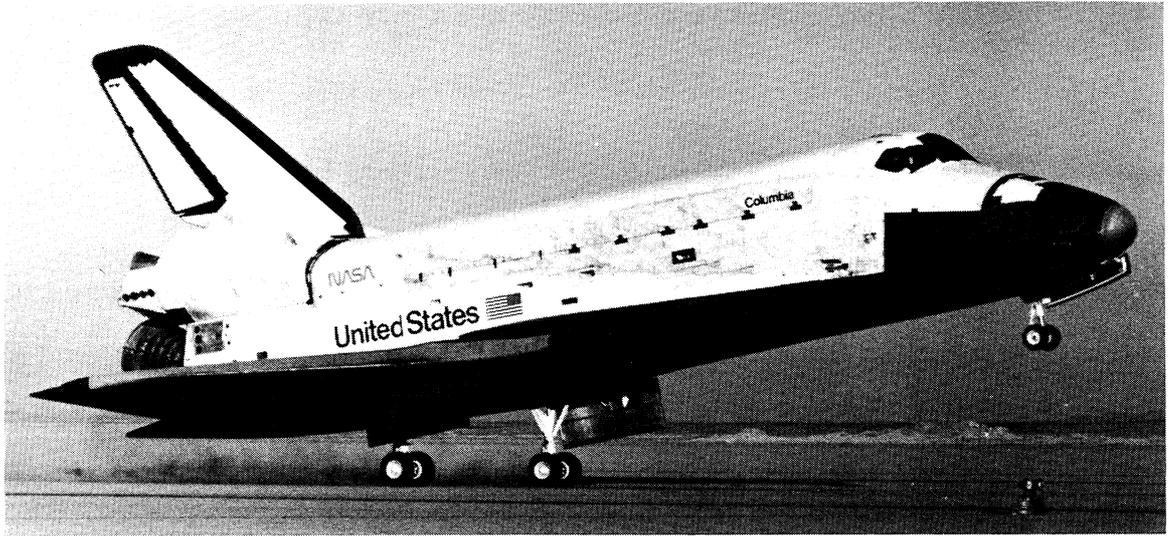


FIGURE 22.— *A landing of the Space Shuttle "Columbia."*

understood. No new technology was required in most instances. The theory of rocket propulsion was worked out in detail by Konstantin Tsiolkovsky, Hermann Oberth, and Robert Goddard between 1900 and 1920, and the atmospheric entry problem had been solved in the 1950s in connection with the ICBM program. Perhaps the only application of new technology in the Apollo program was in the area of guidance and control, since the lunar landing module required a completely automatic electronic control system. The Space Shuttle is very much more complex and did require the development of new technology. The orbiter must survive the shock of launch and reentry and then, given one opportunity, make a "dead stick" landing. Moreover, the management of the Shuttle program brings with it problems more like those of a commercial enterprise than of a government agency. These include how to market the Shuttle and attract paying customers; how to set user fees so that NASA will at least recover direct costs; how to screen proposals for manufacturing operations in a zero-gravity environment; how to improve the orbiter's ability to operate beyond near-earth orbit. With most expendable rockets to be phased out, NASA has staked a great deal — some would say, almost everything — on the success of the Shuttle.

In this chapter we have discussed the most important development in Federally-sponsored technology development over a span of 30 to 35 years. It would be presumptuous to extrapolate from that period to the major programs of the 1980s and beyond. But certain features of Federally-sponsored technology development can be discerned. Three are especially important:

1. The role of new technology, especially in information processing, will continue to increase. Along with the revolution in

microelectronics, there has been a major change in how we process information. Small electronic components make it possible to construct compact and powerful computers that can now perform analysis and other decision-aiding functions which once required hours or even days of work, in a few seconds. Since the size of electronic components can be reduced further, up to a factor of a thousand, the upper limit of this technology has not yet been reached (ref. 53).

2. At the same time, the need for a standard programming language to replace the dozens of languages now used by DOD has never been greater. Thus the Army has a major project, VIABLE, to standardize all of its data processing activities, while the Air Force, in January 1983, awarded a contract to Sperry Corporation to replace all its computers. Most important, DOD has sponsored the development of a single language, ADA, which is intended to replace all of the languages now being used for military systems. This trend towards standardization is certain to continue and will spread to other areas — for example, to the creation of knowledge-based systems, the automatic assembly of software parts, the development of programs which can be built from existing parts, and the creation of very high-level languages with their own control and data structures (ref. 54).

3. Finally, the role of the government laboratory is likely to change. Given the need of many laboratories to broaden their missions, to maintain their current sponsors or else find new ones, they will have to become more flexible than they now are. Consider that few laboratories do any production work and that increasing numbers of installations rely on contractors for base operations. The laboratory's function becomes, more than ever, that of generating new ideas leading ultimately to operating systems. In the remainder of this book, we will look at the problems involved in the absolutely crucial role of the research installation as generator of new ideas, and we will suggest some improvements in the way this role is handled.

CHAPTER IV

Features of Technology Development Laboratories

On Being the Right Size

In the preceding chapters we discussed the history of research institutions and some of their accomplishments. It is important to recognize that the institutions with which we are dealing are not new and have, in fact, evolved over some three hundred years. The merit of the historical approach is in demonstrating the provisional, time-bound nature of *any* institution organized to do fundamental or applied research. What we will attempt in the remainder of this book is to analyze in detail how contemporary research and development institutions are actually managed, and to suggest how the management can be improved.

We touched earlier on the achievements of modern research institutions. Nuclear technology, space technology, and electronic technology such as transistors and integrated circuits have been developed in laboratories of the kind that are the subject of this book. It might also be worthwhile to talk about some failures. Paradoxically, one cannot say that there have been any failures in the development of a particular technology once its physical principles became known. But while technology developments tend to be successful, certain technology development institutions have failed, especially in the Federal sector. Failures in the commercial sector are also known, but in that case we have measurable failures of products rather than of laboratories. In many cases, fully-developed products were brought to the marketplace and for one reason or another — price, inefficiency, supersession by more efficient competing technologies — turned out to be commercial disasters. Some of the best-known of these failed technologies include the rotary engine, Corfam, a synthetic material developed by DuPont, the electronic facsimile transmission system developed by Xerox, and fluidics, a technology using liquids or gases to perform functions ordinarily performed by electronic devices (ref. 55). Despite intensive marketing research and advertising campaigns, these products or technologies were things for which there was either no demand or not enough to justify mass production.

Failure also means the closing of a specific institution after the objective for which it was first organized was achieved.* A good example is the U.S. Naval Radiological Defense Laboratory once located at the Naval Shipyard in San Francisco. Its mission was to apply modern methods of nuclear safety technology to decontaminating naval vessels subject to nuclear attacks. The laboratory was established in 1946 to 1947, and during the next decade achieved most of its goals; decontamination techniques for ships now exist that are probably adequate for what needs to be done. Why, then, was the laboratory shut down? Probably because management recognized too late that its mission was completed and that the organization had to find new problems sufficiently important to justify its continued existence. Either there were no such problems or they were not pursued vigorously enough. The laboratory finally closed in 1968 on the ground that there simply was nothing left for it to do. The laboratory had made its contribution to resolving an important technical problem. That was not enough to save it.

In the private sector, technology development laboratories also occasionally fail. During the 1960s, the Northrop Corporation felt that it had an important future in space technology. Accordingly, a corporate space laboratory was established. Unfortunately, the timing of the step taken by Northrop was not very auspicious. The company's space laboratory was started just as spending on space technology by the Federal Government was beginning to decline. Despite the quality of the people Northrop hired, the management of the company finally concluded that the space laboratory could not be sustained and closed the institution.

Let us take the analysis a step further. Why are some laboratories closed once their missions are accomplished and why do others remain

* Some reviewers wondered why a laboratory which *completed* its mission should be considered a failure. They would argue that it is not failure, but success, if an agency successfully solves a problem or carries out an agency mission, even if in so doing it works itself out of a job. For two reasons we disagreed. The first is that it is inherently inefficient to create a separate institution, staff it with scientists and engineers, and spend perhaps several millions of dollars on equipment, only to shut it down at a certain point. The same objection would hold for a contractor-operated facility, even if the cost to the government were partially concealed. We concede that special circumstances might justify such an arrangement, although the creation of a new division within an existing laboratory might be less costly.

The fundamental objection to regarding the completion of a mission as a criterion of success is that, in such cases, "mission" is construed too narrowly. A laboratory set up to develop one kind of catalytic converter or guidance system could probably make contributions in other aspects of pollution control or navigation technology. By closing the facility after its original mission — or rather, assignment — is completed, the corporate or government sponsor forecloses the possibility of building on the experience gained. Successful laboratories are able, again and again, to reinterpret their missions in light of changing conditions — and that is really what we mean by success.

open? What is it, for example, about a laboratory such as the NASA Langley Research Center or the U.S. Naval Research Laboratory that makes them productive organizations long after their original reasons for being have been forgotten? Both of these institutions are in their seventh decade and yet they have managed to retain their vitality. What, in fact, are the correct ways of evaluating the performance of a technology development institution? One way might be to use past achievements and then make small extrapolations from them. A laboratory that can successfully produce nuclear warheads capable of working from ballistic missiles, another that can develop reentry systems for the Apollo program, and a third that can develop the swept wing principle for high-speed subsonic aircraft probably will continue to make important contributions. But such an argument is often not enough. The authorities within the agency who are in charge of preparing budgets will ask, "What have you done for me lately?" A research facility cannot survive on its record of achievement, by serving as a job shop for other agencies, or even by modest departures from its original mission.

Successful research and development laboratories share certain features that are apparently independent of the particular technology they are pursuing. Three seem to be particularly important:

1. *Cost.* The cost of operating a technology development institution is (in 1983 dollars) about \$75 000-\$100 000 per employee. This number, multiplied by the number of employees, is the institution's budget, and it tends to hold good independently of what the laboratory does or whether it is public or private. The probable explanation is simply that research and development is labor intensive, and that the dollar figure cited equals salary plus overhead, plus some funding for equipment needed to carry out technology development.

2. *Professional and Support Personnel.* The ratio of direct program people to support people in technology development institutions is between one to one and one to two. On the average, in order to keep one person busy in a technology development task, it takes one support person — meaning a secretary, technician, librarian, and the like — who does not work directly for a single scientist or engineer. Those institutions that are more test-and development-oriented tend to have more support people for each direct professional than those oriented toward basic research.

3. *Size.* Almost all permanent research and development institutions range in size between 1 000 and 7 000 people. There are very few operations that are smaller and very few that are larger. The reason for this range of sizes is something like this: The lower limit is determined by the fact that if the institution is too small, there will be too little flexibility for a few people to strike out into new territory, or for new ideas to spill over into research work. Thus the U.S. Naval Radiological Defense

Laboratory mentioned earlier had a total staff of about 800. There were not enough groups of two or three or four people delving into areas unconnected with the laboratory's current mission but that might lead to new missions. Institutions seemingly must have more than about 1 000 people before the kind of flexibility that makes for the institution's survival exists. The upper limit in size is determined by the difficulty, for management, of staying intellectually on top of an institution having more than 6 000 to 7 000 people. (Los Alamos and Lawrence Livermore Laboratories, with just over 7 000 employees apiece and budgets of \$421 and \$515 million, respectively, are near the upper limit. The Sandia National Laboratory, operated by Western Electric for the Department of Energy, has 8 000 employees at three locations and a budget of \$738 million (ref. 56).) Although laboratory directors should not attempt management of research in detail, they must nevertheless understand thoroughly the objectives of work in progress. This is difficult in the largest laboratories; hence a certain creaking of institutional joints, resulting from too many communication channels and not enough feedback. The existence of organizations like Bell Labs, with 19 000 employees, does not disprove this; such organizations are best thought of as federations of semi-autonomous installations of 1 000 to 5 000 persons each.*

Problems of Research Diversification

In the Federal sector, every research installation is always "under judgment" by a variety of groups. The performance of each institution is reviewed annually for budget purposes at the very least. However, there are also a great many other reviews, ranging from those performed by committees of the National Academy of Sciences or Engineering under contract to the agency being reviewed, to those carried out by the General Accounting Office. When a technology development institution gets into real trouble, there are usually a number of special reviews before a decision is finally reached to close it or to make significant changes. Nevertheless, changes do happen fairly rapidly in the Federal technology development establishment. In 1968, the year the Naval Radiological Defense Laboratory closed, the Defense Department also:

- Consolidated several Air Force activities with elements of the Army, Navy, and NASA into the Eastern Test Range (Cape Canaveral, Florida), the Western Test Range (Vandenberg Air Force Base, California), and the White Sands Missile Range in New Mexico;

* As a result of the divestiture of AT&T at the end of 1983, some 3 000 of Bell Labs' 22 000 employees were transferred to Bell Communications Research, owned and operated by and for the divested telephone companies.

- Established the Naval Weapons Center at China Lake, California, by combining the Naval Ordnance Test Station and the Naval Ordnance Laboratory;
- Established the Naval Ship Research and Development Center at Carderock, Maryland, by combining the David Taylor Model Basin in Carderock with the Marine Engineering Laboratory and the Mine Defense Laboratory;
- Approved a plan to reduce the number of Army medical centers from 14 to 6; and
- Closed out the Research and Technology Division of the Air Force Systems Command (ref. 57).

There have been similar instances with other agencies. In 1970, for example, the Electronics Research Center of NASA in Cambridge, Massachusetts, was transferred to the Department of Transportation even before work on the facility was completed. In this case, the rationale for the transfer seems to have been three-fold: With NASA continuing to face budget cuts and layoffs, it seemed preferable to “spin-off” the agency’s newest center, where the sunk costs were much less than for the other centers; the work that the center was designed to do was already being done efficiently in the private sector; and the Department of Transportation had requirements that could be fulfilled by the center.

In fact, few laboratories present so clearcut a case for closure as did the Naval Radiological Defense Laboratory — a small facility with one narrowly-defined mission. On the other hand, a facility may be the “right” size and have the “right” mix of support to professional personnel and still be in danger. The policy considerations that lead a sponsoring agency to close a facility, maintain it in its current mission, or encourage it to seek new clients while continuing to work for its sponsor, are complex. One consideration is obviously cost. The long-term savings from closing a facility may be outweighed by the closeout costs. When the Navy decided in 1977 to “disestablish” the Naval Electronics Systems Engineering Center in Washington, DC, it estimated annual cost savings of \$47 000, annual manpower savings of \$450 000, and “anticipated, one-time costs” of closing the facility as \$818 000 (ref. 58). There are also the long-term costs of losing skilled personnel or paying for their transfer elsewhere, or of losing a capability which the agency may later need. Sometimes the agency can cut its losses by finding another organization to take over the installation; thus NASA transferred the Electronics Research Center and many of its personnel to the Department of Transportation and, in so doing, cut its closeout costs.

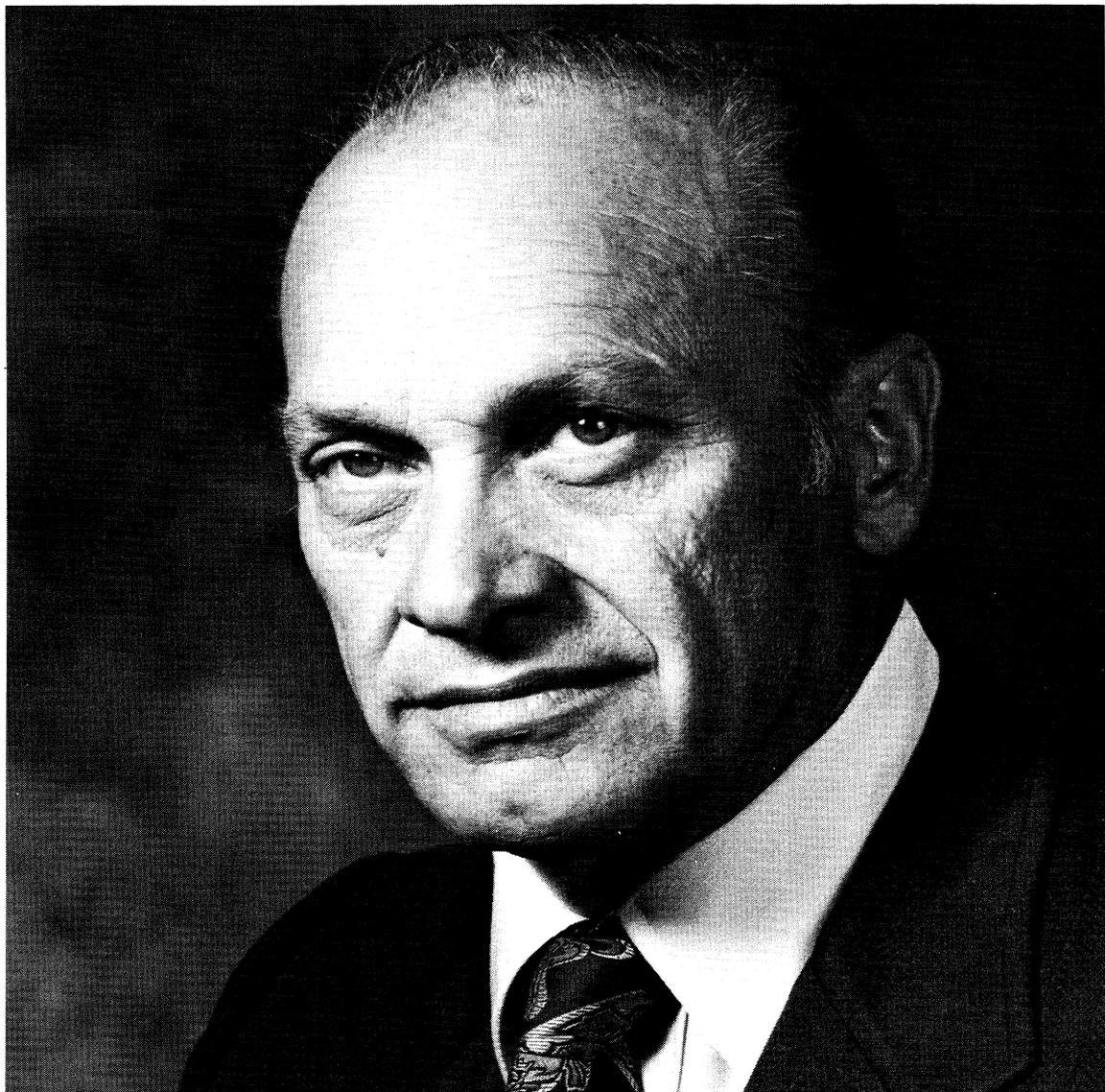
Another consideration, where the agency has more than one research facility, is how a decision affecting one laboratory will affect the others. This problem has different aspects, depending on whether the laboratory

is contractor- or government-operated. A facility like the Jet Propulsion Laboratory or Oak Ridge National Laboratory (ORNL) has somewhat greater freedom to seek new clients when work slackens than a facility staffed by government employees. However, that freedom is usually circumscribed to a degree by the original sponsor.

In this respect, the case of ORNL is unusually revealing. Beginning in 1961, ORNL director Alvin Weinberg sought to diversify, while maintaining good relations with the Atomic Energy Commission and the congressional Joint Committee on Atomic Energy. The need for diversification arose when two of ORNL's biggest projects, the nuclear-powered airplane and the homogeneous nuclear reactor, were canceled. With the encouragement of AEC chairman Glenn Seaborg, Weinberg looked to government agencies for additional work. Between 1962 and 1964, ORNL took on new work in desalting water, cancer research, and civil defense; in each case, Weinberg's criteria for new projects were that they be "big, expensive, strongly in the national interest and . . . not be ready for commercial exploitation." (ref. 59.) Each of these projects was presented to the AEC as something *ad hoc*, "rather than as part of a general laboratory strategy for broadening the base of sponsor support." (ref. 60.) It was only when ORNL sought to become a model laboratory for environmental research that the Joint Committee on Atomic Energy dropped the other shoe, denouncing (without naming) ORNL for "empire building" and for expanding into areas unrelated to atomic energy programs. Moreover, new clients, like the Interior Department's Office of Saline Water, were ambivalent about sponsoring the research efforts of an organization many times their size. Rather than sponsoring the coherent program Weinberg desired, the Office of Saline Water "divided the research effort into small segments, which its program managers could supervise, rather than provide a single, sizable chunk of money to Weinberg for his management . . . ORNL's own ambitions, while couched in ideals and technical jargon, inevitably were rooted in a desire to survive, grow, and serve important needs. The new sponsor knew it and sought to use ORNL, rather than being used for the lab's . . . aggrandizement." (ref. 61.)

It was not precisely that ORNL failed to diversify. By late 1973, when Weinberg left ORNL, outside work accounted for 20 percent of the laboratory's \$100 million budget and even had its own title — "work-for-others." The point is not that Weinberg failed in broadening ORNL's mission but that at a certain point — and the same observation might apply to the Jet Propulsion Laboratory *vis-a-vis* NASA — the laboratory fell out of step with its original sponsor. From the sponsor's point of view, there is "good" and "bad" diversity. Good diversity means diversity within the agency's mission. Thus the Department of Energy, like its predecessor the Atomic Energy Commission, has elected to

operate two national weapons development laboratories, Lawrence Livermore National Laboratory and Los Alamos National Laboratory, and has as we have seen, built competition into its weapons programs; the Director at Lawrence Livermore, Roger Batzel (fig. 23), has stated flatly that "it would be a major mistake to have only one lab." (ref. 62.) Similarly, NASA in the 1960s encouraged (or accepted) diversity within the agency; there were two centers engaged in launch-vehicle development, four in aeronautical research and briefly, no less than five in supporting research and technology. We shall return to the implications of this diversity-within-unity in the next section.



University of California, Lawrence Livermore Laboratory

FIGURE 23. — *Roger Batzel, the Director of the Lawrence Livermore National Laboratory since 1971.*

All three of the large mission-oriented agencies — DOD, the AEC/Department of Energy, and to a lesser degree, NASA — have tried to enunciate policies for evaluating their laboratories. Mention was made in Chapter III of AEC's 1964 guidelines for laboratories desiring to take on outside work. AEC also drafted requirements for appraising contractor performance (ref. 63). But AEC laboratories, as contractor-operated facilities, are in a different category from those of the other two agencies. The staff of AEC/Energy laboratories are not Federal employees; and the AEC has justified its use of contractor employees as being more flexible for institutions which predominantly employ scientific, technical, and management personnel.

For DOD and NASA, on the other hand, the problem has been to justify the continued existence of laboratories staffed mostly by Federal employees. Both agencies have argued that such laboratories are needed to provide a basis for assessing technical alternatives; to develop a body of information leading to design definition; to develop intramurally the skills for selecting contractors and directing their work; and to maintain a continuity of effort, free from commercial pressures. Additionally, since both agencies have more than one laboratory, they have had to face the question of how each laboratory can contribute to the agency's total mission. Should they be organized around a technical discipline, around acquired expertise, or around projects requiring a variety of disciplines?

One concept which was considered, but never completely implemented, by DOD was to convert its larger laboratories into weapon centers. This concept, which dates from a report by the Defense Science Board in 1966, marked a turning away from a state of affairs where competence in a given mission, such as anti-submarine warfare or guidance and control, was dispersed among a number of laboratories at different locations. What the Board recommended was the establishment of large centers, each embracing "a broadly conceived technical program concentrated on a particular military problem associated with general-purpose warfare." (ref. 64.) Among the features of a weapons center singled out by the Board were:

- Size — to achieve "critical mass," the center should have 1 000 or more scientists and engineers;
- That it function as a self-contained organization performing research and technology development, with feasibility models as the end product;
- Its ability to set aside about 70 percent of professional personnel for fundamental development engineering;
- Involvement in determining military requirements and in the initial procurement of equipment; and
- Periodic evaluation of the center to ensure that it maintained high performance standards (ref. 65).

The Board argued that, by combining the capabilities of several laboratories engaged in subsystem work, each weapons center could concentrate on identifying critical military problems and could arrive at optimal solutions independently of technical biases. The task force also concluded that the center's performance would be more easily evaluated, since end products that were clearly the center's responsibility could be tested and evaluated.

DOD could only partially implement the Defense Science Board's recommendation. As we saw, DOD closed or consolidated several of its laboratories in 1968. It also assigned important military missions to its larger laboratories, encouraged installations belonging to one service to work for the other services, developed a program to evaluate those of its contractors engaged in R&D, and created a special category of "in-house independent research funds" — a fraction of the annual laboratory budget set aside without need of prior approval and to be used for work judged by the laboratory director as promising (ref. 66). But the transformation of a number of smaller units into weapons centers was never completed, partly because of political pressures to keep smaller facilities open, partly because of the open-ended, changing nature of the defense missions themselves.

It would be a mistake to close this section without mentioning the evolution of the laboratory itself as it interacts with its sponsoring agency and with other clients that make use of its capabilities. Almost all of the successful large modern technology development centers began with a single mission which changed and multiplied as the original objectives within the original mission were accomplished. The solution of the problems associated with the original mission inevitably led to the development of techniques that could be applied to other missions and it is most important to understand that this happens quite independently of missions that the sponsoring agency might have in mind. Part of the organic development of the laboratory is that it constantly redefines its own missions and therefore also its reason for being.

The original mission of the Lawrence Livermore National Laboratory was to develop thermonuclear explosives. Yet, today, the laboratory has one of the most important capabilities for the *in-situ* recovery of oil from shale deposits and gas from coal deposits. How this happened is shown in figure 24, which illustrates how the various "missions" executed by Lawrence Livermore today evolved from the original purpose of the laboratory. The mission development shown in this diagram was not mandated by the Atomic Energy Commission (later ERDA and the DOE) in Washington. It came about because of actions taken by the staff and the management at the laboratory itself. The Ames Research Center of NASA (fig. 25) began life in 1939 with a mission to explore high-speed flight and yet in 1977 it was designated as the primary

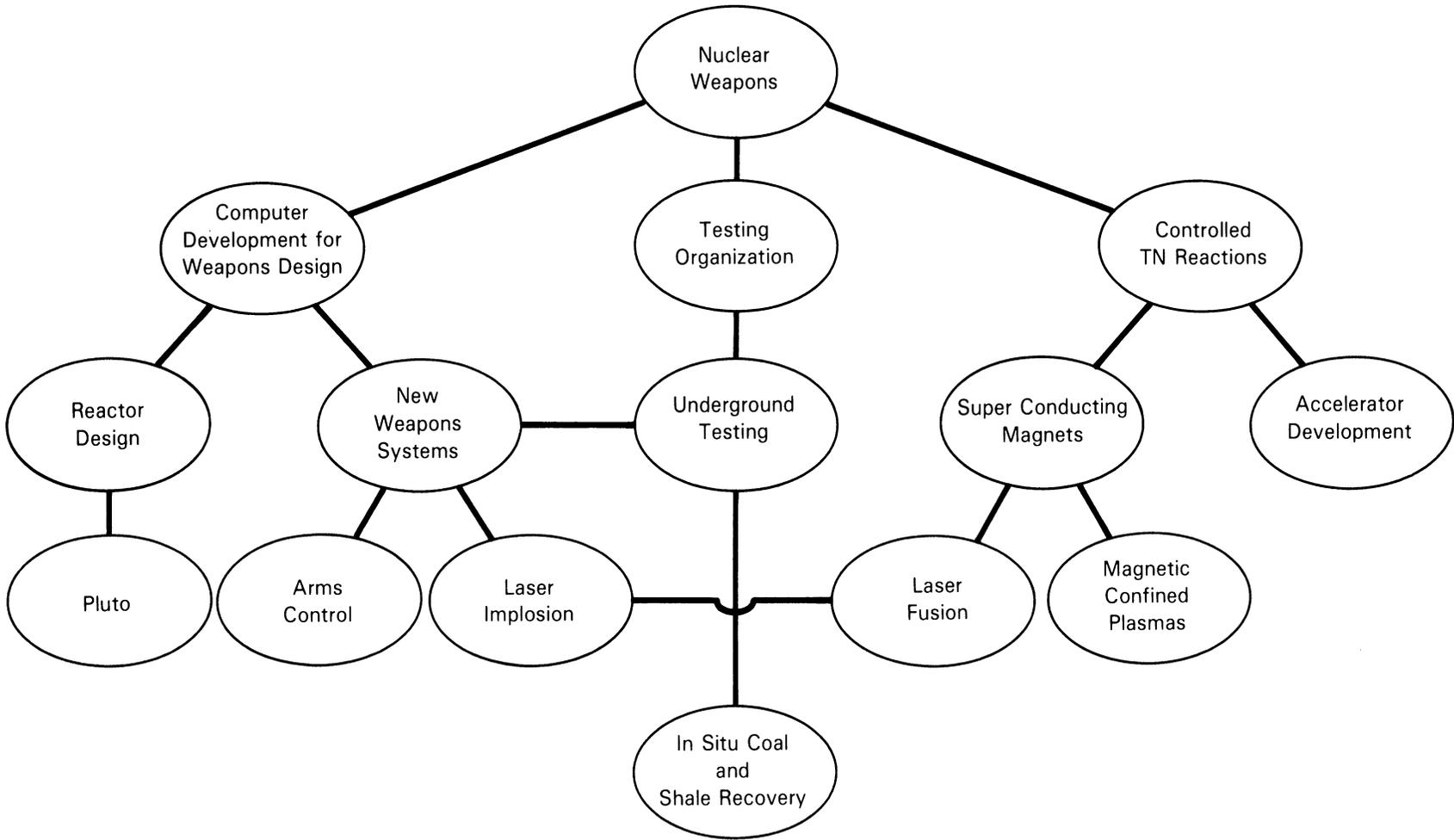


FIGURE 24.—Mission Development of the Lawrence Livermore National Laboratory.



FIGURE 25. — *Aerial view of the NASA-Ames Research Center near Mountain View, California. The large, horn-shaped structure at the left side is the new, full-scale wind tunnel.*

helicopter and vertical take-off and landing machines (V/TOL) technology center of NASA. The mission development diagram for Ames is shown in figure 26, which shows how the logical connection from one to the other came about. Once again, the internal workings of the institution in the field shaped the decisions that were later made (or perhaps ratified is a better word) at NASA Headquarters in Washington. The case of the helicopter development evolution is sufficiently interesting that it will be described in detail in the next section.

A Case Study: The NASA Helicopter Program

For NASA, the problem of devising a policy for evaluating its laboratories was different from that facing DOD or AEC. During the past twenty years the number of NASA centers, including the Jet Propulsion Laboratory but excluding supporting facilities, has fluctuated between eight and ten. For NASA management, the problem has been to strengthen each center's sense of its own mission while making its

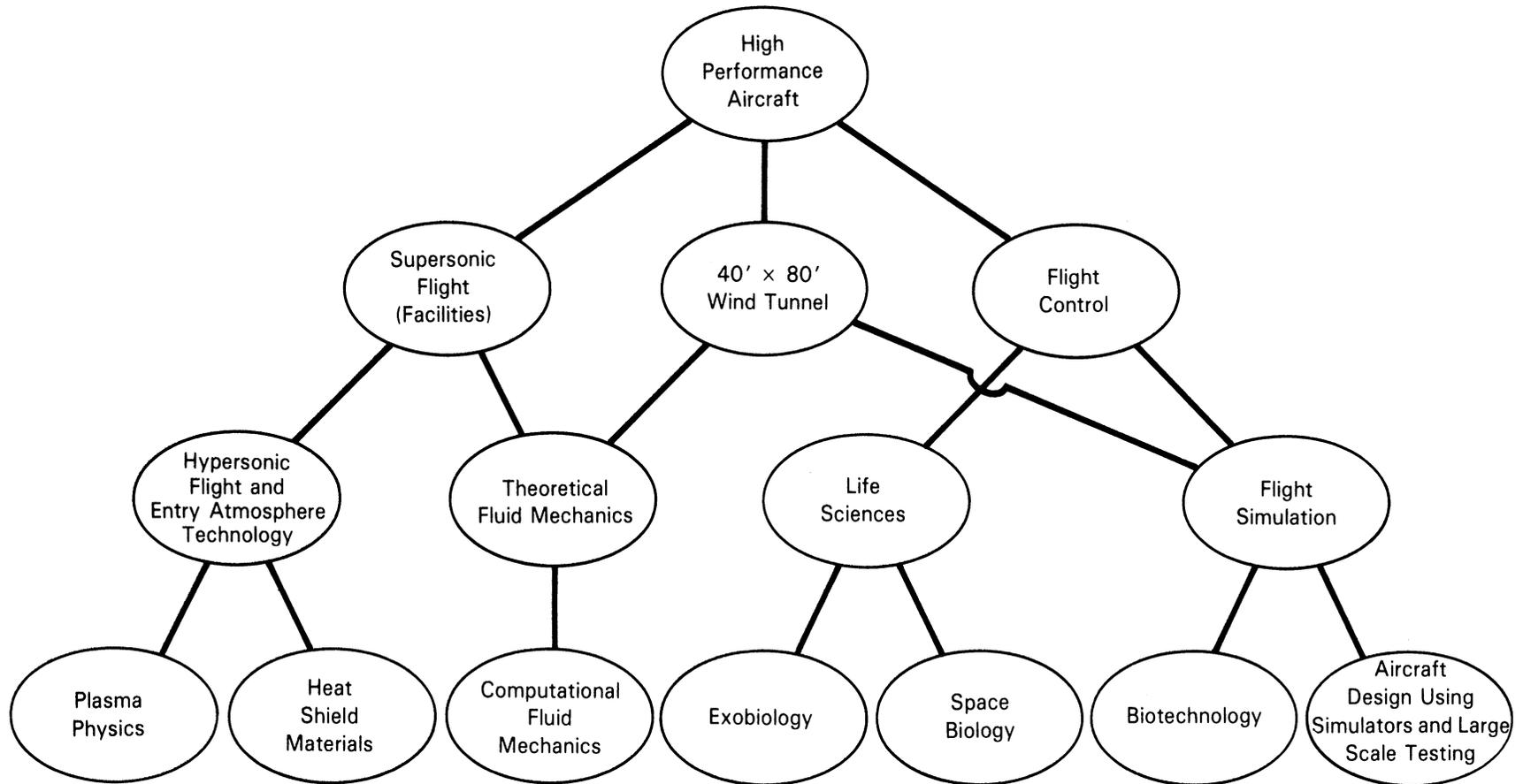


FIGURE 26. — *Mission Development of the NASA-Ames Research Center.*

resources available to the rest of the agency. Where, as mentioned earlier, several centers are working in aeronautics or supporting research and technology, it becomes that much more difficult to avoid a certain degree of duplication. Although never made explicit in a single policy document, NASA has proceeded on the assumption that all but the smallest centers should combine open-ended and discrete projects.* NASA's one attempt to organize a facility around a technical discipline — the Electronics Research Center — failed. Instead, since the early 1970s, NASA has adopted an "area of emphasis" philosophy, that is, assigning work to each center based on the existing facilities and expertise at the center. An account of the consolidation of NASA's helicopter program at the Ames Research Center at Moffett Field, California, will show how this philosophy affected one center. More important, the helicopter program consolidation is a textbook study in demonstrating how one research laboratory adjusted to shrinking budgets, how Headquarters' need to spread work around dovetailed with the center's need for new clients, and the importance of the center's taking an active role to ensure its survival (ref. 67).

The Ames Research Center was established by the National Advisory Committee for Aeronautics in 1939, primarily to test and design high-speed fighter aircraft. The tradition of high-speed aerodynamics that began with this enterprise continued until the laboratory became a center for the development of: first, subsonic jets, rocket and Scout launches at Wallops Island, Virginia; subsequently of atmospheric entry systems; and more recently still, of planetary entry probes. Another kind of mission grew out of the need to develop very sensitive controls for high-speed aircraft. These efforts led to work in flight simulation and the life sciences, since it was necessary to establish physiologically that flight simulators did, in fact, accurately mock up flight conditions. This, in turn, generated strong interest in computers, since high-fidelity flight simulators require very high-speed computational devices to drive them. The work in the life sciences expanded to include space biology after the National Advisory Committee for Aeronautics became the core of NASA in 1958.

An important and independent set of missions emerged in the 1950s around large-scale test facilities originally designed for flight aircraft. It transpired that helicopters and V/TOLs required full-scale testing before they could be flown reliably, owing to the very complex interactions between aerodynamics and vehicle structure. These interactions simply could not be scaled and, thus, full-scale testing was essential. The same

* The Kennedy Space Center at Cape Canaveral, Florida, is an exception, since its mission is to support all of NASA's launches, except for the sounding rocket and Scout launches at Wallops Island, Virginia.

40-by 80-foot wind tunnel originally designed to test fighter aircraft during the Second World War was used later in the design of the most modern helicopters and V/TOL machines.

At Ames, work on rotorcraft research and technology began in 1954, when the Air Force requested tests of two advanced rotorcraft (the McDonnell XV-1 compound helicopter and the Bell XV-3 tilt-rotor airplane) in the 40-by 80-foot tunnel. From this point onward, Ames's involvement in rotorcraft technology increased: In 1956, a rotary-wing research group was formed; in 1958, testing of the Bell XV-3 began; and in the early 1960s, Ames carried out an important series of tests on the UH-1 and H-34 rotor systems. But the key event in Ames's rotorcraft program was the establishment by the Army, in 1965, of an aeronautical research laboratory at the center. In NASA's estimate, the creation of the Army laboratory had significant benefits for both agencies: It led to the refurbishing of an inactive 7-by 10-foot tunnel for small-scale testing alongside the full-scale testing capabilities of the 40-by 80-foot tunnel; to the development of capabilities in noise research and rotor dynamics; to the creation of other Joint Army Research Groups at the Lewis and Langley Research Centers; and above all, to the creation of the Army Air Mobility Research and Development Laboratory in 1970, with headquarters at Ames. This, in turn, caused a rapid expansion of rotary-wing research at Ames; in 1971 a joint NASA-Army agreement to develop a tilt-rotor research aircraft was signed. Thus Ames acquired another sponsor, one that could buffer funding cutbacks in the parent agency.

For reasons that have already been described, it was also true that by 1970 Ames needed all the outside support it could get. When NASA was created, Ames had taken on important new assignments in life sciences and space science. In the early 1960s, NASA built a life sciences research facility at Ames, and assigned responsibility to the center for systems management of the Pioneer series of interplanetary probes. But by their nature, these programs were not likely to grow. There were seldom more than one hundred professional employees working full-time on Pioneer and the early returns on life sciences research — for example, the Biosatellite program for investigating the effects of weightlessness on various organisms — were inconclusive. Also, Ames, in common with other NASA centers, was beginning to feel the pinch of funding cutbacks and layoffs. In mid-1967, there were 2 176 government employees at Ames; three years later, that number had fallen to just under 2 000 — a drop of about 11 percent (ref. 68). Without new programs or sponsors, Ames was in danger of closing or, at best, losing that critical mass of engineers and scientists, without which innovation could not occur.

What permitted Ames to survive was the decision by the center's leadership to concentrate on those areas where it was both strong and

likely to attract support from other funding agencies as well as NASA. When one of the authors (Mark) became the Ames Center director in 1969, there were already proposals to shut Ames down. Something had to be done immediately; and one of the author's first acts as director was to establish a Strategy and Tactics Committee consisting of rank-and-file employees as well as managers to work out Ames' view of its mission. In essence, the committee selected certain areas of emphasis for the center to concentrate on: computational fluid mechanics, V/TOL, flight simulation, airborne sciences, and life sciences. What these areas had in common were a high degree of interdependence and the availability of unique test facilities, such as the 40-by 80-foot tunnel, the Flight Simulator for Advanced Aircraft, and later the ILLIAC IV supercomputer, to support them; the rapid growth of rotorcraft technology for civil and military applications; and the existence of sponsors outside as well as within NASA. Thus Ames used the Army to get funds for V/TOL research and the Defense Advanced Research Projects Agency to procure the ILLIAC IV, operated jointly by DOD and NASA and capable of performing 300 million calculations a second and storing one trillion bits of information at a time (ref. 69). Ames' areas of emphasis were, in a sense, the best horses to ride. New uses for helicopters were being identified in areas as diverse as energy exploration, logging, shipping, and heavy construction; and the Army was considering using the helicopter as an offensive weapon in addition to its traditional support role. As DOD funds became available in the early 1970s, Ames management planned to develop its rotary-wing research capability in ways described in an internal NASA paper as "explosive": repowering the 40-by 80-foot tunnel to increase its maximum speed from 200 to 300 knots, putting the vertical motion simulator into operation, and accepting delivery of an advanced tilt-rotor experimental aircraft.

There was, then, at least as much "push" from Ames as there was "pull" from NASA Headquarters for the center to chart its own course. Indeed, Ames had a long-range strategy in place two years before NASA began an "institutional assessment" of its centers. As it happened, Ames's strategy fitted in well with NASA's strategy of consolidating aeronautics and space technology around areas of emphasis. In late 1975, NASA officials decided to consolidate long-haul aviation at the Langley Research Center in Virginia and short-haul aviation at Ames. From Headquarters' point of view, consolidation would enable the agency to tap Ames's unique test facilities, exploit its contacts with the Army and the Federal Aviation Administration — for example, supporting the latter's air traffic control simulation project — and bring about a division of NASA aeronautical research among the Ames, Langley, and Lewis Research Centers. NASA recommended a consolidation in three phases, beginning with the incorporation of Langley programs for which Ames

had available personnel, followed by the transfer of equipment and key personnel, the transfer to be completed by the 1979 fiscal year. An important point about the program consolidation was NASA's insistence that Ames involve industry to the maximum extent, both for procuring test hardware and in the actual wind tunnel, simulation, and flight test programs. The transfer of the program was executed as planned although very few people were actually persuaded to move from Langley to Ames, a circumstance that caused some program interruptions. Today, five years later, the helicopter research and development program is more active than ever, because of the effective application of facilities and the close collaboration between the Army and Ames.

Conclusions

The consolidation of NASA's helicopter program, the partial redeployment of Oak Ridge National Laboratory, and the setting aside of independent research funds by Department of Defense laboratories represent in different ways the attempts of Federal research installations to avoid becoming captive to a single program or sponsor. The principal lesson to be derived from these case studies is the importance, for the laboratory director and his staff, of honing the entrepreneurial skills needed to get new "business." A center's existence cannot really be justified on the grounds that it is a "national resource"; by the time that argument is trotted out, the game is probably over. There is seldom anything self-evident about the assignment of roles to one laboratory among several; management must build networks and enlist allies, as Ames did with industry, the Army, and the Federal Aviation Administration, or as Lawrence Livermore did with the Office of Coal Research in the Energy Department.

There is a misconception that organizational self-perpetuation is somehow bad or even sinister. But this is not so. If it were, there would scarcely be a major corporation or nonprofit organization that could outlive its original reason for being and find other uses for its resources and experience. This drive to persist and grow exists in private and public institutions alike. Many of our most successful corporations long since outgrew their original businesses. DuPont no longer manufactures dynamite; the Minnesota Mining and Manufacturing Company (better known as 3M) has had no connection with mining for decades; IBM's primary business has nothing to do with selling keypunch machines to the Census Bureau; while the Singer Company has staked its future far more on aerospace and electronics than on sewing machines.

It is impossible to regard this kind of self-perpetuation as anything but beneficial. The important distinction is less between good and bad self-perpetuation, than between the process as it occurs in private and

public institutions. In the eyes of stockholders, the private institution has a single generic objective — profits. As long as a laboratory contributes to a flow of investment opportunities, its perpetuation is in the stockholders' interests. By contrast, Federal research institutions are created and supported as tools to achieve a mission, not to develop technology in any area as an end in itself. The successful Federal laboratory is most often the one whose mission is both open-ended and attainable. Nobody now cares what standards for machine tools were drafted by the National Bureau of Standards in 1910. But the Bureau's mission — to maintain a national measurement system — is almost inexhaustible.

The laboratory director must also know how to redeploy people who are between projects, a skill that some agencies take a long time to learn — and some never do. At NASA, for example, until the late 1960s, few of the centers had much experience in closing out large projects. The usual procedure was for staff to move to a new project or feasibility study; at Langley much of the Lunar Orbiter staff moved to Viking, while at the Goddard Space Flight Center, personnel moved to new projects (Orbiting Astronomical Observatory, Earth Resources Technological Satellite) after the Advanced Orbiting Solar Observatory was cancelled in December 1965. But even in the mid-1960s, and certainly by the 1969 to 1971 retrenchments, NASA experienced serious problems in absorbing project staff, difficulties described in a National Academy of Public Administration study commissioned by NASA: "A number of project staff were left floating without a specific assignment. Others had to take positions considerably subordinate to the ones they previously held or felt that they were employed in make-work tasks. Periods of temporary assignment lasted for periods of six months to a year in some instances . . . dislocation fostered feelings that career progress was being severely stunted, and that technical competence was being dulled by seemingly meaningless assignments." (ref. 70.) It would seem that attracting new clients, working up a portfolio of promising research ideas, and redeploying professionals as projects close down are tasks which must go on simultaneously, if any of them is to succeed.

A second conclusion derives from the complex relationship between personnel and laboratory facilities. Good people come first, with the maintenance of an excellent staff being management's first priority. But it should also be recognized that an excellent staff will generate good facilities and that eventually the facilities will take on a life of their own (figs. 27 to 31). People tend to generate new programs around the facilities, so that when a research and development organization is mature, its roles and missions primarily depend on the facilities available. This is because facilities have a longer "half life" than people. A facility like the 40-by 80-foot wind tunnel at Ames might be used for forty years,

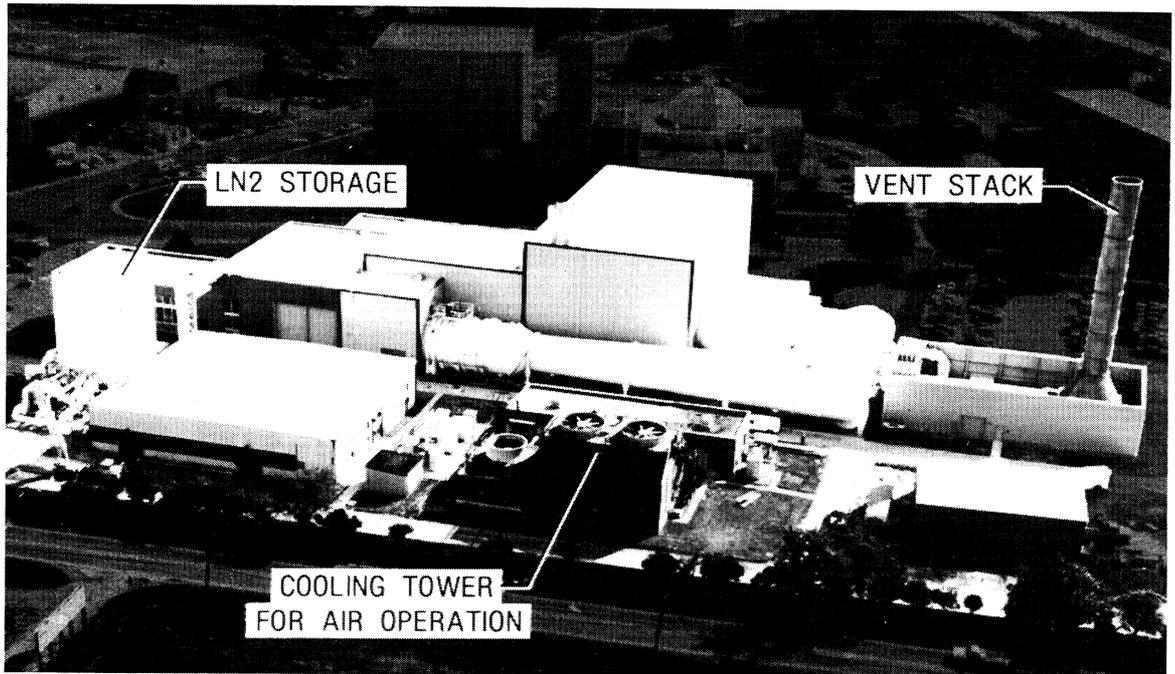


FIGURE 27. — *The National Transonic Facility, a large cryogenically cooled wind tunnel designed to provide the capability to test at very high Reynolds numbers.*

while an individual researcher will change his interests every three or four years and move on to something new. Thus a vigorous research and development program demands an efficient facilities development staff, more particularly where one facility serves a number of projects.

The importance of facilities development is so little understood outside the laboratory that something more needs to be said. A major test facility, such as a wind tunnel, serves many purposes. It attracts the best scientific and engineering talent, frequently because it is the only facility of its kind available; certain research questions can scarcely be posed, let alone answered, without the right facilities; and a major facility itself represents an important but little-appreciated form of technology development. In common with other sophisticated test facilities, the wind tunnel is one of three ways by which discoveries are validated; in the case of aircraft, the other two are theoretical analysis and actual flight testing (ref. 71). As aircraft have become more complex, the demands on wind tunnels at the NASA research centers have grown enormously. As shown in figure 32, the number of wind tunnel tests has grown by several orders of magnitude since the days of the DC-3. What is more interesting, the introduction of high-speed computers and improvements in instrumentation have made the average tunnel-hour much more productive than it was a decade ago.

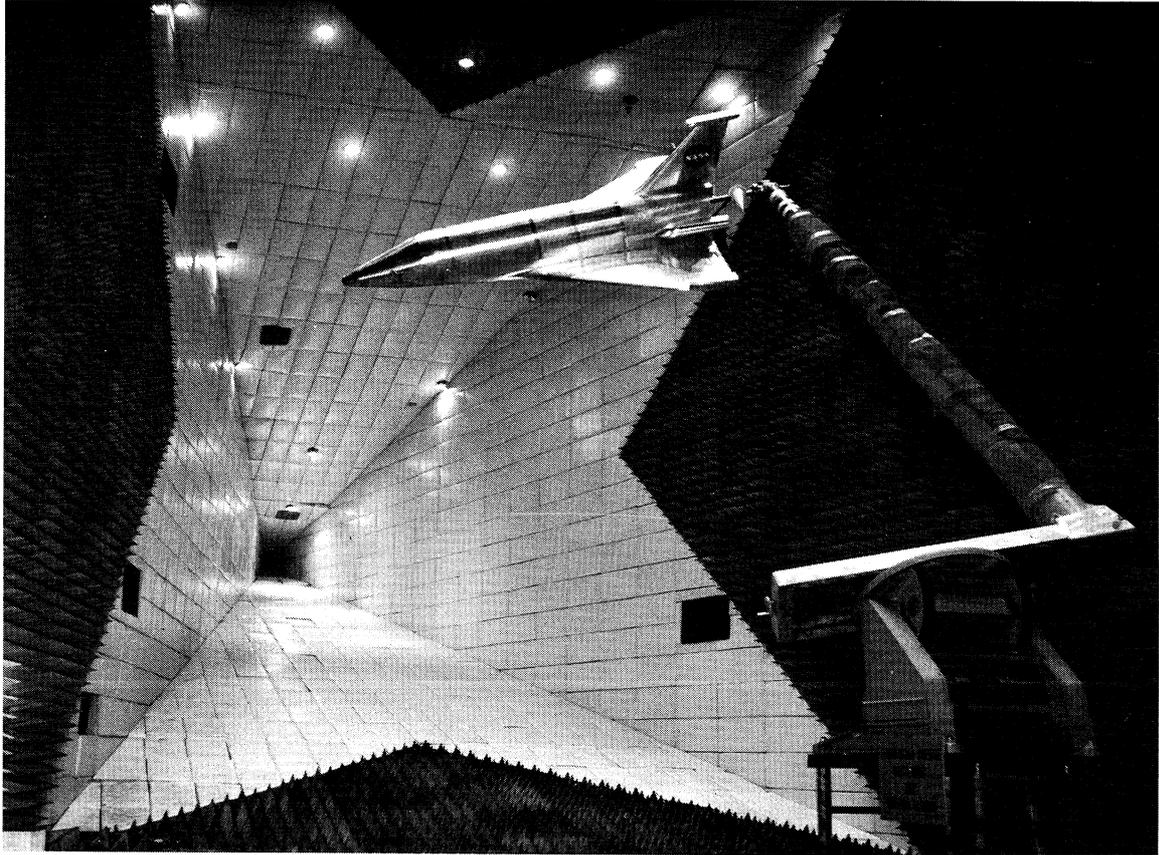
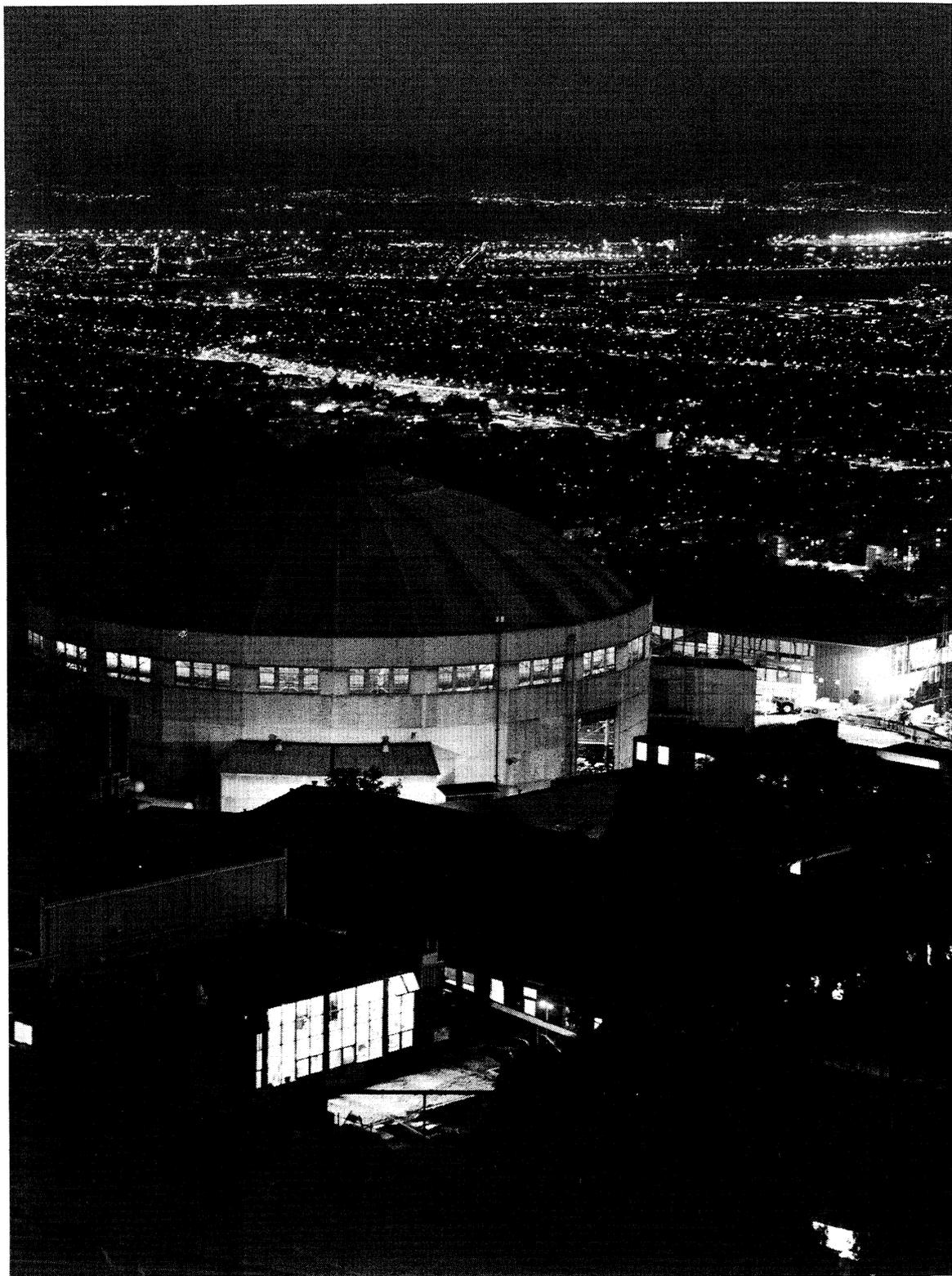


FIGURE 28. — *An anechoic test chamber at the NASA-Johnson Space Center. This chamber is used to measure the electromagnetic properties of space vehicles, such as the Shuttle shown in this picture.*

Thus improvements in test facilities must precede and accompany improvements in the objects being tested; in wind-tunnel technology, improvements over time make it possible to envision a tunnel able to provide a more complete flow simulation of airflow over an entire aircraft (fig. 33) with the correct Reynolds Number, or close to it, to simulate flight conditions.*

* The closing of a major facility or the cancellation of a half-completed facility may have a severe impact on the laboratory housing it. In the summer of 1983, an advisory panel recommended to the Department of Energy that a half-completed particle accelerator at Brookhaven National Laboratory be scrapped, although \$200 million had already been spent on it. The panel's recommendation came shortly after European physicists discovered the particles that the Brookhaven machine was being built to discover. This recommendation imperiled one of the Laboratory's main missions. Such are the perils associated with Big Science. Phillip J. Hilts, "Energy Department Urged to Scrap Half-Built Atomic Accelerator," *Washington Post* (August 18, 1983), p. A3.



University of California, Lawrence Berkeley Laboratory

FIGURE 29. — *The 184" Cyclotron at the University of California's Lawrence Berkeley Laboratory. The magnet of this cyclotron was used during the Second World War to separate isotopes electromagnetically.*

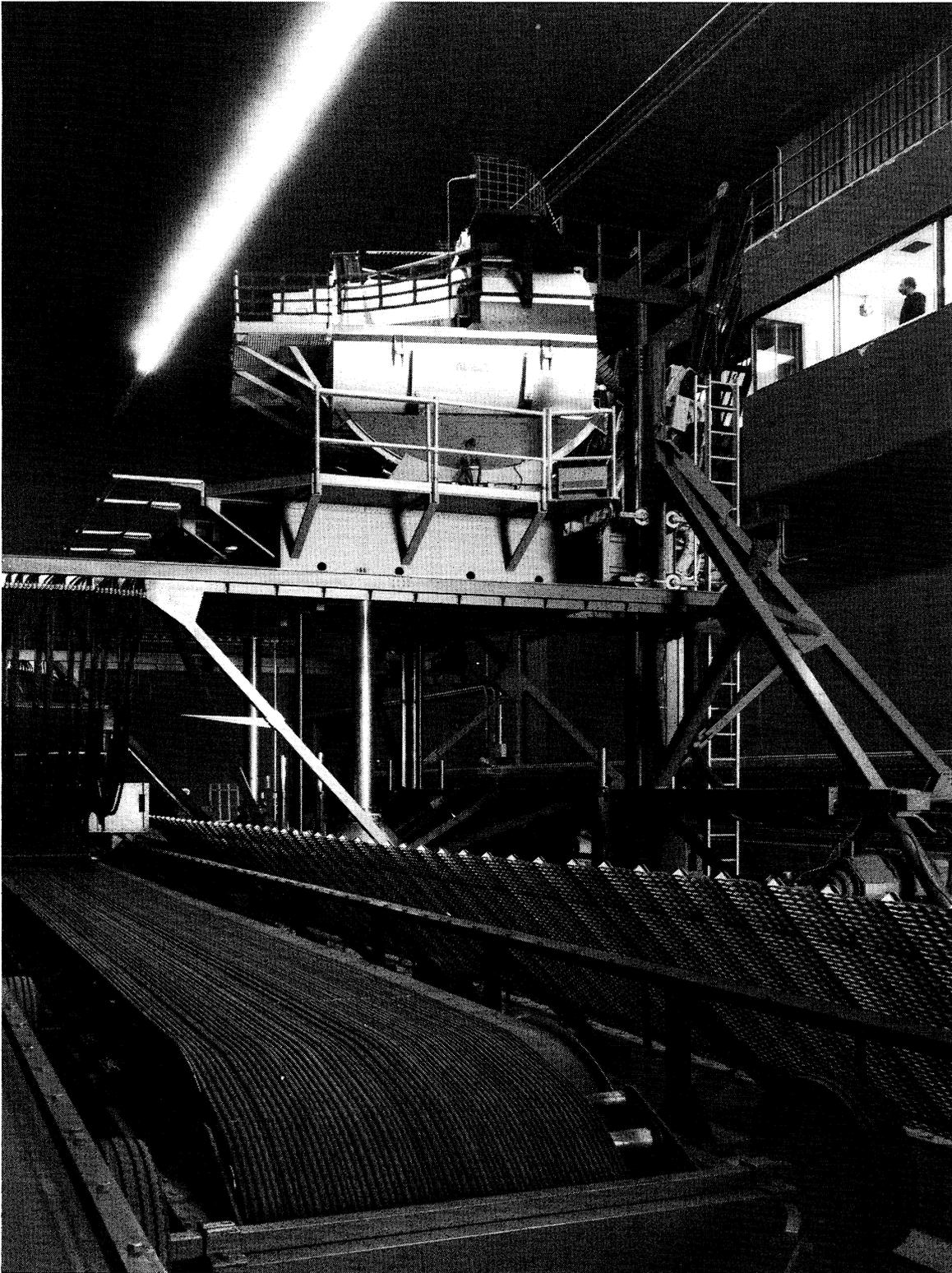
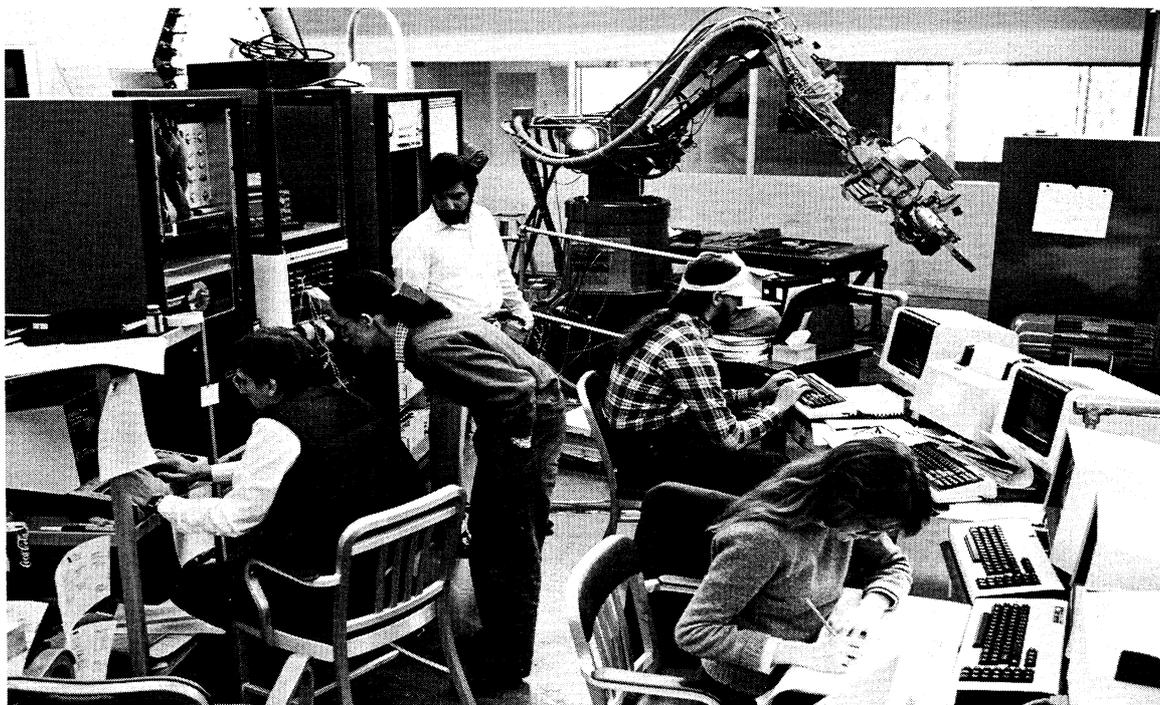


FIGURE 30. — *The Flight Simulator for Advanced Aircraft at the NASA-Ames Research Center. This simulator is used to defined the handling qualities of new aircraft for many purposes ranging from FAA certification to aircraft carrier landings.*



U.S. National Bureau of Standards, Gaithersburg, Maryland

FIGURE 31. — *The Automated Manufacturing Laboratory at the National Bureau of Standards.*

Another point has to do with the differing perspectives of a research center and agency headquarters. An organizational shift or realignment at the field level may be invisible to Headquarters. Our account of rotorcraft research at Ames may appear to make the process more dramatic than it really was; what actually occurred was a series of small, incremental actions that, cumulatively, gave Ames the dominant role within NASA in research on short-haul systems. To a degree, Headquarters in Washington neither encouraged nor discouraged these developments; in many cases, changes in research tasks at Ames never quite rose to the level of awareness of Headquarters officials. As Herbert Kaufman observed, certain organizational changes are the products “of a series of developments so small they are hardly noticed individually as they occur. Collectively . . . these insignificant changes could transform administrative structures without anyone ever having made a single, major deliberate decision to alter them.” (ref. 72.) In effect, many changes in a center’s research agenda occur in just so inconspicuous a way.

One final point pertains to what constitutes productivity in a research and development environment. Defining productivity calls to mind the perplexity of the philosopher in defining time: “I know well enough what it is, provided that nobody asks me; but if I am asked what it is and try to explain, I am baffled.” Of course, there are measurable things — reports,

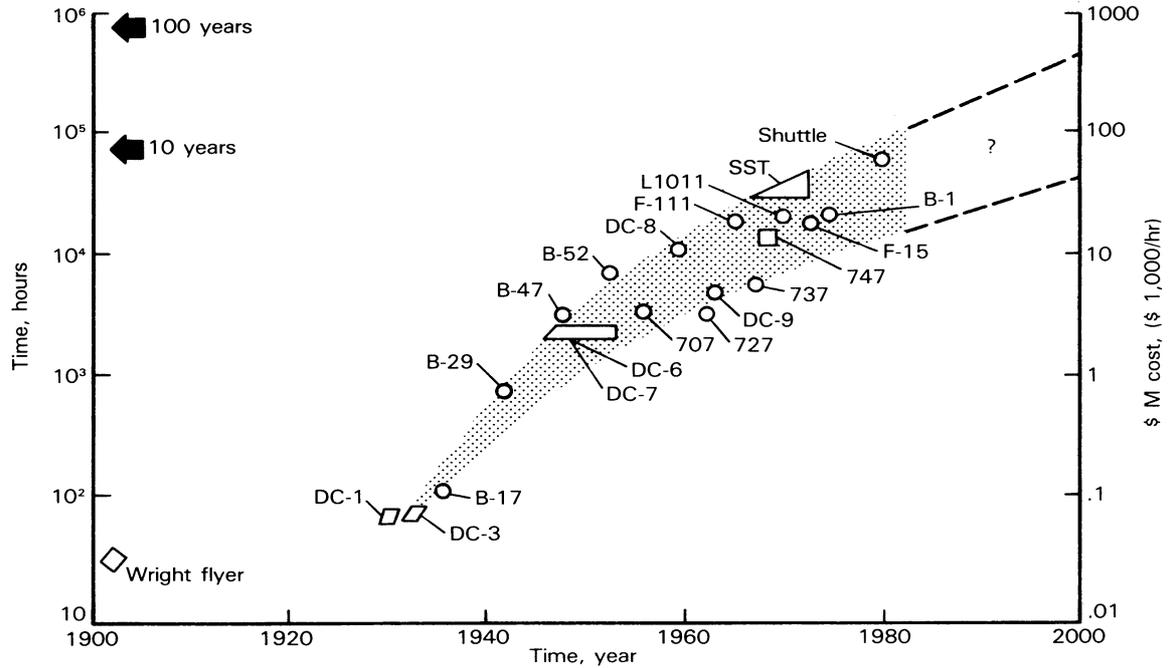


FIGURE 32. — Wind tunnel hours as a function of time.

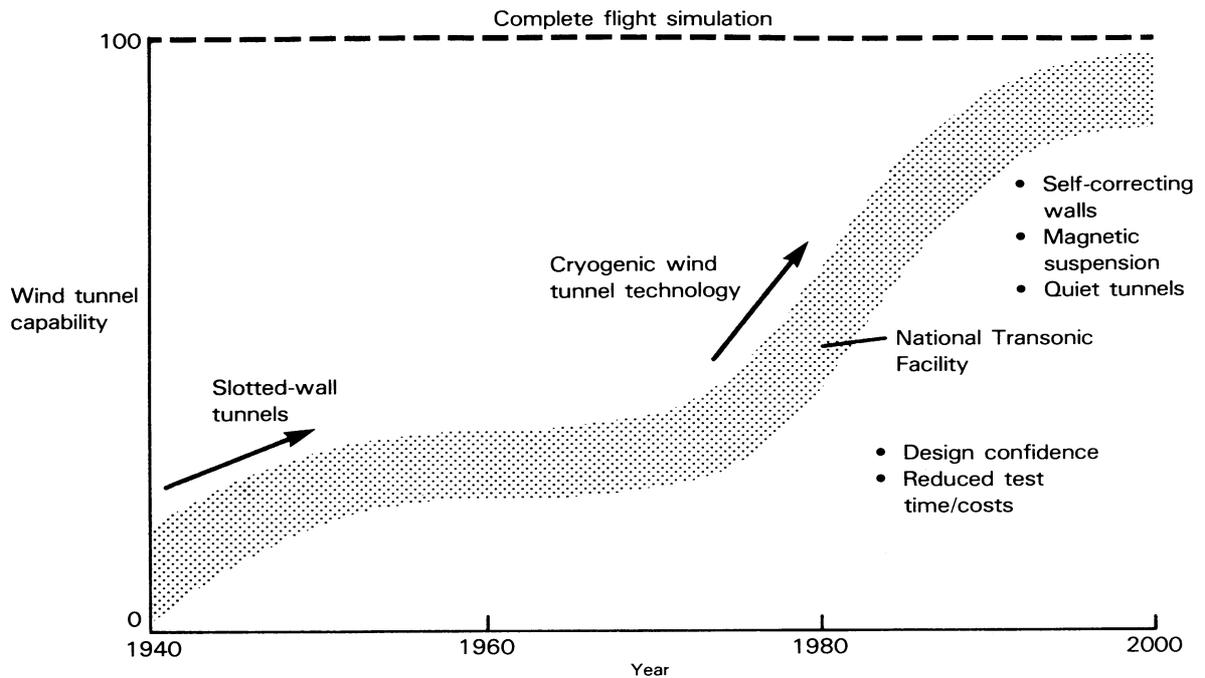


FIGURE 33. — General wind tunnel capabilities as a function of time.

papers, meetings, presentations, various engineering achievements — which can be used as a yardstick. In the case of reports and patents, what matters is that the papers are actually read and the patents actually employed. Responding to an inquiry from the Office of Personnel Management, NASA attempted to measure productivity in its R&D programs. NASA cited productivity improvements in applying new technology within NASA, as well as productivity increases from management initiatives. Among the improvements cited were developing a computer-based, interactive library system at the Johnson Space Center to prepare and disseminate Space Shuttle payload integration documents; computerizing wind tunnels and other test facilities; automating NASA's logistics system; improving facilities management; installing word-processing systems, and the like.

Yet an agency can be “productive” in a certain sense without being productive in any sense that really matters, a point obliquely acknowledged by the authors of the report from which these examples are taken: “Within an R&D agency, only some of the traditional methods of measuring productivity are valid. Analysis of NASA's experience indicates that only about 20 percent of its total civil service effort is amenable to traditional techniques of measuring productivity. These are in areas of general services and technical support where outputs are reasonably structured, routine, and repetitive.” (ref. 73.) The major difficulty faced by research managers in justifying their budgets is the lag between the employment of a research result and its development in the laboratory. The funding agency must, to some extent, take it on faith that the results generated in the laboratory will actually be employed — in other words, be productive. This unresolved dilemma has led to a situation in which research and development is almost impossible to justify prospectively. The solution of this problem — of justifying research for which there is seemingly no payoff — is of the utmost importance, and we will return to it in our final chapter.

CHAPTER V

The Structure of Technology Development Laboratories

Obstacles to Technical Innovation

The laboratory is the linchpin of technology development. To understand where and how (or if) innovation occurs, one must begin here. With certain significant exceptions, the organizational structures tend to be quite similar for all research and development organizations, independent of the organization's functions. This is so because all of these institutions live with the built-in conflict between flexibility and the organization's formal mission. Professional people need the flexibility to start new projects or terminate existing ones; change the distribution of effort between in-house staff and contractors; transfer funds between tasks or projects without the need for prior approval by the laboratory director; or encourage people involved in fundamental research to communicate with those doing applied research, and even to transfer from one group to the other. Indeed, at the project level the most important function of the manager may be to motivate the project team, rather than to make decisions which are both unilateral and final (ref. 74).

Yet there is a limit — easier to sense than to define precisely — to the flexibility a laboratory director will allow his professional staff. The laboratory is constrained (or driven) by its mission, by its budget, by the particular skills it needs, and, paradoxically, by that need to innovate which tends to destroy its stability. In a laboratory without a strong sense of mission, flexibility may degenerate into a situation where professionals all "do their own thing." But in a laboratory where every research task is yoked to an overriding agenda, there may be no room for that relatively modest amount of basic research needed to keep the organization abreast of the state of the art, to prepare for new goals and missions.

Thus the burdens of research management are imposed by the nature of the organization. There is, first, the problem that once any organization attains a certain size, coordinating the work of the various divisions consumes much of senior management's time. Different departments are sealed off from each other; the paperwork needed to process (say) a procurement action increases; and routine tends to drive out innovation.

But — our second point — innovation may be regarded (and quite accurately) as a threat to the status quo.* Where innovation is perceived as a threat, management can deal with it in a number of ways: allowing the effort to continue, but isolating it from the rest of the organization; reducing the level of effort, so that innovation never attains critical mass; compartmentalizing innovation, so that it occurs in one part of the organization, but not in the others; laying down development criteria so stringent that no research effort can ever satisfy them; or converting radical innovation into routine, incremental improvements in existing systems (ref. 75).

In large organizations, whether corporations or government agencies, a major program, one with long lead times and a large budget, acquires enormous momentum. While corporations are more likely than a government agency to cancel an unsuccessful project, the contrast is usually overdrawn. True, even on the most optimistic projections, the hydrogen fusion research sponsored by the Department of Energy will not lead to commercial production until well into the next century. On a smaller time scale, it took the Boeing B-47 bomber 7.8 years and the Boeing B-52 bomber 9.4 years to attain operational capability in the late 1940s and the early 1950s. Other Federal development projects not only met schedules, but surpassed them. The nuclear submarine is only the best-known example; it took the Thor and Atlas ballistic missiles only 3.5 and 5.2 years, respectively, to go from program approval to first operational squadron, instead of the 6.8 years first projected (ref. 76). There are also commercial projects which require extremely long lead times. Consider one current example: Exxon's research and pilot-testing of a surfactant (detergent) method to coax more oil out of the ground. Research on an enhanced surfactant process began in the mid-1960s and led to a pilot project in 1969 to 1971. In turn, the results of the project led to research to develop a process to reduce the salinity content in the test reservoirs. A pilot test of the process began in 1980 and was considered a *technical* success in 1981. It will take another ten years before field testing for commercial viability will make a decision to proceed with full-scale development possible (ref. 77).

* In this connection, Jacques Gansler's observations about the defense industry are worth citing. "The relative inelasticity of . . . demand is a particularly interesting factor in defense R&D; it implies that if you come up with a new idea for, say, a better airplane, you will simply be replacing the old design (which may well have been your own), with very little likelihood of being able to create increased demand, since the number of airplanes to be procured is a function of the force structure, not of cost or performance. Thus, technological advances that originate in the civilian sector are likely to be immediately applied in that sector, with very little thought given by the firm to military application. Only much later is it likely that a defense-oriented firm might pick up the idea and perhaps begin to apply it." Jacques Gansler, *The Defense Industry* (Cambridge, Mass.: MIT Press, 1980), pp. 304-305.

Whether government or commercial, projects such as these tend to force innovation into narrow channels. In a large research organization some, and in a smaller one, most professional staff, may be working on small, carefully-defined areas into which the research effort is parceled. Of course, as work continues, the researchers will be further down the learning curve. But the danger in these larger projects is that once an all-out commitment is made, it becomes difficult, if not impossible, to admit that the organization is on the wrong track. As Donald Schon has noted, "Large-scale developments of the kind undertaken by supercorporations or the military may proceed for months or years beyond the point where they should have stopped; they continue because of massive commitments to errors too frightening to reveal . . . In these cases, the personal commitment of the people involved in the development, the apparent logic of investment, and the fear of admitting failure, all combine to keep the project in motion until it fails of its own weight." (ref. 78.)

It is not that laboratory directors and the officials to whom they report are unaware of these problems. Some Federal agencies have tried to control the development process, whether by stimulating competition between laboratories or by introducing management-decision points at important stages of a project. In the 1960s, NASA, for example, instituted "phased project planning," whereby management could intervene at four stages in the life of a project. The project, so the theory went, would begin with a study of alternative approaches (Preliminary Analysis), followed by the selection of one of them (Definition). This would lead to a Design stage, culminating in the development of a mock-up or "breadboard" of project hardware. In the final stage (Development/Operations), the contractor, in cooperation with NASA scientists and engineers, would prepare the final hardware design, leading to development, fabrication, testing, and operation. While phased project planning was a successful management method for controlling a well-defined project, it did not help at all in determining whether or not a project should be terminated. Projects have occasionally been terminated in NASA but these terminations have little relation to the "phase" in which the project was in at the time of cancellation. In any case, for so complex a project as the Space Shuttle, it is often impossible to state accurately at what "phase" the Shuttle — as opposed to its subsystems — has arrived.

In sum, the pressures on research laboratories are generated externally and from within. The external dangers stem from having to hew strictly to one narrowly-defined mission. The pressures generated within the laboratory are toward routine and conservatism. Where a laboratory is one of several within an agency, it is imperative that one of them, or divisions within all of them, do some basic research beyond their

current mission. Technical innovation is a somewhat mysterious process, one easily swamped by the conservatism of a large organization. In *The Sources of Invention*, perhaps the most thorough investigation of the subject, the authors conclude that “the forces which make for innovation are so numerous that they are not fully understood.” (ref. 79.) It is disconcerting, but true, that important discoveries are as likely to be made for aesthetic reasons — “craftsmanship for craftsmanship’s sake” — as for economic or military reasons. This was the case in aeronautics, in the development of wing flaps, streamlining, and stressed skin metal construction (ref. 80). It is even more disconcerting that some important inventions taken up by government agencies originated outside government laboratories or the industries from which the invention might have been expected to come. The development of the jet engine is a classic example: The pioneering work was done in two countries (Britain and Germany) by men who were either unconnected with the aircraft industry or were specialists in airframe design; no significant development originated with the aircraft engine manufacturers; despite the engine’s military value, governments were reluctant to support it; and not until the engine manufacturers awoke to the significance of the jet engine was its development assured (ref. 81).

Innovation in the laboratory is never a foregone conclusion. Yet some laboratories have been remarkably productive, and it is important to understand why. We shall briefly consider the formal organization of technology development centers, although the only justification for any organizational scheme may be fairly arbitrary and tailored to the particular individuals who work in the organization. We shall then examine three cases of technology development in search of clues — perhaps the nature of certain test facilities, the freedom of researchers to bypass the organizational structure, the coupling of the researcher with the ultimate user — as to what generates new ideas, new hardware, new technologies. Finally, we shall consider at length how one agency, NASA, manages its research and technology program and, in particular, how it reconciles centralized control with discretionary research at the field centers.

Organizational Structure of Research and Development Institutions

Most, if not all, technology development centers are organized at four levels: the branch or group level, the division, the directorate or department, and the office of the director and his staff (fig. 34). A group working in basic research or the early stages of technology development is generally quite small — somewhere between five and twenty people, or ten to forty if support personnel are included. What distinguishes a branch or group or section is that it generally has one and only one research objective.

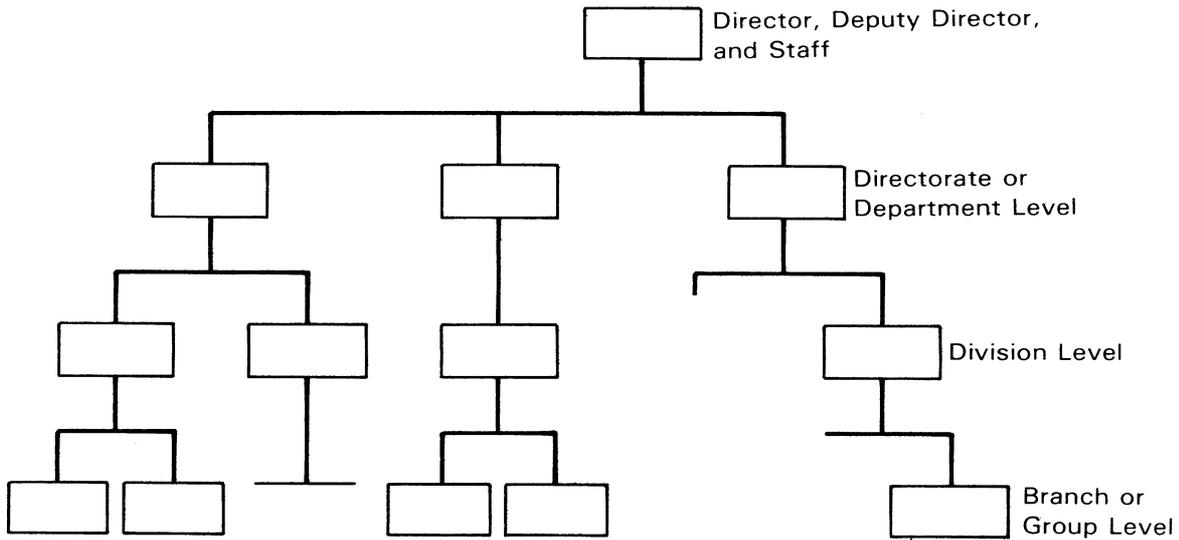


FIGURE 34.—*Typical schematic organization of a large research institution. The actual technical work is done at the branch or group level. The division tends to be the most important organizational element because it is still close to the technical work yet has enough resources to manage that it can claim management attention.*

The next organizational level is the research or technical division, consisting of two to five research groups working on related topics. Thus division-level organizations tend to have 50 to 200 people and form the first organizational unit whose leader has formal financial relations with the headquarters financial office. It is normally the lowest administrative level at which business with the sponsoring agency is transacted. A division-level organization may also be responsible for one or more of the laboratory's research and test facilities: a large particle accelerator at one of the former Atomic Energy Commission laboratories, a major wind tunnel at one of the NASA research centers, or a contractor-operated computer facility supporting all of a laboratory's operations.

At most research installations with 1 000 or more people on the staff, the next level is the directorate or department. A department will normally have 200 to 500 people and will be organized according to function, to discipline, or — where the laboratory is responsible for a large project — to project. This, for instance, was the case with the Viking project, which led to the successful landing of two probes on Mars in 1976. Viking was run out of the NASA-Langley Research Center, and the project manager reported directly to the center director. A directorate or department may also be organized according to discipline, as in the case of the Chemistry Department at the Lawrence Livermore National Laboratory. This department coordinates the laboratory's work in various areas of chemistry as well as providing the support for chemical diagnostics of nuclear explosions. The individuals heading departments

form what might be called a corporate board of directors for their research institutions. An institution of the size we are discussing will typically have between three and seven departments.

The final level of organization in a research center is the laboratory's management. This consists of a director, in most cases a deputy director who is formally designated as the director's second in command, and a supporting staff including the legal office, public affairs, health and safety operations, and perhaps a planning and financial staff.

Figure 34 shows the formal basis of authority in a "typical" research laboratory. In general, supervisors at each level are responsible for technical programs in their organization; an important part of this responsibility is personnel development, including hiring and setting salaries. In most laboratories certain supporting functions — fiscal management, libraries, machine shops, data processing, and the like — may be located in one directorate designated as a support organization. The remaining directorates or departments are then the functional line organizations. But while the formal organization outlined here is quite common, it must be stressed that very often things get done in any organization outside normal channels. While most laboratories have, of necessity, adopted the kind of organization just described, it is essential that any organization remain sufficiently flexible to prevent routine from driving out innovation, and that bright and energetic young people be able to bypass it.

Innovation in Technology Development Centers

How do laboratories get into new fields? This is an important question and deserves a detailed answer. Advanced development, fundamental research, and exploratory engineering are all terms used to describe ways in which innovative new things may be done in a research and technology development center. There are essentially two ways in which such work can be organized in a large research institution:

1. Independent Advanced Research and Development Division or Department. In this approach (fig. 35), all of the basic research or advanced development for the entire organization is placed in one division or department.
2. Integrated Advanced Development Groups. Here, each of a research center's functional departments or directorates has its own basic research or advanced development group (fig. 36).

Each of these organizational methods has its advantages and disadvantages. An independent basic research department gives stature to people engaged in work of this kind, and sometimes makes it possible to get better people into the department. The main disadvantage of an

independent unit is that the work tends to become uncoupled from the functions of a technology development center. The most common method for developing relationships between the basic research directorate and the remainder of the laboratory is to make certain that people transfer in and out of the basic research directorate to other units in the course of their careers. The difficulty with the “people transfer” method is that the “stature” argument becomes less important if, in fact, people are too mobile within the organization. Additionally, it is often difficult to transfer people, owing to various personnel rules operating within the organization.

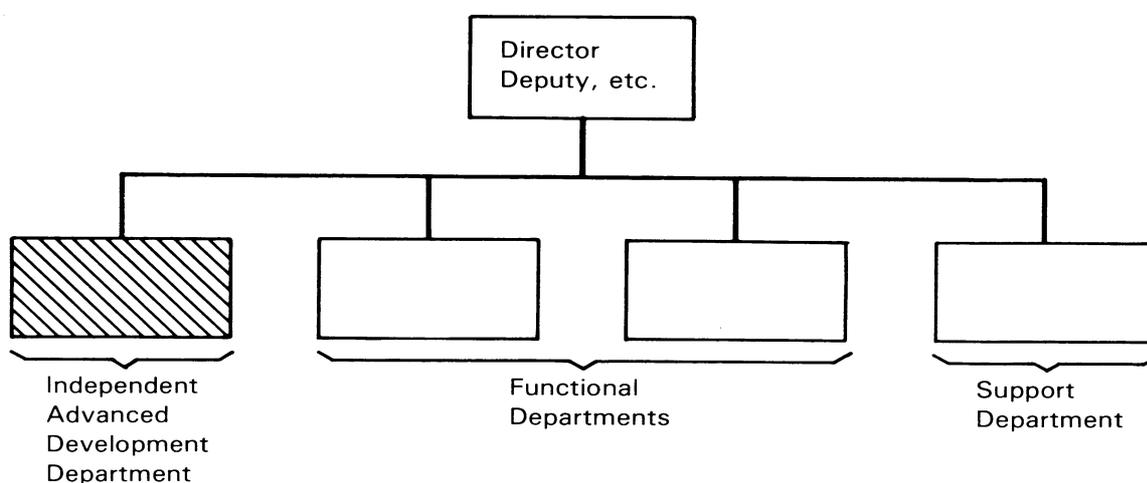


FIGURE 35.—An organization chart showing the Basic Research Department of a large research institution as an independent unit.

For integrated basic research groups, the major advantage is, of course, that the research agenda is directly related to the laboratory's functional organizations, and can be more readily utilized by those responsible for carrying out the laboratory's mission. The method's major disadvantage is that since the laboratory's functional departments must put out a product, functional group managers tend to be unsympathetic toward the longer term work that basic research implies.

Both of these systems can be made to work — it is really a matter of choice for the management of the laboratory to decide which one to employ. But whatever system is chosen, there are some general rules regarding the number of people in the various research groups. Where a functional technology group consists of a few hundred persons, there may be twenty or thirty doing basic or advanced research. Roughly speaking, ten percent of the people in the organization are (or should be) devoted to long-term work. If such research is concentrated in one directorate, then

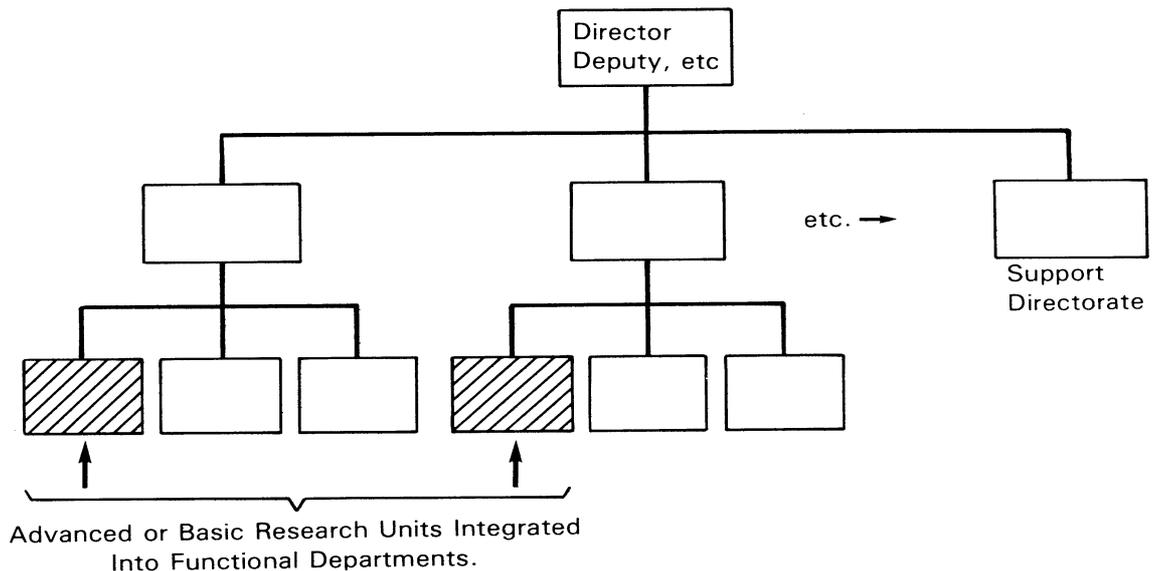


FIGURE 36.—An organization chart showing the way basic research organizations can be integrated in a large research institution.

once again such a group tends to represent about ten percent of the laboratory's total work force. There is no *a priori* reason why there should be a ratio of one full-time researcher to every ten laboratory staff, but this is approximately the size of the basic research effort in many technology development centers.

Whatever institutional arrangements a research center adopts, the important thing is that they lead to innovation, whether in research concepts, hardware, the design of facilities, or the way in which the laboratory is managed. It may even be (as some people have claimed) that the only productive work in a technology development laboratory is done outside the normal organizational channels. A new development usually requires a concentration of the laboratory's facilities and people outside the normal organizational structure. There is no more famous example of a small, quasi-independent research organization embedded within a larger one than the Lockheed Corporation's "skunk works" directed for many years by Clarence (Kelly) Johnson. Johnson's team, which has never numbered more than 200 to 300 people and operated as an independent unit, has designed some of the most advanced military aircraft, including the F-104, the U-2, and the SR-71.

Again, in 1960 to 1961, when NASA had to decide on the method for executing the lunar landing, there was strong sentiment in the agency for direct ascent to the Moon using a giant booster, or for earth-orbit rendezvous, assembling a lunar-landing vehicle in earth orbit and sending it on to the Moon. Wernher von Braun and his collaborators at the Marshall Space Flight Center favored earth-orbit rendezvous because

they recognized that it would require the construction of an earth orbiting space station that would have many other uses beside the one for which it was intended. But in late 1961, it became apparent that there would not be enough time or money to use the method von Braun preferred. A group of engineers from the Langley Research Center, led by John Houbolt, proposed lunar-orbit rendezvous, which involved launching the Apollo spacecraft into lunar orbit, detaching a small landing vehicle — the lunar module — and providing it with an ascent engine so that the astronauts could return to the parent ship for reentry. Houbolt was able to sell lunar-orbit rendezvous to NASA management by demonstrating that it was technically feasible and that it would be the least expensive and most rapid way of achieving the objective of reaching the Moon before 1970. But to make his case, Houbolt had first to go outside channels and get the attention of Associate Administrator Robert Seamans; once Houbolt managed this, it became a question of gradually winning over the major technical elements within NASA, a process consummated in July 1962, when NASA announced that it had selected lunar-orbit rendezvous as the mission mode for going to the Moon (ref. 82).

The same organization that brings skilled personnel together may also, through conservatism, inertia, or the compartmentalization of research, make generating new ideas difficult. After all, it is easier to decide to do nothing than to strike out in directions which may imperil the laboratory's existence. There is also, especially among government laboratories and their sponsors, a tendency to persist in a particular line of research long after evidence has accumulated that it would be unproductive; examples of such failed systems would include the B-70, nuclear powered aircraft, and the Manned Orbiting Laboratory. To illustrate just how innovation does occur in the Federal laboratory, we have selected three examples of successful research — successful in the sense that the research proved a concept or led to new instruments for acquiring basic scientific knowledge. These are: the "area rule" in aeronautics, the alternating gradient synchrotron, and the laser program at Lawrence Livermore Laboratory.

The Area Rule. The Area Rule is a formula for minimizing the drag of an airplane at transonic speeds (fig. 37). The so-called "coke bottle" shape of high-performance aircraft results from applying the area rule. The idea of the area rule — that drag is minimized if the rate of change of the area of the vehicle is a linear function of distance along the center line of the aircraft — was first proposed by Dr. W. Hayes in his doctoral thesis submitted to the California Institute of Technology. It was buried in the thesis without attracting attention for several years, until the concept was picked up independently by Dr. Richard Whitcomb of the NACA's Langley Memorial Aeronautical Laboratory (as it was then called) in 1951. Using the center's new 8-foot, slotted wall high-speed

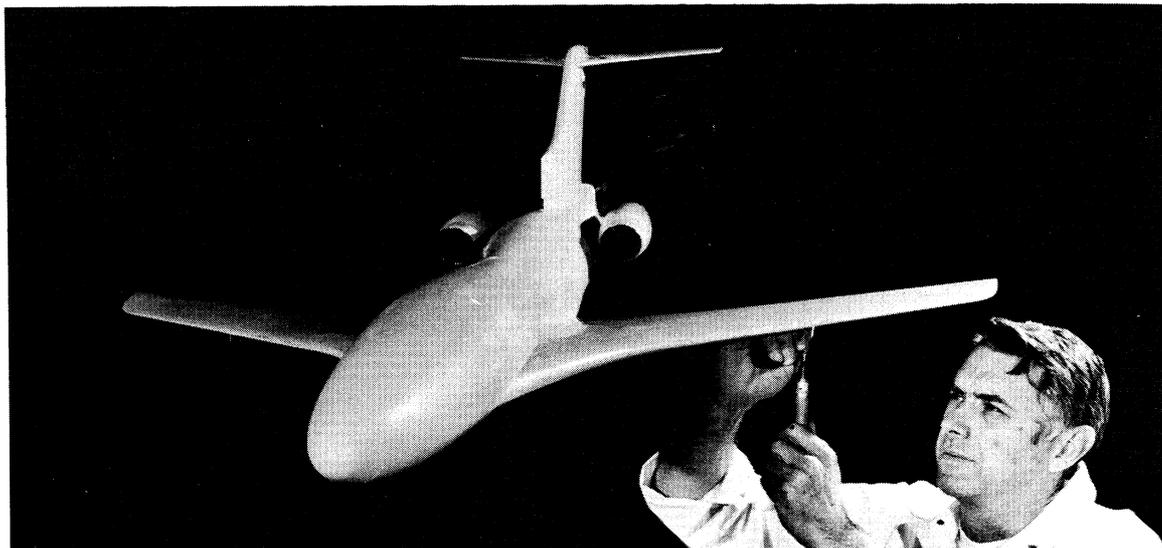


FIGURE 37. — An F-106 aircraft showing the “coke bottle” fuselage shape demanded by transonic area rule.

wind tunnel, Whitcomb performed a series of exquisitely planned and executed experiments to prove the concept. His tests demonstrated that bulletlike aircraft, such as the F-102 fighter being developed by the Air Force, would not attain supersonic flight; that the prototype's streamlined fuselage should be redesigned so that the total cross-sectional area of the plane, and not simply the fuselage, was that of an ideal streamlined body; and that the fuselage should be constricted at the wing attachments and expanded at their trailing edges. When the Air Force made these changes, a new version of the F-102 called the F-106 easily reached supersonic speeds (ref. 83).

This account of the area rule illustrates certain features of the integrated basic research group. It underlines the importance first, of good test facilities in enabling the investigator to develop an idea once it seems the right thing to do. Dr. Whitcomb later used this same transonic pressure tunnel to prove the concept of the supercritical wing, as well as to design small vertical airfoils or “winglets” capable of reducing drag over an entire aircraft by 4 to 8 percent (ref. 84). Relations between the research sponsor, the National Advisory Committee for Aeronautics, and the user — in this case, the Air Force — were particularly close; and once tests made clear that the F-102 in its original form would never go supersonic, the Air Force promptly — within 117 working days — redesigned the aircraft according to the area rule. Perhaps most important, Whitcomb's basic research branch was integrated into a larger functional directorate at Langley dealing with the technology of high-speed aircraft. In effect, the directorate structure built a bridge between those doing the basic and applied research on aircraft

drag — and note that the same people who hit on the area rule also proposed the redesign of the F-102 — the user agency, and the senior managers within their own organization. There was, in short, a close coupling of the people involved in research, development, and production.

The Alternating Gradient Synchrotron. Like the cyclotron, the synchrotron is a device for investigating the structure of the atom by bombarding it with high-energy particles accelerated by electromagnetic fields. But the synchrotron differs in two respects: It accelerates particles with velocities very close to the speed of light, and while the orbit radius of the particle and the rotational frequency remain constant, the magnetic field increases. Since there is a practical upper limit to the magnitude that can be produced, the particle energy ultimately depends only on the radius of the machine. The problem in building a synchrotron is to get larger and larger radii, while minimizing the use of iron in building the magnets of the machine. The solution — what became the alternating gradient synchrotron — was hit upon independently by Dr. Nicholas C. Christofilos and Dr. Stanley Livingston. Christofilos began to study the problem of accelerator design in Greece during the Second World War, when the development of hardware would have been completely out of the question. In 1947, he sent a proposal for an alternating gradient synchrotron to the University of California Radiation Laboratory, then headed by Ernest Lawrence. After an evaluation by Laboratory staff, the proposal was rejected because Christofilos had not explained his ideas using the conventional mathematical notations. This is an example of the “not invented here” syndrome, a lack of interest by research professionals in new ideas originating outside their organization.

Quite independently, in 1950 to 1951, Livingston, who was then finishing work on the first truly high-energy accelerator at Brookhaven National Laboratory, the “Cosmotron,” accidentally hit on the same idea. He was trying to get the beam of particles out of the Cosmotron and suggested that one of the C-magnets of the ring should be turned around so that the beam would be free to leave if it were properly steered. He asked two of his theoretical collaborators, Hartland Snyder and Ernest Courant, to calculate the effects on beam focusing if one of the magnets were indeed turned around. Courant and Snyder quickly discovered that the gradient set up by this method of arranging the magnets would actually focus the beam into a tighter bundle and would thus lead to the alternating gradient focusing principle. Snyder, Livingston, and Courant published their results and, in the normal course of events, discovered that a U.S. patent on the principle had been taken out by Christofilos (ref. 85). Accordingly, Christofilos was invited to join the staff of Brookhaven National Laboratory in 1955, where he worked on the development of the first large alternating gradient synchrotron. He then transferred to the

Lawrence Livermore National Laboratory, where he was one of the most productive researchers until his untimely death in 1972.

As with the area rule, the alternating gradient synchrotron was developed by a small group of theoretical researchers operating in a functional organization. Here, what began as theoretical research eventually reshaped the agenda of the sponsoring laboratory. The work of Livingston, Snyder, Courant, and Christofilos led, not only to the creation of a large new functional group at Brookhaven — the department that operates the alternating gradient synchrotron — but to an entirely new national laboratory, the Fermi Accelerator Center in Batavia, Illinois, where the country's largest and most powerful particle accelerator is located. What began as a problem in engineering design ultimately led to improved research tools which, in turn, made it possible to investigate atomic structure in entirely new ways. But unlike the area rule case study, where basic research in aerodynamics fed directly into aircraft design, in this case theoretical work on synchrotron design led to a machine embodying that design. But at all stages the work being done was basic research; in the whole of physics, there is no research more "basic" — less applications-oriented, if you like — than the work carried on at the great particle accelerators.

The Laser Program at The Lawrence Livermore National Laboratory. This case illustrates two features of "big science": the desire of researchers to get into areas that appear technically ripe, and (as with the alternating gradient synchrotron) the influence of a basic research department separate from the laboratory's operating or functional groups. In 1962, shortly after Theodore Maiman built the first laser (light amplification by stimulated emission of radiation), two researchers at Lawrence Livermore, Drs. Ray Kidder and Charles Violet, became interested in lasers and their possible application. One of the authors (Mark), who headed the Laboratory's Experimental Physics Division at the time, went to Dr. John Foster, then director of Lawrence Livermore, to ask if Kidder and Violet could set up a small section to work on lasers in the division. Foster agreed. The Experimental Physics Division had the mission of performing basic research that might be important to the Laboratory's major mission. What happened then was that a number of researchers decided that lasers, because of their extremely constant frequency and sharp concentration of radiation into a beam, would become important to the weapons business — and Lawrence Livermore is primarily a weapons laboratory. There was, so to speak, a "gut feeling" that research into high-energy pulses might be important.

And so it proved to be. Kidder and Violet established their laser research group and it grew rapidly. By 1965, enough progress had been made to convince the Laboratory's management that high-energy pulsed

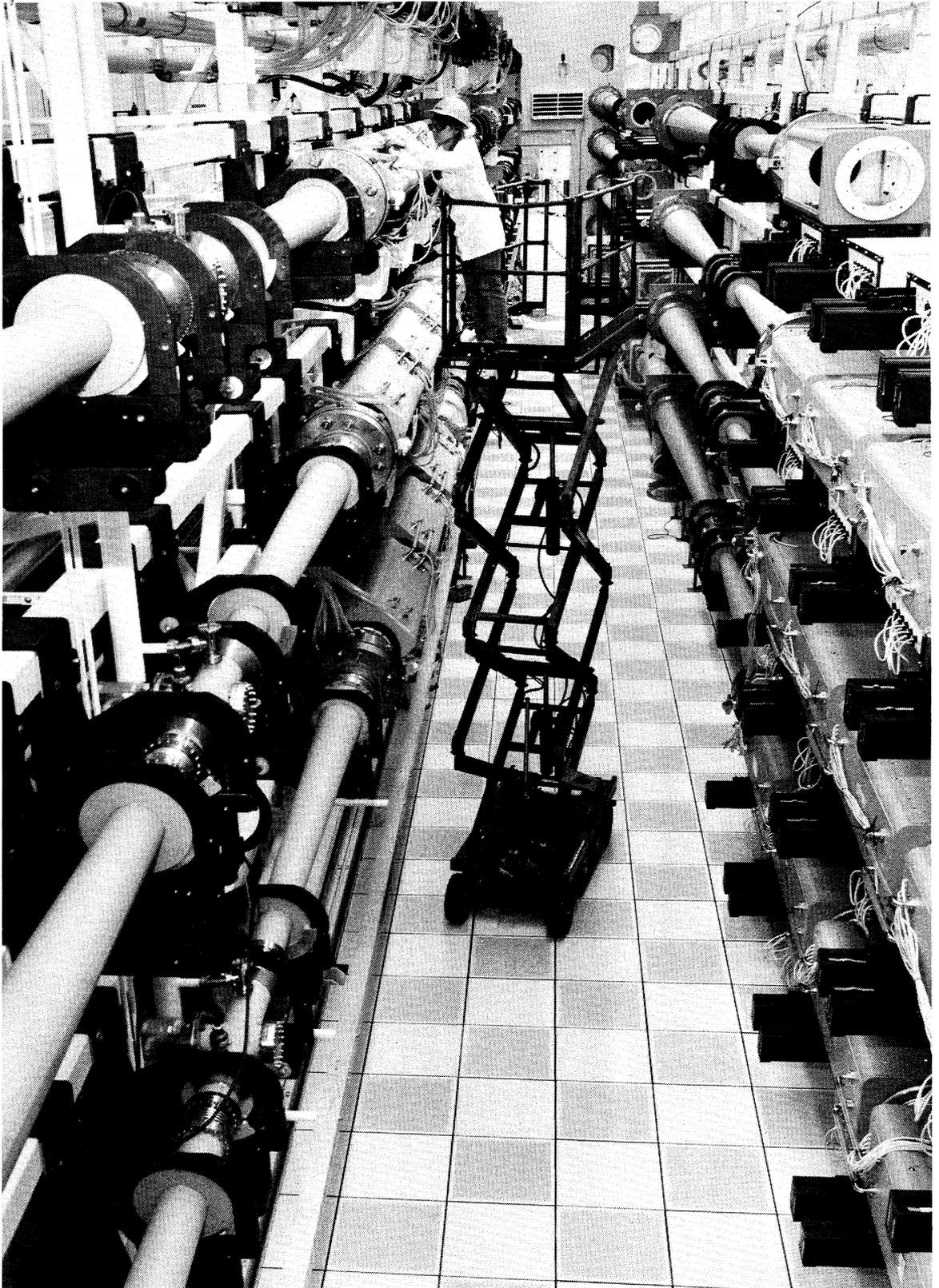
lasers might be important in initiating fusion reactions for both civil and military purposes. Once this was established, the laser group, which by then numbered 15 to 20 people, was removed from the Experimental Physics Division and given organizational stature of its own, with Kidder as director.

Since then the laser fusion program has expanded further. A few years later, Dr. John Emmett was brought in to lead the program. The Laser program came to employ about 500 people, had an annual budget of approximately \$30 million, and was at the department or directorate level (fig. 38). It also stimulated research elsewhere: Los Alamos set up its own laser division, using gas lasers, rather than pulsed-glass lasers, as at Lawrence Livermore. The case of lasers at Lawrence Livermore illustrates two things: how an organization within a laboratory devoted to basic research can spawn a small group that then grows into one of the laboratory's programmatic efforts; and the importance, for every large laboratory, of making a place for small, non-mission-oriented research groups.

The starting of new work is crucial, but it is equally important that work that is no longer productive be stopped. How are research and development groups disbanded? The easiest way is simply to stop funding the project. This was the case, for instance, with the Biosatellite group at Ames Research Center. This function was basically a contract management operation which was shut down once the contract money disappeared. Because the group had no facilities associated with it, the shutdown procedure was fairly straightforward. It is more difficult to shut down those research and development groups that have facilities, since facilities acquire a momentum of their own. Normally, a technical requirement generates a certain facility to carry on the project. Once the project is completed, the existence of the facility may generate new projects. This is perfectly legitimate; in fact, in many cases the new programs are better and more useful than the ones for which the facility was constructed. But it is important not to be trapped into a condition where the facility generates new projects simply to keep the facility alive.* Once this happens, it is unlikely that anything of importance will emerge from the research group operating the facility.

In the case of a large project, the shutdown can cause serious dislocations merely because of the enormous number of people involved, as in Apollo. What mitigated the damage in this case was that by the time

* This does not really contradict our observations in the final section of Chapter IV concerning the importance of facilities. The situation discussed here refers to a facility which was: set up to support one project, rather than to provide support across the board; or was overtaken by new technology; or was underused, during or subsequent to the project for which it was designed.



University of California, Lawrence Livermore Laboratory

FIGURE 38. — *The large-scale "Shiva" laser at the Lawrence Livermore National Laboratory.*

Research and Technology encompasses means knowing the structure of Headquarters-center relations.

There has been an almost cyclical rhythm to Headquarters-center relations in NASA. From 1961 to 1963, the centers reported directly to the NASA Associate Administrator in Washington; from 1963 to 1974, they reported to associate administrators for each of the substantive program offices; in 1974, the centers were placed, for administrative purposes, under an Associate Administrator for Center Operations; in 1977, another change led to the center directors reporting directly to the Administrator; while currently NASA has returned to the post-1963 arrangement, by which each center is under a separate program office (ref. 87). There are three substantive program offices: the Office of Space Science and Applications; the Office of Space Flight, which manages the Space Shuttle program; and the Office of Aeronautics and Space Technology, which (among other things) carries out much of NASA's basic research. These reorganizations have not been arbitrary. It made sense to have the centers report to one very senior official when all of the agency was expected to pull together on one major program — in one case Apollo, in another, the Space Shuttle. But once these programs were underway and it became clear that there were separate programs within the larger mission, it made equal sense to place each center under its own program office, while making it possible for the center to do work that would contribute to all of NASA.

Part of the problem is that there is a certain antagonism between project work and long-term research. Projects have precise goals and schedules; therefore, project staff will tend to avoid large risks and settle for small advances in the state of the art. Project success often involves the selling of the project idea, to the point where the supporting research and technology is often overwhelmed by the need to push the project through the bureaucracy (ref. 88). Thus Headquarters officials, from the Administrator down, cannot simply take a permissive attitude toward Research and Technology; they must actively promote it, if it is to accomplish anything. Headquarters cannot very well monitor everything going on at the section level; what it needs is some document at a low enough level to provide Headquarters with an understanding of what is to be accomplished and at a high enough level to provide the flexibility the centers need — in short, a system that is partly self-regulating.

In NASA, this document is the Research and Technology Operating Plan (RTOP). The RTOP system was created in 1970 to replace an earlier system by which discrete "workunits" were the level at which reporting occurred. This was far too detailed for Headquarters program planning. Instead, as will be seen, the RTOP is organized around a technical discipline; in the Office of Aeronautics and Space Technology (OAST), for example, some 500 RTOPs replaced over 4 000 work unit

the last Apollo mission (Apollo 17) flew, in December 1972, NASA had already received the charter to develop the Space Shuttle (President Nixon had made the decision in January 1972). Thus, many of the engineering development people who were leaving the Apollo program were immediately put to work solving the technical problems presented by the creation of a fully reusable spacecraft.

A Case Study: NASA's Management of Its Research and Technology

For the purposes of these case studies, we treated the laboratory as a closed system. This is far from being the case. The sponsoring agency has to defend its mission before Congress and the Office of Management and Budget. There may also come a time when it appears to headquarters officials that a particular installation no longer justifies continued support; in that case it may be cut back, closed, or merged with another installation. But there is a range of activities which are neither projects nor routine administrative operations, but which set the terms for future development work. In NASA these activities are grouped under "Research and Technology" (R&T), and are a crucial part of the agency's mission. As an internal report put it, NASA's Research and Technology program: "encompasses basic research, provides both a near and far term technological base for the future, creates essential capabilities for the next project or mission, provides options for future mission selections, serves to strengthen American industry, and assists other agencies of government. It helps to support universities, educate students, and develop new markets for technology. It contains the 'corporate memory' in science and technology and maintains a scientific and technical institution in government, industry, and universities that is a basic national strength." (ref. 86.)

NASA has invested heavily in its R&T; it accounts for about ten percent of average annual expenditures — some \$600 to 700 million — and involves some 10 000 professionals spread about among NASA, its contractors, and the universities. Yet, as the excerpt above illustrates, the importance of research and technology is even greater than the figures indicate. At the same time, the Research and Technology program raises the kinds of vexing questions that a mission-oriented technology development agency confronts: Can basic and applied research coexist within the same installation? How can basic research tasks support the agency's mission or missions without becoming applied research? How can research tasks be evaluated, that is, what are the criteria for success or failure? Finally, how can Headquarters monitor thousands of research tasks, while giving center directors freedom to explore new areas? To begin to answer these questions, a description of the way the Research and Technology program is organized is needed; and to understand what

Research and Technology encompasses means knowing the structure of Headquarters-center relations.

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statements (ref. 89). The RTOP is an agreement between Headquarters and a center to perform the described research and technology within a specified time and using specific resources; it includes the center's technical approach and contracting plan; and it normally has finite life of one to three years. Under the system, center directors are given enormous flexibility, not only to start new research but also to reprogram funds across approved RTOPs. The RTOP is the product of negotiations between the center director and Headquarters program offices; the latter defines the broad objectives of the office's Research and Technology program, and the former drafts a proposal setting forth how and with what resources the work will be done. The RTOP is, at least in theory, at the lower end of an integrated budgetary system. As one ascends from one level to the next, one goes from the particular to the more general, from the RTOP to a subprogram or discipline, to a unique project, to a budget line item.

Suppose, for example, that OAST issues an RTOP for work on actively cooled structures. An ascending sequence might look like this (table 2):

Table 2. Research and Technology Objectives and Plans Structure

<u>Level</u>	<u>Budget Structure</u>	<u>Examples of Program Work</u>
V	RTOP	Actively Cooled Structures
IV	Specific Objective	High Temperature Material
III	Subprogram/Discipline	Materials
II	Unique Project or Program	Aeronautics Research and Technology Base
I	Budget Line Item	Aeronautics

Thus the RTOP system was designed so that senior NASA officials could determine that the centers had measurable goals within the agency mission; that these goals were subsumed within a variety of research and development activities not tied to specific projects; and that these activities had been properly scheduled and costed out. Yet within a few years of the introduction of the RTOP, it became apparent that the system was not working precisely as intended. Thus OAST, which for many years had overall responsibility for coordinating the Research and Technology program, was unable to do this well, partly because OAST was not working closely with the "using" program offices, and partly because these offices saw OAST as a competitor for funds (ref. 90).

There was, then, little transfer of technology from those developing it to those who could use it.

But there were more general problems as well. First, the system did not adequately support the budgetary process, since the budget request had to be formulated before that year's RTOPs were drafted. An unfortunate byproduct of the budgetary process was NASA's tendency to justify its R&T program in terms of future benefits rather than past accomplishments.

Second, almost all of the centers complained of the level and detail of control exercised by Headquarters. With over 1 100 RTOPs in force at any time, they appeared to be not so much contracts between Headquarters and the centers as "agreements between specific people at Headquarters and specific research groups within the centers . . . In nearly all cases, the center Directors felt that they should be more a part of the management process." (ref. 91.) There was consensus among center Directors that the number of RTOP's needed to be reduced to the point where each RTOP stood for one specific objective per center.

What the centers wanted, and Headquarters could concede only to a limited degree, was greater flexibility: whether in starting new work, reprogramming funds, or making changes in the scope of approved work. NASA in the 1970s was making a slow, painful transition from being an agency dominated by a few very large projects to one of many smaller interdependent tasks; to a blurring, a running together, of aeronautics and space technology, so that the same test facilities were being used for both; and to a greater interdependence among the centers themselves, so that centers were supporting program offices other than those to which they formally reported. (Unlike Apollo, whose goal involved landing on and exploring the Moon, the Space Shuttle was a vehicle intended to provide more effective support for a variety of other programs.) Because the transition was so difficult, and because the research and technology program would play a crucial role, NASA created a task force in 1976 to consider how the centers could retain a certain autonomy while remaining accountable to Headquarters for the ways programs were conducted.

Two of the task force's findings were especially interesting. Its members concluded that the very complexity of NASA's Research and Technology work made it difficult to reduce the number of RTOPs by more than thirty percent. As the authors of the final report noted: "Spanning basic research into fundamental processes of nature, of fundamental space sciences, of applied research into high risk endeavors, of developing new instruments and sensors, of demonstrating the worth of a new technology, of building the capability for new missions, the array of activities . . . is truly large. Any significant reduction in the number of RTOPs would be damaging to the visibility and communications between Headquarters and the Field Centers . . ." (ref. 92.)

The task force also noted that the freedom of center directors was limited to approved programs. The members wanted centers to have the freedom to start new — probably high risk — work, without the need for prior approval by Headquarters. This, it will be remembered, was what the Defense Science Board had recommended for DOD laboratories in 1966. Under this concept, NASA would create a “Center Directors’ Discretionary Fund” as a source of seed money for establishing new product lines or for bringing a center to a level of technical excellence in new areas; no task would run for more than two years; and the fund would be used neither “to bail out on-going projects that may be in trouble nor to meet deficiencies in construction, operation and maintenance or for laboratory equipment funds.” (ref. 93.)

Yet the task force members were unwilling to overhaul a system they regarded as fundamentally sound. Nothing could better illustrate the difficulties in doing technology development than the efforts of the task force to reconcile the centers’ need for the freedom to pursue research wherever it might lead with the agency’s need for accountability. The task force’s recommendations might be described as a series of injunctions to both sides: Reduce the number of RTOPs and, where possible, use a one-or two-page RTOP that references project documentation that serves the same purpose as an RTOP; encourage the Headquarters program directors to visit the centers once or twice a year for a comprehensive program review; set up a discretionary fund so that centers may start new projects; select a small number of goals to provide guidance to the centers; remove the job of coordinating the Research and Technology program from the Office of Aeronautics and Space Technology and give it to a special assistant within the office of the Associate Administrator.

Underlying the task force’s conclusions was the sentiment that Research and Technology work must be supported for its own sake and not as something ancillary to a project. The problem facing NASA was this: How could the agency coordinate a number of small-scale efforts and organize them in related fashion, yet not tie each research task to a specific mission or completion date? This is not NASA’s problem, so much as it is a problem inherent in any organization trying to discover new knowledge about physical phenomena.

Conclusions

The more complex the task of technology development becomes, the harder it is to assert that there is one best form of organization to get the job done. Compared to the 1960s, NASA is a much less “projectized” agency now; there are fewer big, discrete flight projects like Apollo or Skylab, more of a give-and-take involving all of the centers. As Sayles

and Chandler observe: “. . . it would appear that the greater the inability to define interfaces, the more the agency must rely on relatively decentralized coordination . . . the locus of control has shifted from the field level to the headquarters level and back again as technology has become more complex.” (ref. 94.)

The RTOP case study bears this out. Headquarters oscillates between giving the centers great discretion in beginning new work and asking for a full accounting of work completed; between making one program office responsible for coordinating research and technology and spreading that responsibility among the other program offices; between demanding detailed statements of what the centers intend to do or settling for brief summaries. The fact is that there are many ways of doing research and technology development and all of them may be appropriate, depending on what is to be done. An organization appropriate for a manned lunar landing may be the wrong one for bringing about a revolution in microelectronics.

What one can do is lay down some general rules which, with the greatest of luck and support by the sponsoring agency, may lead to some worthwhile research and development. The first rule might be called, with a touch of cynicism, the Law of Grantsmanship: “Innovation” is a very useful point in selling a project to the potential sponsor. Much of what goes by that name is really a kind of fundraising. Consider, for example the history of the Air Force’s Office of Scientific Research. From the official history of this organization, it appears that the only way to get funding was to call things by other than their proper names. The director of Air Force Research and Development made it clear that the Office “could not hope to get any money unless it accepted a certain amount of semantic perversion in its programming . . . Basic research and applied research were dropped from the programming idiom, replaced in turn by exploratory research and supporting research . . .” The more practical a category sounded, the better (ref. 95). What is more, this approach worked. The Office of Scientific Research “talked of applications, and the Bureau of the Budget loosened the purse strings.” One official could justifiably brag that “we sold them the sizzle, not the steak.” (ref. 96.)

The second rule is to know when to move from research to development. Here, timing is everything. In NASA, virtually every flight project from the early 1960s to the mid-1970s was conceived in a brief period — from mid-1958 to the end of 1961. It was not that NASA scientists and engineers ran out of ideas; rather, it would have been self-defeating to remain in the conceptual phase much longer. In many ways the ultimate test of a good research idea is its incorporation in a discrete project with a definite beginning and end. Even for a new agency, the quality and quantity of ideas for NASA flight projects were

remarkable. But large Federal agencies are not, as a rule, the best place for generating new ideas. The Atomic Energy Commission explicitly recognized this when it turned over its research and development work to contractor operated facilities. Without going so far, NASA officials have conceded that many of the most creative ideas for its programs will come from outside, from contractors, universities, and advisory committees. The point is that the seminal ideas — the capital off which the sponsoring agency will live for a generation — may be generated in a short span, but their working out is a complex, lengthy, and very expensive process that only a large organization can manage.

Our third and final point is that a research installation of the right size (see Chapter IV) should *always* do some research in advance of current needs. Defensive research, such as NASA's Research and Technology program, serves many purposes: It provides the agency with a portfolio of ideas which *may* reach the development stage — in some cases, 20 to 25 years after they were first conceived (the orbiting Space Telescope is an example); it is a way of making effective use of young engineers and scientists just starting their careers; and as our case studies illustrate, it may reshape the entire installation so that its mission coincides with, or becomes part of, those disciplines which appear to be at the leading edge — microelectronics or artificial intelligence as opposed to civil engineering. The more formal a management system is, the more time-consuming the review process becomes and the less likely it becomes that anything genuinely new will emerge from the organization. Basic research not tied to a specific project is one of the ways by which a technology development organization stays alive.

MAJID & JANDIYETHI

CHAPTER VI

Projects: The Ultimate Reality

Definition of the Subject

In the past five chapters, we have described the process of technology development in some detail and we have tried to define the features of the institutions in which it is practiced. We have stressed that the process of technology development in the end should lead to some "practical" application of the technology that is being created and developed. In the institutions that we are concerned with we do not study solid-state physics for its own sake but rather to create (say) small light sensitive detector elements that ultimately will be used to take better pictures of the planet Mars, or perhaps of Russian ICBM installations to monitor arms control agreements. The application of the technology being developed requires that it be used in some kind of a "system" designed to accomplish some end. The creation of this system is usually accomplished by carrying out a project. The word "project" itself is quite neutral and can mean anything from building a bridge or a group of tract houses to creating a Broadway show. In the context of technology development, however, it has come to mean something special, with sometimes unfortunate results.

The use of project methods is nothing new in facilitating the application of new technology, and history abounds with good examples — such as the construction of the "Monitor" in about six months in 1862 under the leadership of John Ericsson. The Manhattan Project and Apollo were much more complex and the results were apparently so much greater than anything heretofore attempted that some people began to believe that there was something magical about the project approach, independent of the technological substance of the project. Indeed, many government officials believed that the project approach could be transferred bodily to the solution of hitherto intractable social problems. The project approach was to be adapted to developing modular, low-cost, factory-built housing; to transferring available technology to municipal and county governments; to starting a Dial-a-Ride program intended to combine the advantages of urban mass transit with the convenience of an automobile; and even to building a "Personal Rapid Transit System" (with the Jet

Propulsion Laboratory as prime contractor), a guideway with small cars which could pick up and discharge passengers on demand (ref. 97). None of these projects was an unqualified success and some (the Personal Rapid Transit System in particular) were, frankly, white elephants.

Despite these reservations, we regard the project approach for applying new technology to be of central importance. That is why we are devoting a chapter to it. But to understand the advantages and limitations of projects better, we must begin with some definitions. Whatever else may be said about them, all projects have the following four features: They are planned to have a definite beginning and end; they have a specific goal; there is a fairly precise limit to the number of people, below which the activity is a research task, above which it becomes coextensive with the agency's mission; and they all involve more than one science or engineering discipline, so that the larger the project, the greater the need for coordination.

It is rarely the case in a large project that the manager will be accountable to only one official in one organization. In a NASA flight project, for example, the manager will interface — to use a horribly technocratic but useful word — with the Headquarters program manager, representatives of the prime contractors, principal investigators who design and develop experiments to be flown, subsystems managers, contracts officers, and, of course, the management of the laboratory at which the project is located. Thus a project of any complexity leads to a systematic, continuous review of all the elements, as they move from conception to hardware; to a breaking down of organizational barriers, so that the manager may draw on the requisite skills, wherever in the organization they may be located; to a sharing of authority; and to a constant flow of communication among all the managers in the project organization (ref. 98).

Having defined a project, the question we would ask is, What are the advantages and disadvantages of the project approach? The principal advantage is that, in the words of a National Research Council report, "projects often provide the ultimate reality." (ref. 99.) It is one thing to originate a concept for a new aeronautical vehicle, nuclear reactor, computer, ship, or spacecraft, another to "prove" the concept, in NASA jargon. As the report notes, "Projects are practical demonstrations. New equipment must function well, performance is measured against the previous experience, and success needs to be achieved."

By bringing together people in many technical disciplines, the project may lead to interactions that could occur in no other way. Another, quite different, advantage of a large project is that it builds political constituencies willing to support the agency's mission. A project, even a small one, has a visibility that a technology development task lacks. And when the project is very large, as with Apollo, it may

even bring in research and technology development work on its coattails that might never have been funded on its own.

Yet the project approach sometimes entails heavy penalties when it is pushed to the exclusion of other approaches and becomes a brute force effort to achieve a goal, or freezes technology prematurely. The tendency of large projects to close out options is one of the hidden costs associated with this approach. There is no better example of this effect than the choice of the mission mode for the lunar landing. In the preceding chapter, mention was made of the events leading to NASA's selection in 1962 of lunar-orbit rendezvous for Apollo. On strictly technical grounds, lunar-orbit rendezvous was a great success, since the lunar landing was achieved on schedule. But it also ensured that Apollo would be a dead end. By 1969, it was apparent that there was no logical sequel to the lunar landing, and that the agency would have to redeploy its resources in a radically different direction. Had NASA selected earth-orbit rendezvous instead, the lunar landing could still have been achieved and NASA would have had at least a ten-year start on deploying an orbiting space station, rather than waiting until 1982 to let study contracts for its design.

Another example of premature commitment to a certain approach refers to several of NASA's more advanced scientific satellites. As we shall see, there is considerable (although not conclusive) evidence that the Orbiting Astronomical Observatories launched by NASA between 1966 and 1972 represented too great a forcing of the available technology. Several of the agency's scientific advisors argued unsuccessfully in favor of cheaper, less ambitious satellites that might have returned data earlier. What is more interesting, some senior NASA officials came to believe, after the fact, that their advisors had been right.

It may be that what was wrong with these decisions — if, in fact, they were wrong — was the decisions and not the project approach itself. But we would argue that the decision to select one method to the exclusion of others is inherent in the project approach to technology development; that it is usually neither possible nor even desirable to attempt all feasible alternatives simultaneously; and that it is precisely the business of an institution's or agency's senior management to study the long-range implications of projects that the line organization wants. Whatever the merits of a phased project approach, it becomes exceedingly difficult to alter the design, as opposed to the purpose, of a project, once it goes beyond the advanced study stage. Once the decision to proceed with (say) Apollo had been made, a projectized approach was inevitable. But as a result, the Apollo project, in the words of one scholar, "could not capitalize on most post-1962 developments, and therefore placed less relative emphasis on basic development. Advanced development in the NASA program as a whole had to be a matter of secondary emphasis." (ref. 100.)

It is instructive to compare the approach described above with the way private industry pursues some of its research and development goals. The most obvious difference is one of size; a project running to \$10 to \$15 million would be relatively minor for NASA or DOD, but a major effort for all but the largest companies and their research arms — Bell Labs, IBM, General Electric. In a few cases, these companies have been willing to invest enormous sums in new technology; IBM is reported to have staked \$5 billion on its 360 family of computers (ref. 101). But such cases are even rarer in the commercial than in the government sector. Cost considerations aside, the emphasis in industrial research is often on incremental improvements in existing products.

Where genuine innovation occurs, there are many routes besides the project approach to it. Important innovations have been the work of inventors and small companies, whose inventions were taken over by large firms (phototypesetting). They have occurred when individuals at small firms sponsored the initial commercial development (oxygen steelmaking); when individuals in several organizations contributed the basic ideas for a working system (television); when two inventors, one of whom worked in a research laboratory under conditions resembling those of an individual inventor, recognized the outstanding properties of a new product (stainless steel); when a scientist working in a large corporation pursued a promising idea as a sideline but was encouraged to concentrate on the work (long-playing records) (ref. 102). The case of radio is particularly interesting, since the majority of the basic inventions: "... were produced by individual inventors who had no connections with established firms in the communications industry, or by men who worked for new firms. . . . large firms only entered the field after the potentialities of the invention had been established. Once the large firms started work, they made valuable contributions to radio technology, but . . . a few individuals in the large industrial laboratories did the outstanding work. Nor have all the recent innovations come from these laboratories; an outside worker contributed to the most radical of them." (ref. 103.)

The precise word for the behavior of the large industrial laboratories is "opportunistic." That is, they are prepared to exploit technical breakthroughs made outside the organization — unless the "not-invented-here" syndrome is at work! In the same way, government laboratories position themselves to take advantage of innovations generated elsewhere — by industrial contractors, advisory groups, non-profit research institutes, universities, and the like. Some of the major innovations of the past forty years — jet engines, integrated circuits, radar, lasers, rockets — have not been developed in response to a firm statement of military or commercial requirements (ref. 104). There were and are advantages to letting development occur in this apparently random fashion. The large

organizations, commercial and governmental, can let new ideas work themselves to the point where they are ready to be exploited. (For every Bell Labs or IBM, there are dozens of laboratories of large corporations that have produced little of importance.) If this view is correct, the research and technology work carried on in governmental laboratories is not intended to lead immediately to advanced development. Rather, it gives researchers the experience they need to evaluate ideas originating elsewhere; and it enables them to generate their own ideas which, in turn, may evolve into projects requiring the kinds of outside innovations already mentioned.

For two reasons, then, the large goals-oriented project is not always an important source of innovation. Once the commitment of resources and manpower to a large project has been made, the main consideration for the sponsoring agency or company is achieving the goal. Any spinoff of new technology is incidental. The second reason has to do with the systems approach used by many agencies in their technology development work. A system may be defined as a series of "complex, interrelated elements or components working effectively together . . . to yield a single desired result." (ref. 105.) Where the system is defined broadly, even marginal improvements in the performance of (say) an Air Force fighter plane may be very expensive. They will be expensive, first, because the engineers working on the system will make relatively little use of available components; second, the greater the percentage of the system to be replaced, the greater the changeover costs will be; finally, the larger the boundaries of the new system, the greater the amount of manpower needed to make the tradeoffs inherent in treating a large number of components as a single operating system (ref. 106).

In sum, there are certain situations which lend themselves to the project approach. These are where: there are specific, discrete goals; considerations of costs and scheduling, especially the latter, are paramount;* and the technology to achieve the goals is available, even if that technology must be vastly extended. The first two situations may be independent of the third, since projects can and do occur outside a technology development environment. The combination of severe cost and time constraints with a definite goal accounts for the popularity of the project approach in public works and heavy construction generally. Where goals are open-ended to the point of continuing through the

* There are differences between government and ordinary industrial projects. For government, and particularly the Defense Department, it is the reliable achievement of technical performance that is preserved in a crunch. Schedules slip and costs rise, unless it becomes clear that a further change in goals imperils the project. In practice, the project approach as exercised by government rewards well the achievement of technical success, but does not strongly penalize cost and schedule variances unless they affect that success.

sponsoring agency's lifetime, there may be less to be said in favor of the project approach. Whatever else may be said about them, Apollo and the Space Shuttle represent two very different kinds of missions, if only because the latter is open-ended and continuing, where the former was not. In many cases it is no longer possible for NASA to do what it did in the 1960s: issue a Project Approval Document which tracked the project from cradle to grave and was revised annually. The Space Shuttle is best thought of as a vehicle rather than a substantive program. As such, its connections with NASA substantive programs, with those of DOD, with commercial users, and with foreign governments are so complex that the single-mindedness of a conventional project is missing.

Having examined those conditions that militate for or against the project approach, we turn to the kinds of organizational issues — how projects are selected, approved, and implemented — that can be analyzed under the head of project management.

What Project Management Entails

If it were the case that no two projects are alike, nothing useful could be said about them. However, when research officials — whether in the laboratory, at Headquarters, or contractors pursuing independent research and development — consider starting a new project, the same questions normally arise. Consider these questions from the perspective of a laboratory director. At a bare minimum, these are the kinds of considerations that will enter into the selection of a project:

- What is the relation of the new project to the laboratory's charter, its traditional missions, and the sponsoring organization's mission?
- Are resources available to do the project? Will it adversely affect other work? If so, to what extent must the other work be modified to accommodate the project? Are qualified people available to do the job?
- What are the long-term possibilities of this project? Will it develop a new capability in the laboratory?
- Does the project really serve some outside user or merely the laboratory's interest in maintaining itself?
- Shall the work be done in house, by contract, or by some mixture of the two?
- To what extent can the laboratory draw technical personnel from all its operating divisions, or limit itself to that division to which the project is assigned?
- Shall the project be managed by an autonomous group or by functional specialists? Shall the project be structured according to the type of technology or project objectives, or with one center as "lead" within the sponsoring agency?

These are only some of the questions which must be faced early in the life of the project. A method called phased project planning is normally employed in NASA, and its four stages correspond rather well to the logic of project management (table 3).

Taking table 3 as a schema of a typical large technology demonstration project, there are several points to be made. First, the key project decisions are almost always made in the first two phases. Even the decision by a field center or a laboratory (let alone Headquarters) to authorize an advanced study sends a message to other centers, to other agencies with the capability for doing the work — indeed, all the way up to Capitol Hill. A study contract is normally interpreted by the successful bidder as an instruction to review means rather than ends. Studies may examine a proposed project from many angles: A study may be *exploratory*, analyzing an idea for a new program or system; it may be an examination of *feasibility*, to determine the possibility of accomplishing a given project within a specified period; it may be *parametric*, a study of tradeoffs between the different elements of a project; it may be a *preliminary design* study, which makes detailed assessments of the assumptions underlying earlier study phases; or it may be a *detailed engineering design*, in which the design is specified to the point where contracts for hardware production could be let (ref. 107). But the point to stress about advanced studies is that they have an importance out of proportion to their dollar values or findings. From the laboratories' or centers' perspectives, advanced studies create a portfolio of ideas, some of which may be dusted off years later; where multiple study contracts are let, the laboratory or center can evaluate the project from many angles; and it can also begin a preliminary, informal winnowing of firms which may be qualified to bid on the big production contracts, except in those cases where a "hardware ban" is written into the study contract.

But the advanced study is only part of the broader strategy of the research center or laboratory. The center director will attempt to stake out his territory as early in the project as possible. His concern is to keep the sponsoring agency at arm's length, to prevent the project from being run *de facto* out of Headquarters; to use the know-how of the study contractor(s), while keeping all major decisions in the hands of his technical staff; and to signal other operating elements within the agency that the project, as approved, will be run out of his shop. It cannot be emphasized too strongly that in a multi-center agency like NASA, almost all of the ideas for new projects or programs — everything from sounding rockets to the Space Shuttle — have flowed upward from the line organization rather than from the top down.

When, in June 1969, the Associate Administrator for Manned Space Flight created a task force to study the possibility of a reusable space vehicle, the internal debate on a sequel to Apollo was already concluded.

Table 3. Phased Project Planning

Method: Phased Project Planning

Objectives:

1. To create an option-preserving sequential decision process, with four major management-decision points.
2. To progressively refine project requirements to produce a detailed work statement that will permit the use of contracts containing appropriate forms of incentives.

Project Stages	Objectives	Parties Involved	
		In-House	Contractor
1. Planning and Definition Phase A — Preliminary Analysis	Analysis of alternate overall project approaches and concepts	Primarily an in-house effort	Support role for university and industry study contractors (FP or CPFF contract); need not be capable of Phase B, C, or D.
Phase B — Definition	Selection of one of several project approaches for further definition and eventual development, if this seems advisable; effort may be cut off here	An analysis role	Study contractors develop information (FP or CPFF contract); not a competition for Phase D contract.
Phase C — Design	Definition in detail of the project approach selected in Phase B	Integration and validation of contractor data	Major portion of work is contractor conducted (CPFF or incentive contract); generally two or more prime contractors selected; only firms capable of performing through Phase D are eligible since Phase C provides competition for Phase D.
2. Project Implementation Phase D — Development / Operations	Final hardware design, development, fabrication, testing, and operation	Monitoring and review functions	Major portion of work is contractor conducted; restricted to Phase C contractors except in unusual cases; one prime contractor (incentive contract).

Note: FP = fixed-price contract CPFF = cost-plus-fixed-fee contract
 Source: Leonard Sayles & Margaret Chandler, *Managing Large Systems: Organization for the Future* (New York: Harper & Row, 1971), p. 33.

(The membership of the Task Force consisted mostly of people from the field centers.) With two-thirds of the agency's budget, the Office of Manned Space Flight (OMSF) was the most powerful program office in NASA. And conditions were uniquely favorable for Dr. George Mueller, who headed OMSF, to take the initiative. The first lunar landing was anticipated within the next month; the Air Force had just cancelled its Manned Orbiting Laboratory, the only remotely conceivable contender in manned space flight; and NASA had a new Administrator brought in from the outside who did not yet have his hands on the levers of influence. By then, there was a consensus among OMSF managers that the Shuttle was feasible, that it would draw on the expertise gained in Apollo, and that there was enough support behind it to sell it to the rest of the agency and — possibly — DOD. What applies to the Shuttle applies with equal force to other flight projects. What happened was that the field organizations — Marshall Space Flight Center (headed by Wernher von Braun) and Manned Spacecraft Center (headed by Robert Gilruth) — took the lead in proposing new projects and convincing the Headquarters people to go along.

In project work, considerations of substance can (and should) determine considerations of procedure. The decision on whether work will be done in house or by contract depends on the nature of the project. Except in special cases, the option of doing everything in house exists mainly as a theoretical possibility; most laboratories lack design, test, and fabrication personnel and the necessary facilities. A more common approach is for the laboratory to do the conceptual studies and overall management, with the detailed design and most of the hardware contracted out. Depending on the laboratory's strengths, one of two approaches is used:

- The laboratory acts as project manager, designer, and systems integrator, with specific components, or "black boxes," farmed out to subcontractors.
- The laboratory acts as project manager, with a prime contractor handling the complete hardware program from detail design through delivery.

It is important to understand that, in contracting out, an agency like NASA is not simply making a virtue of necessity. NASA stands in relation to its contractors somewhat as an industrial firm like General Motors stands in relation to its suppliers. Antitrust considerations aside, General Motors has been in a position to encourage competition among its suppliers, playing one off against another, as well as seeking (and getting) the most favorable combination of cost and design: turning to this firm for tires, to another for suspension system components, to a third

for engine valves and temperature controls.* Vertical integration could hardly promise as much, and that only at much greater cost and internal complexity. Similar considerations have been at work in NASA, the Air Force, and the Atomic Energy Commission from the beginning. For reasons discussed in Chapter III, a reversion to the arsenal system after the Second World War was not politically feasible. But especially since the mid-1950s (see the Polaris case study later in this chapter), the large Federal technology development agencies have deliberately encouraged potential contractors to bid for agency work, to sponsor their own self-initiated independent research and development reimbursable as a percentage of overhead, and to submit unsolicited proposals against the time when they would be incorporated in formal requests for proposals.

Whether the benefits of large-scale technology development carried out by contractors still outweigh the costs is open to question. The cost of entry into aerospace technology development in the late 1950s and early 1960s was lower than it is today but it was still large. Today it is prohibitively expensive for a firm to enter in any capacity other than that of subcontractor for a federally sponsored program. Some of NASA's largest contractors in the 1960s, like Boeing and Grumman, have largely withdrawn: to concentrate on commercial work in the case of Boeing, on defense work in the case of Grumman. All of this has greatly enhanced the Federal role in technology development and has made careful planning and thinking within the government and its advisory structure much more important than it has been in the past. It has also enhanced the importance of giving more freedom to contractors and to put as much of the responsibility for the project management and execution on the contractor. All of this must be done using procurement regulations that were written by people who were not always sensitive to these considerations.

Contracting out projects has important advantages over doing work in house, especially where agency personnel ceilings are fixed. In Chapter VIII we shall consider the use of contractors for support services — everything from trash removal to writing computer programs to

* Indeed, GM may consider it even more important to find suppliers for a variety of parts than to stimulate competition among suppliers of a single part — “Firm A for tires today, Firm B for tires tomorrow.” The reason is that GM's overwhelming size *vis-a-vis* its suppliers gives it formidable bargaining power. Uniroyal, which manufactures the tires for many GM cars, needs GM far more than GM needs Uniroyal. Moreover, the recent agreement between GM and Toyota will place the original equipment manufacturers who supply GM in a position analogous to those defense contractors who are losing business. As more parts are manufactured overseas, these suppliers will be stuck with excess capacity, while coming under pressure from GM to reduce costs still further and improve productivity.

managing entire installations. But NASA's philosophy of using contractors has been based on certain principles which carry over into its technology development work: The rapid buildup of large projects has precluded reliance on government employees alone; it is Federal policy not to develop capabilities that are already available in the private sector; and it is better to let the up-and-down swings in manpower take place in the contractor, rather than the civil service, work force (ref. 108). Also, contract employees do not normally count against an agency's personnel ceiling. Within limits, agencies like NASA and DOD have great flexibility in their use of contractors, even extending to the right, affirmed by the Federal courts, to lay off their own employees before laying off contracted personnel (ref. 109).

Thus the final shape of a project will depend on a good deal more than the availability of funds to get the work done. There are many interdependent elements: the ability of the lead center or laboratory to define a mission, the availability of contract support, the particular capabilities of the lead center or laboratory, and the speed with which the center or laboratory can move from preliminary analysis to design definition to a definition in detail of the project approach. Clearly, center and laboratory managers must be able to "cost out" projects. Project costs are normally estimated by one of two methods. The first is through detailed comparison of previous similar projects of known costs; the other is to generate costs by a complete "from-the-ground-up" work breakdown, sometimes with the assistance of computer models. This latter approach, a kind of zero-based budgeting, examines the efforts involved in every element by the required manhours. Finally, all the elements are added up to develop the overall cost. The first method is easier and is as reliable if, and only if, the comparison program really is similar.

Just how difficult cost estimating of large projects really is, can be shown by table 4, which reviews NASA's early projects.

Where the agency is buying production-line items, where (for example) spacecraft and experiment design were established before the start of the project, accurate estimates of project costs are possible. But cost estimating is and will remain very much an art, until completely standardized project hardware with experiments that can be "plugged in" has been developed. That time probably will never arrive for advanced technology development projects of the kind considered in this book.

There is considerable variation in project structure, most of it involving the degree to which the organization is "projectized" versus reliance on "functional organizations."* Suppose a project requiring 200

* An intermediate approach is known as matrix management. Here, employees are temporarily assigned to a project, while remaining on the rolls of the parent organization.

Table 4. Cost Growth in Selected R & D Projects, 1958 to 1966, in Millions of Dollars

Project	Date ¹	Initial Estimate			Final (or Current) Estimate			Unit Cost Ratio (Final/Initial)
		Total Cost	Number of Spacecraft	Unit Cost	Total Cost	Number of Spacecraft	Unit Cost	
Tiros	10/58	16.9	3	5.6	72.6	12	6.1	1.1
OGO	1/61	25.3	3	8.4	218.5	6	36.4	4.3
Surveyor	1/61	115.9	7	16.6	468.5	7	67.0	4.0
OSO	1/61	14.7	3	4.9	95.7	8	12.0	2.4
OA0	1/61	53.4	3	17.8	280.7	4	70.2	3.9
Ranger	1/61	49.2	5	9.8	170.0	9	18.9	1.9
Nimbus	1/61	33.5	4	9.6	308.4	7	44.1	4.6
Relay	1/61	17.8	2	8.9	33.6	3	11.2	1.3
Syncom	8/61	12.7	2	6.4	27.6	4	6.9	1.1
Pioneer	11/62	27.0	7	3.9	133.9	7	19.1	4.9
Mariner Mars 1964	8/63	48.2	2	24.1	82.8	2	41.4	1.7
Orbiter	8/63	77.3	5	15.5	163.1	5	32.6	2.1
Biosatellite	9/63	49.9	6	8.3	160.0	4	40.0	4.8
ATS	4/64	109.3	5	22.1	143.7	5	28.7	1.3
Mariner Venus 1967	3/66	30.2	1	30.2	26.5	1	26.5	0.9
Mariner Mars 1969	3/66	98.0	2	49.0	130.3	2	63.2	1.3
Subtotal OSSA		784.3			2 515.9			1.9 ²
Mercury	2/59	200.0	—	—	384.0	—	—	2.4 ²
Gemini	12/61	529.0	—	—	1 283.0	—	—	1.6 ²
Apollo	5/63	12 009.0	—	—	19 741.0	—	—	
Subtotal OMSF		12 738.0			21 408.0			
Total		13 522.3			23 923.9			

¹ These are the dates at which the cost calculations start.

² Cost ratio based on total cost.

Source: Arnold Levine, *Managing NASA in the Apollo Era*, Washington, DC: NASA Scientific & Technical Information Branch, 1982), p.155, Assistant Administrator for Program Planning and Analysis, April 10, 1969.

people at its peak. In a fully projectized operation, all 200 people would work directly in the project office and would "get their paychecks" from the project manager. For example, the office that managed the Pioneer 10 and 11 missions to Jupiter and Saturn was fully projectized (fig. 39).

There were three major groups — spacecraft, experiments, and operations — each with its manager. Each person had specific assignments: to follow the spacecraft subsystem as it was developed by TRW, the prime contractor, to follow from one to three experiments, or to follow functions like launch, tracking, and data acquisition.

With the functional approach, a small project office of (say) ten people would be established. They would control the project, but the bulk of the work would be farmed out by task order to functional organizations such as the Mechanical Design Section, the Test Laboratory, the Electronic Design Section, and the like.

Each of these approaches has certain advantages and disadvantages. A projectized organization can maintain tighter control of the project, both technically and fiscally. Personnel can devote all of their attention to the project; the project office is responsible for a given assignment from beginning to end; and the organization is tailor-made to fit the job (ref. 110). The principal drawback is the inefficient use of manpower. In the functional approach, when someone is needed on the project only one day a week, he can do other work during the remainder of the week. In a project organization, he tends to sit on his hands the other four days. The problem with the functional approach is threefold: It conflicts with the desire of functional managers to build a technical expertise in their sections; responsibility for a given job is diffused; and only part-time attention can be given to any one project (ref. 111).

Obviously, no idealized description of a project approach can do full justice to the range of projects within even one agency. A given project will be affected by many variables other than those discussed. It may be affected by a midcourse change of goals by the sponsoring agency; by an increase in the length of the project approval process relative to the length of the project; and by the jeopardy to the careers of project managers in committing themselves to long-term projects running to five or more years. To examine how goals of a sponsoring organization tend to shape the kinds of projects sponsored, we have selected three projects for analysis. One was a large space-flight project sponsored by NASA; the second was a successful weapons development project; and the third involved work on several fronts by a leading industrial laboratory.

Case Studies

The Orbiting Astronomical Observatories (ref. 112). One of the interesting byproducts of the Second World War was that it made space astronomy possible. With the delivery of captured V-2 rockets, American

astronomers began to launch scientific instrument packages above the atmosphere. By the late 1950s, balloons and rockets like the V-2 and the more sophisticated Viking and Aerobee had returned a wealth of information: the first photographs of solar spectra from altitudes up to 75 kilometers, the discovery of X-rays in the upper atmosphere, and the detection in 1957 of ultraviolet radiation from a star.

But rocket-borne astronomy had many drawbacks. Viewing time was limited to a maximum of 45 minutes, payload weights were restricted, and stabilization was a problem. Many astronomers believed that something more permanent, like a telescope orbiting the Earth, was needed to provide continuous coverage above the atmosphere. When NASA was established in 1958, that agency, rather than DOD or industry, became the focus of their hopes. What had once been a theoretical construct was now being pushed as a national commitment.

But the positions of the scientific community on the one hand and NASA on the other were considerably more complex. Dr. Homer E. Newell, who directed NASA's science programs through 1967, wanted the assistance of the scientific community, more especially the Space Science Board created by the National Academy of Sciences in August 1958 for that purpose. But he wanted advice on his terms. The Board sought to be an independent advisory group but Newell, while welcoming its advice, expected it to respond to tasks "within carefully prescribed limits ... specified by NASA." (ref. 113). Almost at once, the Board and NASA disagreed. Many, but not all, Board members thought that NASA should concentrate its funding on rocket- and balloon-borne observations. The technology of these instrumented packages had improved remarkably over the decade; by the late 1950s, a telescope lifted by balloon (Stratoscope I) was taking remarkably sharp pictures of the Sun (ref. 114). In short, the Board wanted NASA "to use good 'obsolete' vehicles to send up instruments routinely rather than attempt to develop new vehicles. NASA answered that it was aware of space science needs, but that its emphasis on vehicle development was appropriate." (ref. 115.) Because NASA had the funds and the Space Science Board only had a shopping list, it was Newell's view that prevailed.

In 1959 and 1960, NASA defined its plans for an advanced scientific satellite. Based on consultations with leading astronomers, NASA elected to develop a satellite similar to a ground-based observatory. Because it would be designed to accommodate a variety of experiments, it would be considerably larger than the Vanguard and Explorer satellites then being launched. The primary mission of the Orbiting Astronomical Observatory (OAO) would be to analyze ultraviolet radiation from stars. As such, the OAO would have to meet stringent technical criteria. It would have to "lock on" to those stars that would provide a frame of reference for the observatory, maintain constant temperatures, and provide a stable

platform that could be pointed to any position in the celestial sphere. Confident that these problems could be overcome, NASA gave overall management of OAO to the Goddard Space Flight Center, established a coordinating program office at Headquarters, and, in October 1960, selected the Grumman Corporation of Bethpage, New York, as prime contractor.

As NASA's largest and most elaborate satellite, it might have been predicted that the development of the OAO (fig. 40) would not be easy. But the actual course proved even more painful than anticipated. The initial estimate of \$50 million for the entire program rose to \$200 million for three missions, and the date of the first launch slipped from late 1962 to mid-1966. Weighing 1 773 kilograms and containing more than 440 000 parts and 48 kilometers of electrical wiring, the OAO had to undergo rigorous qualification and testing procedures that were not included in the original plan (ref. 116). As one project official explained NASA was pushing the state of the art, especially in star-tracking; the problems of integrating the subsystems were solved as they arose and were not postponed to await systems testing, and new parts had to be added, owing to the effects of high-altitude, high-yield nuclear explosions set off by the Atomic Energy Commission in 1962 to test their effects on worldwide communications (ref. 117). Most, but not all, of the early technical difficulties were overcome. But even as late as the end of 1964, one of the two principal experiments was dropped from the first launch.

Yet the early frustrations of building the OAO were nothing as compared to the failure of the first launch on April 8, 1966. Liftoff of OAO-1 was perfect, but within a few orbits the primary battery overheated (probably because of electric arcing), and by April 10, the mission was given up as lost. Goddard and Grumman immediately began a review of the mission, and on April 21, Newell created a special review board chaired by Robert Garbarini, one of his deputies, to examine the design and management of NASA observatory-class spacecraft. The review board examined every facet of the project—management, system design, manufacturing, reliability, and quality assurance. Issued in October, the Garbarini report conceded that OAO was conceived during an early period of NASA's history and, therefore, could not take advantage of advances in the state of the art.* Even so, it was debatable whether "even the best of practices could eliminate all the hazards in moving forward into previously unexplored areas." (ref. 118.) The report

* This does not contradict the remark cited earlier that OAO was pushing the state of the art. It was, but it was against 1960 technology that the OAO project team was doing the pushing. Once OAO's design was frozen, it was in fact the case that OAO "could not take advantage of advances in the state of the art."

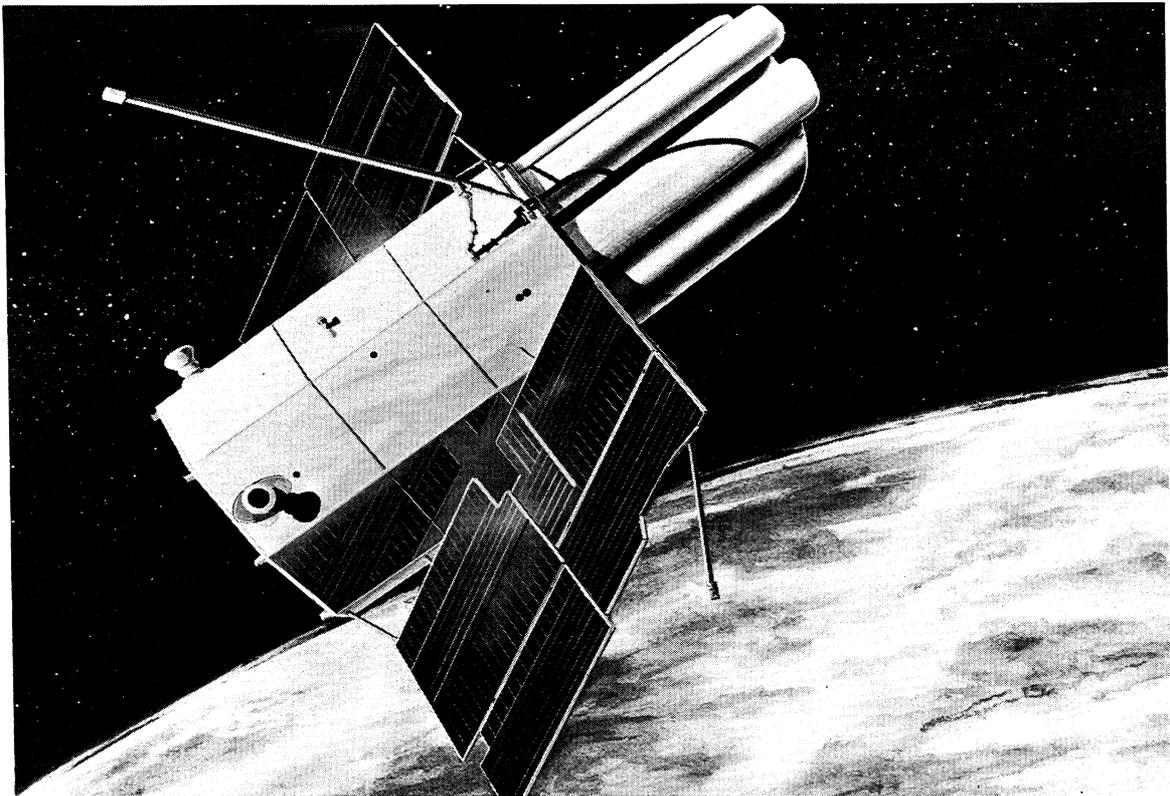


FIGURE 40. — *The Orbiting Astronomical Observatory. This spacecraft was launched in 1968 and was managed by the NASA-Goddard Space Flight Center.*

went on to make more than 100 recommendations, most of which NASA accepted. In the area of management, the report recommended that NASA assign a full-time technical representative to each prime contractor, centralize all testing at Goddard, and apply the Program Evaluation and Review Technique (PERT) to tracking all phases of the project.

Based on the Garbarini report, NASA did a “ground up” evaluation of the OAO design. It replaced the Atlas-Agena launch vehicle with the more powerful Atlas-Centaur, doubled the Goddard project staff, redesigned the OAO power system, doubled the computer memory of the data storage system, and added a special backup system to prevent the spacecraft from being lost. All this caused the OAO to go from the initial 1 773 kg to 2 000 kg for OAO-2 (launched December 7, 1968) to 2 227 kg for OAO-3 (launched August 21, 1972).*

* Another OAO failed, in November 1970, to reach orbit, because the nose fairing of the launch vehicle failed to separate.

These changes probably saved the project. OAO-2 and OAO-3 operated for several years, the former from 1968 to 1973, the latter from 1973 to 1981. Both returned vast amounts of data concerning the composition of interstellar clouds, the relative abundance of different elements in the interstellar gas, and the existence of stellar winds flowing from very hot stars. Up to a point, then, the OAO was highly successful.

No doubt, a great deal more could be said about a project as large and complex as the OAO. But three observations are in order. First, the OAO is almost a classic model of the technology demonstration program with long lead times, high uncertainty, and cost overruns. But aside from the difficulties that dogged the program in its early stages, OAO shows the ability of a large organization to move quickly down the so-called learning curve. OAO-2 and OAO-3 were replicas neither of each other nor of OAO-1. OAO-3, named Copernicus, had a spectrographic resolution down to 0.005 nanometer and its mirror, which was twice the diameter of the OAO-2 mirror, could stay pointed toward a star for several minutes with a maximum deviation of 0.02 arc-second (ref. 119). Because of the program's complexity, the experimenters, or principal investigators, could not take a purely passive role. In OAO, for example, the data acquisition system was so complex that, as one principal investigator acknowledged, the task of the astronomer was to select spacecraft pointings and generate experiment commands to the spacecraft for execution. "It is likely that no other astronomical observing program has been planned in as great a detail over as sustained a period of time." (ref. 120.) NASA insisted that scientists and engineers talk and understand each other's language and that scientists, in the most literal sense, master the nuts and bolts of projects.

Second, NASA managers refined, if they did not invent, certain techniques for controlling these very large projects. One was the adoption of reporting systems like PERT and the agency's Management Information and Control System; a second was to pair scientists and engineers at each level of the space science program, so that where the head of one division was a scientist, his deputy was an engineer; a third was to make a formal distinction between the project manager, who was the chief line officer, and the program manager, who developed the Headquarters guidelines under which the project was run. Many technology development agencies have implicitly recognized this distinction, but only NASA made it formal. A program, for NASA, is "a related series of undertakings which continues over a period of time and which is designed to accomplish a broad scientific or technical goal... NASA's long-range plan..." (ref. 121.) Thus there was no anomaly in program manager having only one project to supervise, as was often the case. His responsibility was to ensure that the project was not pushed

the expense of broader goals, while concurrently defending the project at Headquarters.

Third, the recovery that took place after the OAO-1 failure shows what could be accomplished by an agency with strong in-house technical capabilities. Goddard's physical proximity to Headquarters cut both ways: It may have made access to senior NASA officials a little easier, but it made it easier for those same officials to find out what had gone wrong. In the end, what mattered was, as one Goddard official said at a briefing for the NASA Administrator on OAO-1, the ability of project staff to "get up there in their [the contractor's] plant and you are right over their shoulder. And if you think their controls man isn't very good, you go back and get your best controls man to come up with a better idea and hit him over the head with it." (ref. 122.) Goddard could also draw on other centers for support. The Lewis Research Center was developing the Centaur upper stage used for OAO-2 and OAO-3, while the Ames Research Center was brought in after the OAO-1 failure to help redesign the spacecraft's control system (ref. 123).

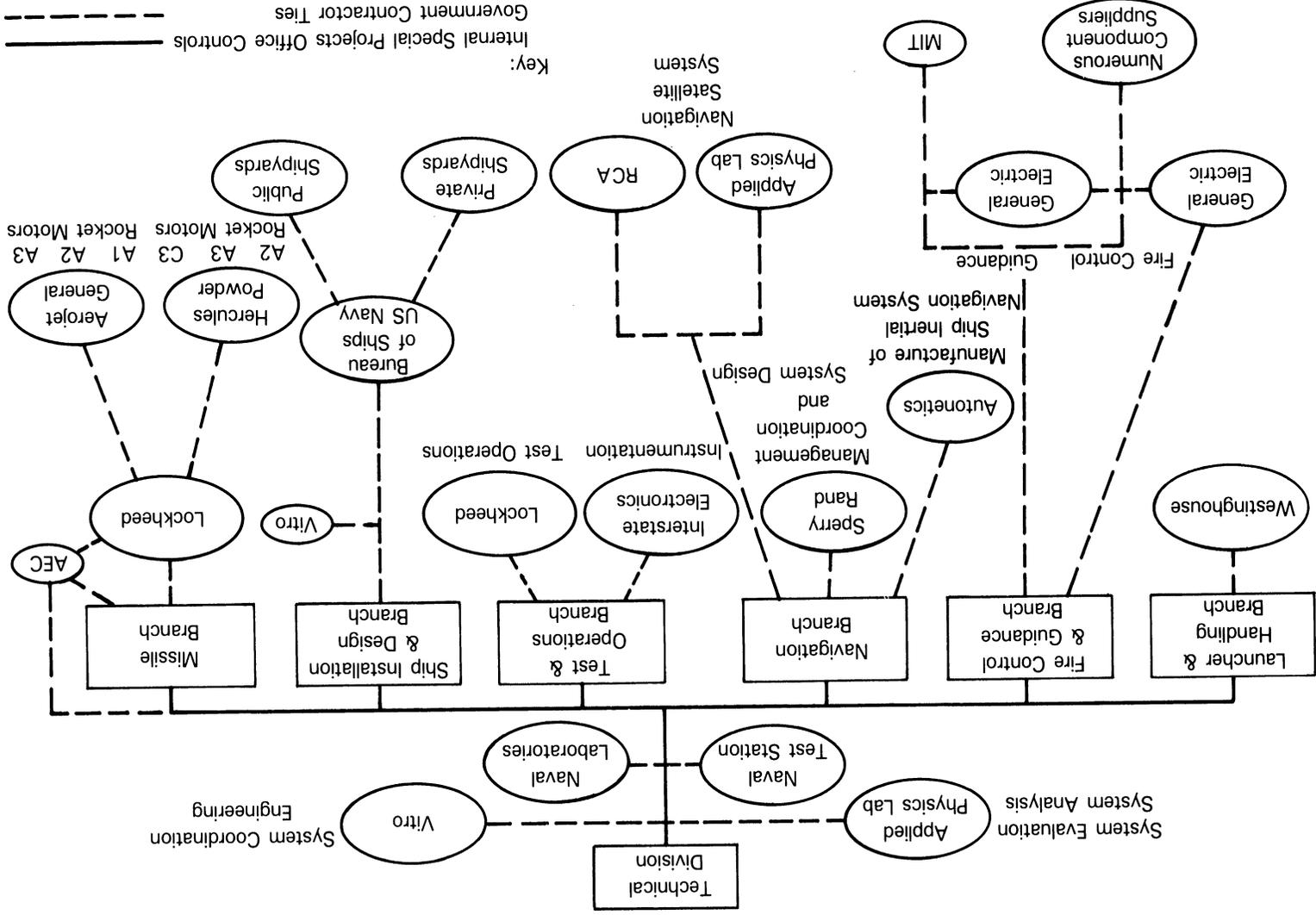
Yet there still remains the question as to whether the OAO program was the most appropriate way for NASA to use its resources. NASA's position in 1959 and 1960 was that the agency could handle projects of this scope and, as a technology development agency, *needed* to do so. But in retrospect some officials, including Newell, came to believe that OAO had pushed too hard against the state of the art, and that a more modest program might have met the needs of astronomers sooner and more cheaply. Many years after leaving NASA, Newell discussed the OAO with remarkable candor: "The observatory finally proved to be a powerful astronomical facility. But in retrospect it can be seen that NASA might have done better to follow the recommendations of its advisors, who would have preferred to start with a less ambitious astronomy satellite that would have permitted astronomical observations sooner. Having the less capable . . . satellite sooner, the astronomers would have been content to wait for the larger one. . . . one could detect the feeling that OAO was a bit out of step. The satellite had been sufficiently difficult to construct that it had delayed satellite optical . . . astronomy for about a decade, whereas a series of cheaper, simpler satellites could have kept research moving while work on a larger instrument proceeded. Also, now that it had come, OAO was well behind both existing telescope technology and current needs." (ref. 124.)

Perhaps this was not the entire story. Where so much was being spent on manned projects, it was probably necessary to have at least a few unmanned scientific projects of the order of OAO to maintain the appearance of a "balanced" program. Nor was the OAO without progeny. Although the first studies for a large orbiting space telescope were

concurrent with the development of OAO, the telescope as finally designed owed a great deal to OAO technology: precision star trackers, large sealed batteries, unicon tubes, and the advanced thermal control system (ref. 125). By a not uncommon paradox in technology development work, OAO was both ahead of and behind the times, too advanced for its users and yet not advanced enough when it finally flew. *Polaris* (ref. 126). There was no such ambiguity about the success of the Fleet Ballistic Missile program, better known as Polaris. Managed by the Fleet Ballistic Missiles Office of the Department of the Navy, the Fleet Ballistic Missile program involved the development of the Polaris missile and the nuclear-powered submarine that launched it. From the start of the project in the mid-1950s to the first test firing in 1960 to the development of the final submarine in 1967, Polaris was a success: It met all its goals, stayed within cost, and was completed years ahead of schedule. This success owed much to the approach of the Special Projects Office (SPO) and to its management techniques, some of which have proved very influential. Yet, on the strength of what is known of the history of the project, the real secret of the SPO's success was the ability of its officials to master all the elements of the bureaucratic shell game and to outflank potential competitors in the Navy and throughout DOD.* In his *Polaris System Development*, Harvey Sapolsky has shown convincingly how SPO promoted and protected Polaris, and we will use his analysis as a point of departure for our own.

As mentioned in Chapter III, the SPO was emphatically *not* a large in-house organization monitoring every phase of contractor work. Quite the contrary. If we compare Polaris with OAO, a smaller albeit still large project, perhaps the principal difference is that in the former project there was no prime contractor. Instead, each system—fire control and guidance, missiles, test and operations, ship installation and design—had its own contractor. Polaris was a highly decentralized operation, and this was one of the secrets of its success. A network of multiple contractors ensured that the SPO would not be the captive of any one of them; the SPO's Technical Director could compare competing proposals; and each subsystem contractor became in effect an advocate for the entire project. The better to bind the contractors to the project, the SPO insisted that all of its major contractors set up project-type organizations working solely on Polaris (fig. 41).

*Having said this about Polaris and the SPO, it is still obvious that the real credit belongs to the unusually talented Navy flag officers who headed the SPO and worked in over the years—above all, Admiral "Red" Raborn, who organized the SPO in 1955, his successor, Admiral Levering Smith, and others like Robert Geiger and Robert Wertheim. The fact that people of this caliber were available to the Navy when they were needed speaks well for the Navy's personnel system—a subject we will discuss in a subsequent chapter.



Source: Harvey Sapolsky, *Polaris System Development* (Cambridge, MA: Harvard University Press, 1972), p. 92.

FIGURE 41.—Major contractor network for the Fleet Ballistic Missile Program.

Even more crucial to Polaris's success was the ability of SPO to neutralize opposition and, particularly, to achieve maximum flexibility in attaining its ends. As Sapolsky notes, the creation of the SPO was of greater consequence to the Navy than the establishment of Polaris. "The Navy had undertaken many complex tasks prior to the [Fleet Ballistic Missile]. It had not previously, however, formed a major subunit whose sole mission was the development of a single weapon system." (ref. 127.) How did the SPO achieve this organizational autonomy? The SPO benefited from the national commitment to a ballistic missile program—a commitment similar to those which led to the Manhattan Project and Apollo. It was relatively easy for the SPO to sell Polaris to Congress and the public. It was more difficult to sell it within DOD. In order to accomplish this, the SPO strategy was to co-opt potential critics by seeking them out and soliciting their advice (for example, the naval laboratories and outside scientists generally), accommodating the submarine specialists and the Nuclear Power Directorate under Admiral Rickover, using its own contractors to sell the project, and focusing on systems development rather than advancing the state of the art. "Improvements in subsystems were encouraged, but not allowed to interfere with the object of meeting ship deployment schedules . . . The test program was designed to develop a missile system and not to research its components." (ref. 128.) Contractors were encouraged to avoid any task which did not contribute to the development of a submarine-launched ballistic missile.

The attitude of the SPO toward outside scientists and the naval laboratories shows how the Office could co-opt outside groups while still keeping them at arm's length. The SPO established the positions of Chief Scientist and Engineering Consultant, but refused to institute a general advisory committee of outside scientists which might slow the pace of development and even remove some of the SPO's control over Polaris. (For much the same reasons, NASA in 1966 rejected a proposal that it establish a general advisory committee of outside scientists.) It was also SPO policy not to use naval laboratories in development work unless no commercial source was available and the laboratory's technical competence was superior to that of SPO contractors. SPO avoided using government laboratories because their behavior could not be controlled as easily as that of its contractors, and because of the laboratories' vulnerability to government-wide funding cutbacks (ref. 129).

Another reason for the SPO's success in starting Polaris and maintaining control over it was the office's reputation for managerial innovation. The SPO, using the consulting firm of Booz, Allen and Hamilton, invented the Program Evaluation and Review Technique (PERT) for tracking resources and activities throughout a system. Generally computerized, PERT describes the discrete steps needed to

roduce an end item, develops estimates of the time needed to reach an event in the network, and charts a "critical path" of the longest anticipated time sequence for realizing each event (ref. 130). When the SPO introduced PERT in 1958, it was an immediate success, eventually becoming a requirement in Defense, NASA, and Atomic Energy Commission contracts. SPO officials constantly proclaimed the merits of PERT, and they had every reason to do so. By gaining a reputation for managerial effectiveness, the SPO could insulate itself still further from competition from other organizations or, indeed, from any outside attempt to weaken its control over Polaris.

Yet there is no evidence that PERT by itself had anything to do with the successful management of Polaris. Neither contractor executives, nor the technical engineers and evaluators in the SPO, nor the Navy plant representatives claimed to have used data generated by PERT in their segment of the project (ref. 131). Its effectiveness was a myth that SPO officials needed to sell the project. As Sapolsky shows, ". . . these techniques either were not applied on a significant scale in the operations of the Special Projects Office until after the successful test and deployment of the initial [Fleet Ballistic Missile] submarines, or they were applied, but did not work, or they were applied and worked, but had a totally different purpose than that officially described." (ref. 132.)

The success of Polaris depended on the ability of the SPO to devise an organizational structure which combined decentralization with internal competition; to fend off potential competitors and to remove the SPO from the normal chain of command; and to give a role to scientists, other elements in the Navy and DOD, and to contractors in such a way as not to jeopardize the Office's power to control the project. The success of the SPO in achieving these ends was due rather more to its superb skill in playing bureaucratic politics than to innovations in management methods.

Systems Engineering at Bell Laboratories. Both OAO and Polaris owed a great deal to the concept of systems engineering. As we saw earlier in this chapter, a large and complex piece of hardware may be treated for design purposes as parts of an interrelated whole. From the 1930s to the present, Bell Telephone Laboratories has made important contributions in systems engineering, both in government-sponsored work and in its internal research. For the government, during the Second World War, Bell Labs designed the gun director for anti-aircraft defense, perfected techniques for locating submarines and homing torpedoes to their targets by sounds generated by the submarine, and designed more than half the radars used by the United States armed forces during the war (ref. 133). This support continued after the war, with work ranging from basic physics and chemistry to operations research on guided missile systems. Bell Labs was also involved in two special projects. The first was to contribute its research expertise to Sandia Laboratories, set up by

the Atomic Energy Commission to develop nuclear weapons and operate by Western Electric, the manufacturing arm of AT&T. The second was to set up a special dedicated organization, Bellcomm, working for NASA on systems engineering for the lunar landing.

Before turning to the contributions of Bell Labs, it would be well to look more closely at what systems engineering entails. As noted earlier, there is a marked difference between military systems engineering and civilian applications. For the former, military urgency leads to an emphasis on very small margins of performance. For the same reason, military equipment tends to be designed from the bottom up, while in such civilian areas as telephone systems engineering, there is the requirement noted by a Bell Labs official, that "any new telephone system must be compatible with all existing equipment in the telephone network." (ref. 134.) Unlike operations research, which tries to make the best use of existing equipment, systems engineering tends to be most active at the earliest and the latest stages of the design process. In the former, the engineer studies the overall design concept; in the latter, he prepares the operating instructions, test procedures, and logistics of the operating system. What matters, in this formulation, is that the end user and the ultimate need are in the forefront of the engineer's research. In systems engineering, there is less emphasis on incremental improvements and more on exploiting advances in (say) electronics or physics. Systems engineering "needs new technology to work with." (ref. 135.)

Viewed in this light, the achievement of Bell Labs during and after the Second World War has been to translate the concept of systems engineering into operating systems. How was this accomplished? One answer has to do with the relative ease with which technical people were able to move from basic and applied research into areas with which they had no previous connection. For example, during the war, persons whose previous work had been in telephone and switching and transmission systems worked to develop microwave systems and radar equipment or computing devices for fire control (ref. 136). Where fundamental research was being carried on along a broad front, the boundaries between civilian and military work were quickly obliterated.

Or consider Bell Lab's work on long-distance transmission. This continuing development of transmission capability has had to be matched by other, parallel lines. Some of these areas include:

- Development of solid-state devices from the transistor to the integrated circuit;
- Materials development—synthetic crystals, magnetic alloys, and plastics for insulation and weather protection;
- Switching technology, ranging from mechanical stepping relays to densely packed solid-state devices;

Computer development and applications, from computer-assisted design to computer operation of complex systems;

- Information theory—the general specification of conditions which must be met for messages to be transmitted and decoded with predictable probability of error.

What these cases show is the feedback that occurs—must occur—between fundamental research and engineering design. Research in solid-state physics and information theory begets improved communications which, in turn, suggest new possibilities in research. Because of the importance of this synergism, Bell Labs has even located some of its groups—for example, the development-and-design-for-manufacture group—in laboratories on Western Electric premises (ref. 137).

A second feature of the work undertaken at Bell Labs has been the importance attached to long-range planning. It is fundamental to define one's objective—to decide what the laboratory or a group within it should be doing now and perhaps a dozen years downstream. At Bell Labs, management has often been willing to wait for years before a commercial application becomes practical. Projects, even major ones, are small, rarely employing more than a dozen persons. What has characterized much of Bell Labs' work has been the persistence with which management has stuck to a certain objective. It will be interesting to see whether this long-term view of the world survives the changes in AT&T under the current order of divestiture. One of the advantages of the AT&T monopoly was that it could, and did, take the long view.

Take the development of electronic switching systems. The first studies for a "transistor switching system" were begun in 1951 at Bell Labs' New York offices and ran parallel to work being done by the electronic apparatus development organization. These studies, as well as other exploratory work, led to the conclusion that high-speed electronics could provide smaller and less expensive switching systems. At that point (1954), Western Electric authorized a field trial of an electronic system at company facilities in Morris, Illinois. This trial was a success, in the sense that it proved the soundness of stored-program control for telephone switching systems; but the Morris system as originally designed was not put into production, because "a revolution in technology had made obsolete all its major components." (ref. 138.) Instead, work began on a parallel effort, using many of the concepts proved at Morris, but incorporating magnetic memories, newer network elements, and silicon transistor and diode circuits. The Bell System introduced its first commercial system in 1963, an improved version in 1965, and an automated government network in 1966. In effect, the lead time from the first exploratory studies and first commercial networks took more than a

dozen years, involved parallel efforts at many of the Laboratories facilities, led to several false starts and, once on the right track, resulted in new services and features.

If we seek to isolate the special features of the more important Bell Labs' projects, they would include the interdependence of fundamental and applied research, the broad base of knowledge in many disciplines; the continuity of teamwork among small groups of scientists and research engineers, the willingness of the sponsoring organizations to commit the resources to projects with very long lead times, and the close working relationships established with the ultimate user, Western Electric. Of these features, the most important was the interdependence of basic and applied work—and not at this laboratory only.

In this and the two preceding chapters, we have looked at technology development institutions as a whole—their common features, the problems, and the ways in which they interpret their charters. We now turn to the roles of the professional staff, the men and women who *are* the laboratory. Reversing the Marxist formulation, we can say that the overwhelmingly important job of the research administrator is to move from the administration of things to the management of people—rather, to manage *with* them.

CHAPTER VII

The Management of the Professional Staff

Employment Patterns Among Federal Scientists and Engineers

The vitality of a technology development organization resides in its professional scientists and engineers. This group proposes the ideas that are starting points for development programs, examines their validity in the light of fundamental scientific principles, and devises strategies by which ideas are converted, step by step, into operating systems. Other elements of the organization are essential — skilled technicians, shop machinists and electricians, administrative personnel. But unless the professional scientists and engineers function imaginatively and competently, the organization will lose momentum, stop generating worthwhile ideas, and either cease to achieve the goals set by the sponsoring agency or lower them in favor of what is considered attainable. In this chapter we will review the tactics of managers and senior officials in technology development to maintain high levels of staff performance. Specifically, we want to provide at least tentative answers to these questions: How does a laboratory acquire and retain a competent staff? What are the effects of aging on research? How do scientists and engineers make the transition to management? How important is professional mobility, both between divisions within a laboratory and between laboratories? Finally, how do managers and the officials to whom they report evaluate the quality of research within their organization?

Before these questions can be answered, we would like to make our assumptions about personnel management explicit. Our first assumption is that there are no personnel policies which are guaranteed to work across organizational lines. Continuing education may or may not work; indefinite employment or term employment may or may not work; rotation of staff between divisions may or may not work. It is as if, in the best laboratories, the organizational culture is strong enough to impose itself on any program of personnel management.

This brings us to our second point. Personnel issues are synonymous with the organization's goals. Arnold Deutsch notes that: ". . . the primary motivating factor in job selection by technical people is the

nature of the work itself. The interest, the challenge to the technical abilities, and the opportunities for significant professional achievement offered by the work to be done determine whether or not an engineer or scientist will consider a position." (ref. 139.) Consequently, nothing less than a thorough understanding of the functions of a given laboratory will enable one to decide which personnel policies are effective and why. Personnel management is affected by (for example) levels of agency funding and the discretion of laboratory directors in spending what is allocated; the diversification of laboratories into work for others; the advent of new technologies which suggest new programs; and the competition for the brightest graduates that government laboratories face from industry and the universities. Many personnel theorists have somehow forgotten that since the environment of any organization is constantly changing, no theory which ignores this truism will hold water.

Our third point has to do with nature of this environment, and may be summed up thus: Despite declines in funding for nondefense basic research, the aging of staff in Federal laboratories that have experienced manpower restrictions, and the tendency of agencies to locate their new programs in existing facilities rather than creating new ones, the function of the Federal scientist and engineer are not likely to change greatly. As to the relative decline in Federal support for research and technology development, the evidence is clear enough since 1967. Only in the last few years have we started to reverse this trend, and it will take some time for the positive results to become apparent.

Consider, for instance, nine of the national laboratories operate under contract to the Energy Department (table 5).

These figures show that for seven of the nine laboratories there was little or no growth in staffing during the preceding decade and that the two exceptions — the Los Alamos and Lawrence Livermore National Laboratories — were and are heavily involved in weapons development. At NASA the decline in the work force has been even steeper, with a drop of 30 percent, from over 30 000 to 22 000 between fiscal years 1971 and 1982. Possibly of more significance is the rise in the proportion of professional employees to the total workforce (fig. 42). From these figures it is apparent that scientists and engineers alone comprise just over half of NASA permanent employees and that, combined with professional administrators, they account for two-thirds of permanent employees. A further breakdown of the figures illustrates two important trends. The first is the increase in the number of supervisors as a percentage of total NASA employees. In fiscal 1982, there was approximately 1 supervisor for every 6.7 employees agencywide, while at Headquarters the ratio was almost 1 to 5. At many government laboratories there has also been a decline in the number of technical staff available to support professional staff.

Table 5. Full-time Staff Equivalents from June 1967 to September 1982 for Nine National Laboratories*

	Ames	ANL	BNL	LANL	LBL	LLNL	ORNL	PNL	Sandia	Total
June 30, 1967	635	5 000	3 400	4 014	2 920	5 629	4 870	2 413	8 214	37 095
June 30, 1968	635	5 200	3 400	4 120	2 792	5 898	5 040	2 617	8 149	37 851
June 30, 1969	570	5 000	3 350	4 330	2 697	5 937	4 850	2 619	8 179	37 532
June 30, 1970	530	4 600	3 200	4 428	2 545	5 568	4 619	2 372	7 581	35 443
June 30, 1971	455	4 435	2 894	4 168	2 342	5 276	4 345	842	7 291	32 048
June 30, 1972	410	4 356	2 542	4 183	2 342	5 538	4 143	935	7 223	31 672
June 30, 1973	380	3 982	2 621	4 501	2 002	5 233	4 048	1 008	6 401	30 176
June 30, 1974	365	4 114	2 472	4 585	2 148	5 445	3 726	1 213	6 477	30 545
September 30, 1975	395	4 570	2 609	5 062	2 122	5 704	4 255	1 328	6 539	32 584
September 30, 1976	405	4 719	2 829	5 565	2 294	6 257	4 445	1 506	7 022	35 042
September 30, 1977	440	4 887	3 049	6 035	2 905	6 731	4 710	1 825	7 264	37 846
September 30, 1978	443	5 045	3 304	6 370	3 235	7 035	4 980	2 127	7 478	40 017
September 30, 1979	448	5 002	3 491	6 802	3 350	7 087	5 001	2 169	7 618	40 968
September 30, 1980	463	4 925	3 604	6 938	3 278	7 313	5 025	2 249	7 847	41 642
September 30, 1981	479	4 727	3 502	7 165	3 009	7 717	5 073	2 156	8 018	41 804
September 30, 1982	427	4 200	3 308	7 000	2 500	7 525	4 775	2 050	7 677	39 487

* Estimated. Some of the data consist of FY averages.

Source: Final Report of the Multiprogram Laboratory Panel, Energy Research Advisory Board, Vol. II (Oak Ridge, Tenn.: Oak Ridge National Laboratory, September 1982), p. 24.

Key:

Ames = Ames Laboratory

ANL = Argonne National Laboratory

BNL = Brookhaven National Laboratory

LANL = Los Alamos National Laboratory

LBL = Lawrence Berkeley Laboratory

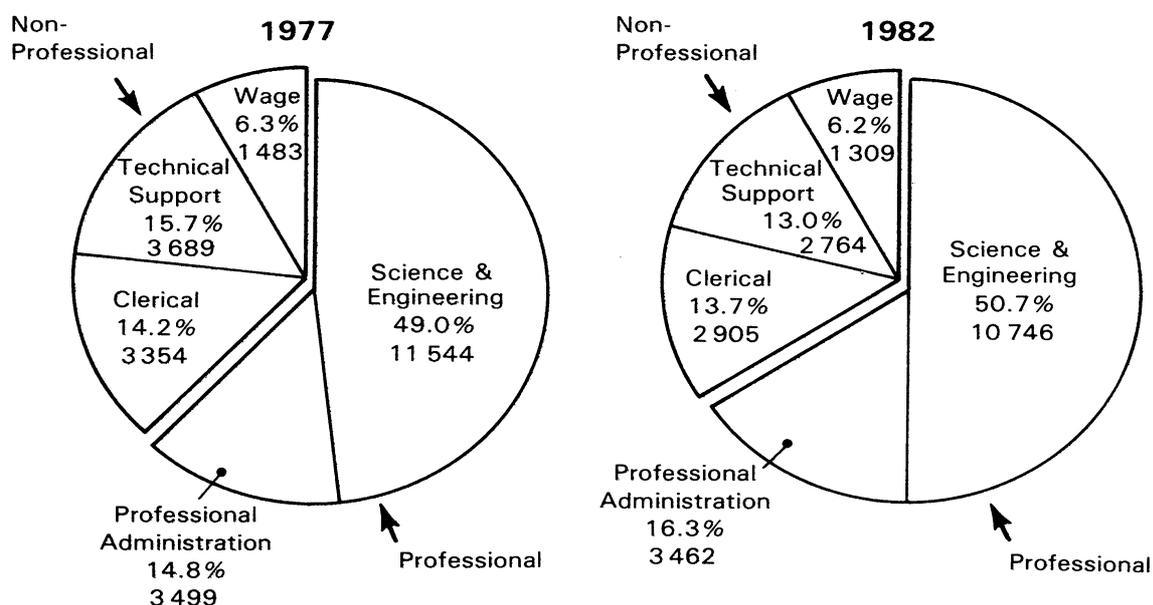
LLNL = Lawrence Livermore National Laboratory

ORNL = Oak Ridge National Laboratory

PNL = Pacific Northwest Laboratory

Sandia = Sandia Laboratories

THE MANAGEMENT OF RESEARCH INSTITUTIONS



	1977	1978	1979	1980	1981	1982
Professional	15 043	14 938	14 705	14 749	14 418	14 208
Non-Professional	8 526	8 231	7 928	7 864	7 426	6 978
Total	23 569	23 169	22 633	22 613	21 844	21 186
%Professional	63.8	64.5	65.0	65.2	65.2	67.0

Source: NASA, Personnel Programs Division, *The Civil Service Work Force as of September 30, 1982*, p. 3.

FIGURE 42. — *This figure shows the number of permanent civil service employees in NASA and how they are divided into various occupational groups.*

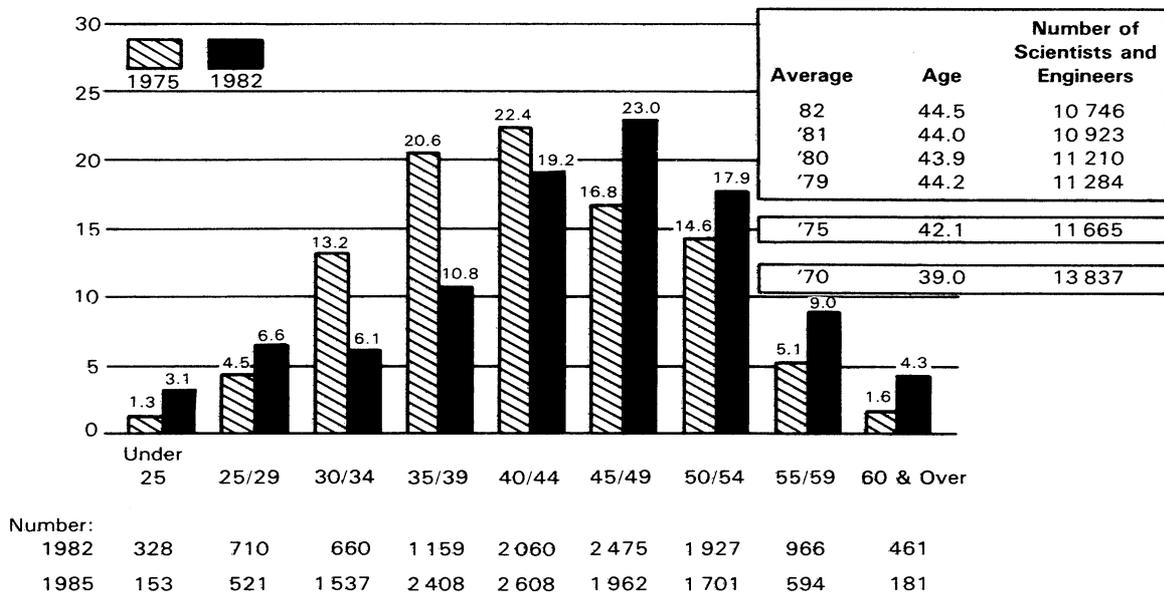
The other notable trend is in the age distribution of scientists and engineers which is, of course, a consequence of the manpower reductions. In this respect, NASA is typical (fig. 43).

Since 1975, there has been a quite dramatic increase in the average age of scientists and engineers employed by NASA. Note particularly the decrease in the number of scientists and engineers in the 30 to 34 and 35 to 39 categories — precisely the ages when the most productive research is being done.

The Problem of the Aging Staff

Why is the aging of the professional staff considered a problem? Most research and technology development organizations seek to bring in those who will contribute to the store of new technology. Recent graduates with advanced degrees have been trained to attack problems using the most advanced instrumentation, design concepts, and computer techniques. The received wisdom is that younger candidates are likely to

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Source: NASA, Personnel Programs Division, *The Civil Service Work Force as of September 30, 1982*, p. 15.

FIGURE 43. — This figure shows the age profile of NASA civil service scientists and engineers in 1975 and in 1982.

be more productive than older employees, and the hiring age distribution reflects this preference. Note that this preference is not a matter of discrimination on the basis of age, but rather that the desired qualities are usually found among younger employees.

However, there are some functions that can more effectively be carried out by persons with broad experience. Project management is an obvious example, since it involves coordinating the work of many contractors and subcontractors. Also, as programs change, an organization needs to find expertise not represented within its present staff. Thus, some new employees will be hired at mid-career or beyond.

In fact, most laboratories have a spectrum of needs, ranging from basic research to project management to the continuing administration of the laboratory itself. There is, then, no single approach to hiring and retaining technical personnel, because no single approach can meet all the needs of a large organization moving simultaneously on many fronts. At Bell Labs, for instance, there appear to be several concurrent policies calculated to maintain a high level of competence. Bell Labs predominantly hires young degreed scientists and engineers out of the best universities; weeds out by termination those who do not come up to Bell Labs' standards of performance within three to five years; continues to let go those whose performance lags in mid-career; and retains a core of highly qualified people up to the mandatory retirement age of 65.

Such policies inevitably raise this question: How many new employees does a given research and technology development organization need to hire each year to maintain a high level of effectiveness? Where basic research is a source of new ideas or programs involving highly sophisticated techniques, an influx of new employees is essential. In a study carried out by staff at Lawrence Livermore National Laboratory in 1970, the data in table 6 were gathered, showing the average turnover rates in fifteen research and technology development organizations.

Table 6. Professional Personnel Turnover in Research and Technology Development Organizations

Organization	Annual Percentage of Professional Staff ^a	
	Hired and Transferred In	Terminated and Transferred Out
(5-Year Averages through 1970)		
Aerospace Corporation	7.1	12.1
Oak Ridge National Laboratory	8.9	10.2
Lockheed Missile & Space Company	9.8	13.2
Bell Laboratories	11.0	9.1
Standard Oil Company of California, ^b Engineering Department	19.8	16.8
(10-Year Averages through 1970)		
Argonne National Laboratory	6.3	6.2
Los Alamos Scientific Laboratory	7.5	4.1
Naval Ordnance Laboratory	8.4	7.1
Chevron Research Company	9.2	10.8
Sandia Corporation	9.7	7.6
Applied Physics Laboratory	9.8	6.7
Lawrence Livermore Laboratory	9.9	7.5
Battelle Memorial Institute-Columbus	11.1	10.0
RAND Corporation	13.2	13.6
Jet Propulsion Laboratory	17.8	10.5

^a Includes all full-time, regular, technical, professional employees who were added to the staff by any means and those who left for any reason, including layoff. The percentages were calculated for each year separately and then averaged. Some figures were based on calendar and some on fiscal years.

^b Includes those engineers who, after hiring and training, were transferred to other company departments.

Source: C. M. Van Atta, W. D. Decker, T. Wilson, *Professional Personnel Policies and Practices of R&D Organizations* (Livermore, Cal.: Lawrence Laboratory, December 1971), p. 10.

Many of the organizations listed above reported more people hired than terminated and were in a period of growth. For example, Bell Lab

had known sustained growth throughout its history, save for a five-year period in the 1930s. In such periods, growth plus normal attrition may provide sufficient vacancies to accommodate an adequate hiring rate. But where there is no real growth, the hiring rate may drop to only three or four percent annually unless management stimulates attrition by selective termination. On the limited evidence available, an annual influx of six to eight percent is needed to avoid deteriorating performance.

During periods of rapid growth, the opposite condition may prevail — too many new employees for the organization to assimilate efficiently, resulting in inadequate supervision and inefficient use of resources. Rapid growth is followed by moderate or no growth, and errors in hiring during the preceding period become obvious and present management with difficult choices. To compete with other comparable organizations, senior managers may have to terminate incompetent professional employees and shake up the organizational structure.

To sum up the argument to this point. Within Federal laboratories and laboratories operated under contract to Federal agencies, we have noted the following: a decline in total employment at various Federal laboratories, combined with a rise in the percentage of professional employees, a steady rise in the average age of scientists and engineers, and a drop in the number of employees per supervisor. These figures suggest a sluggishness — a stagnation, even — in bringing new blood into the organization. But against these data, there are certain considerations which tend to mitigate their impact. The first consideration has to do with the effects of an aging staff. It is true that in scientific research, the most significant work is done before the age of forty and in development work, before the age of fifty; that performance tends to decline in the forties as employees fail to keep up with advancing technology; and that sometimes it becomes necessary to make older employees take early retirement to open up positions for applicants with specialized skills. But this is scarcely the whole story; professional obsolescence is at most a statistical tendency, not an iron law. For one thing, personnel chiefs at some major industrial laboratories have denied that there is a significant relation between age and value to the organization (ref. 140). More important, the attitudes of a laboratory's senior managers may themselves determine how productive older professionals are. If the laboratory tries to maintain a low average age and an "up or out" policy in promotions, older staff members may indeed feel undervalued and their work will suffer accordingly.

There has also been much research sponsored on the effects of aging within organizations — "industrial gerontology," as it has been called. On the evidence, productivity need not decline after the age of forty. Indeed, it appears that after fifty productivity tends to climb. As the Lawrence Livermore study mentioned earlier notes: ". . . contributions

made during this late period are usually more 'productive' than 'creative' . . . a pulling together of one's life work, guidance of younger professional employees, etc. The older employee is generally secure in his position, has fewer family pressures, and is willing to take risks again . . . Older scientists continue to be useful to the organizations in many ways despite their drop in strictly creative scientific achievement. Although they tend to produce a lesser amount of major creative work than their younger counterparts, they do continue to produce over the span of their careers. When the working environment is otherwise favorable, only two conditions can effectively keep older employees from performing worthwhile creative tasks: negative attitudes and failure to try." (ref. 141.)

These conclusions must be regarded with caution. In the literature of the social science, it is rare to find a study unambiguously supporting a theory — the effects of school busing, the negative income tax, and medical care policy immediately come to mind — for which equal and contradictory data are not available.

Yet there is evidence that some scientists — those who are highly motivated to begin with, and (equally important) who are afforded the opportunity to work on important research — continue to produce throughout their careers. Management can encourage these people in a number of ways: rotating assignments, offering continuing education and sponsoring some individuals for leaves to study at a leading management school or the Federal Executive Institute. It is not, then intuitively obvious that aging and obsolescence are synonymous *if* the sponsoring agency provides for at least some influx of new staff, *if* there is a spectrum of opportunities within the organization, *if* some scientist and engineers can be transformed into managers (a point to which we shall return), and *if* the agency can devise ways to measure the productivity of research as a step to increasing it.

Research Productivity in the Federal System

In the context of this chapter, productivity implies several not necessarily compatible things. On one level, productivity means the automation of work, the supersession of manual effort by automated instruments, or the use of techniques like simulation programs which reduce the burden on expensive test facilities. So productivity may include any of the following: the use of computer-aided design to custom-design very large scale integrated circuit chips; the use of interactive graphic design systems in preparing production drawings; the programming of wind tunnel tests from startup to shutdown; the development of laser flow diagnostics; and the use of remotely piloted research vehicles for particularly hazardous tests (ref. 142). On a deeper

level, productivity refers to the quality of the research carried out at the laboratory; in turn, this cannot be isolated from the sponsoring agency's mission. Personnel management and a sense of mission are reciprocal; an agency without a strong sense of mission will not keep its best research people for long. The problem in evaluating research productivity is actually three-fold. It is difficult to measure something as intangible as an idea; research scientists, like other professionals, resist attempts by non-scientists to evaluate their output; and any evaluation of research productivity must use multiple criteria, instead of a simple-minded enumeration of research products (ref. 143.)

In any discussion of research management, we are ultimately driven to consider the sponsoring agency's goals. We have discovered that a rise in the average age of professional staff need not be a disaster, that the measurement of research productivity is difficult, and — as we must now disclose — that there are not simply “scientists and engineers,” but professionals of very different temperaments who may not coexist peacefully in the same laboratory. There are, on the one hand, those scientists who go deeper and deeper into a single area of research, and for whom the institutional setting within which the work is done is a matter of indifference. They are as likely to do their best work in a Swiss patent office, a prisoner-of-war camp, or the laboratory of a small college as in a large multidisciplinary research installation. The kind of people who feel at home at NASA or the multiprogram laboratories of the Energy Department are quite different: They are more likely to go from discipline to discipline while fitting their investigations into the content of some larger mission or project. It is this kind of scientist or engineer who, within the same laboratory, will move from aeronautical engineering to an analysis of the physics of lunar cratering; from advanced control theory to design techniques for control systems for powered lift aircraft; or from information theory to developing methods of noise suppression and error correction in deciphering telemetry signals (ref. 144). It is these persons who can move freely between disciplines, who can see practical applications in the most abstruse research, who are the essence of the laboratory's reasons for being. And the number of such researchers is not large — perhaps one or, at most, two dozen in a facility of 3 000 people.

But the distinctions between one kind of scientist and another go even deeper than we have indicated. The scientist who moves easily between disciplines is perhaps more an ideal type than the norm. Instead, scientists seem to fall into one or another category, depending on whether their work is weighted to fundamental science or to the application of their findings to their agency's interests (ref. 145). Scientists in the first category are more apt than those in the other to demand a large degree of freedom in selecting research projects, to identify more with the scientific profession than with the employing organization, and to work on research

activities which are organizationally separate from development.* In the other category, the most productive scientists — those who have had an opportunity to translate research findings into useful applications — are more likely to desire promotion into management positions than either the least productive scientists in their own category or the most productive scientists in fundamental research. It is the business of the laboratory's management to accommodate both categories under the same roof.

We seem to have drifted rather far from the nuts and bolts of personnel management, but in fact this is not so. Taking the perspective of a laboratory director, we have tried to analyze personnel issues in their full complexity. After all, it is the director's responsibility to accommodate basic and applied researchers, and to deal with perceptions that one group is gaining at the other's expense; to sponsor discretionary research; to maintain at least the minimum complement of professional staff needed for worthwhile research (the opposite problem — too large a professional staff — is seldom a problem nowadays!); to evaluate the work done; and to decide how much of the laboratory's manpower can profitably be diverted to work for others. (To anticipate our conclusions in Chapter XI, work for others only succeeds where it has a *particular* relevance to the laboratory's mission. Additionally, laboratories with a good record in basic research are better able to diversify than those handling large development projects, because the work of the former is more apt to spill over into a variety of disciplines.) The director can do much to maintain the vitality of the laboratory, whether this involves changing assignments frequently, identifying candidates for management positions, instituting leave programs for the professional development of people, retraining people and the like.

But even the most capable director can do only so much. To repeat Personnel policies cannot be appreciably changed or even understood apart from other policies which determine how the Federal Government gets its research and technology development done. And dissatisfaction with these policies is growing. From the Bell Report of 1962 to the reports of the Defense Science Board in the 1960s and 1970s to the recent report (May 1983) of the White House Science Council, the diagnoses have been much the same, although the more recent the report, the more

*These attitudes are more characteristic of highest productivity scientists within each category. Thus while in one survey 66 percent of the most productive scientists in the first category regarded selecting research projects as "extremely important," only 1 percent of the least productive scientists agreed. And while only 19 percent of the most productive scientists desired promotion into management positions, 52 percent of the least productive did so. See Howard M. Vollmer, "Evaluating Two Aspects of Quality in Research Program Effectiveness," in M. C. Yovits et al., eds., *Research Program Effectiveness* (New York: Gordon & Breach, 1966), pp. 160-161.

caustic the criticism: too much red tape and too little discretionary authority at the workbench level; salaries low in comparison to salaries paid by industry for comparable work; inability of many laboratories, particularly those under civil service rules, to attract, motivate, and retain the most qualified staff; and too much direction of the laboratories by their sponsoring agencies (ref. 146).^{*} And up to a point, the proposed remedies have been the same: make government salaries “comparable” to those offered by private industry; eliminate excessive layers of management; delegate more authority to laboratory directors to make decisions and control funds; and give the directors of government-owned, contractor-operated facilities complete authority to set and carry out personnel policy (ref. 147).

Considering this unanimity of opinion, this dissatisfaction with things as they are (or have been), it is surprising that so little has changed in twenty years. At contractor-operated facilities especially, the freedom from civil service requirements seems not to have produced the intended good effects. There seems to be a rule that, with time, contractor-operated and government-operated laboratories tend to become more like each other. The reason is quite simple: In each case, operating funds ultimately derive from congressional appropriations, and the senior officials of sponsoring agencies are held accountable for their proper use. The failure to understand this is at the root of the conflict between many contractor-operated facilities and their sponsors — for example, NASA and the Jet Propulsion Laboratory (JPL). When JPL’s functions were transferred to NASA in 1958, JPL officials assumed that the Laboratory would remain a quasi-independent institution working for one principal client — in this case, NASA rather than Army Ordnance. But NASA officials saw matters very differently. To them, JPL would take on the functions of a NASA center, although with a freedom not available to centers staffed entirely by government employees. These two views were irreconcilable and, under pressure from NASA, JPL did become more like a government laboratory, especially after 1964. JPL created the position of general manager, dropped the clause from its contract with NASA requiring tasks to be set by mutual agreement, and agreed to be paid on a cost-plus-award-fee basis rather than by an annual lump sum. The case of JPL can stand for many others. In practice, no Federal agency has been willing or able to give its contractor-operated facilities complete

^{*} One criticism found in the Bell Report seems to have dropped out recently — namely, that the Government was contracting out essential functions that were properly its responsibility. That less is heard of the problem twenty years later may mean one of two things: that the problem has been addressed and resolved, or that the fusion of government and contractor work has advanced so far that no separation of functions is really possible.

independence to set policies within the framework of its mission, even when there were no specific regulations to prevent this.

These conclusions will strike readers as pessimistic. But there's the rub: Federal laboratories, like other large organizations, do not change quickly. Without recognition of this fact — for fact it is — there is not the slightest possibility that the recommendations of the latest panel will have any greater effect than its predecessors'. Within the interstices of the Federal personnel system, laboratory directors can do much to start or redirect work, move younger staff into management positions, and, through a variety of tools, breathe life into the laboratory and maintain its technical competence. To understand the limits and possibilities of personnel management in a government environment, we shall look at the subject from an agency perspective. Specifically, we shall examine NASA in the 1970s and a recent experiment by two Naval centers to simplify their personnel management systems by introducing more flexibility. In this context, flexibility includes the ability to link pay to performance, drop outdated position standards, increase turnover of low performers, use the agency's block of senior level and excepted positions to retain the most desirable people, and move people between different divisions or even different laboratories within the same agency.

One word of caution. Many of the methods that make for flexibility are available. Many technology development agencies already have authority to retain certain appropriations until they are spent, reprogram within their accounts, use a percentage of laboratory funds for discretionary research, and terminate unsatisfactory employees. But as mentioned earlier, the theoretical freedom to do certain things is usually limited by the accountability of Federal agencies for the ways in which public monies are spent. They are limited still further by the authority of the Office of Personnel Management to set policy for executive agencies and by the authority of the Office of Management and Budget to set personnel ceilings for the same agencies. At every turn it seems as if the checks and balances of the Federal personnel system work against the interests of the government laboratory. Let us see if this is the case.

Two Case Studies in Personnel Management

NASA in the 1970s. One of the severest tests to which a large mission-oriented agency can be put is how it reacts to the completion of its original mission. Confronted by this problem as early as 1967, NASA responded by closing its Electronics Research Center, cutting back at most of the other centers, and creating a Personnel Management Review Committee of senior employees to advise the agency on personnel matters. But as NASA moved into the 1970s and cutbacks instigated by

the Office of Management and Budget (OMB) continued without letup, the agency decided to take a much harder look at its personnel management. The evidence, as shown by the work of internal task forces and contractor reports, reveals a consensus about NASA's problems and some possible approaches to solving them.

As early as 1966, an agency task force had concluded that certain options were no longer available to NASA for matching manpower with programs (ref. 148). Every subsequent review of NASA personnel accepted this conclusion as a starting point for its own investigations. The task force dismissed out of hand the possibility of mass transfers between centers; the conditions that had led to the transfer of the Space Task Group from the Langley Research Center to the new Manned Spacecraft Center at Houston in 1961 no longer existed. Transfers of individuals (unless voluntary) were even less likely to succeed because of the morale problems such moves usually created. Reductions in force were to be viewed as a means of enabling a center to move "from one step in technology to the next," but they were a poor way of redistributing manpower. In the end, the task force endorsed three methods of matching personnel to programs. It cautiously approved the concept of assigning tasks where the manpower was available — cautiously, because each center had its special capability and because "the prime interest of a center can properly change only very gradually." The task force also approved using attrition to move slots between centers and the "float" (the gap between authorized and filled positions) to handle emergencies (ref. 149).

What these recommendations amounted to was a consensus that some of the methods available to NASA for shaping its programs were, in effect, either unusable or unlikely to do much good. The problem, in the eyes of officials and the panels that advised them, was compounded by government-wide policies limiting the ability of officials to exercise their discretion. Among these policies were veterans preference, retreat rights, by which a senior employee whose job was abolished could "bump" a lower grade employee, the inability of agencies to retire older, marginal workers, and the use of position controls by OMB as a budgeting mechanism. Without exaggerating, we believe that these policies are almost as dominant now as they were a decade ago. Government reforms, notably the Civil Service Reform Act of 1978, have simply not yet had the good effects intended. It is still the case that executive agencies (NASA among them) are bound by the position controls imposed by OMB; that attempts to institute a new system of performance appraisal have left most managers confused and uncertain; and that the creation of a Senior Executive Service, a corps of high-level managers accountable for their successes and failures, has not fulfilled its purpose owing to the cap on salaries (ref. 150).

Perhaps the initial hopes as to what these reforms might accomplish were unrealistic. But the principal reason why these reforms fell short of expectations was that they ran up against certain political realities. It is unlikely that OMB would agree to the permanent elimination of manpower ceiling controls, or government unions to seniority rights, or Congress to the closing of some installations in order to preserve the jobs of employees elsewhere. Instead, research and technology development agencies have had to content themselves with modest incremental changes in their personnel management, while seeking permission from the Office of Personnel Management to conduct small-scale experiments to determine if changes in personnel regulations might lead to greater efficiency.

Since 1967 to 1968, NASA has sought to make the best use of certain powers vested in its senior officials. First, NASA has required of its centers that each have definite roles and missions; this, as we saw in Chapter IV, was what led NASA to assign responsibility for short-haul aeronautical research to the Ames Research Center and for long-haul aeronautical research to the Langley Research Center. Second, NASA has come to rely more heavily than before on the use of support contractors to shoulder the burdens caused by declining personnel ceilings. It is not that NASA is using more support contractors now than it was at the high point of Apollo. Rather, NASA has drawn a much sharper line between so-called "government functions," which may not be contracted out, and everything else. NASA's position was set forth lucidly in a 1976 staff paper: ". . . all functions *except certain fundamental 'Government functions'* have been at least partially contracted in NASA as long as improper supervision of contractor employees by Government employees was not required and there was no significant cost differential between the civil service mode and the support contractor mode . . . It is clear . . . that support contract manpower has become an essential in-house resource. Support contracting provides an effective . . . way of carrying out certain routine operations to 'keep the plant going' at NASA centers and also a valuable flexibility in meeting fluctuating program workloads with minimum disruption to other activities at the Centers." (ref. 151.)

This policy reserved to NASA employees functions that they may not delegate: responsibility for general management, external affairs, resources, procurement, and, most important, determining the work to be done. But it left to the agency sufficient authority to provide common support services to a number of divisions, rather than having each division provide them separately.

A third area for NASA officials to control was career development — identifying their best people and, in particular, grooming those scientists and engineers who wanted to make the transition to

management. To find and develop these professionals meant moving on several fronts: improving the agency's personnel management information system so that it contained data on what employees actually did and how well they were doing it, rather than relying on the formal (and often inaccurate) position classification system; encouraging inter-center transfers and selecting employees for tours of duty at Headquarters; stressing the importance of executive development, assigning responsibility for it to a very senior official, and keeping a confidential roster of key jobs and promising middle managers identified as top management potential; and devising training programs for staff whose skills were becoming obsolescent (ref. 152).

This matter of identifying future managers among professional staff needs some elaboration. At the NASA centers most successful scientists and engineers must eventually decide whether to remain in research or take on management responsibilities — even though, officially, both career paths have equal merit. As a kind of rule of thumb, to be on the fast track one should be a branch or division chief by the early thirties, and a center director by the early forties. Some professional staff already have that loyalty to the organization rather than their technical discipline which is a prerequisite for good managers. For others — for example, those people who are technically competent but are somewhat reluctant to take the plunge and become managers — the transition is clearly more difficult. It is not easy to take on the problems of a supervisor and retain the skills needed to step back into the role of the scientist or engineer. The specialist must switch his loyalty from his profession to his organization; he must learn to work more through others; he must become an expert in organizational procedures, while having few rules on which to fall back (ref. 153). Moreover, among engineers particularly, there is a kind of "instrumentalist" approach, an often unarticulated faith that organization and managerial problems are amenable to technical fixes, and that "politics" is an excrescence in a supposedly value-free technical organization. The engineer preparing to become a manager must often begin by consciously striving to overcome the effects of a narrowly technical education.

As important as identifying and training staff for management positions are, it is equally important that the agency recognize how difficult the transition process normally is (ref. 154). This was the principal finding of a study commissioned by NASA and the National Institutes of Health from the National Academy of Public Administration in 1971. The study also noted a defect of the civil service selection system: Where scientists and engineers believed (rightly or wrongly) that the only path to promotion and salary advancement was through managerial assignments, they often took on work for which they were unprepared. What emerged from the investigations of the study team

were the recommendations that the selection of managers be a continuous process, and that agencies use a variety of tactics — internships, supervisory training programs — to prepare employees for managerial roles. Implicit in the study's recommendations was the philosophy that, indeed, many are called but few are chosen, precisely because so few professionals choose themselves.

An Experiment in Federal Personnel Management. Is the Federal personnel system a shield or a target? Does the system permit any deviation from its rules? Every system has room for variances from the norm but, if too numerous, the system ceases to be a system. In fact, Federal personnel regulations do tolerate certain freedoms. There is an entire category of "excepted" positions, which is just what the term implies — positions excepted from the regular civil service screening and classification procedures. All large agencies have them and, indeed, regard them as one of their most valuable resources. From the beginning NASA officials fought hard to extend their authority to fill such positions "by persuading Congress to revise the (number) of excepted positions, hiring nonquota* personnel where excepted personnel were unavailable, reserving (as far as possible) a certain number of positions for 'quick hires' of executives from outside the agency, and doing what they could to keep the center directors and program associate administrators at the highest levels authorized by the various salary acts." (ref. 155.) But such positions seldom comprise more than two percent of an agency's permanent employees; and even if the percentage were higher, it would not change the current system at all. What would really be interesting would be an effort at modifying the system to give laboratory officials greater discretions in hiring, promoting, and retention. Absent a national emergency, it is unlikely that such an overhaul would be permitted.

There are, however, intermediate steps which some agencies can take. Without any changes in the law, agencies could rewrite the contracts governing their contractor-operated facilities to give the director complete freedom to set personnel policies (ref. 156). But if such authority were given, it might well lead to the wholesale conversion of laboratories from government to contractor operation. Another possibility is to permit agencies to conduct small-scale experiments with a simplified personnel system. The 1978 Civil Service Reform Act mentioned earlier makes this possible: It authorizes up to ten demonstration projects to determine if a simplified system would lead to greater efficiency (ref. 157). It should be stressed that these projects are quite limited in scope. They are limited to a maximum of 5 000 employees; they may not waive

* Nonquota positions are a category of scientific and research positions which do not count against an agency's block of supergrade (the old GS-16/18) positions.

leave, insurance, annuity, Hatch Act, or equal employment opportunity regulations; and they are limited to five years. To date, the Office of Personnel Management has approved only one project. This is a joint project — begun in 1980 and continuing to the present — of the Naval Ocean Systems Center in San Diego and the Naval Weapons Center at China Lake, California.

Clearly, any assessment of a project which is not yet completed would be premature. The avowed purpose of this project is to address problems acknowledged to be widespread. These include outdated position standards, the inability of supervisors to measure employee effectiveness, lack of the right incentives (including merit pay), and the adverse effects of reductions in force on good performers and newly-hired employees. Under project guidelines, the position classification system is to be simplified and made more flexible; performance appraisal will be linked closely to compensation and organizational effectiveness; pay will be linked to performance, with employees who exceed performance expectations receiving salary increases exceeding government-wide comparability increases; while performance will be “a primary criterion in the retention process while retaining tenure, veterans preference, and length-of-service factors.” (ref. 158.) To repeat: This project represents a modest step toward rationalizing the existing personnel system. What it promises is the establishment of performance criteria to be used in evaluating the project’s anticipated effects. Thus flexibility of workload assignments can be measured by the cost, quantity, and quality of recruits; increased turnover of low performers by the turnover of critical employees; retention of high performers by the overall retention rates; and increased recruitment success by the cost per recruit and recruit quality. Should the project succeed — that is, if the anticipated effects occur — the possibilities for change throughout the Federal service are evident.

Conclusions

The complexity of professional personnel policies is not so great as to make generalizations impossible. If anything, the opposite is true. There is, in fact, a wide consensus within the Federal community that there are serious problems — in the aging of professional staff, in the perceived lack of authority by laboratory directors to hire and fire, in that inflexibility in adapting regulations to changing conditions which is one of the system’s salient features — hampering the conduct of Federal technology development. Rather than repeat what has already been said about these matters, we will conclude with some remarks about the special nature of personnel management in a laboratory environment.

The presence of a few individuals of exceptional talent has, to a very large degree, been responsible for the success (and even the existence) of outstanding research and technology development organizations. It is not the function of the laboratory to be "representative," to be a cross section of the population, but to nurture exceptional talent wherever it may be found. A technology development organization that cannot tolerate and nurture a few eccentrics is halfway toward *rigor mortis*. In many Federal laboratories, the best fundamental research projects are almost always built around one outstanding individual. This person has demonstrated that he is capable of performing basic research of high quality, and he is accordingly granted the freedom to pursue his interests. It should be noted that in these smaller projects the question of duplication of effort usually does not arise, since usually no large resources are involved.

The most talented individuals will make their way, but what of the majority of professionals? For them, continuing education is essential — not because, as some allege, the exponential growth of new technology is pushing back the age of staff obsolescence, but to improve peripheral skills and to make employees familiar with subjects outside their areas of expertise. Note, also, that continuing education does not necessarily improve the performance ratings of people whose skills are becoming obsolescent.* Studies of continuing education programs in laboratories indicate that the highest performers took the fewest continuing courses; that a staff member's level of *initial education* is more important in determining his or her rating and salary than subsequent course work; that the younger the staff member at the time of receiving his highest degree, the higher the rating; and that there is little evidence of improvement in relative performance as a result of participating in continuing education courses.

These findings imply that the cost to an organization in man-hours lost and dollars spent may not pay off in improved staff performance. Alternatively, the findings may be interpreted as indicating that the particular programs of continuing education covered in these studies were

* Based on an admittedly limited survey taken by the General Accounting Office at two Air Force laboratories, it appears that many scientists and engineers consider management training more likely to lead to reward than technical training. More than half of the interviewees "considered that maintaining technical expertise is not an effective way to enhance promotion prospects. Promotions and awards are based upon job performance and, while special recognition may not be given to skill enhancement through education, there would be an indirect benefit when such activities improve performance upon which rewards are based. Nevertheless, their perception of nonrecognition might discourage some individuals from participating in continuing educational activities." Letter, R.W. Guttman, Director, Procurement and Systems Acquisition Division, U.S. General Accounting Office, to Secretary of the Air Force, March 6, 1978.

poorly designed or administered. Continuing education for mature scientists and engineers should not attempt to repeat or update graduate courses. The mature staff member needs a clear presentation of material he can use on the job, preferably presented by successful professionals within his own organization. The fact that so many outstanding research and technology development organizations with tough-minded managements provide continuing education, often wholly or partly during working hours, indicates a prevailing belief in continuing education as a means of raising the general standard of staff performance.

Finally, we assert that, in principle, simple measures exist for evaluating the aggregate productivity of professional staff and that they need not exclude one another. Productivity can mean the number of publications produced by an individual within the past five years, the frequency with which these publications are cited by the author's colleagues, or the extent to which the staff translated their own or others' research into applications (ref. 159). Such measures are essential for planning and budgeting purposes but, even more important, they are the yardsticks by which the laboratory's reasons for being may be judged. And it is surprising, but also encouraging, that productivity does not seem to be clearly related to staff age, age distribution, tenure, or turnover rate. Perhaps it remains only for research administrators in some laboratories to recognize this.



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CHAPTER VIII

Supporting Functions and Personnel

Supporting Functions Defined

Whatever the technology development laboratory may be, the last thing we can call it is a one-man show. The presence of a few exceptional individuals may account for the quality of the laboratory's work, but hardly explains how the work gets done. The exceptional scientist or engineer can accomplish little by himself. He needs colleagues with whom to exchange ideas, as well as to serve as collaborators in the working out of those ideas. But even after we subsume all the scientists and engineers at a single installation, there remain several categories of workers without whom the laboratory could not function at all. These perform what are called, for want of something better, "support" functions. There is no really adequate definition of support, but provisionally, we can say that whatever is not included in scientific and engineering work at one end and administration and clerical work at the other is a supporting function.* Support activities include everything from mowing the lawn or carting trash to writing sophisticated computer programs or running a tracking station. The more complex these support functions are, the less distinguishable they are from research and technology development.

But even this way of putting matters scarcely elucidates the significance of support functions for a research installation. Consider any large NASA center. It may have a photographic laboratory or image-processing facility for converting digitized information transmitted by satellite into pictures, storage facilities for holding expensive

* The functions we will discuss are covered by NASA Occupational Codes 100 and 300.

100-Wage System- (Trade and Labor Positions): Includes trade, craft, and general laboring positions (non-supervisory, leader, and supervisory), compensated on the basis of prevailing locality wage rates.

300-Technical Support Positions: Includes scientific and engineering aid, technician, drafting, photography, illustrating, salaried shop superintendents, quality assurance specialist, production planning, and inspecting positions.

one-of-a-kind equipment, tracking or telemetry stations for communicating with satellites designed by its staff, and (almost certainly) a data-processing facility for supporting the center across the board. While all of these facilities "support" the center's mission, we need to go somewhat deeper in order to understand the problems supporting functions pose. There are really three questions we have to consider: Can anything useful be said about functions as diverse as lawn mowing and providing ground support for a deep space probe? How can productivity or efficiency in general support functions be first measured and then evaluated? And what are the public policy implications of the contracting out of many support activities by Federal agencies?

To answer our first question: There are two broad classes of support activities. The first comprises those functions that cannot be managed and controlled as a direct function of the levels of primary technology development tasks of the laboratory. Suppose we call these *general support* functions (table 7). Included in general support are the personnel office, procurement and supply, financial management, administrative computing, and a number of more specialized staff offices. General support activities are typically organized along functional lines; that is, there exists a single personnel office or procurement office serving the entire installation. General support is also designated "research administration," "base support," and "indirect support."

Table 7. General Support Functions at a NASA Center

General Support	Direct Support
Communications	Graphics and Exhibits
Property and Supply	Publications
Financial Management	Reproduction Services
Personnel, Equal Opportunities	Photographic Services
Security	Electronic Instrument Services
Procurement	Model Shop
Library	Machine Shops
Health, Safety	Metal Fabrication Shop
Facilities and Engineering	Technical Computing
Plant Services, Maintenance	Also includes professionals, technicians, and facility operators assigned in supporting roles in line technical organizations.
Legal Services	
Public Affairs	
Technology Utilization	
Resources Management	

The second kind of support can be closely adjusted to the laboratory's primary work. Under this head we include graphics, reproduction, publications services, technical computing, photographic services, and shop services (table 7). It is clear that costs for a part produced in a shop, for example, can be charged to the technology

development activity requiring the part. Thus there is a built-in mechanism for controlling costs that does not exist in the case of general support organizations. We will call this second class of functions *direct support*. As with general support, direct support is often organized along functional lines; for example, there may be a single comprehensive machine shop or computer organization serving the entire center. Under direct support we would also include those technicians, craftsmen, and engineers assigned to line organizations to work alongside the engineers and scientists in primary technology development. Further, it is common in technology development organizations to locate special purpose shops or support laboratories within a line organization to facilitate direct support of primary research and development such as a special purpose computer installation. Figure 44, with direct support outlined heavily, makes this clear. Note that all direct support units are located in the Directorate of Research Support. But note, also, that direct support personnel "reside" in the technology development directorates (Aeronautics and Flight Systems, Astronautics, Life Sciences) as well.

To return to our first question — can anything useful be said about this diversity of functions? — we would reply that all of them can be classified as either general or direct support. A more important (and difficult) question is how we can measure the productivity of general support elements in particular. A related question is, what is the appropriate size (whether numbers of workers or expenditures) of the general support elements? Answers to these questions cannot be based on a laboratory's mission; they must be sought, rather, by considering the specific activities included under each general support function. To take an example to which we shall shortly return — warehousing, the management of a supply depot, may be broken down into broad categories such as housekeeping services, material operations, traffic management, and inventory management, and then further broken down into discrete, measurable activities. In short, we can first consider the appropriate mix of support to professional personnel, before trying to hit on other methods for assessing productivity. And that is precisely what we shall do, using NASA's Ames Research Center as an example.

Measuring Productivity: The Method of Support Ratios

Support ratios may be defined as the ratios of support personnel to the total center population. These ratios can be calculated at both a gross and at a *functional* level — that is, the ratios of numbers of workers in each support function to the total population. But such ratios mean little unless there are comparative standards to go by. Unfortunately, these do not exist. Each center's special mission will lead it to devise its own approach to the organizing of support activities, which may be reflected

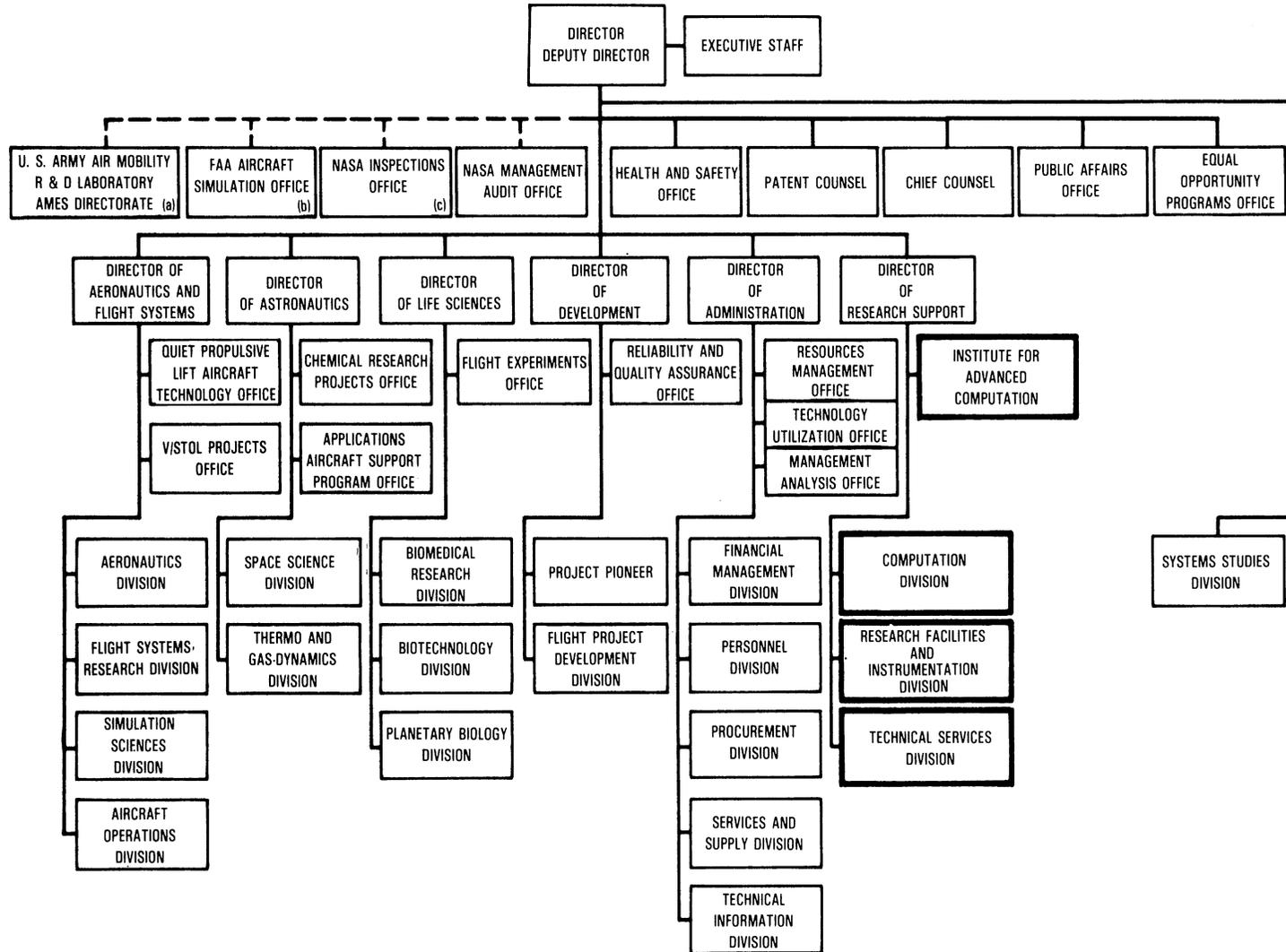


FIGURE 44. — The direct support organization at the NASA-Ames Research Center is shown on this chart.

in the use of more or fewer persons, or personnel at higher or lower levels.

Despite this lack of standards or norms, we might approach the problem of identifying support ratios by comparing them at a number of ostensibly similar technology development centers. In table 8, the functional level general support ratios at Ames are compared with those obtained in a survey of industrial laboratories undertaken in the early 1970s, and for nine Navy laboratories based on data furnished to one of the authors (Mark) by the Director of Naval Laboratories.

Table 8. General Support Ratios — Functional Level

<u>General Support Function</u>	<u>NASA Ames</u>	<u>Navy¹ Labs</u>	<u>Industrial² Survey</u>
Communications	0.3%	—	0.4%
Supply, Property Management	2.0	3.8	1.2
Procurement	2.0	—	1.3
Financial Management	2.6	2.3	3.3
Personnel	1.3	1.6	2.2
Security	1.3	—	1.5
Library	0.9	—	1.3
Health and Safety	0.3	—	—
Facilities Engineering	1.5	—	1.2
Plant Services and Maintenance	5.5	7.7	7.5

Notes:

1. Data for nine Navy labs provided by Director of Naval Laboratories, Dept. of the Navy, Washington, D.C.
2. The Industrial Survey is reported in: Jones, Richard A., *Research Administration: Its Relative Size in the Organization*. SRA Journal, Spring 1974.

At first glance, one would note the relatively good agreement between the ratios for Ames and those of the surveys. The problems begin when we try to interpret the data. For example:

- Though great care was exercised, there was considerable difficulty in matching the definitions of functional categories at Ames with those of the two surveys.
- Even where functional definitions could be relatively easily reconciled, there was no assurance that the activities of the functional organizations were really comparable. An example was *administrative computing*, which was subsumed under Ames' *financial management* function, but was not mentioned in the information available for the two surveys.
- The definition of "similar," in comparing the Ames Center with the firms covered in the survey, was unclear. For example, the industrial

organizations all had fewer than 1 000 employees, while Ames had about 2 500, including support contractors.

- Some organizations resided within another facility and may have shared certain general support functions. An example was the autonomous U.S. Army Aeronautical R&D Laboratory, a tenant at Ames Research Center, which used a number of the Ames general support services, such as the library, technical computing, and security.
- An organization with dispersed facilities may have some centralized functions. Again, the Army Aeronautical R&D Laboratory is an example, with personnel and payroll offices in San Francisco.
- Detailed comparative data were difficult to acquire, as shown by the blanks in the Navy laboratory data (table 8) and the paucity of published papers on the subject.

There are two gross ratios for which some comparative data are available. These are the general support ratio as defined earlier, and the administrative and nonprofessional support ratio. The latter can be defined as the ratio of total complement less all scientists and engineers to total complement. The available data are shown in table 9. The problems listed earlier still apply. The rather large difference between the NASA and military administrative and nonprofessional support ratios cannot be reconciled on the basis of available data. It may be that the NASA centers employ more professionals in supporting roles than do the military installations, rather than that the work of military laboratories requires larger nonprofessional supporting staffs.

Table 9. Gross Support Ratios at Selected Laboratories

Organization	Gross General Support Ratio	Administrative and Nonprofessional Support Ratio
NASA Ames Research Center	17.6	56.9
NASA Johnson Space Center	17.0	—
NASA (all centers)	—	57.3
Air Force Laboratories	—	70.3
Army Laboratories	—	66.2
Navy Laboratories	—	67.9
Industrial Survey	19.9	—

Note:

The Air Force, Army, and Navy ratios are derived from a report of hearings before a Subcommittee of the House of Representatives Committee on Appropriations, 93rd Congress — Investigative Report on “Utilization of Federal Laboratories.” USGPO, Washington, DC 1974.

Measuring Productivity: The Job Analysis Method

Our first attempt to measure the productivity of support personnel seems to have left us where we began. It seems that the use of support ratios may be of limited value in managing general support; that problems of comparison across organizations are formidable; that detailed data are generally unavailable; and that developing criteria for making decisions on the basis of these ratios has not been possible. Support ratios may have some value in reviewing variations in general support with time *within* (rather than *across*) organizations. We had better forget about support ratios, and consider how best to measure the productivity of selected general support services. In principle, it should be easier to measure the adequacy of support services, which are normally discrete and repetitive, than to measure the quality of research ideas or personnel. In the case of the former, we have a simple criterion by which to proceed — output. More precisely, it should be possible to take any support activity and, by means of a detailed analysis, determine acceptable performance. Such an analysis would require three steps: (1) enumerate the steps required to perform the work; (2) list those things — input — needed to perform the work; and (3) list those things produced by the work — output (ref. 160).

But as soon as we begin to consider any particular service, it becomes apparent that evaluating the outputs of even the simplest service is no easy matter. Consider, for example, a taxi or shuttle bus for taking employees from one part of a large research installation to another. How would we measure the quality of the service? We could develop any number of criteria as, for example, that a passenger should be picked up within four minutes of the agreed time, or that a vehicle should not be out of commission more than X hours in a given period, or that a certain number of drivers should be available on all working days. Even as simple a case as this demonstrates three things: To be evaluated, a service must be broken into discrete components; to each activity there must be assigned a quantifiable standard, along with the acceptable deviation from that standard — in other words, an acceptable quality level; and tradeoffs between standards must be made.

To avoid misunderstanding, some cautionary words are in order. While, for example, personnel and procurement certainly qualify as general support, they are so intimately related to the public interest that they cannot be easily separated from an agency's mission, as we might do with operating a mess hall or a supply depot. These latter are commercial activities in the sense defined by the Office of Management and Budget as "work that is separable from other functions or activities and is suitable for performance by contract." It is these activities which lend themselves, whether as direct or general support, to job analysis; and it is in this context that we are applying the method.

It is also necessary to warn against a simple-minded application of job analysis or any other method. Imagine this method applied to analyzing what a symphony orchestra does. An analyst might observe that for long periods the second clarinetist had nothing to do, that the tympanist only played repeated notes, and that the winds only repeated what the strings had introduced. Such an analysis would be correct as far as it went, but would simply miss the point of the performance. No method can substitute for judgment or a knowledge of what is being evaluated. Or as one writer put it, some works are like mirrors; if a donkey looks in, no apostle will gaze out.

A brief case study will serve to make these points clearer. One of the authors (Levine) was commissioned by the National Oceanic and Atmospheric Administration (NOAA), an agency within the Department of Commerce, to draft a plan to evaluate prospective contractors who would manage NOAA's supply operations. NOAA is itself an agency made up of other agencies, of which the largest and best known is the National Weather Service. All of these agencies are supported by NOAA warehouses which stock instruments, electronic equipment, common use technical and administrative forms, and NOAA publications, handbooks, and operating manuals. The largest of these warehouses, the NOAA Logistics Supply Center, is located in Kansas City, Missouri. For the moment, we can disregard the agency's intention to contract out the management of its warehouses; the performance criteria would be identical if the system was managed, as in fact it is, by government employees. The question remains: How can NOAA evaluate what is, in effect, a range of support services?

From what has been said, a general approach to evaluating NOAA's supply depot can be easily described. For each service, develop a standard; assign an acceptable quality level for the performance of the service; and design a surveillance method to determine if acceptable quality levels have been met (ref. 161). In practice, the task of drafting a quality assurance plan is a little more complicated. The Logistics Supply Center stocks some 8 600 line items, in addition to sophisticated one-of-a-kind equipment furnished to the National Weather Service; some items are inactive, while there are shortages of others; and in other cases, information on items in stock may not be readily accessible to users. Moreover, a quality assurance plan, to be effective, must be capable of being entered into a data-processing system; otherwise, the supply system will temporarily collapse whenever the one or two persons who carry it in their heads leave. The plan, as approved, allowed for the complexity of the system. Supply operations were broken down into some 65 to 70 discrete activities; to each was assigned a performance standard and an acceptable quality level; finally, one of three surveillance methods — random sampling, 100-percent inspection, and customer

complaints — was specified to determine that standards really were being met. (For contractor operations, there was also a category of deductions for failure to meet the acceptable quality level.)

How does this system work? Suppose we have a requirement that the operations manager must check all incoming shipments. We could then specify, as a standard, that the correct number of containers as noted on the carriers' freight documents has been received. To find out if this standard is being met, we could carry out a random sampling of verified items received on randomly selected days. Finally, we would check to see that what was received met our acceptable quality level — say, that no more than 5 percent of incoming items were not properly verified. Table 10 gives three more examples of this kind of job analysis. None of these procedures is novel. The method of job analysis simply means that an agency looks at work as it is being done to see what actually results. This method has long been used by private industry and the Department of Defense, and the Office of Management and Budget now requires Federal

Table 10. Job Analysis of NOAA Warehousing Operations

Requirement	Standard	Acceptable Quality Level	Surveillance Method
Establishes and implements control over NOAA-owned equipment	Establishes written procedures which prescribe the relationships, operations, and specifics of NOAA Logistics Supply Center equipment control system	5%	100%-inspection
Identifies all non-expendable and selected items of expendable equipment	Controlled and identified in the records and on the equipment by a uniquely numbered tag	5%	Random sampling of relevant documents
Maintains an accountability system for equipment management	Establishes register with equipment control numbers listed sequentially and containing, as a minimum: (a) the equipment control number, (b) date assigned or tagged, (c) noun, and (d) acquisition document identification number	5%	Random sampling of register

agencies to do job analyses before contracting out support services. The important thing to note about the job analysis method is that it can be extended to evaluate any support functions, no matter how complicated, and that in this respect it is easily superior to the support ratio method discussed earlier.

The Legal Status of Support Services

We have not yet answered the last of the questions posed earlier: What are the implications of the contracting out of many support functions? The first thing that needs to be said is that the question of who shall provide commercial services to the Federal Government is hotly debated. By now, analyses of the Office of Management and Budget's (OMB) Circular A-76, the controlling document on the subject, have reached talmudic levels of complexity. Since 1955, there have been two opposing philosophies regarding the use of contractors to provide support services to Federal agencies. The first, as summed up by OMB, is that it is the government's policy not to compete with its citizens, but instead, to "rely on competitive private enterprise to supply the products and services that Government needs." (ref. 162.) The other philosophy is that government shall attempt to get its work done by its own employees, only contracting with the private sector when the nature of the work makes full-time use of government employees impracticable or the skills needed to do the work in-house are unavailable (ref. 163). By the late 1970s, the former philosophy had completely superseded the latter, to the point where some officials and congressmen began to wonder out loud if the government was not losing its ability to evaluate its contractors' work.* To understand what the official policy implies about the government's conduct of research and development, we need to understand what OMB Circular A-76 prescribes. As we shall see, there is nothing self-evident about the procedures for converting a government, commercial, or industrial activity to contract operation.

* Of course, there are circumstances where programs are best evaluated from the outside, on the principle that no one should be judge in his own cause. This is why the Securities and Exchange Commission requires independent audits of publicly-held corporations, why Congress in authorizing certain education and social services programs requires evaluations by outside contractors, and why the Defense Department created RAND and the Aerospace Corporation as sources of independent technical evaluation. What is at issue here is a narrower question: How does an agency determine if it is getting value for its money? It can hire an outside evaluator, but once the evaluator submits a final report, what then? There seems to be a danger of an infinite regress: The agency selects a second evaluator to evaluate the first evaluator, followed by a third evaluator . . . If government employees have a stake in boosting their own programs, outside evaluators may have a stake in telling their clients what they want to hear. Otherwise, they may not be invited back.

Four principles enunciated in OMB Circular A-76 define how and by whom commercial and industrial work shall be done. First, there are restrictions on how contractors may be used: Contract employees may not supervise government employees, nor may government employees be involved in close, continual supervision of contract employees; and in-house work may not be converted to contract solely to avoid personnel ceilings or salary limitations (ref. 164). Second, there are certain functions which may not be contracted out, functions enumerated by a former director of the Bureau of the Budget (OMB's predecessor) as "the decisions on what work is to be done, what objectives are to be set for the work, what time period and what costs are to be associated with the work, what the results expected are to be . . . the evaluation and the responsibilities for knowing whether the work has gone as it was supposed to go, and if it has not, what went wrong, and how it can be corrected on subsequent occasions." (ref. 165.) This is as succinct a justification for an in-house staff as one could wish, but its practical application is less clear. As will be shown, there is no longer a firm line dividing research and development from "routine" support services.

Third, Circular A-76 outlines a procedure for agencies to follow in deciding whether to contract out their industrial and commercial activities. To simplify greatly, an agency does a cost-comparison study; that is, it develops an estimate of the cost of government performance of a commercial activity and compares it to the cost of contract performance (ref. 166). If studies warrant contract performance, the agency solicits bids and, in effect, competes against commercial firms to see who can do the work at least cost. If a firm's low bid is at least 10 percent lower than the agency's, the government is required to contract for performance of that service. Finally, OMB authorizes Federal agencies to carry on commercial activities if no satisfactory outside source is found to be available. This is, one might say, an escape clause for the government.

If the reader suspects that interpreting Circular A-76 is rather like walking through a minefield, our point is made. The circular wavers among the various reasons for justifying contracting out: because a commercial source is *available*, because of *cost*, or on general philosophical grounds. These may be good reasons, but they do not account for the main reason agencies have contracted for services: *It is often the only way to get the job done*. Agencies like NASA and the Defense Department have not let huge service and base support contracts simply because commercial sources were available, or because costs would be lower, and least of all for philosophical reasons. Rather, they have let these contracts because they were subject to continuing civil service manpower reductions, and the extensive and sophisticated use of support service contracting was the only way in which these agencies could continue to perform the functions for which they were responsible.

In this sense, government agencies may be violating the spirit if not the letter, of the provision that support service contractors should not be used as a substitute for civil service employees. The political climate in the past fifteen years has been such as to make this situation unavoidable. Government employment has not been held in high esteem ("There are too many bureaucrats") and yet the government has been called upon by the Congress to render ever more complicated services to various client groups. The pattern has been to appropriate the funds to do a given job but then — in the name of government "efficiency" — to cut back the number of civil service people necessary to do the work.

We can now examine the role of support services in a research and technology development environment.* There are three basic contractual arrangements that agencies use to manage their programs, ranging from management of entire installations to providing support for specific activities:

1. The agency awards a fixed-term, renewable contract to a commercial firm, a university, some other not-for-profit organization or a consortium, to manage an installation. The contractor "gets no proprietary benefits from laboratory research or facilities (whatever is available to the contractor is also available to other parties on the same terms). Its role is almost entirely administrative." (ref. 167.) The classic example of this relationship is the government-owned, contractor-operated facility, such as the Jet Propulsion Laboratory and the multiprogram laboratories of the Energy Department. Employees at these installations are on the contractor's payroll; thus workers at Sandia Laboratories are employees of Western Electric, not the Energy Department.
2. The agency installation is managed by government employees, but the agency awards a master contract for housekeeping and base support, and separate contracts for more specialized functions. NASA's Kennedy Space Center is an example. Trans World Airlines provides base support, firms with major development contracts provide checkout and launch support services, and the Air Force Eastern Test Range provides joint support for services such as photoprocessing. A comparable arrangement is the use of base support contractors at military installations.
3. Again, the installation is managed by government employees but there is no master contract for base support. Instead separate contracts are let for particular activities such as technical writing, janitorial

* OMB Circular A-76 does not apply to the conduct of research and development. However, "severable" commercial activities in support of research and technology development are subject to the circular.

services, image processing, ground support, computer programming, and the like. This is the practice followed at most of the larger NASA centers, as well as some Defense laboratories.

There are, of course, all kinds of intermediate arrangements, such as a contractor-operated test facility at an installation managed by government employees. The important point is that government practice is frequently at odds with government policy. Contractors are used for work that could be done in house, while government employees provide services which could be provided more cheaply by contractors (ref. 168). As a 1981 report of the General Accounting Office (GAO) said, “. . . since the executive branch first established a general policy of reliance on the private sector . . . emphasis has shifted from almost outright reliance on the private sector to reliance with exceptions.” (ref. 169.) Federal policy *has* been inconsistent, but the reasons for inconsistency go even deeper than those given by the GAO. For one thing, the existence and thriving state of government-owned, contractor-operated laboratories suggest that “make or buy” decisions may be a side issue. If Federal agencies can contract out of the civil service system entirely, why the fuss as to whether government or contract labor should be used for a given activity?

What Circular A-76 obscures is that the nature of support services in technology development agencies has changed significantly during the past decade. Leaving basic research to one side, we can hardly distinguish government from contract employees simply by what they *do*. If we try to subtract all support services from research and technology development, we will be left with no “development” and precious little “research.” What distinguishes government employees from support service contractors is not *what* they do or even *how* they do whatever it is that they do; it is, as we saw, the power of the former to decide what work will be done and to evaluate the results. But as supporting an agency’s mission has become very complex, the government is at a real disadvantage in dealing with its contractors, since the latter can hire the most competent (who will usually be the most experienced) technical personnel.*

* This shrinkage of in-house technical competence may have something to do with the persistence of “mutuality clauses” — requirements that research tasks shall be set by mutual agreement — in the contracts negotiated by the Energy Department with its multiprogram laboratories. A mutuality clause may mean that an agency has enough confidence in the quality of its managers to permit them to negotiate work with its contractors; but it may also mean that the agency must, perforce, defer to its contractors’ good judgement. NASA dropped its mutuality clause with the Jet Propulsion Laboratory precisely because its senior officials were confident that they had the in-house competence to assign research tasks unilaterally.

Two examples of services in support of NASA will illustrate both the nature of the more sophisticated support services and the difficulties faced by government managers in monitoring them. In 1978, the Goddard Space Flight Center awarded a contract to the Space Systems Division of General Electric for operating a ground station to support the launch of the Landsat-D satellite, for which GE was also the prime contractor. By early 1982, the Landsat Ground Segment had become a project in itself, with more than 300 engineers, computer scientists, and technicians housed in a building at Goddard. The Ground Segment was a very large, complex, totally integrated network of automated data processing equipment; the software alone amounted to more than 680 000 lines of programming code (ref. 170). Even GE seems to have been a little awed by the magnitude of the task. As the Space Systems Division's house organ boasted, the final configuration of the Ground Segment consisted of "15 computers, 44 disk drives, 36 tape drives and 30 terminal interfaces, as well as 22 racks of special purpose hardware . . ." (ref. 171.) The Landsat Ground Segment was a support activity in the strictest sense of the term. Yet the lack of in-house staff available to design a system such as the Ground Segment also made the job of evaluating the contractor's effort — that is, whether the government was getting good value for the money — difficult, if not impossible.

As our second example, consider the use of the computer systems in wind tunnels. In engineering jargon, the development of computer programs is the "pacing item" in the field of computational aerodynamics. The basic equations needed to simulate airflow over a complete aircraft free of any approximations are extremely complex, with up to 60 partial derivative terms (ref. 172). Only supercomputers with speeds about 25 times greater than that of current computers can handle such equations. The real bottleneck, though, is in programming. In its final report on *Aeronautical Research and Technology Policy* (1982), the President's Office of Science and Technology Policy identified precisely the reason why NASA centers like Ames and Langley were having trouble acquiring the software they needed: "Software development by in-house staff is extremely difficult because the government cannot attract and retain . . . personnel in this area. The result is a large contract effort to develop and maintain software. This, unfortunately, results in no in-house expertise. Industry figures indicate that 20-30% of programming costs are required to just maintain software. The standardized wind tunnel data system software being developed at Ames will take at least 35 man-years of programming effort and should have begun before the data system was acquired. Because of funding limitations, this was not possible, and hence it will be three years after hardware delivery before the new system can be used to its full capability." (ref. 173.)

To sum up the debits and credits of contracting for support services: contracting out is often less expensive, especially in base operations; it gives the agency greater leeway in rapidly building up the work force at the start of new programs and phasing it out when the work is completed; it frees government employees from routine chores; and it is often the only way to tap expertise unavailable in the government. On the debit side, contracting out may create a vicious circle: Industry attracts the most capable technical people, the agency must perforce contract for a particular service, and the government ends up indirectly paying its contractors what it could not pay them directly.

But contracting out has still other disadvantages. There is no conclusive evidence that contracting out, except for routine base operations, is less costly than having the work done in house. Contracting is time-consuming and does nothing to relieve immediate manpower shortages. The complexity of the process gives contracting officers reason not to contract out. As the former head of OMB's Office of Federal Procurement Policy observed: "The [A-76] handbook is so complex and detailed, you need a training program to teach people how to use it." (ref. 174.) (He spoke better than he knew; A-76 seminars and training sessions are a thriving cottage industry in Washington.) Finally, government managers are concerned that contracting out can lead to work stoppages because of strikes, and that a decision to contract out is usually irrevocable, even if it is less desirable than keeping the work in house (ref. 175).

In short, no conclusive case in the abstract can be made either for or against contracting out. Yet the percentage of support services contracted out is likely to increase, especially for the more sophisticated services on which technology development agencies depend. One thing is quite clear: Cost is not, and for many years, has not been an overriding factor in make or buy decisions. To repeat, the principal reason why NASA and the Defense Department have contracted for the most sophisticated (and expensive) services, especially those involving data processing, is that there was no other way to get the job done. It needed no Circular A-76 to encourage NASA to rely on the private sector. Long before Circular A-76 was promulgated in 1966, NASA had been routinely contracting out 85 to 90 percent of its Research and Development appropriations and it continues to do so. And this dependence on the private sector can only grow, given the nature of the work carried out at the NASA centers and the technology needed to support it: the short (5 to 10 years) life cycles of computer systems, the costs associated simply with maintaining software, the move to computer-aided design, manufacture, and simulation, and the development of supercomputers able to perform up to one billion calculations per second. There may be some changes in the management of these contracts. For example, many support functions may be

physically consolidated at a single "operations center," just as support contracts themselves may be consolidated into long-term master contracts. But the combination of a slowly declining government work force and work that is both demanding and very expensive means that contracting for support services will remain unavoidable.

There is, then, something paradoxical about support services in a research environment. The output of any service can be measured and, therefore, evaluated. But the legal status of these services is ambiguous. We have found government employees providing commercial services, contract employees performing governmental functions, and a certain disregard for the rule that functions shall not be converted to contract to avoid the limit on personnel ceilings. But too much attention paid to legal issues may cause us to miss an important point. For the center or laboratory director, these functions are among the easiest to control, since input and output can be defined in advance. What is still lacking is some model to integrate support services with the management of the laboratory's internal resources — people, money, equipment. In other words, something is needed to tie a particular function — say, data processing or micrographics — with the laboratory's mission. Somebody has to be able to set objectives, develop long-range plans, assemble the resources, and do everything needed to make the divisions within the laboratory work as one. It is to this function of internal resources management that we now turn.

CHAPTER IX

What the Research Executive Does

Toward a Theory of Research Management: Search for a Method

By the nature of their work, senior research administrators are not likely to spend their time reading textbooks on public administration. There is never enough time to do everything that needs to be done, there are never enough people available to do the work, and there is never enough money for all the programs needing funding. All too often the motto of the senior administrator — someone with the power to hire and fire, to select project work assignments — is: Sufficient unto the day is the evil thereof. The circumstances under which he works leave him neither time nor inclination for a theoretical analysis of the organization in which he has chosen to spend his career. As is not uncommon in large organizations, the people most familiar with their operations are often the ones least able to describe what is going on.

This lack of introspection on the part of research officials has had some undesirable consequences. We have no really adequate theory of the organization of Research and Development institutions: indeed, there are no generally agreed definitions of basic or applied research or, for that matter, any consensus on how basic research (however defined) feeds into industrial productivity. Again, it is probably unwise for senior officials to ignore the broader implications of what they do. All of us carry a picture of the world around in our heads; but only to the extent that we are sufficiently aware of our assumptions to criticize them can we match them against the world “out there.”

Thus most of our knowledge of organizations comes from persons on the outside. There are, broadly speaking, two kinds of theories concerning the behavior of organizations, research installations included. The first approach attempts to develop certain extremely general axioms applicable to every kind of organization. A well known example of this approach is March and Simon's *Organizations*, the core of which is a series of propositions about organizations (ref. 176). We learn, for example, that “both the amount and the locus of uncertainty absorption affect the influence structure of the organization.” Again, “the greater the standardization of the situation, the greater the tolerance for subunit

interdependencies." Yet again, "the greater the amount of past experience with a decision situation, the less probable that intraindividual organizational conflict will arise." Readers may or may not find these assertions intuitively obvious. The problem begins when we try to conceive how we could test, let alone validate, assertions of such generality. It might even be the case that a theory which accounted (say) for the operations of a retail chain would, for that reason be inadequate to explain the working of a laboratory. The most we can say is that, while these theories might be confirmed by experience, they are logically anterior to experience.

This is not to deny that much of what March and Simon assert is of considerable importance. In particular, we accept with some reservations the central thesis of *Organizations* that "most human decision-making, whether individual or organizational, is concerned with the discovery and selection of satisfactory alternatives; only in exceptional cases is it concerned with the discovery and selection of optimal alternatives. (ref. 177.) Given the constraints under which research executives normally labor, they will indeed have reason to prefer the alternative which is satisfactory to one which is optimal. To use the word coined by March and Simon, they will "satisfice;" in research, as elsewhere, the best is often the enemy of the good. But to understand how a research organization really behaves, we need something more descriptive, less abstract, and less on the theoretical plane.

A second approach is inadequate in a different way. This approach claims to be empirical: The theorist puts a hypothesis before us and then tells us to look and see how admirably it squares with our experience. A famous example of this approach is Luther Gulick's acronym POSDCORB defining the work of a chief executive: Planning, Organizing, Staffing, Directing, Coordinating, Reporting, and Budgeting (ref. 178). Many a member of the Senior Executive Service, reading Gulick's analysis, might well feel drunk with power! The problem with this formulation is not that it is wrong — chief executives do all the things Gulick claims they do — but that it is incomplete. Executives such as the research management people with whom we are concerned are limited in many ways: by lack of funds, by agency mandates which they cannot significantly change, by government-wide policies over which they have no control. A purely formal description of what executives are supposed to do will be doubly misleading. It will say nothing about the constraints just mentioned, and it will ignore the informal strategies executives normally employ to achieve their ends.

There is, however, a third approach, which we tentatively advance. For purposes of analysis, it is possible to classify organizations in various ways. One such attempt, by the British sociologists Burns and Stalker in the early 1960s, is particularly suggestive. Based on a study of the British

electronics industry, they posited two types of working organizations, the "mechanistic" and the "organismic." The mechanistic organization is the classic hierarchical, bureaucratic system described by Max Weber nearly eighty years ago (ref. 179). A mechanistic system can be easily identified by the specialized differentiation of functional tasks, the precise definition of each person's rights and obligations, the elaborate code governing superior-subordinate relations, the greater importance attached to local rather than to general knowledge, and the concentration of information at the top of the hierarchy. Mechanistic organizations are appropriate to stable conditions.

Where conditions are changing rapidly, where unforeseen requirements occur, a very different kind of organization is needed. In the organismic system, what is posited of a mechanistic system is turned around. The system is characterized by a network structure of control and authority, a lateral (rather than vertical) direction of communications, greater emphasis on commitment to the concern's mission than to decisions and judgments by one's superiors, and acceptance of the fact that knowledge may be located anywhere in the organization. Burns and Stalker summarize the nature of the two systems, thus: "Mechanistic systems tell (the individual) what he has to attend to, and how, and also tell him what he does *not* have to bother with, what is *not* his affair, what is *not* expected of him — what he can post elsewhere as the responsibility of others. In organismic systems, such boundaries disappear. The individual is expected to regard himself as fully implicated in the discharge of any task appearing over his horizon. He has not merely to exercise a special competence, but to commit himself to the success of the concern's undertaking as a whole." (ref. 180.)

These observations may provide a clue to the kind of conceptual framework we seek. The purpose of this book is to describe a certain institution, the technology development laboratory, and a certain kind of executive, the "satisficing" research administrator, as they really are. We are not out to substitute one full-blown theory for another, but to test our working assumptions to see if they correspond to reality. Our first assumption is that the *technology development laboratory is a mechanistic organization trying to behave like an organismic one*. Routine always threatens to drive out innovation, but most of what is done at any laboratory involves complying with directives from the head of one's division or directorate, or from the laboratory director, or from the administrative offices of the sponsoring agency. The problem is not so much to rein in a group of free spirits out to "do their own thing" as to ensure that the laboratory is not smothered by requirements imposed from the outside.

It is no accident that some of America's most successful corporations are organismic organizations. In their remarkable book *In*

Search of Excellence (New York: Warner Books, 1982), Thomas Peters and Robert Waterman have identified, with a wealth of detail, the attributes common to these firms, whether their products are hamburgers (McDonald's), integrated circuits (Texas Instruments), or tape and allied products (3M Company). These attributes include: a bias for action ("do it, fix it, try it"); respect for the individual; autonomy and risk-taking; simple form and lean staff. For our purposes, the most interesting attribute of these companies, and the one most relevant to the laboratory, is that they are simultaneously centralized and decentralized. In the world of advanced research, where the environment is uncertain and changing, executives need to push decision-making down to the lowest practicable level, with great flexibility as to methods of getting things done. This flexibility must *not* extend to mission objectives; otherwise flexibility equals plain chaos, as opposed to the "organized" chaos found in the successful organizations examined by Peters and Waterman.

Our second assumption is a corollary to an observation made in the preceding chapter. There, we noted that it was becoming harder to distinguish "support" from "research and technology development." The corollary is that, to a growing degree, *process and production are becoming interrelated*. For example, the recent development of powerful systems of computer-aided engineering and automated systems of text editing may profoundly affect the kinds of work laboratories do. Technology development laboratories are not simply the beneficiaries of these systems; they are heavily involved in (for instance) designing video discs to replace field operation manuals, storing engineering drawings and parts information digitally, and creating an electronic file of designs, such that the laboratory or its contractors can move from one product generation to the next with parts modified from existing designs. If these systems can be brought to fruition, we may expect two changes in the ways laboratories carry out their missions. Productivity will be enhanced, and the laboratory will concentrate more of its resources on *process*, that is, on the best procedures — whether in design, distribution of product information, or testing — for accomplishing substantive programs.

Our third assumption is that in any large laboratory, *the way work is funded often determines what kinds of work get done*. Patterns of institutional funding vary from funding by task order to one where the laboratory is given considerable freedom in allocating money to its projects. One approach found at the NASA centers and, in different form, at the National Bureau of Standards, is level-of-effort funding. This pattern has been lucidly described in a recent report on the contract-operated laboratories of the Energy Department: "A laboratory may receive a funding allocation each year based on the size of its staff, its buildings and equipment and the nature of its work. Agency programs then negotiate with the laboratory (and perhaps an agency coordinator)

for their piece of the pie . . . For example, each NASA research center receives a lump sum annually based on the size of its staff, its buildings and equipment, and the nature of its work . . . In addition, a certain proportion of NASA's R&D support comes to the laboratory on a prorated basis related to the level of effort, covering such items as capital equipment, computer service, and other kinds of general R&D support. Clearly, this makes tasked R&D in such centers very inexpensive compared with a laboratory without such base support . . ." (ref. 181.)

The point is not that one method of funding is better or worse than any other, but that each sets the terms on which the laboratory goes about its business. A laboratory where most funding is assigned by task effort is likely to be organized differently from one which receives an annual lump sum. In the latter case, the laboratory will probably negotiate a total package with its sponsoring agency; in the former, there may well be a complicated process of bargaining before laboratory and sponsor can agree on which tasks (with overhead) shall be funded.

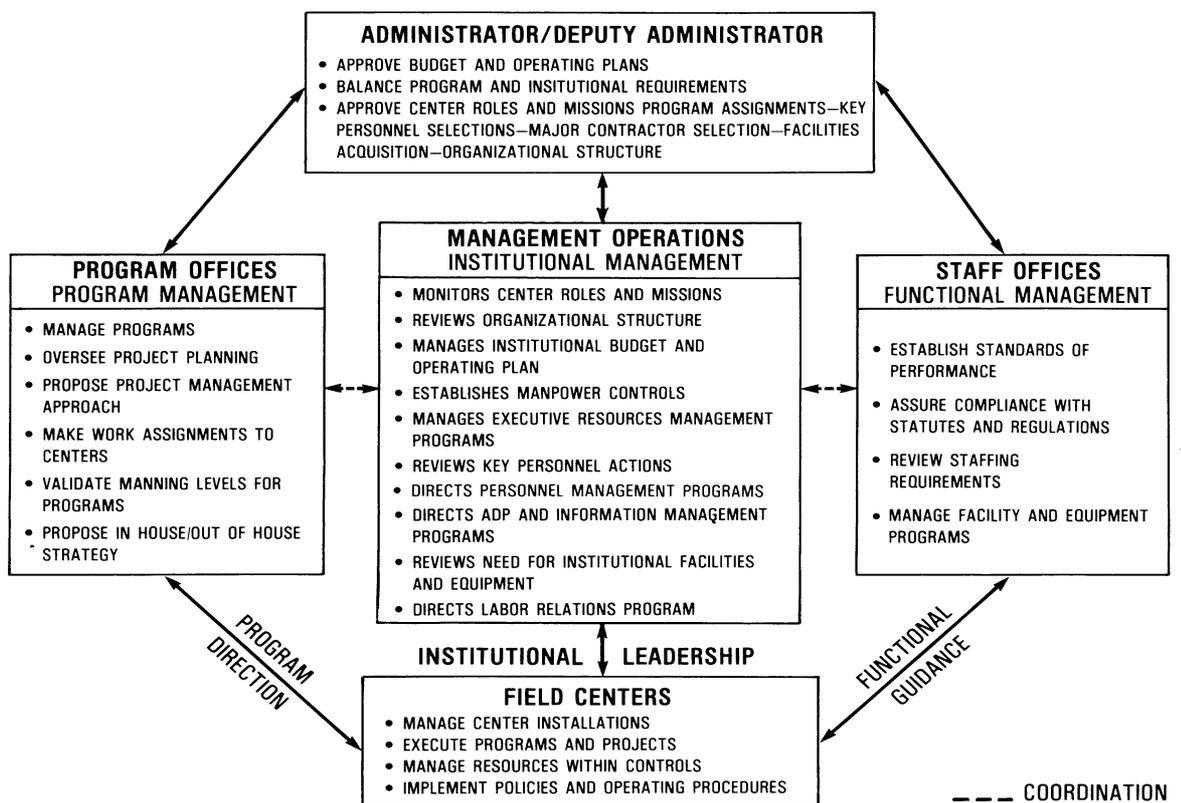
It is time to piece together these remarks into something more coherent. We want to understand this curious phenomenon, the modern technology development laboratory. Much of what we need for that understanding has been presented piecemeal in earlier chapters: the nature of project management, the roles and missions of one laboratory in a multilaboratory agency, the nature of support functions, and so forth. But something is still needed if we are to understand how a laboratory's resources — people, money, facilities, and equipment — are pulled together to accomplish useful work. The approach adopted here is to focus on the senior laboratory executives, those officials ultimately responsible for the institution's performance — to their bosses at Headquarters, to Congress, and to the taxpayers. (In the case of large privately funded laboratories, read "Board of Directors" for "Congress" and "Stockholders" for "Taxpayers.") In analyzing what it is they do, we shall concentrate on resources management at the NASA centers, for three reasons. First, they are all government-owned, government-operated facilities, and do not present the more complicated policy issues of a contractor-operated facility like the Jet Propulsion Laboratory. Second, NASA is a multilaboratory agency, and much of the interest in examining the agency's resources management is in seeing how different institutions are grouped in related fashion. Third, the planning and control of NASA resources is usually well documented and more accessible to the analyst than much of what occurs in Defense and Energy laboratories or in privately funded technology development institutions.

Starting, then, with the NASA centers as our "case," we shall review in detail those functions that justify the existence of a center's senior executives — or, Who needs the boss? These functions are:

- Setting objectives
- Short-term planning
- Structuring the organization
- Reviewing, measuring, and evaluating results

Each of these functions is susceptible to further breakdown. "Short-term planning" includes budgeting, facilities management, drafting program operating plans for Headquarters approval, and much more. This process is known in NASA as Institutional Management, which is understood to include "all those activities and resources involved in the development, utilization, and maintenance of the NASA in-house capability in an effective and efficient manner." Figure 45 provides an overview.

OVERVIEW OF THE INSTITUTIONAL MANAGEMENT PROCESS



Source: *The Planning and Control of NASA Resources* (NASA Technical Memorandum 83090, 1981), p. 24.

FIGURE 45. — This is an overview of the institutional management process as used by NASA.

The Functions of the Senior Executive

We begin with the most all-embracing, long-range functions, working our way from the drafting of long-range plans to the improvement of performance based on those plans.

Setting Objectives. This is the overt act of deciding what the organization's reasons for being are. At the level of research and technology development agencies such as NASA and the Energy Department, objectives are enunciated in very broad terms in their enabling legislation. Within each center, the objectives become much more specific, and in the long term they may be nothing more than a delineation of the center's current or evolving expertise.

To a degree, the circumstances leading to the creation of a laboratory are those "objectives." Often, a laboratory is created as a vehicle for an outstanding scientist or research administrator, as the Lawrence Berkeley Laboratory was built around Ernest Lawrence, Lawrence Livermore National Laboratory around Edward Teller, and (in a very different way) the Marshall Space Flight Center around Wernher von Braun and his rocket experts. The problem for such installations is how to adjust once their founders are gone. For example, the Marshall Center moved from a strong emphasis on propulsion systems into many other areas: the Apollo Telescope Mount that was the principal payload for Skylab, the orbiting Space Telescope, the High Energy Astronomy Observatory, and similar projects more in the realm of space science than development engineering. At the same time, the center continued its role in propulsion technology, with responsibility for developing the main engines, external tank, and solid-rocket boosters of the Space Shuttle.

What these examples suggest is, first, the complexity of changing objectives and, second, the limited role of laboratory directors in this process. A really major change in the mission of a laboratory must normally be approved by Headquarters. Moreover, the decision to change the mission of a center is seldom made on technical grounds alone. The 1962 decision to remove the development of the Centaur upper-stage vehicle from Marshall to the Lewis Research Center was taken in order to minimize interference with Apollo. The decision four years later to assign the Apollo Telescope Mount to Marshall was made because work there on the Saturn rocket was phasing down, leaving the center vulnerable to reductions in force and raids by other centers on Marshall personnel. Neither of these decisions would have taken the precise form that they did if center officials, led by von Braun, had not made their position clear. But these were primarily Headquarters decisions made at the highest levels of the agency.

In what ways, then, do center executives, set goals? The answer seems to be that they do so incrementally — by shifting resources

from one directorate to another, by the use of discretionary funds, by sponsoring research and technology work which may blossom into full scale projects several years later, by improving the center's support capabilities (data processing, test facilities, technical documentation) so that they attract new sponsors, and most important of all, by selecting talented people to do the jobs they deem most important. They rarely do zero-based budgeting, save as a requirement imposed from outside. Whatever its merits, zero-based budgeting is ill-suited to most government functions. Laboratory directors do not start their mornings with a clean slate; they deal, most emphatically, with certain givens which they can only alter with difficulty. What the center director *does* is more important than anything written into a formal plan. The Chapter IV case study of work on short-haul systems sponsored by the Ames Research Center is an example of this method. But there are many others. In his *Functions of the Executive*, Chester Barnard has concisely explained what the method of defining organizational purpose really is: ". . . strictly speaking, purpose is defined more nearly by the aggregate of action taken than by any formulation in words; but that . . . aggregate action is a residuum of the decisions relative to purpose and the environment, resulting in closer and closer approximations to the concrete acts . . . It is more apparent here than with other executive functions that it is an entire executive organization that formulates, redefines, breaks into details . . . *No single executive can under any conditions accomplish this function alone, but only that part of it which relates to his position in the executive organization.*" (ref. 182.)

So far from being identical, goal setting and formal long-range planning, as commonly understood, may even be antithetical. The conventional wisdom about long-range planning is that the planning organization must develop plans that are bases for current decisions; that plans should, so far as possible, be written; that each major suborganization in an agency should have its own long-range planning group and that all long-range planning groups should be permanent (ref. 183). But experience shows something else: While *plans* may or may not exist, *planning* occurs in all the operational divisions of a good organization. Laboratory executives are reluctant to draft long-range plans, partly out of a fear of committing themselves prematurely to an untried course of action, partly out of the feeling that such planning is a waste of time, and partly from the knowledge that next year's budget is plan enough. One may say that planning is about what the organization does *now*, not what it will be doing five years hence.

Thus laboratory executives are not forced to choose between supposedly rational analysis and muddling through. If we take Barnard's analysis a step further, we would say that a capable research executive defines goals most effectively by conveying them to those managers who

must flesh them out. By making his managers partners in a common effort, by delegating to them authority to work out new tasks in detail, the executive engages in a most effective (if informal) kind of planning.

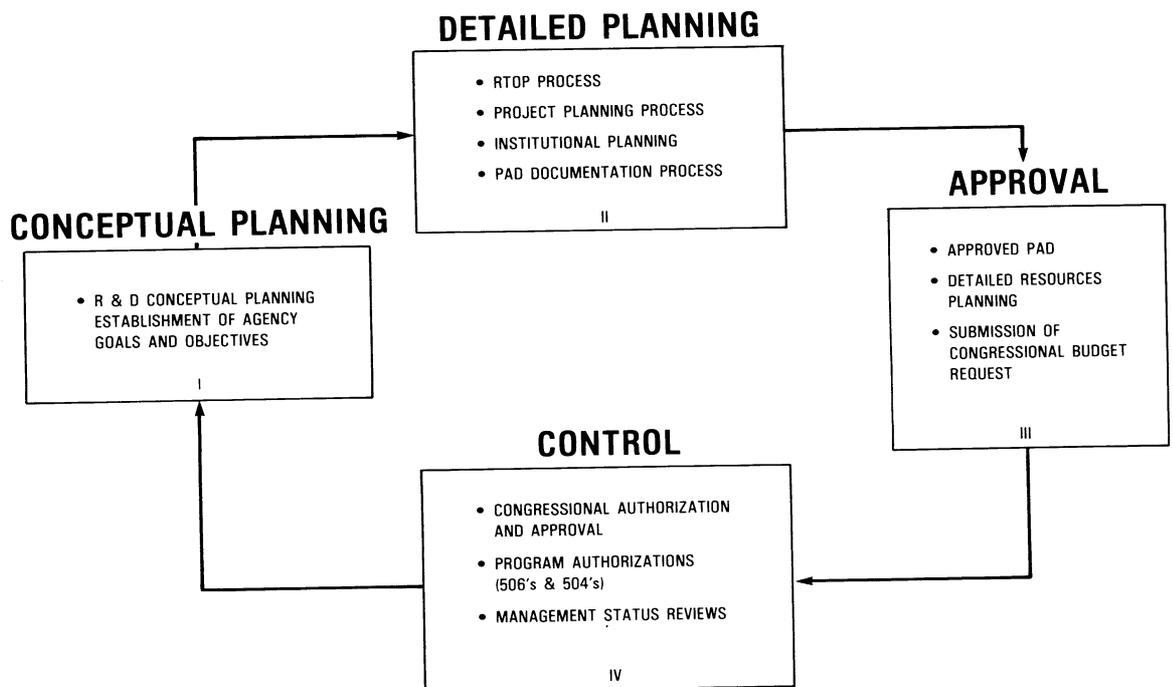
It is strange that the inadequacy of formal long-range plans is not better understood. Such plans are too often criticized for the wrong reasons. In a recent study of the National Bureau of Standards (NBS), the Congressional Research Service criticized that agency's Long Range Plan because: ". . . there is no systematic use of program implementation timetables . . . nor does the plan present any clear prioritization of programs in support of NBS goals . . . it is not clear how the Bureau's long-range planning effort is tied into the development of the Bureau's annual budget—a relationship which is essential to the effective use of long-range planning as a strategic management tool." (ref. 184.)

But the report has already made clear that the stimulus for the creation of a central planning office came from outside the Bureau — from the Office of Management and Budget. It would be absurd, though, to suppose that NBS did no planning before 1978, when OMB inserted six positions for central planning in NBS's budget. The excerpt cited above raises some further questions. How, precisely, should NBS have "prioritized" its programs? More than any other Federal laboratory, the Bureau of Standards provides services to industries and other agencies. This work for others is an exceedingly important part of its work, as we shall see in the next chapter. It is not clear how NBS could set priorities over the next five years, when changes in the economy — and presumably the Bureau's efforts to improve the nation's science and technology base — would cause the plan to be overtaken by events. The faults singled out by the Congressional Research Service may be real faults, or they may show a shrewd awareness by Bureau officials of the limitations of any formal plan. Conditions are always changing, but a long-range plan, once ratified by the sponsoring agency, can seem as though set in concrete.

All of this should not be taken to mean that long-range planning — or more accurately — long-range *thinking* is unimportant. Perhaps the most important contribution that the leadership of an institution must make is to provide the institution with a vision of the future. This vision must be easy to understand both by people inside and outside the institution, it must be intellectually challenging and exciting, and it must be credible so that the people in the institution actually believe that the vision can be turned into reality. The mechanisms for creating this vision are complex and diffuse — certainly no "long-range planning staff" has ever succeeded. It is in this process that the management of the institution must strictly apply, for want of a better term, the "organismic" approach. A great many people in the institution must be involved in many different ways to create this vision of the future. Only then will the

vision fulfill the reason for its existence, which is to make the institution something much more than simply the sum of its parts.

Short-Range Planning. Short-range planning will be defined here as the continuing process of developing programs, procedures, schedules, budgets, and forecasts. Ideally, such planning is performed at all levels within the organization. It entails intensive interaction of managers at all levels to agree on a rationale to guide the organization. The products of such planning normally cover specified periods — the schedule for a technology program, a fiscal-year budget, or a five-year forecast. The main sequences in this planning and control process, as developed with NASA, are shown in figure 46. This figure illustrates the most important steps in the process, but it omits several features of interest. Like those of most technology development agencies, NASA's programs are authorized annually. This means that every year the agency must seek authorizing legislation from both houses of Congress before it can request appropriations. More will be said about annual authorizations in the next chapter. Their significance here is that they add greatly to the centers' paperwork burden. With the full range of NASA programs under constant review, the centers must specify in even greater detail than would otherwise be the case as to how they will use their resources, the status of current programs, what new work they will need approved, and what changes in their need for manpower can be anticipated.



Source: *The Planning and Control of NASA Resources* (NASA Technical Memorandum 83090, 1981), p. 11.

FIGURE 46. — *The NASA planning and control process is shown on this chart.*

The budgetary process is as much the driving force in center planning as the programs to be funded. To describe the center budgetary process, two mechanisms need to be understood. The first pertains to the NASA budgetary structure. NASA funds its programs under three accounts: Research and Development (R&D), Research and Program Management (R&PM), and Construction of Facilities (CoF).

It should be noted that the CoF account only funds the construction of buildings and often not their collateral equipment, which sometimes actually costs more. As such, equipment is normally paid for out of R&D or R&PM, depending on whether the equipment is special-(R&D) or general-purpose (R&PM). The R&PM budget absorbs the cost of NASA employees — salaries, training, and benefits — while the costs of support service contracting are borne either by R&D or R&PM, depending on whether tasks support R&D programs or the agency's institutional base. Thus funds are disbursed from Headquarters either as direct program funding or institutional support. Note that institutional support funds consist of both R&D and R&PM monies. Such an approach is NASA's way of affirming the policy that direct support (computer services, graphics, machine shops) be charged directly to projects benefiting from that support.*

The major part of NASA's budget is contained in the Research and Development account. This is actually a serious misnomer because such things as the funds required to construct Space Shuttle orbiters as well as, say, technology development for advanced space communications systems, are carried in the R&D account. For all practical purposes, the R&D account carries the entire NASA program activity and amounted to

* At the contractor-operated laboratories operated for the Energy Department, the budgetary process is markedly simpler than NASA's. The primary budget parameter is funding, and there are no arbitrary limitations on staffing or grade levels. As with NASA, the budgetary process begins with informal discussions, at the project level, with a Headquarters counterpart. Budgeters must develop detailed costs for *all* aspects of their projects, including all technical/scientific and direct support personnel, facility operations, direct support services, and capital costs. Then, a "fair share" of laboratory general support and other overhead costs are included through a standard factor, currently about 60 percent of direct costs. Each project budget is analogous to the RTOP budget in NASA, except that the project budget at (say) Lawrence Livermore Laboratory accounts for *all* costs. The NASA RTOP budget includes all direct costs, but excludes civil service salary costs, and in no way accounts for a share of general support costs.

It should not be concluded that the comparatively straightforward budget process at Lawrence Livermore National Laboratory, for example, is "painless" for project managers. Technical people usually have a low threshold for pain due to administrative demands on their time, and justifiably so. Still, the process is simpler than NASA's, and the absence of anything comparable to NASA's system for handling institutional costs makes administering resources at Lawrence Livermore markedly easier, and probably less costly.

almost \$6.0 billion in Fiscal Year 1984. The Research and Program Management account is also misnamed — before 1970, it was actually called Administrative Operations (AO) which much more accurately describes what the money in the account is used for. In round numbers, the R&PM account contained a billion dollars in Fiscal Year 1984. The dominant items in the R&PM account are those related to personnel costs and these are, to first approximation, fixed. But in addition to the control imposed by the limit on salary money, NASA also has to live within a separate personnel ceiling imposed in terms of the average number of full-time equivalent (FTE) people that can be on the payroll in any given fiscal year. It happens on occasion that these two “controls” contradict each other. Thus, it is possible to have more FTEs than R&PM to pay the salaries and vice versa. The reason for the separate controls on salary money and FTE is not quite clear. It is probably due to the fact that the *number* of federal employees (FTEs) has a much higher political visibility compared to the dollars in the Federal budget devoted to the payment of civil service salaries.

In addition to these considerations, there are two small items carried in the R&PM account that are unusually important for managers of research institutions. One is the travel account. This is divided into two parts, program and non-program travel. Program travel is defined as travel necessary to accomplish a specific program, such as visiting a contractor, or going to the launch site when a spacecraft is being readied for launch. Non-program travel is intended to make it possible for members of the scientific and technical staffs of the several NASA institutions to attend technical meetings and so further their professional development. Managers of research and technology development institutions need to recognize the importance of non-program travel and to make sure that their best technical staff members have ample opportunity to travel for the purpose of professional development. Non-program travel should be recognized as an important investment in the “human resources” available to the agency, despite the possibility that some of the travel opportunities provided for technical staff will be abused.

The other small but important item in the R&PM account that deserves special management attention is the cost of utilities. Ever since the large change in energy costs ten years ago, utility costs have fluctuated in ways that have become less predictable. A sudden rise in electrical power costs may therefore have to be accommodated at the expense of other things — personnel costs, travel — in the R&PM account. In view of this situation, it has become necessary for management to become sophisticated about cost trends, so that negotiations with the utility suppliers can be carried out at the highest levels if necessary.

The Construction of Facilities is a very small fraction of NASA's overall budget, but it is unusually important. (In Fiscal Year 1984, the NASA budget had \$137 million in the CoF account.) The facilities available to a center have an extraordinary impact on what can be done and what technical objectives the center management can adopt. Since facilities normally take a long time to develop and construct, they form an important part of the 'vision of the future' that the management must provide. There is no doubt that the rank-and-file at a center view the facilities plans in just this manner. Accordingly, the development of an imaginative facilities plan is unusually important even if the amount of money spent on its implementation is relatively small. All of this is well understood by most NASA senior managers, and most of the NASA centers have quite elaborate long-range facility plans that are updated annually. These plans are not just "bricks and mortar" proposals. They are a vital part of the future of the center.

The second mechanism we mentioned pertains to the work of developing budgets at the centers. The most important of these budgetary instruments is the Research and Technology Operating Plan (RTOP), discussed in Chapter V. This, it will be recalled, is the agreement between a center and Headquarters on the nature and size of research tasks the center elects to sponsor. The RTOP is a microcosm of the entire budgetary process. A division within a center may start the ball rolling, but what ensues will be the outcome of prolonged negotiations between different divisions within the center, between a division and the director's office, and between the center, the relevant program offices (Space Science and Applications or Aeronautics and Space Technology), and the NASA Comptroller. If approved, the RTOP will describe the planned work, accomplishments to date, the schedule for the coming fiscal year, and manpower and funding requirements, which may be run out to five years if appropriate.

Two aspects of this process often lead to considerable complexity. One is the fragmentation of the budgetary process, owing to the split at Headquarters between the programmatic and the center operations sides, and to the existence of separate program offices, each with its own procedures and schedules. The second is the extreme difficulty experienced by center technical staff in breaking out their R&D budgets at the RTOP level into separate direct program and general support portions.

What has not so far been mentioned, but must always be kept in mind, is that the total NASA budget is generally being reviewed by OMB and Congress, even as the center prepares its operating budget. A further complication is that controls are placed on manpower, as well as funding, levels. These controls affect the actual numbers of government and

contract employees, the funds available to compensate civil-service workers, and the average grade level at the center.

What applies to the RTOP applies to full-scale projects as well. In both cases, there are standard forms whereby the center indicates what it plans to do, how it plans to do it, and what it will need in the way of resources. In the case of discrete projects, Headquarters issues a project approval document (PAD), which authorizes a new project; on the center side, the project manager drafts a detailed project plan. The actual authorization for work to proceed comes when Headquarters issues two forms (the "506s" and "504s" in figure 46) to the center. Briefly, these are the principal authorizing and resource allocation documents. Without them, the center cannot obligate funds.

It was necessary to give a brief account of NASA's planning and control process in order to make the role of center executives comprehensible. Consider, for example, centers of the size and diversity of Goddard or Langley. At any time, the directors and their staffs are accountable for several dozen research tasks, perhaps one or two flight projects, and a multitude of activities which support other centers as well as their own installations. A capable director can give a certain direction to all of this, but he cannot hope to control all — perhaps not even a majority — of the decisions that must be made by his managers. What he *can* do, for starters, is to ask the right questions at the right time: Do we need a new RTOP, or can we sponsor new research tasks within existing authorizations? Is it, perhaps, time to phase out research work that has already run 2 to 3 years? Regarding Project X, should we move from the conceptual phase to the drafting of a project plan? Do we need heavier support — in other words, more contractors — for our computation laboratory? How can we absorb personnel from Project Y, which is winding down? Should we establish a small working group to look into areas not covered by our existing directorates? Many such questions suggest themselves.

Recall our earlier discussion of the "organismic" system. We noted as one of its features the dispersal of knowledge throughout the organization. As Burns puts it, "Each individual has to do his job with knowledge of overall purpose and situation of the [center] as a whole. Interaction runs laterally as much as vertically, and communication between people of different rank tends to resemble 'lateral' consultation rather than 'vertical' command. Omniscience can no longer be imputed to the boss at the top." (ref. 185.)

This describes precisely the situation at any NASA center or, indeed, most government laboratories, save for small installations with missions of a narrowly technical kind. Whether it is a budget, a contract requiring the director's approval, an RTOP, or the allotment of discretionary funds to a directorate — a senior research executive can

seldom handle these matters unaided. What we have called short-term planning is not so much the cause, as the outcome, of the negotiations, meetings, and informal consultations involving a center's senior executives, their managers, functional staff, and Headquarters officers. According to this view, center executives are neither "top-down" managers nor constitutional monarchs at the mercy of their staff and advisors. Rather, they are *advocates* for the projects and programs thrashed out at all levels of the center. In effect, these interactions are a kind of analogue to the free market, since it is in this form that center executives answer the key questions pertaining to R&D budgeting:

- What should be the total amount allocated to R&D for the next planning period?
- How should the total amount available be allocated to various types of R&D?
- Which specific projects should be initiated, which continued, and what level of support should be given to each? (ref. 186.)

Structuring the Organization. Richard R. Nelson has made a most useful distinction between decisions *within* an organizational structure and decisions *about* that organizational structure, which he sees as the major decisions. Decisions about an organization occur when "a given organizational regime is limited in the range of contingencies it can handle effectively . . . when circumstances evolve outside of this range, the symptom is a growing restiveness (on part of one group or another) with the routine flow of events and decisions; and . . . successful resolution requires some kind of significant reorganization." (ref. 187.) It is in structuring the research organization that senior executives often make their most significant contribution.

Note that it is not a question here of rearranging the boxes in an organization chart. Reorganizations do matter, but less for what they accomplish than for what they signify to center employees. The advantage to a center director, especially a new one, in reorganizing is that in doing so, he can shake up the organization, and avoid the danger of becoming captive to existing institutional arrangements. But the real question a senior executive must face is, What kind of an organization do I want this laboratory to be? A laboratory director can put his stamp on the organization in many ways. In personnel selection, for example, he will try to find the right mix of skills, so that the center can perform the full range of work from basic research to flight projects. More than likely, he will try to control civil service job classifications to maintain a reasonable average grade level; control the total number of civil service positions to meet the OMB ceiling; use special hiring authorities for temporary special skill needs; enter into cooperative agreements with

local universities; establish special entry-level programs to attract new blood; and encourage direct and informal communication between gifted researchers and senior management outside the defined channel of the line organization (ref. 188). Taken by themselves, none of these measures is radical; taken as a whole, they can shape the organization for a generation.

To make our point as strongly as possible — that a laboratory's senior management has great freedom to shape the organization — we selected examples of possible changes from one area, personnel management. But there are many other ways in which laboratory managers can and do subtly change the nature of their installations. A new (or even modified) research or test facility can create new sponsors or a new research agenda. In the realm of what are, strictly speaking, organizational changes, an executive can transform the laboratory by, for example, merging several research efforts into one directorate, setting up small groups working in fundamental research and, particularly, moving the center into disciplines adjacent to its historic mission.

A short case study will illustrate how some laboratory directors move their organizations in new directions. In 1973, the NASA Deputy Administrator wrote to the directors of the older research centers, asking them to explain the objectives they planned to set for their institutions. In his reply, Edgar Cortright, director of the Langley Research Center, mentioned that several years earlier, center officials had decided to concentrate more resources on space applications: "We selected environmental quality as best suiting our skills and interests. Langley has always been involved in studying the atmosphere since it is the medium of flight . . . In addition, we have extensive experimental and analytical research under way on a wide variety of related subjects, i.e., circulation and dispersion modelling; currents of the continental shelf; pollutant chemistry; tunable lasers for pollution detection, etc." (ref. 189.) At the Ames Research Center, one of the authors (Mark) listed six "areas of emphasis" where, it was felt, the center should lead NASA. One of these, theoretical fluid mechanics, "is the basic science on which all of NASA's activities ultimately depend . . . Our objective at Ames is to hold the leading position in the world in this field." (ref. 190.)

The wisdom of these choices is not at issue here. We cite these choices because the underlying rationale is so clear; in one case (Langley) that the center already had the expertise needed to move into new, but adjacent, fields and in the other (Ames) that the center's expertise underlay every kind of development work funded by the sponsoring agency. We can go still further and state certain principles which, we believe, should guide executives in shaping their organizations. First, do not move too far out ahead of the rest of the organization. Build on what is already there. It is exceedingly rare for a laboratory to move abruptly

from one kind of research to one altogether different. While this was once possible — for example, the Jet Propulsion Laboratory moved from guided missile research to lunar and planetary exploration, when it was transferred from the Army to NASA — it has become rare to take such major steps. In the past, when money was easier to come by, the usual procedure when an agency moved in new directions was to create a new research installation. Today, the same objective must be accomplished by causing more gradual changes in the existing institutions.

Second, structuring the organization and making the restructuring stick are two different things. A really determined executive can create “facts,” but there is even greater challenge in training the future branch chiefs and managers who will inherit that structure a decade hence. As the director of the Lewis Research Center, responding to the Deputy Administrator’s 1973 survey, warned, “Because we all started here together, most of us will be leaving together.” (ref. 191.) Personnel development and considerations of organizational structure cannot be separated.

Finally, we would once more stress the importance, in any research organization, of leaving room for research not tied to specific missions. A major difference between government and industrial laboratories is that, in the latter, basic research is a staff function conducted at one (or at most a few) location(s) instead of being dispersed throughout the organization (ref. 192). Even in companies as large as General Electric and DuPont, there is one central research organization which serves several functions: as the long-range research arm of the corporation, as a centralized facility for specialized services, as a technical consulting group to corporate management, and to do research for company divisions on a contract basis (ref. 193). In an agency like NASA, on the other hand, most of the larger centers — the Kennedy Space Center is an exception — have their own long-range basic research groups. It falls to the center director to justify these groups, to guard against duplicating work other centers are doing, and to ensure that some communication outside formal channels is always possible.

Reviewing, Measuring, and Evaluating Results. This is the process by which center executives assure themselves and those to whom they report that work under their direction is being adequately carried out. A single word for this function is “control.” Examples of control are the day-to-day monitoring of individual tasks by supervisors, periodic progress reviews, meetings, presentations to Headquarters, testing of hardware, and the like. In a somewhat different sense, the accounting function is the tool by which the budget is controlled. Moreover, limitations on the workforce in the form of costs, number of employees, and average grade level all lead to elaborate control systems.

Although this function is no less important than the first three, we shall not say much about it here. First, evaluating results depends on the goals set, the game plan under which the laboratory functions, and the structure drafted for it by its senior management. It is not something independent that can easily be treated from a theoretical viewpoint. Planning, operations, and evaluation merge into one another. The same budget which maps the center's strategy for the coming fiscal year is also a commentary on those activities which executives think should be terminated, sustained at current levels, or augmented.

But there is a more important reason why the evaluation of results cannot be treated here. No Federal laboratory is completely independent in choosing what it does. If its work is of national importance — and if not, what is its reason for being? — it will be under pressure from its sponsoring agency, from Congress, from the scientific community, from industry, and from any number of other outside players. It is useful to treat the laboratory as a closed system, but it is not realistic. In the next chapter, we set matters right by analyzing the relations between laboratories and their sponsoring agencies. And we shall look especially closely at a single question: How do laboratories interpret their mission to apply their research to “national” needs? In accordance with our practice throughout this book, we shall approach an answer by way of two case studies, one of the multiprogram laboratories operated for the Department of Energy, and the other of the National Bureau of Standards. We will then be in a better position, through these examples, to show how the work done in a laboratory is actually evaluated in practice.

CHAPTER X

The Laboratory and Its Sponsors

"Can two walk together, except they be agreed?"

—*Amos, III, v. 3*

The Role of the Sponsoring Agency

There is a delusive simplicity to studying Federal laboratories as though they were closed systems. What we propose is to get behind appearances and review some of the many ways in which laboratories interact with their external environments. As a general rule, research and technology development laboratories tend to be more stable than the agencies that justify and provide the funding for the laboratories. While it is expensive to set up a research institution, the reorganization of an agency in Washington does not automatically require the construction of large new facilities. A good laboratory is a more or less permanent institution and it may shift between agencies as the political climate dictates. Examples abound: the transfer of the National Bureau of Standards from the Department of the Treasury (1901) to the Department of Commerce (1903-1913); the transfer of segments of the Bureau to the National Oceanic and Atmospheric Administration; the organization of the multiprogram energy laboratories, first under the Atomic Energy Commission (1946), followed by the Energy Research and Development Administration (1975) and the Department of Energy (1977); and the selection of the research centers of the National Advisory Committee for Aeronautics to be the core of NASA when it was created in 1958. There is nothing immutable about the ties of a laboratory to its sponsoring agency. The agency may be target as well as shield.

It often happens that the reason for being of a technology development agency becomes politically irrelevant, once the objectives for which the agency was created been achieved. Since 1967, NASA has had to face this problem, shutting down its Electronics Research Center and transferring it to the Department of Transportation, as well as cutting back support to other centers. Similarly, the Army transferred part of the Fort Detrick Chemical Warfare facilities to the National Cancer Institute when the Nixon administration terminated all biological warfare research in 1969. In both cases, there was a sponsor able and willing to take on a

new facility. In other cases, a laboratory, while retaining a primary loyalty to one agency, tries to pick up additional support by diversifying or by arranging to do work for other Federal agencies. We will call this the "resource sharing" model. Mention was made in Chapter IV of the location of the Army Air Mobility Development Laboratory at NASA's Ames Research Center. Another example of resource sharing is the location of the Army Corps of Engineers Nuclear Cratering Group at the Lawrence Livermore National Laboratory, which is operated under contract to the Energy Department. In each case the agency operating the laboratory is funded by another that wants work done there. Almost every large laboratory operated by a given agency does some reimbursable work for other agencies.

More will be said about resource sharing in the case studies included in this chapter, but certain general rules (which account for successful resource sharing) can be mentioned here. The first is that "user" groups should be small compared to the "host" laboratory — probably no larger than 20 percent of the host. Second, the host laboratory must be extremely careful not to interfere with the programmatic function of the user group. Resource sharing will not work if the program of the user group is so skewed that it only benefits the host laboratory and its parent agency. Finally, the user group should operate under roughly the same personnel, procurement, and fiscal regulations, if the host laboratory is to execute these functions properly. Unless some administrative uniformity exists, it is possible for purely administrative problems to wreck a relationship that otherwise makes sense on technical grounds.

Perhaps enough has been said to show why easy generalizations about the proper roles of government laboratories are implausible at best, misleading at worst. Government laboratories exist in every phase of dependence or freedom. A laboratory may work exclusively for one sponsor or for several; perform reimbursable or non-reimbursable work for other agencies; be a joint venture of an agency and one or several universities and be operated as a distinct organizational entity;* do fundamental research or perform work very closely tied to an agency mission; or even carry on work which is not closely tied to any agency mission.** No generalizations are adequate to encompass such varied institutional possibilities, and so we have elected to explain matters indirectly, by detailed case studies of two very different kinds of laboratories: the National Bureau of Standards in Gaithersburg, Maryland

* An example of this is the Joint Institute for Laboratory Astrophysics operated by the National Bureau of Standards and the University of Colorado.

** A possible example might be the National Center for Atmospheric Research in Boulder, Colorado, which is funded by the National Science Foundation and operated by a consortium of universities.

and the nine multiprogram laboratories operated under contract to the Energy Department. Once the operations of these laboratories have been reviewed, it will become easier to understand the complexity of the transactions between a laboratory and its clientele, or the reasons for the difficulties encountered by Energy laboratories in moving into work in nonnuclear energy research, development, and demonstrations. A laboratory may be deemed "national" because it is supported out of public funds and supplies certain public goods in, for example, national security, space exploration, or the maintenance of a national measurement system. But such an assertion tells us almost nothing about the way these laboratories operate or why they choose certain courses of action over others apparently as rational. Only selected case studies can bring out the reality behind a laboratory's organic legislation.

But even the case studies set out later in this chapter need something by way of a preface to make them intelligible. We need to explain two of the most important features that make technology development laboratories what they are. First, we will set forth in general terms the functions performed by a parent agency for its laboratories. Then, we shall briefly examine those conditions which militate for or against the independence of laboratories *vis-a-vis* their sponsors.

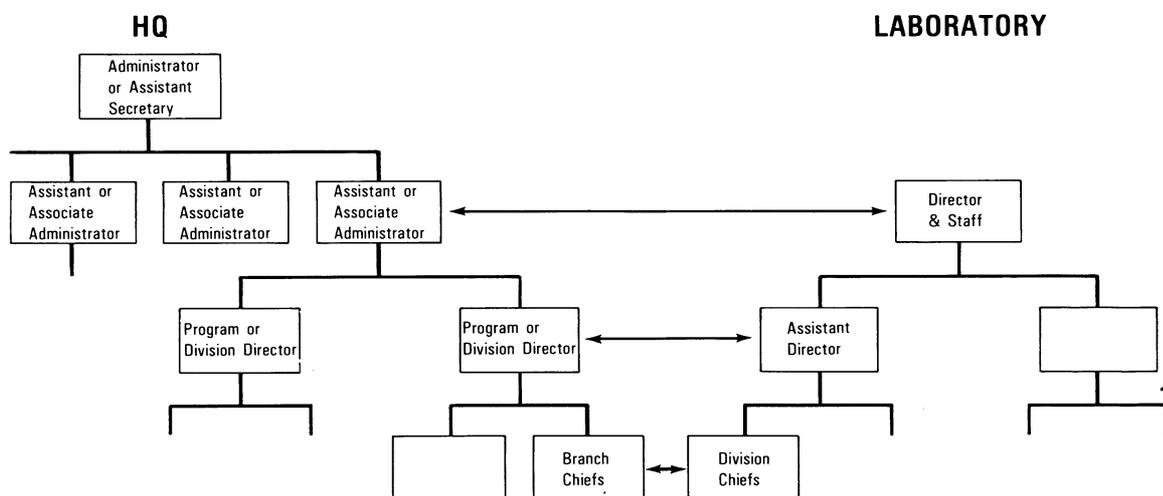
Justification of the Technology Development Function. The most important function of the parent headquarters organization is to justify the research and technology development functions carried out by the laboratories and to see to it that the necessary funds are appropriated. Relationships between laboratory and sponsor are complicated and decentralized; only in this way can all the talents, both at the laboratory and headquarters, be brought to bear productively on the problem of how to set the right amount of funding. Figure 47 illustrates these relationships schematically.

The top laboratory managers establish their primary relationship with the second tier of managers at headquarters, and so on down the line. The laboratory director will normally deal with the program associate administrators (or assistant secretaries), the department heads of the laboratory with the various headquarters division heads, and the laboratory division chiefs with the group leaders in the headquarters organization. There is, of course, considerable cross-talk between the various tiers, and independent negotiation regarding programs and funding. These negotiations are keyed to the Federal budgetary cycle and the three main groups involved: the agency itself, the Office of Management and Budget, and the congressional authorizing and appropriations committees. The Federal budgeting cycle is illustrated in figure 48.

The agency formulates a budget which is presented to the President through the OMB sometime in the fall of the calendar year, usually in

September. (Preliminary budgets may be submitted to OMB earlier in the year as a basis for subsequent negotiation.) OMB then reviews the agency's budget, compares it with those submitted by other agencies, and responds by giving a "mark" to the agency in October or November. Final negotiations then begin, and by the following January a final budget for the executive branch is developed. This budget is part of the President's annual message, and by law must be submitted to Congress before January 20.

Once the budget is presented to Congress, it is referred to the appropriate authorizing committee. The distinction between authorization and appropriation is subtle, but it is absolutely crucial. An agency whose appropriations have run out is normally kept alive through a continuing resolution, which permits the agency to continue spending at the funding level of the prior fiscal year until a new appropriations bill is passed. But an agency whose authorization expires ceases to exist. What has made this process especially important has been congressional insistence on periodic, or even annual, authorization. Prior to 1959, most agencies — the Army Corps of Engineers was a notable exception — were permanently authorized by their organic legislation. In that year Congress added a rider to the NASA appropriation bill, requiring that agency to seek authorization before it could request appropriations. The annual authorization requirement was subsequently extended to all new military research and development programs and to the Atomic Energy Commission.



Relationships are generally those indicated by the horizontal arrows.

FIGURE 47. — This chart shows the formal relationships and lines of communication between the headquarters and a large research institution. The most important point to recognize is that these communications take place at many different levels.

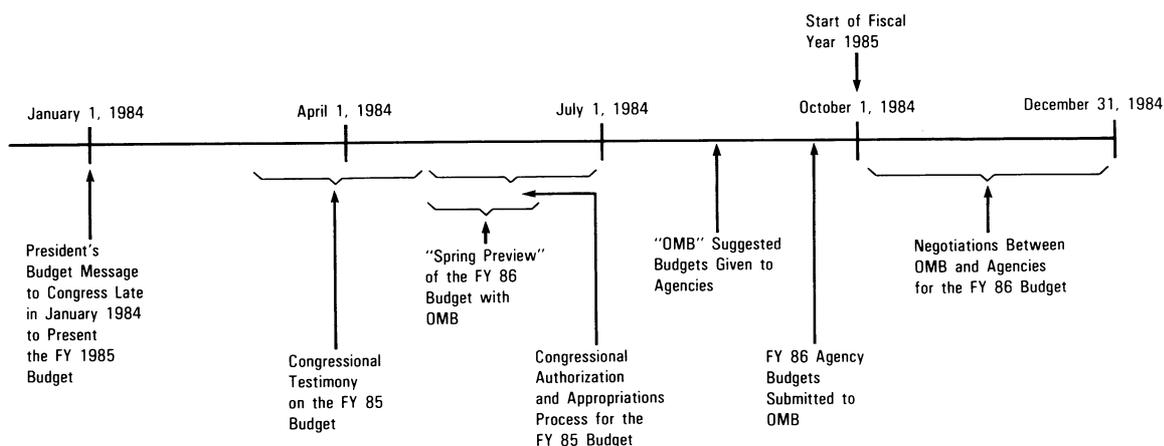


FIGURE 48. — *The Federal Budgetary Cycle (Fiscal Year 1984-85).*

The authorizing committees shape the agencies for which they are responsible in three closely related ways. The bills reported out of committee set the ceiling for appropriations; the reports accompanying the authorizing legislation are recognized as limitations and preconditions on how the funds made available can be spent; and committees prescribe the conditions under which “their” agencies can reprogram or transfer between accounts, as well as the percentages and sums involved (ref. 194). Over and above these functions, the authorization process has an impact which is hard to measure: The authorization committee will be the advocate in Congress for its agencies. Where most appropriations subcommittees deal with a hodgepodge of unrelated agencies, authorization committees usually oversee, at most, two or three. Thus, the House Science and Technology Committee authorizes the budgets of NASA, the National Science Foundation, and the National Bureau of Standards. On balance, regular authorizations probably work to an agency’s advantage. The process is undoubtedly cumbersome, with distinct sets of hearings in each house on essentially the same programs. But it does give an agency greater visibility in Congress, although with the implied threat of Congress trying to control its programs too closely.

Once the authorization bill is passed, Congress must actually appropriate funds for the work authorized. Because appropriations subcommittees deal with groups of unrelated agencies, it sometimes happens that items in one agency’s budget are traded off against items in another. What we have here is not randomness but, to quote a phrase, a failure to communicate. There is, for example, the case of NASA’s Pioneer Venus program as it made its way through the 1974 appropriations hearings. Pioneer Venus was a planetary exploration program for which the House Science and Astronautics Committee — the predecessor to the Science and Technology Committee — had

authorized \$50 million in the summer of 1974. When the bill reached the House Appropriations Committee, the item was deleted with the proposal that Pioneer Venus be delayed one year so that a program in another, unrelated agency could be funded. For reasons of celestial mechanics, it was impossible to delay the planetary program. Failure to fund would have meant a delay not of one, but of four, years and would have effectively crippled the program. Once this was properly explained to members of the Appropriations Committee, the funds were restored; but this example illustrates how tradeoffs in the appropriations process are sometimes made.

Negotiations with Congress and OMB are the responsibilities primarily of the program associate administrators (or assistant secretaries) in the technical agencies. They will generally be the ones who conduct final negotiations with OMB examiners and senior officials, and who deliver the agency's testimony before its authorizing and appropriations committees. The most senior agency executives (the Secretary or the Administrator) will also testify before Congress, but their main function tends to be, at least during negotiations, to deal with the staff in the President's Office who formulate the President's Budget. At the end of each calendar year there are usually some points still at issue between the agency and OMB, and it is at this time that agency heads tend to meet with the President and his immediate assistants to agree on the final form of the budget.

Formulation of the Program. The second important function of the Headquarters organization is to formulate the agency's program in general terms, and then to assign it, once it has been funded, to the appropriate research installations. In assigning roles and missions, Headquarters is usually guided by certain principles:

First, a given role and mission should be consistent with the facilities at that laboratory. Facilities are usually expensive and require long lead times to bring "on line." Prudent management dictates that, normally and in periods of level or declining budgets, the mission of a particular laboratory should be largely determined by the facilities it can bring to bear on research problems. We have chosen our words carefully. Qualified people are, of course, extremely important but, in general, people can be hired more quickly than facilities can be built.

Second, the work of a mission-oriented laboratory should result in some final product. The assignment of a mission should not be open-ended; achieving a goal must take precedence over exploring a particular discipline. (There are, as always, exceptions; an institution like the National Accelerator Laboratory in Illinois has the mission of developing the discipline of high-energy physics.) Missions tend to be associated with projects, such as Apollo, Viking, or the fast breeder reactor; with finite military programs, as in many Defense laboratories; or

with the determination of the properties of materials important for industry, as with much of the work of the National Bureau of Standards. The work done at the Bureau raises many of the kinds of questions that research directors and their superiors at Headquarters must consider: How are the areas of work to be chosen and priorities to be set? How will it be determined whether the work of the Bureau is relevant to concerns of industry? How active or passive should the Bureau be in its relations with industry? How do relations with industry intersect with those of consumers, regulatory agencies, and, in the case of computers, with the preparation of mandatory Federal standards for automatic data processing (ref. 195)? It is well to emphasize the *generic* nature of these questions since an agency like the Bureau, preoccupied as it is with matters of test and measurement, might seem to be the farthest removed from an agency like NASA. Yet the same principle holds for both: By orienting missions around projects with finite lifetimes, an agency can assure that its research is dynamic.

Evaluation of Results. There is a final function which a headquarters organization has, and that is to evaluate the quality of the product of a technology development institution. Despite all that can be said about evaluation, this remains the most difficult and perhaps the most hazardous headquarters function. It must be done, yet no good rules that apply across the board exist. The simplest way to make this evaluation is to ensure that a laboratory's output — whether this output is a new research concept, new hardware, or new standards for data processing equipment — is closely coupled to the ultimate user. One example might be a reactor design used in ship nuclear propulsion; a second might be a standard reference material, like carbon steel or rice powder, used to calibrate instruments; a third might be an electrophoretic system to separate and map human serum proteins; while a fourth might be an aircraft engine design that improved fuel efficiency.

The coupling will be less direct where the work done is either more exploratory, or is devoted to programs like space exploration which often have no first-order practical consequences. Here the question is not so much, Did this bird fly? as, Should it have flown in the first place? (Recall our discussion in Chapter VI of the Orbiting Astronomical Observatory.) Where the work requires technology that scarcely exists when a program is sponsored, or when any payoff is far in the future, the distance between output and the end user becomes still greater. In this category are programs like the supersonic transport and the breeder reactor. Here, the technical problems are (or were) so complex and the technologies so unproven that the private suppliers whose cooperation was essential — the airframe manufacturers in one case, the public utilities and reactor manufacturers in the other — refused to participate until the government agreed (as eventually it did) to put up all or most of the development money.

In general, evaluation works well primarily in the first case, where the output of the laboratory (the supplier) is closely coupled to the industry or the agency that uses it. In the other two cases, evaluation is much more problematic because it involves not only technical but also political, regulatory, and environmental considerations that technology development organizations are ill-equipped to review. Evaluation and the work being evaluated tend to succeed where: (1) the government has a direct procurement interest in a technology and is the actual buyer and (2) where precedent exists for the work being sponsored (ref. 196). Even in these, the best cases, evaluation will be difficult because it must be made before the practical impact of the technology being developed can be properly judged.

Autonomy of the Federal Technology Development Laboratory

The other question raised earlier was this: What conditions militate for or against the independence of laboratories *vis-a-vis* their principal sponsors? "Autonomy" is probably a more precise word: autonomy to set research priorities, to structure the laboratory's budget, to manage its programs without excessive headquarters interference. There are obvious limits to what even the most free-wheeling headquarters organization can tolerate, but within these limits there is considerable variation in the autonomy of Federal laboratories. In relation to headquarters, a laboratory or laboratories is likely to have a relatively high degree of autonomy: (1) where the resources devoted to research and technology development are large in relation to the agency's budget, (2) where the laboratories manage one or a few large high-priority programs, rather than many small research tasks, (3) where these same programs require frequent congressional authorization, and (4) where the units of a multilaboratory agency are grouped in related fashion. This last condition is especially important, because it permits the formation of coalitions of laboratories against what field personnel see as the efforts of headquarters to engage in "micromanagement."

In view of these criteria, NASA emerges as an agency whose laboratories enjoy relatively high autonomy, the National Bureau of Standards as one with limited autonomy, with the laboratories of the Energy and Defense Departments falling somewhere in between. Consider the NASA centers during the Apollo program. Throughout the 1960s, the manned spaceflight programs accounted for about 70 percent of NASA's budget. Given Apollo's status as a program of the highest priority, the three centers under the Office of Manned Space Flight (OMSF) — the Manned Spacecraft (MSC), Marshall, and Kennedy Centers — enjoyed considerable freedom to run their programs in their own ways. Between 1962 and 1967, NASA was a more decentralized

agency than before or since. OMSF and its centers devised tactics enabling them to become for a time the preponderant organizational element in NASA. First, OMSF and its head, Associate Administrator Dr. George Mueller, built a powerful coalition, almost a "manned spaceflight family." (ref. 197). Through the Apollo Executives Group, it dealt directly with all of its prime contractors; through the Manned Space Flight Subcommittee of the House Science and Astronautics Committee, it had close ties with its authorizing committee; and in the OMSF Management Council, it had a forum for resolving its disputes internally. And despite strains, particularly the philosophical differences between Marshall Space Flight Center and the Manned Spacecraft Center (now the Johnson Space Center) on the proper way to manage Apollo, the coalition held.

Second, there was the possibility of coalitions between OMSF and the older research centers that got their start under the National Advisory Committee for Aeronautics. By their nature these coalitions — alliances, really — were weaker and more temporary, but they were no less real for that. The older centers were doing much of the supporting research and technology work for Apollo; many of their technical people were eager to get into large-scale development work; most important, the core of the Manned Spacecraft Center, when it was constituted late in 1961, consisted of senior managers like MSC Director Dr. Robert Gilruth and his distinguished colleague Dr. Max Faget, who had spent their careers at the Langley Research Center. The possibility of cross-communication between OMSF and the older centers was not foreclosed. Third, Mueller and his OMSF staff were careful not to challenge NASA's senior management, the Administrator and the Deputy Administrator, directly. The autonomy of OMSF was brought into play, not against NASA senior management, but against other organizational rivals, notably the Office of Space Science and Applications (OSSA). OSSA and OMSF were far apart on the kinds of experiments to be flown on Apollo missions, and some OSSA scientists and advisors believed that the size of the manned programs imperiled NASA's very ability to do space science at all. It was precisely in these circumstances that OMSF could make the other program offices feel the size of its foot. OSSA was outflanked, and space science on Apollo was done very much on Mueller's terms.

Two things combined to diminish OMSF's autonomy: the leveling off of the manned space program budget as early as 1966, and the January 1967 fire that killed three astronauts on the launch pad at Cape Canaveral. The decline in OMSF's budget could not, by itself, have diminished OMSF's autonomy very much; relative to the other program offices, it remained the largest entity within NASA. But the Apollo fire was enough to convince NASA officials, especially Administrator James Webb, that OMSF had been kept on far too long a leash and that management of the

program had been faulty in certain areas. For more than a year prior to the fire, both the Apollo program manager and NASA senior officials had known that Apollo was running into serious trouble and that the prime spacecraft contractor, North American Aviation, seemed to lack the technical and managerial skills needed to keep the program on schedule. Following the fire, Webb brought Apollo — and through it, OMSF — under much stricter control: shaking up OMSF management at Headquarters and in the field, creating a special office to monitor NASA's contracts with North American Aviation, and appointing an Associate Administrator for Organization and Management to bring the entire field organization under central control. Thus OMSF tended to become more like a normal program office and, less like a project organization embedded in a Federal agency.

By comparison with NASA, the National Bureau of Standards has had much less autonomy relative to its parent agency, the Department of Commerce. The principal reasons for this are that the Bureau is small in relation to the Commerce Department, that Commerce has many responsibilities other than research, and that even within that restricted area there are rival research and development organizations within the Department of Commerce, such as the National Oceanic and Atmospheric Administration. To complicate matters, much of the Bureau's work *could* be done elsewhere: by firms large enough to have their own test facilities, by trade associations, and by private organizations involved in drafting voluntary standards, like the American National Standards Institute. Because some of the Bureau's research appeared to duplicate work being done elsewhere, OMB put pressure on the Bureau to justify what it was doing. In 1974, OMB went further and enunciated a "lead agency" policy, requiring that an agency, in drafting its program, "should be the principal or primary source of support for carrying out that mission . . . Ideally, this . . . should help avoid duplication of effort (and) . . . result in better knowledge of and the control over the cost of carrying out specific mission." (ref. 198). But in the eyes of Bureau officials, this policy did not always seem to be applied in the same way, and it was often applied after the fact (ref. 199). By 1980, there was a consensus that the Bureau should be considered the lead agency in measurement sciences, but even this consensus left important questions unanswered, as the next section shows (ref. 200).

In sum, the Bureau's mission makes it very difficult to build the constituencies available at certain times to NASA and the Department of Energy. The work of the Bureau is exceedingly important; one thinks, for instance, of the ASCII code set by the Bureau, which enables several computers by the same or different manufacturers to talk to each other. By its nature this work is unglamorous and highly technical. Moreover, it is not work that lends itself to the project approach. It is a matter, instead,

of research tasks, most of which are performed by Bureau employees; the Bureau has never been a large contracting agency, again in marked contrast to NASA and the Department of Energy. The truth is that, aside from its specialized user communities, the Bureau has rarely attracted much outside attention, and that such attention as it has received has often been of a rather unwelcome kind. An example of such attention was the notorious battery additive affair of 1953, when the Secretary of Commerce demanded the resignation of Dr. Allen V. Astin, the Bureau's Director, after the Bureau tested a battery additive and found it worthless. The subsequent outcry by the trade associations and the press and congressional hearings saved Astin's job; also, an investigation of the affair by the National Academy of Sciences concluded that the behavior of Astin and the Bureau's scientists had been entirely proper (ref. 201).

Then, in the 1970s, Congress turned its attention to the Bureau and began imposing direct responsibilities on it in, for example fire prevention research, energy conservation, packaging and labeling, and evaluating energy related inventions — all areas outside the Bureau's competence, strictly defined. Finally, in 1978, Congress authorized appropriations to the Bureau for 1979 and 1980 only. This was a change since, from its establishment in 1901, the Bureau had been permanently authorized. This move toward regular authorization may work to give the Bureau more powerful political sponsorship; as mentioned earlier, a congressional authorizing committee tends to become an advocate for its agencies. But the fact that congressional interest in the Bureau was so sporadic for so long may serve to explain why the Bureau has found it difficult to develop programs different from what the Commerce Department, OMB, and Congress deem necessary.

It might appear that the large multiprogram laboratories operated under contract to the Atomic Energy Commission (and later the Energy Department) would face relatively little interference from Headquarters. Because the AEC elected to contract out almost all of its technology development work, it might have come to pass that all the scientific and engineering expertise would reside in the laboratories; that Headquarters would be in no position to challenge the laboratories' assessment of their missions; and that they would have autonomy to move into areas of their own choosing. But the actual relations of the laboratories to the AEC were far more complex than this. In one area, weapons development, the Lawrence Livermore and Los Alamos Laboratories did enjoy something like autonomy, owing to the unique triangular relationship of the laboratories, the AEC, and the Defense Department. Although these two laboratories were operated under contract to the AEC, their principal client was the Defense Department. In a sense, the laboratories fell between the cracks; the customer and funding agencies were distinct.

In other areas, above all in reactor design, officials at AEC

Headquarters were not shy about imposing their preferences on the laboratories. The decision by the AEC to sponsor the development of a liquid-metal, fast breeder reactor (LMFBR) is a case in point. Although in 1961, the AEC had authorized Oak Ridge National Laboratory to begin work on a molten salt reactor, the Commission was under pressure to come up with an even more advanced technology. The Joint Committee on Atomic Energy wanted to move from commercial light water systems to reactors that would "breed" new fuel; while by the early 1960s, the Atomic Energy Commissioners were moving from "assisting studies of the technology . . . to paying a share of the capital costs and subsidizing reactor design costs . . . to the building and monitoring of the full panoply of equipment needed for a commercial LMFBR." (ref. 202.) The molten salt reactor was a breeder technology, but in the judgment of AEC technical people it was not the technology of choice. The crucial event in the AEC's decision to sponsor the LMFBR was the appointment of Milton Shaw in 1964 to head the AEC's Division of Reactor Development and Technology. What happened next is described by Irvin Bupp: "During 1965, Shaw successfully reoriented the AEC program. His liquid-metal breeder reactor program would attempt to do for this new technology what Admiral Rickover's naval propulsion program had done for light water systems: provide a solid technical base for a prototype construction project. For the first time, the AEC's power reactor research-and-development program would meet the demands articulated for years by the Democratic majority on the Joint Committee on Atomic Energy." (ref. 203.) By 1967, Shaw had won over the Joint Committee and many senior AEC officials. Work on the molten salt reactor continued, but at a very low level. It would have been terminated in 1973 but for the Arab oil embargo, additional funding for energy related work, and perhaps a disposition by energy officials to hedge their bets (ref. 204).

The history of the LMFBR is a clear case of the AEC imposing its policies on one of its contractor-operated laboratories. But this leaves unanswered the question of how the AEC managed to do this. The intuitively obvious answer is that the AEC, as the funding agency, could do as it pleased, once it was certain of Joint Committee backing. In the absence of AEC support, the laboratories had nowhere to turn; and so long as the AEC favored one program (LMFBR) over another (molten salt core reactor), some laboratories stood to gain from the new dispensation and would support it. But we can go further in explaining why the laboratories were unable to turn aside AEC policies with which they disagreed. The AEC laboratories are not comparable to (say) the NASA centers; where the latter formed powerful coalitions within their agency in promoting the Space Shuttle program, the former proved unable to do so in the case of the breeder reactor. These laboratories have

widely differing missions and management styles, and they are even more geographically dispersed than the NASA centers are. They have been subjected to a variety of controls, especially since the stage-by-stage transformation of the AEC into the Department of Energy. Unlike NASA, the AEC interposed civil service field organization offices between Headquarters and its laboratories, enabling it to supervise the laboratories more closely than NASA would have deemed necessary or desirable.* As will be shown later, the transition from the AEC to the Energy Department has burdened the laboratories with some of the less desirable features of the old regime. But the point to grasp is that the multiprogram laboratories have never had a monopoly on technical expertise, and that self-confident AEC managers, backed by a powerful congressional committee, could always make policy for the laboratories — if need be — from the top down.

We need add only a few words about laboratories under the Department of Defense. Strictly speaking, there are no Defense laboratories; all such laboratories (except the U.S. Naval Research Laboratory in Washington) are operated by one of the services and are envisaged as mission-oriented agents for the respective service research and development commands. At its best, the system has made an organization like the Office of Naval Research possible — an organization which has been permitted to sponsor research in many areas not directly related to its agency's mission but with the potential for long-term payoffs. Examples of such work range from elementary-particle physics to the discovery of the Van Allen belts to supporting the earliest work on time-shared computer systems to the invention of a method for the rapid freezing of blood (ref. 205). At the other end are the many research installations working in more narrowly-defined areas; because these laboratories are attached to one of the services, there is the likelihood that some research problems will be "solved" three times over.**

* NASA does have an office in Pasadena to monitor its contract with the Jet Propulsion Laboratory. The important point, surely, is that NASA has chosen to operate its centers with government employees and not to turn the centers over to contract management — so necessitating an elaborate field organization.

** In fairness, it should be noted that this is not necessarily bad, especially in defense, where the penalties for falling behind are severe. In basic research, intellectual competition is inherently valuable, and it is not imperative that the competition be tightly structured to allow close comparisons. Problems arise in the early stages of development. There are legitimate reasons for each service to have its own weapon system; it is also true that the cost advantages of joint acquisition by the services have been overstated. No service will willingly permit another service to dictate its requirements; what is necessary for one service may be superfluous to another; service doctrines which tend to dictate military requirements are not lightly surrendered; finally, there are objective technical

There are two things about the DOD style of laboratory management that are noteworthy: reliance on outside laboratories for sophisticated exploratory work, and the existence of a special organization, the Defense Advanced Research Projects Agency (DARPA), to serve as a kind of "venture capitalist" for DOD. Relatively little of the most advanced exploratory work is being done in house. Much of the work sponsored by the Office of Naval Research is supported by contracts to industry and grants to universities. DARPA has carried this approach to its limit. It has *no* research installations of its own; rather, DARPA program officers define a research agenda, arrange for one of the services to be its procurement manager, and work with contractors to bring a particular concept — say, a tank autoloader or a robot with three-dimensional vision — to demonstration. Many of DARPA's most promising ideas are culled from unsolicited proposals. This approach has led to major advances in supercomputer technology, computer-to-computer communications (for example, the nationwide packet-switching network known as ARPANET), electronic warfare, materials science, and lasers (ref. 206). Indeed, DOD's tendency to go outside the walls is of long standing; one thinks of the establishment of RAND and the Aerospace Corporation as contract research centers for the Air Force, and the reluctance of the Navy's Special Projects Office to use naval laboratories in developing the Fleet Ballistic Missile. Since the early 1970s, DOD in general and the Air Force in particular have moved to reduce the proportion of basic and exploratory research carried out by government employees. The autonomy of DOD laboratories has thus been further reduced.*

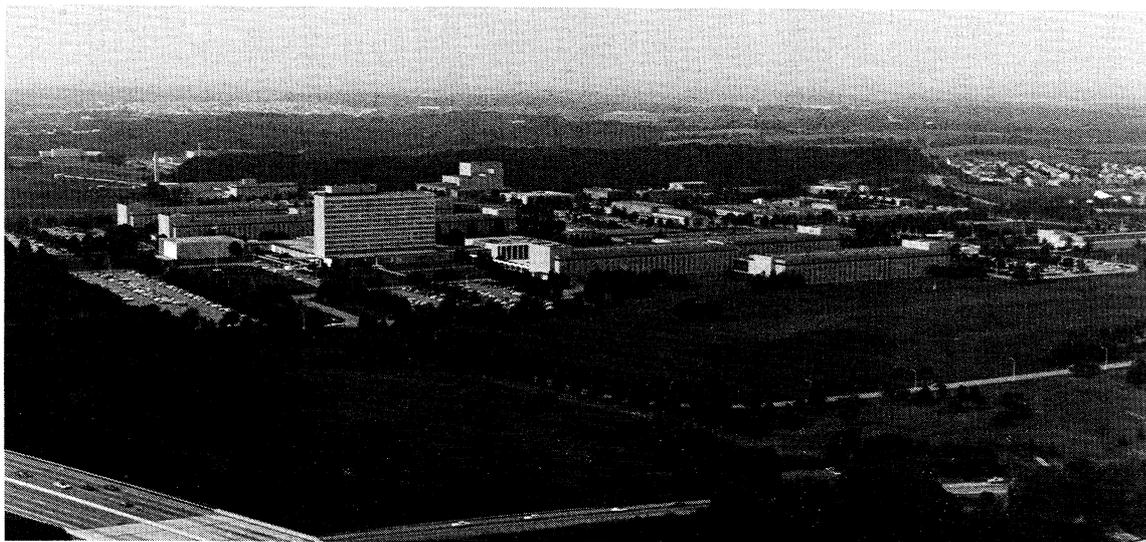
differences in service needs. This is why joint programs, beginning with the TFX plane of the early 1960s, have proved so difficult to manage. As the General Accounting Office noted of an attempt to merge three laser-guided missile seekers into one: "... the Navy Bullpup was ready for production at merger time, the Air Force laser Maverick was beginning advanced development, and the Army Hellfire was still in the conceptual stage. All the services eventually dropped out of the joint seeker program." GAO, *Joint Major System Acquisition by the Military Services: An Elusive Strategy* (NSIAD-84-22, December 23, 1983), pp. 14-15.

* According to the National Science Foundation, between 1976 and 1983 DOD funding for intramural basic research increased at an annual rate of 9.5 percent, compared with 3.2 percent from 1973 to 1976. However, the laboratories' share of basic research at 5 percent remained unchanged. It seems to us that increased funding for intramural basic research — including that carried out at government-owned, contractor-operated facilities, which are not counted by NSF as intramural — is very desirable, even if the laboratories' share of such research remains the same. The defense industry is unique in many ways: There are formidable barriers to the entry of new firms, especially the small ones which are so fruitful a source of innovation; only the few large firms already established as defense contractors are capable of managing sophisticated programs; and the shifting of funds to routine development work means that less money is available for

In the end, the ability of a laboratory to deal on an equal footing with its principal sponsor is affected by many things: the reasons that led to the laboratory's creation (which may not be the same as the reasons for keeping it in being), the existence or absence of competing organizations able to do the laboratory's work, the size of laboratory programs relative to its agency's mission, and much more. In the next two sections, we see how two quite dissimilar kinds of research installations have interpreted their missions in the light of changing environments.

Strengthening Productivity: The Role of the National Bureau of Standards

Founded in 1901, the National Bureau of Standards (NBS) (fig. 49) is charged to maintain and develop a national measurement system (ref. 207). Although it has been given many subordinate responsibilities, the Bureau's primary mission remains its reason for existence today. This national measurement system is not easy to describe because the boundaries of the system shift constantly. Even if we divide the system into user and supplier sectors, it will quickly appear that many institutions are both (table 11).



U.S. National Bureau of Standards, Gaithersburg, Maryland

FIGURE 49.—A view of the National Bureau of Standards complex near Gaithersburg, Maryland. The large building at the left houses the administrative offices.

fundamental research. Under the circumstances, DOD laboratories serve an essential purpose as sources of new ideas, as a means of making DOD a more sophisticated buyer, and as points of contact with the universities. On the structure of technology development carried out on contract to DOD, see Jacques Gansler, *The Defense Industry* (Cambridge, Mass.: MIT Press, 1980), Chapter 4 and p. 304, n. 17.

Table 11. Supplier and User Sectors in the National Measurement System

Supplier Sectors	User Sectors
The Knowledge Community	The Knowledge Community
International Meteorological Organizations	Instrumentation Industry
Standard Organizations	Regulatory Agencies
Instrumentation Industry	Department of Defense
Other U.S. National Standards Authorities	Civilian Federal Agencies
State/Local Offices of Weights and Measures	State/Local Government Agencies
Standards Testing Laboratories and Services	Industrial Trade Associations
Regulatory Agencies	Construction
Industrial Trade Associations	Food, Tobacco, Textiles, Apparel
	Lumber, Paper
	Chemicals, Petroleum, Rubber, Plastics, Clay, Stone
	Primary and Fabricated Metal Products
	Machinery, except electrical
	Electric and Electronic Equipment
	Transportation Equipment
	Transportation and Public Utilities
	Trade, Retail and Wholesale
	Health Services General Public

Source: Science Policy Research Division, Congressional Research Service, Library of Congress, *The National Bureau of Standards: A Review of Its Organization and Operations, 1971-1980* (May 1981), p. 6.

What is the role of the NBS in this diffuse and voluntary system? To simplify somewhat, we would argue that the NBS performs three closely-related functions which are vital to maintaining the system. It provides the basis for uniform and accurate measurements throughout the economy; it drafts voluntary standards for determining that a product, a process, or a service meet certain criteria; and through its Institute for Computer Sciences and Technology, it recommends uniform data-processing standards in the Federal Government. This mission, in turn, can be carried out because of certain special features inherent in the Bureau's organization. The first is scientific competence in many areas: in materials processing, calibration, computer-aided design, mathematical modeling, structural analysis, and instrument design. So much may appear obvious. But without competence across many disciplines, the Bureau could scarcely play a significant role in developing standards. This system is fragmented, decentralized and, above all, *voluntary* (ref. 208). Without regulatory authority to enforce standards, the Bureau can gain acceptance for its standards only on the strength of the quality of the work that goes into drafting them.

A second distinguishing feature of the Bureau is its responsiveness to the needs of other Federal agencies and industry. NBS is unique among Federal laboratories in the amount of work for others it performs. Much of this extramural work is required by law; but in any case, the Bureau has accepted many assignments, the better to carry out its mission. During the Second World War and for several years thereafter, the Bureau received up to 85 percent of its funds from other agencies, principally the military. Other agency funding has since tapered off and, as a matter of policy, Bureau officials prefer to maintain it at about 40 to 45 percent (ref. 209).

What has kept the level of support this high has been the proliferation of Congressional mandates since the late 1960s. Congressional committees which have to do with NBS have seen it as a national resource, capable of contributing to economic growth and supporting the science and technology infrastructure. These mandates led to many problems: They caused the Bureau to carry on work that went beyond its competence in the physical sciences; Congress neglected to appropriate funds to cover these programs, forcing the Bureau to divert funds and manpower from its core activities; and it led to disagreement with OMB over whether a Congressional mandate in a given area — say, in studying the economic effects of metallic corrosion — sufficed to make the Bureau the lead agency in that area. The point is that the Bureau's work for others affects its mission in ways which have no parallel in other Federal laboratories.

Third, the Bureau's effectiveness depends not only on technical competence but on the perception by its sponsors as an objective, neutral authority. The Bureau has shunned a regulatory role and it has been generally cautious in taking on work that might give the appearance of competing with industry. It has been NBS policy to avoid the later stages of product development. At NBS, “. . . product development is usually not taken as far as it is in other Federal laboratories, where the usual policy is to continue developing a new technology to the point of pilot demonstration. Development at NBS usually ends with an early-generation prototype, a few steps prior to pilot demonstration. Then other Federal agency or private sector laboratories are left to continue development.” (ref. 210.)

Even where the Bureau has taken an activist view of its mission and has moved further into technology development, it has avoided large demonstration projects.* In 1983, for example, the Bureau installed an

* Note, however, that it is not the case that if a Federal agency avoids supporting large demonstration projects, that any work up to that point is appropriate for Federal support. The test is the judgment — by the sponsoring agency, Congress, OMB, or all of them — that the results of a research and technology development activity would be appropriable by private industry.

Automated Manufacturing Research Facility (AMRF) in its machine shop (ref. 211). The AMRF is to be a small, modular flexible manufacturing system that will serve to test certain measurement concepts. The novelty of the facility is not in its hardware; all of AMRF's components are off the shelf. Rather, its purpose is to determine if different components from different manufacturers can exchange information without the need to modify software or protocols. This work is being supported by industry, through the loan of equipment and personnel; and the transfer of technology — in this case, standards for the interfaces between components of computer-aided manufacturing systems — will occur through the usual channels: industrial and trade associations, and the American National Standards Institute.

The features so far discussed — technical competence, neutrality, responsiveness to other organizations — suggest that the Bureau is a rather conservative organization. They also suggest that, given the structure of the national measurement system, technical competence and mission performance are linked in a peculiarly intimate way. What remains to be discussed is how the Bureau's conception of its role in stimulating industrial productivity has changed under outside pressure. Of all Federal agencies, the Department of Commerce, the Bureau's sponsoring agency, is the one most closely tied to the ups and downs of the U.S. economy. As data gatherer (Census Bureau, Bureau of Economic Analysis), as a regulator of international trade, as a registrar of patents and trademarks, and as the parent agency of the National Weather Service, the Commerce Department touches the economy at many points. To some Congressmen, it seemed reasonable that Commerce in general and NBS in particular should play their part in stimulating technological innovation.

But there is not (to put it mildly) unanimity as to the best approaches to government sponsorship of industrial innovation — let alone what the Bureau's role shall be. In the past ten years, the Federal Government, through the National Science Foundation, has provided seed money for university/industry cooperative research centers; these centers are intended to work on the cutting edge of technology, in biotechnology, polymer processing, robotics, computer graphics, microelectronics, and much more (ref. 212). Again, in 1980, Congress passed the Stevenson-Wydler Act, which authorized the Secretary of Commerce to establish an Office of Industrial Technology to assess the climate for industrial innovation and to propose methods for advancing it; and to create Centers for Industrial Technology, similar to the cooperative research centers sponsored by the National Science Foundation. The Act also requires every Federal agency operating one or more laboratories to set aside 0.5 percent of its research and development budget to support

the transfer of Federally-owned or originated technology (ref. 213). It remains to be seen whether this and other legislation will really lead to greater productivity or whether, in pursuit of aims set forth in high-sounding language, government employees will shuffle paper. Where the government is not itself the buyer, such policies lack all focus. In particular, the experience of NASA and some AEC laboratories in setting up technology utilization programs has not been encouraging (ref. 214).

In promoting innovation, the role of the Bureau will be a limited one. And necessarily so. The Bureau, after all, comes under pressure from many directions: from Congress, to move directly into product-oriented research; from the scientific community, not to compromise the quality of its basic research; from OMB, to do only such work as can be justified on the ground that the Bureau is the lead agency; and from industry, to establish standards in cooperation with, and not competing against, the private sector. Bureau officials have tried, with some success, to steer a course between two opposing policy models, each fraught with political consequences. One position would be to restrict the Bureau to its traditional role in measurement science, on the ground that this in itself represents an important contribution to economic growth. According to this view “. . . the Bureau performs basic research, not *for* industry or for a particular *technology*, but in the limited area of physical measures . . . While these physical measures may be used for producing information about technologies . . . or technical or natural processes . . . or the distribution of physical particulates in a media (sic), the emphasis is on the measurements, not on the processes or technologies.” (ref. 215.) The other position would put the Bureau in an activist role. It would move into exploratory research and would concentrate on those industries that, for whatever reason, could benefit by the research they could not themselves perform.

In practice, the Bureau has tried to strengthen those areas where its basic competence resides and to move gradually into targeted exploratory research. This category of investigation has been defined by the National Science Foundation as “the early stages of research in areas not yet well enough defined or understood to merit full programmatic support.” Since the late 1970s, the Bureau has managed to get Congress to vote full funding for mandated programs. It has also persuaded Congress to fund special “competence building” projects, which are intended to sustain the Bureau’s scientific and technical base. The Bureau’s rationale is that, with so much of its competence diverted to short-term work, some way must be found to keep its best people in the advanced research which is the basis of its special competence. Such research can be justified for its own sake and for the way it fits NBS mission objectives. As part of this

competence building, NBS scientists and engineers have done (or are doing) important research in wave optics, organic electrochemistry, quantum radiometry, and advanced robot vision (ref. 216).

The same rationale sustains the Bureau's sponsorship of the Joint Institute for Laboratory Astrophysics (JILA) in Boulder, Colorado. Founded in 1962, JILA is an independent research institute, a joint venture of NBS and the University of Colorado. The staff at JILA engage in research of the most fundamental kind in atomic and molecular physics; part of this work is supported by NBS direct appropriations, part from other-agency contracts, and part from direct Colorado state funds. As with the Bureau's competence-building program, the existence of JILA is justified on the ground that ". . . basic understanding in [atomic and molecular physics] would increase the Bureau's long-term ability to respond to measurement needs in emerging technologies, or industrial areas, for example, lasers or chemical reactions in chemical processing, and improve the precision of measurement in fundamental physical constants. The case for such work was coupled with the assertion that, in the future, the Bureau's role in standardization and measurement would shift from that of a *developer* and *doer* to a *teacher* and *innovator*." (ref. 217.) JILA's existence can be justified on other, related grounds. New standards demand greater accuracy and flexibility and as such, are important to the economy. Tighter standards are only possible with better understanding of physical properties at the most elemental level, and ways to control them. More important still, the development of the knowledge behind the standards is, like the standards themselves, a public good not sufficiently appropriable by any likely group of firms or universities to be undertaken in an organized way without Federal support.

The appeal of this arrangement is that JILA is perceived as an elite organization with a minimum of bureaucratic overlay. Less obvious is the role of JILA in advancing the Bureau's mission in applied research and engineering. Nevertheless, other joint institutes have been proposed — for example, in microelectronics and membranes for chemical separation (ref. 218) — and the concept is one that fits nicely with the Bureau's strategy of maintaining and subsequently expanding its traditional areas of competence.

Thus the Bureau is trying to pull off something quite difficult. This "something" is to continue its role in advancing measurement science something essential to the infrastructure of science in the United States while moving into exploratory research in areas that Bureau staff see as vital to industrial growth — above all, in automated manufacturing (ref. 219). The next group of laboratories we consider is very different.

The Multiprogram Laboratories of the Energy Department

In moving from the National Bureau of Standards to the multiprogram laboratories operated for the Department of Energy, certain differences are obvious (ref. 220). Where the Bureau is operated by government employees, the latter are government-owned, contractor-operated facilities. Where the Bureau's principal clientele is the industrial sector, the former AEC laboratories have worked primarily for the AEC, the Department of Defense, and other agencies. Where the Bureau has traditionally avoided contracting out, the Energy laboratories subcontract on a massive scale to industry and universities. And lastly, where the Bureau has, for good reason, shunned product development, the Energy laboratories have been involved in many kinds of production — from nuclear warheads to prototypes of commercial reactors to synchrotrons.

The nine laboratories we shall consider represent one of the heaviest investments in basic and applied research made by the United States or any government. By "multiprogram" we emphasize that each laboratory has capabilities in many areas of basic and applied research, as well as in energy and weapons technology development. This sets them apart from other laboratories established either to operate a very large research facility — for instance, the Stanford Linear Accelerator Center — to do research in one discipline, such as inhalation toxicology, or to work on one product or technology, as the Knolls and Bettis Atomic Power Laboratories specialize in nuclear propulsion. The nine multiprogram laboratories, with their locations, primary mission or core area, and operating contractors are shown in table 12.*

To generalize about the work of these nine laboratories is difficult. Suffice it to say that the laboratories handle close to half of the Energy Department's research and development budget and about nine percent of total Federal research and development; that they conduct about 70 percent of the Energy Department's weapons development and about 24 percent of energy-related research and technology development; and that they represent an investment of over \$3 billion (ref. 221). It should also be recognized that, besides doing research and development, the laboratories have other important roles. They serve as technical consultants to the Energy Department, drafting environmental impact statements, making technical assessments of safety and health-related work, and helping the Department to be an informed buyer of industrial services. They maintain special research facilities, some of them unique. Several laboratories produce items of commercial value, like the radioactive isotopes manufactured at Oak Ridge. All of the laboratories

* See Appendix III for more information about the nine multiprogram laboratories.

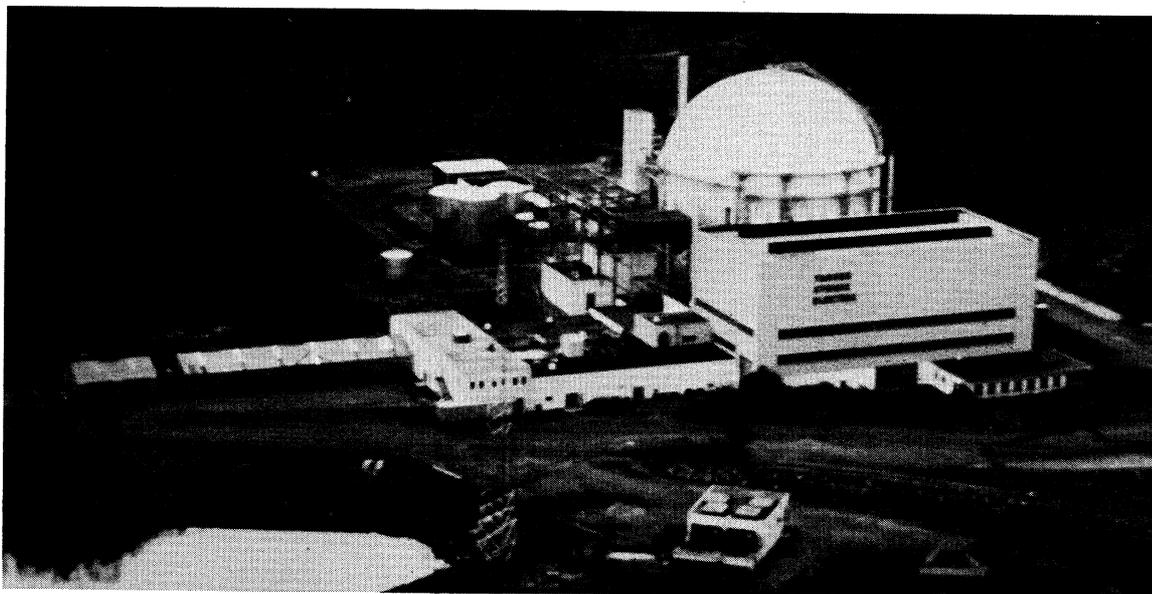
are involved in education and training; many allow doctoral work to be done at their facilities and hire postdoctoral students for short periods. As systems engineers, as consultants to state and local governments, and as stewards of unique facilities, the multiprogram laboratories contribute in many ways to the nation's science and technology base (ref. 222).

Table 12. Multiprogram Laboratories

Multiprogram Laboratory	Location	Primary Mission or Core Area	Operating Contractor
Ames Laboratory	Ames, Iowa	basic research in materials and chemical sciences	Iowa State University
Argonne National Laboratory	Argonne, Illinois	advanced nuclear technology research and development, fundamental research in high energy and nuclear physics	University of Chicago in cooperation with the Argonne Universities Association
Brookhaven National Laboratory	Upton, New York	fundamental energy science	Associated Universities, Inc.
Lawrence Berkeley Laboratory	Berkeley, California	fundamental energy science	Regents of the University of California
Lawrence Livermore National Laboratory	Livermore, California	research, development, and test of nuclear weapons designs	University of California
Los Alamos National Laboratory	Los Alamos, New Mexico	developing nuclear warheads and maintaining an innovative weapons design program	University of California
Oak Ridge National Laboratory	Oak Ridge, Tennessee	fundamental energy science, magnetic fusion, nuclear energy (fuel cycle, primarily)	Martin Marietta Corporation
Pacific Northwest Laboratory	Richland, Washington	waste management	Battelle Memorial Institute
Sandia Laboratories	Albuquerque, New Mexico; Livermore, California; Tonopah, Nevada	research, development, and engineering of nuclear weapons systems (except for the nuclear explosive)	Western Electric Company

The roles and missions of the laboratories have changed significantly since their establishment. Most were created as part of the Manhattan Project, although Brookhaven was founded in 1946, Sandia in 1948 to 1949, and Lawrence Livermore in 1952. Since then the laboratories fortunes have depended largely on national policy toward the uses of atomic energy. The Atomic Energy Act of 1954 envisaged industry and

the AEC laboratories working in tandem to develop commercial light-water reactor systems (fig. 50). As the first commercial nuclear plants came on line, the amount of nuclear energy research and development at the laboratories dropped. It was at this time that laboratory directors, notably Alvin Weinberg at Oak Ridge, began to emphasize their laboratories' abilities to move into areas of nonnuclear research where they could make a real contribution — for example, in civil defense, air pollution control, and water desalination. The problem, as Weinberg saw it, was that most large Federal laboratories were tied to missions that might or might not matter a decade hence. "When a government laboratory finishes a project, it cannot ask, What is the most important national problem . . . to which our talents can be put? Rather, the laboratory must ask, What is the most important problem, *coming within the purview of our sponsoring agency*, to which we should next turn? This narrower set of problems may not be as important to the nation as are other problems which are the responsibility of a different agency, but which the laboratory may be equipped to handle. Such rigidity reduces the efficiency with which we deploy our federal scientific apparatus." (ref. 223.)



U.S. Nuclear Regulatory Commission, Washington, D.C.

FIGURE 50. — *The Yankee Atomic Reactor, a commercial light water nuclear power plant operated by New England Power and Light Co., Inc.*

As it turned out, the AEC and its laboratories moved in two directions concurrently. On the one hand, as we saw earlier, the AEC shifted its emphasis in the mid-1960s from encouraging commercial

sources to build complete, or "turnkey," reactor systems to (in the words of a GAO report) "building Government-owned facilities to resolve uncertainties, improve and test reactors, and in effect, set standards for all future builders of nuclear reactors." (ref. 224.) At the same time, Congress began to encourage the laboratories to diversify. In 1967, Congress expanded the 1954 Atomic Energy Act to permit the laboratories to conduct research in public health and safety; in 1972, it further amended the Atomic Energy Act, enabling the laboratories to conduct nonnuclear energy research for the AEC and other agencies; it increased the laboratories' nonnuclear budgets after the Energy Research and Development Administration (ERDA) was established in 1975; and in 1980, it passed the Stevenson-Wydler Act which, as shown earlier, required large Federal laboratories to set aside funds to stimulate the transfer of technology developed under laboratory sponsorship.

Both ERDA and its successor, the Department of Energy (DOE) drafted guidelines to regulate the ways in which the laboratories planned their long-range research, considered new work, or took on work for others. In the last case — work for others — the Department hewed to the AEC guidelines mentioned in Chapter III: Work for other Federal agencies may be undertaken if private laboratories are unavailable to do the work, the work will not interfere with DOE programs, and the work is consistent with basic DOE responsibilities (ref. 225). The Department has also developed a strategic planning procedure known as the Institutional Plan. This is supposed to represent the consensus of the Assistant Secretaries and other officials to whom the laboratories report the responsible field operations offices, and the laboratories on the kind of work to be sponsored over five years (ref. 226). The Plan is reviewed and updated annually. Additionally, the laboratories submit ideas for future work under a system known as "Form 189." None of this is unique to DOE laboratories. NASA, for example, has a five-year operating plan the centers report to program directors at Headquarters; and Form 189 corresponds roughly to the Research and Technology Operating Plan. The point, of course, is not what constitutes the formal system, but how well it works.

One way of analyzing the work of the laboratories is to take up the roles and missions that laboratory directors consider appropriate. Specifically, we want to return to something touched upon in earlier chapters: the attempts by laboratories to diversify into areas related to their core programs. More precisely, we shall examine the issue involved when the laboratories' technical capabilities have been applied to nonnuclear energy research, development, and demonstration (RD&D). It is important to understand what, exactly, is at issue. Diversification and work for others are *not* identical. All of the laboratories are engaged in work for others, ranging from 6 percent (i

1982) at Ames Laboratory to 19 percent at Oak Ridge and 20 percent at Pacific Northwest Laboratory (ref. 227). But much of this work is closely related to the laboratory's primary mission; thus at Los Alamos, a weapons laboratory, a large proportion of work for others is sponsored by the Defense Department. Some of the work, as in laser technology, may have important commercial application; and there are programs, like Lawrence Livermore's work on *in situ* coal gasification, which grew out of the AEC's research into the peaceful uses of nuclear explosives. But such technology is, in a sense, diversification *within* the laboratory's primary mission, rather than *outside* it.

What conditions lead a laboratory to diversify? Much depends on whether the laboratory executives wish to diversify, or whether the sponsoring agency desires it. In fact the ERDA and the DOE have called on the laboratories to enter into nonnuclear energy research: when the laboratories had existing capability from similar work in other programs; when they had capability from work in the same program; when ERDA needed technical support (ref. 228); or when ERDA assigned tasks to the laboratories because Headquarters officials felt that it would be inappropriate to assign them to industry — for example, in developing energy conservation performance standards for buildings (ref. 229). It would be a serious mistake to assume that the efforts of the laboratories to diversify were taken *only* on their directors' initiative. Still, there are circumstances which would predispose laboratories to diversify, among them the termination of key projects or a decline in the laboratory's budget, forcing the laboratory to seek sponsors to make good the shortfall. But many laboratories have had activist directors who were convinced that the experience gained in their core activities was transferable to other programs, other technologies.

There are several scenarios which research executives have advanced to justify their laboratories' moves into related areas. In ascending order of probability of success, these are: (1) The laboratory works to develop a new technology, where the chances of success are either low or very long term, but where the payoff would be high. Examples of such work would be programs to develop an inexpensive method of extracting oil from shale, or to create temperatures hot enough for hydrogen fusion to occur. (2) Several laboratories work on different pieces of a major technology development program, in the hope that their work will coalesce to form one "solution." This really reduces itself to Scenario 1, except that the work is now parcelled out among several institutions. (3) A laboratory takes on new programs simply because of their intrinsic worth or importance and without thought of a global solution to major national problems. Examples of such work are legion, and they are the kinds of diversification with which we are concerned. It is often very difficult to predict what line of research will be most fruitful,

and it is probably well for a research executive to dismiss out of hand the possibility of solving (say) the energy crisis as a result of work pursued in his laboratory. With the greatest of luck and effort, the preconditions may be created for removing bottlenecks to resolving a major problem in the development of a specific technology.

Nonnuclear energy research and development is one area which did attract the multiprogram laboratories, once the amended Atomic Energy Act enabled them to engage in it.* However, it is still a relatively small proportion of the laboratories' total workload, only rising from 8.9 percent in 1977 to 9.5 percent in 1982 (ref. 230). It is not that the laboratories failed to make impressive use of the expertise gained in work on nuclear energy. To take a few cases at random. Sandia Laboratories, whose activities used to be 100 percent defense-related, has developed more than two dozen devices used by industry — coring devices, new hardrock drill bits, hot gas solder levelers, and the like. Lawrence Livermore is developing a prototype metal-air power cell for use as an alternative energy source. The Ames Laboratory developed a method for preparing large-area films for solar cells. And there are many other examples. If the laboratories' nonnuclear energy research has not grown significantly since the late 1970s, it is less for lack of technical skills or enthusiasm than because of features inherent in the relations between the laboratories and DOE, features singled out by the General Accounting Office in a 1978 report and confirmed in part by DOE's Energy Research Advisory Board in 1982.

The roles of the laboratories in nonnuclear energy research and development have been far less clearly defined than they were for nuclear energy and weapons R&D. The laboratories report to at least three different Assistant Secretaries, as well as a Director of Energy Research. There has been, in short, a real lack of uniformity in program planning. The Institutional Plan has been only one of several vehicles by which the DOE defines its laboratories' agenda. The same holds for project evaluation: Headquarters program offices use support contractors, technical review panels, and consultants in reviewing laboratory work. Whatever one may say about the AEC's shortcomings — and they were many — the Headquarters program officers usually knew what they wanted. The DOE, by comparison, is a multiheaded monster, charged with regulatory, data-gathering, and project management functions (like filling the Strategic Petroleum Reserve), as well as research planning and

* As defined here, nonnuclear energy research and development includes work in (1) solar, geothermal, electrical energy, and storage, as well as (2) conservation. As shown, these activities are broken out as two separate budget categories. It does not include work for others, a separate category, although some nonnuclear energy research is incidentally performed for other Federal agencies.

management. For the laboratories, the danger is a combination of micromanagement by Headquarters and Congress, and a crossfire of demands from line and staff officers who do not communicate with each other. Since the time of the Joint Committee on Atomic Energy, both Congress and congressional staff have been involved in directing energy research down to the field level. As the Department's own advisory board concedes, the laboratories are grossly overburdened with reporting requirements; and it cites the case of Argonne National Laboratory, which must respond to 137 separate DOE orders and policy directives and which receives funding from 129 separate, noninterchangeable accounts. This is micromanagement with a vengeance! (ref. 231.) Carried to a conclusion, such policies (or lack of policies) would drain all flexibility from the system under which the laboratories must perforce operate.

But this does not exhaust the reasons for the laboratories' failure to do more nonnuclear research. First, the DOE's attempt to place each laboratory under an Assistant Secretary responsible for specific program areas has not worked well. Under this system, for example, the Lawrence Livermore, Los Alamos, and Sandia Laboratories report to an Assistant Secretary for Defense Programs. But such a system tends to increase pressure on the laboratories to intensify specialization, and it makes it more difficult for one laboratory to "cross service" another. Work outside the core program becomes expendable, especially if it is only a small portion of the laboratory's budget. Under ERDA and DOE, there has been a reluctance to expand the multiprogram laboratories, for fear that such expansion would blunt their effectiveness in weapons development.

Second, the multiprogram laboratories face competition from other, in-house research facilities. It is not quite correct to say that the DOE has no government-owned, government-operated facilities. The DOE has set up, as Congress required, a Solar Energy Research Institute (SERI); and it inherited from the ERDA five "energy research centers" specializing in fossil energy research and technology development. DOE officials have strong incentives to place work with SERI and the energy centers. In so doing, they avoid the appearance of duplication, free the multiprogram laboratories to concentrate on their core areas, and save the money that would be needed to bring the laboratories up to scratch in, what is for them, an unfamiliar technology, when the capability for getting the work done already exists.

As paradoxical as it may appear, the laboratories' missions do *not* ultimately depend on the existence of consensus as to a national energy policy. It cannot be said that such a policy, or such consensus, exists; there is at this time no policy as to the roles of fossil, nuclear, and solar technologies. Moreover, even if the nonnuclear work being pursued at the laboratories continues, more thought must be given to marketing new technologies and getting them into use. What are the "national" needs to

which the laboratories are being invited to respond? Does it make sense, for example, to pursue research into synthetic fuel technology, in the absence of projected demand for shale oil, liquefied natural gas, or liquefied coal? At what point can the laboratories hand over a prototype technology for commercial development by the private sector? The existence of a coherent policy on energy research and development would certainly help the laboratories to answer these questions and to sort out their various functions. However, it is unrealistic to expect the laboratory managements to wait until such a policy exists (if it ever does) before coming to grips with the problems they face. Indeed, the converse is more likely to be true: An energy policy may emerge (at least in part) because the management of the various laboratories responsible for energy related research and development will create one — either explicitly or at the very least implicitly.

It is at the crossroads of policy making that relations between Federal technology development institutions and their sponsoring agencies are ultimately determined. It has become fashionable in many quarters to blame the “lack of a rational policy for X (whatever X happens to be)” for all our troubles. This view is naive, to say the least, especially when it comes to technology development. People who demand “policies” generally do not distinguish between *genuine* policies and individual programs that they support or oppose for one reason or another. The fact is that in a democracy, the policy-making process is difficult and lengthy and there may be long periods in which “there is no policy.” This is not necessarily bad because many individuals and groups are still forced to do things that will eventually be beneficial and may even lead to a “policy.” Furthermore, having a bad policy is much worse than following the democratic practice of leaving people (including laboratory directors) free, within broad limits, to do their own thing. In other words, no policy at all is better than a bad one.*

Most of the individuals in the leadership of our technology development institutions are completely aware of the situation just described. They also recognize something else, which is that policy making often starts in their own institutions. It is no exaggeration to say that their policy of deterrence — that is deterring the Russians from major military adventures — is based on nuclear weapons that originated either at Los Alamos or at Lawrence Livermore. It is possible to assert (although perhaps difficult to prove) that we would not be in a position to pursue the activist foreign policy that we have adopted since the end of

* In reviewing an earlier draft of this book, one official observed that “no policy” usually means “I don’t like the President’s (or whoever) policy.” Having a “policy” is synonymous in an advocate’s mind with proposing major Federal initiatives.

the Second World War were it not for our leadership in areas such as air transportation and communications. Both of these technologies grew from work done in the institutions that are the subject of this book. This, then, is the really important point: In the policy-making process, there is a vital interplay between technology development laboratories and the political establishment. The technical capability to do something is often the trigger that causes the establishment of a national policy based upon that capability. This has been generally recognized by the political establishment and it is, of course, for this reason that the nation's technology development laboratories continue to enjoy broad political support.

CHAPTER XI

Conclusions

In this book we have tried to explain the workings of a special kind of research institution, rather than research institutions in general. The technology development laboratories we have dealt with, whether federally funded or the "corporate" laboratories of such large corporations as DuPont, General Electric, or General Motors, require that three basic irreducible conditions be fulfilled: There must be a "critical mass" of personnel and facilities; the laboratories must have the multidisciplinary capabilities needed to move from one mission to the next; and there must be a promise of relatively stable financial support extending over several years or even decades.

The missions of the laboratories range from developing new spacecraft or non-conventional hard materials to devising new measurement techniques. All of these entail the development of some new process, procedure, technique, or capability, and this is essentially what the term "technology development" means. The missions of these laboratories differ from that of a university, which is the creation of new knowledge in a series of well-defined disciplines. This distinction is easy to draw. It is not quite as easy to define the difference between what we have called technology development and product development, as it is usually understood in the industrial sense. Very often, the precise function of technology development laboratories is determined by the attitude of their sponsoring agencies or corporations toward product development. Some, particularly the National Bureau of Standards, elect to stop well short of final products; while others, like the multiprogram laboratories under the Atomic Energy Commission and later the Energy Department, have preferred to push a given technology — say, a fast breeder reactor — all the way to a demonstration prototype. In the private sector, General Electric's corporate laboratory at Schenectady does no product development, but in smaller corporations the process of technology development and product development may be combined.

Depending on the agency's philosophy, each laboratory will provide a different answer to the question of when the mission, or the project within the mission, has been accomplished. That point may come at any one of a number of stages: when a prototype of flight hardware has been

successfully tested; when a component of a new weapons system is delivered to a contractor for systems integration; when research into the structure of composite materials leads to improvements in the design of helicopter blades; or when a deep-space probe returns new knowledge about the structure of Saturn's rings. The point to be emphasized is that, in a good laboratory, there is something open-ended about its mission. Research tasks and projects will have discrete beginnings and ends, but the mission itself will continue in some other manifestation.

It may go without saying, but let it be said anyway: This book is about *successful* technology development laboratories. In the case material in earlier chapters, we have postulated certain features of the successful mission-oriented laboratory. We find a small but significant segment of laboratory resources devoted to basic research and, along with it, considerable discretion in the allocation of funds to promising new areas; good communications between groups involved in basic research and those in applied research and subsequent development; an ability to diversify creatively within the organization's primary mission; strong overlap of all phases of research and development; a policy of early identification of future managers among professional staff; and emphasis on a lateral, rather than a vertical, direction of communications (ref. 232). The great research executives of the last thirty years have had an uncanny insight into the relation of the work done at the division and directorate level to their laboratories' missions. What is more, they have often been able to move their organizations into new areas, even — or should we say especially? — in the absence of a guiding national policy. It was not foreordained that the next step after Apollo would be the Space Shuttle or that ship nuclear propulsion might become the basis for a new commercial technology.

Thus, the tendency of a successful technology development laboratory is to apply the word "mission" in both a very narrow and a very broad sense. In the former case, the emphasis is on getting the immediate job done; in the latter, it is more a matter of maintaining the organization's technical competence over the long run. Why is this competence building so important? Because it positions the organization to exploit its own discoveries and, as desirable, to move into new areas. As Harvey Brooks has observed, "A company — or for that matter, a nation — that has a broad technical capability can quickly exploit the ideas of others, and can catch up on the bets that it misses provided it has the technical sophistication to identify promising ideas at a sufficiently early stage . . . Just as a company or a nation cannot expect to exploit every promising scientific discovery, so every discovery that it exploits need not be its own." (ref. 233.) Viewed in this light, mission and capability tend to blend into one another. It cannot be emphasized too strongly that, while most research organizations live to an extent from an

inherited intellectual capital, this can lead to stagnation and decline without the stimulus of new ideas. Building on an organization's scientific competence then becomes almost as important as any programmatic mission, because it is the principal way of assuring that such missions can be accomplished.

It is time to consider what conclusions can be derived from the analyses of the preceding chapters. It is not to be expected that we can here offer specific recommendations about what should be done in space, defense, or energy. Nor can we be certain that the structure of the U.S. science and technology base will resemble the present state five or ten years hence. But if we assume, not unreasonably, that the Federal Government and our large industries will have a continuing stake in promoting scientific research and technology development, then the following conclusions should prove useful.

1. *The particular management arrangement under which a laboratory is operated is, and should be, primarily a matter of administrative convenience.*

Technology development has occurred in a large variety of organizational arrangements. Laboratories may be operated by government employees or by contractors; they may be Federal contract research centers, like RAND, working primarily for one sponsor; or they may have a variety of sponsors, as in the case of SRI International. One mission may be perfectly compatible with different arrangements, while a changing mission may provoke the sponsoring agency into changing the structure of its laboratories. There is nothing final about the structure of a research organization. To take one example: Within the past decade the Air Force has converted its Cambridge Research Laboratories from basic research to an "exploratory development" institution, closed the Aerospace Research Laboratory and transferred part of its staff to other Air Force installations, converted one Federal contract research center, ANSER, into a conventional contractor, and authorized other contract centers to create units separate from their work for the Air Force (ref. 234). Since a laboratory may well outlive its original functions, there is no reason to expect its structure to remain unchanged.

As a corollary, the distinction between government-owned, government-operated laboratories and those run by contractors is losing much of its sharpness. One reason was given in Chapter VII: No Federal agency can abdicate responsibility for seeing that congressional appropriations are used properly. But the nature of Federally-sponsored research and development is itself changing, to the point where the distinctions we are talking about have become, if not meaningless, then at least less relevant than they might have been in the 1960s. These changes are of several kinds. First, the distinction between research and development functions, which are reserved to government employees,

and support services, which may be contracted out, is eroding. It is difficult to draw an absolute boundary between a support service, such as writing a complex computer program, from ostensible research and development functions like designing the spacecraft for which the program is being written. Both may be — indeed, they usually are — performed under contract. Second, the kinds of technology that support large mission-oriented projects are also changing. No Federal laboratory any longer has the capability for designing and building all the major components of systems as complex as the Space Shuttle or a ship-launched cruise missile. What the laboratory must retain is the ability to specify the research or systems concepts, select its prime contractors, and certify at any point along the line that the system meets all requirements. At an agency like NASA, these functions would be performed by government employees, with guidance from Headquarters; at the multiprogram laboratories of the Energy Department, the ultimate authority to specify the work to be done would reside at the Washington headquarters, with field offices monitoring work in progress. But the point to bear in mind is that the structure of the NASA centers, on the one hand and of the Energy laboratories, on the other, reveals very little about what these institutions actually do. Even within NASA, the work done at the Jet Propulsion Laboratory is not easily to be distinguished in kind from work carried on at the centers that are staffed largely by civil service employees.

What emerges from this analysis is the conclusion that the organization of most laboratories owes as much to the origin of the institution as to the present purpose. We believe that if a mission is of sufficient importance to justify establishing an installation to carry it out, then that mission can be carried out even as the institution changes. What is remarkable about older laboratories like the National Bureau of Standards and the Langley Research Center is how closely they have adhered to their original mission — to maintain a national measurement system in one case, to conduct advanced aeronautical research in the other — despite all the changes that have intervened. With some slight exaggeration, we would assert that any institutional arrangement that sustains the laboratory's mission is justified. The right arrangement, under any regime, means that laboratory executives have the discretion to assign work and to start research in new areas without the need to seek prior approval; that professional staff are involved in defining the design of major systems; and that the laboratory is something more than a funnel through which funds from the sponsoring agency pass through to contractors.

2. *A capability for performing basic research is essential to the work of a technology development laboratory.*

Mention has been made of the importance of basic research in a

laboratory oriented to long-term missions. But we would like to draw out the implications of this conclusion still further. Whether research is termed "basic" or "fundamental" or "applied" largely depends on the context. Research ceases to be basic at just the point where it suggests practical applications. For two generations superconductivity was an interesting laboratory curiosity; it is now recognized as a phenomenon with important potential applications, such as power transmission and medical imaging, and research on superconductivity has now moved into the applied category. This is true even though some work in superconductivity remains research about fundamental properties of matter. Or consider a more famous case, the development of the transistor. As Brooks notes, once the transistor was discovered, "almost any research on the properties of Group IV semiconducting materials could be considered to be potentially applicable, and this has indeed proved to be the case . . ." (ref. 235.)

To put the matter briefly, basic research in a mission-oriented laboratory serves several closely-related purposes. It explores areas *contiguous* to the laboratory's mission, in the hope that such areas may later become relevant to that mission. As we saw in Chapter X, this was the rationale for much of the work sponsored by the Office of Naval Research and the Defense Advanced Research Projects Agency (DARPA). But even within the compass of a single laboratory, units may be set up with "DARPA-like" characteristics; that is, units may be created which, without getting into development work themselves, sponsor new ideas, particularly in those fundamental areas where with "time, strength, cash, and patience" (Herman Melville) basic research may transcend into new technology.

Thus a commitment to some basic research positions a laboratory to move into new areas, while sustaining its ability to work in current programs. The importance of such research may be gauged by the differing philosophies of Federal and industrial laboratories. In the former case, the majority of large laboratories have their own basic research units; in the latter, it tends to be concentrated at one or (at most) a few facilities. But whatever the institutional arrangement, it is important that laboratory executives encourage a spillover of such research into applied work, whether by encouraging scientists to publish their work in professional journals, by encouraging collaboration with university researchers, by sponsoring interchanges of personnel between laboratory divisions and private firms, or by sponsoring improved instrumentation as a link between basic and more applied work.

3. *Successful diversification is most likely to occur where it has a particular relevance to the laboratory's mission.*

The emphasis here is on "particular." When an agency is large and has many laboratories, there may be few problems in other agencies

which are not, in some sense, relevant to it. The issue for laboratory executives becomes one of *particular* relevance: Is work in a new area related to work currently being pursued in this laboratory? Does it represent an addition to our capabilities, or is it merely a reimbursable service for another agency? Can we get into new areas which are interesting in themselves but might be construed by our sponsor as interfering with our primary mission? But relevance is not a simple concept; it needs to be analyzed. Where a laboratory is charged with a broad mission, such as research into the peaceful uses of atomic energy, there is in principle almost no limit to the disciplines which impinge on the mission.

How, then, do laboratories diversify successfully? Almost by definition, diversification means establishing links with organizations outside the system. In a laboratory where scientists and engineers may talk mainly to each other, the possibility of diversifying will be somewhat remote. As a necessary (though not sufficient) condition, laboratories need to cultivate external relationships; the richer and more varied these relationships, the greater the number of opportunities for new work that will suggest themselves. The multiprogram Energy laboratories are a case in point (ref. 236). They have been charged by the Atomic Energy Acts and subsequent legislation with transferring technology generated by the laboratories to external users. They have made their facilities available to industry, frequently without charge; subcontracted research tasks to industry; collaborated with industry on subjects of mutual interest; and selected individuals from industry to serve on advisory committees. Note that these are *continuing* relationships. It is not as though a laboratory director must stake everything on an all-or-nothing decision to commit his institution to something radically new and unfamiliar. This is not how successful diversification comes about. Rather, the network of relationships is drawn tighter; opportunities for new work seem to arise "serendipitously"; the laboratory exchanges personnel and equipment with industry, universities, or both at once; and by a judicious mix of subcontracting, advisory services, and published summaries of work in progress, it creates a climate favorable to research collaboration and joint ventures.

But a description of how diversification does in fact occur leaves our earlier question unanswered. Put differently, what are the preconditions for successful diversification? The first is the presence of a second party willing to sponsor the laboratory's venture into new fields, whether it is industry sponsoring Sandia's work in drilling technologies, the Interior Department's Office of Saline Water sponsoring Oak Ridge's work in water desalination, or in the work conducted jointly by Argonne National Laboratory and DuPont on neutron diffraction studies of catalysts. Second, there should be a feeling on the part of laboratory executives that

while the organization's mission remains relevant, current programs do not exhaust the organization's capacity to carry it out. Third, there should be few institutional barriers preventing laboratories from taking the broadest view of their mission. Here, the sponsoring agency can play an important part by, for example, drafting a liberal policy of permitting work for others, bringing in scientists and engineers from industry for advice and technical assistance, and improving conditions for cooperative work. Indeed, the removal of obstacles may accomplish more than well-intentioned, but largely fruitless, efforts to stimulate two-party ventures.

In the latter category we would include the various congressional mandates imposed on the National Bureau of Standards in the 1970s, many of which have fallen into desuetude. Or consider the case of technology transfer. In the 1960s, there were two opinions at NASA regarding the best way to transfer technology from Apollo and the larger unmanned programs to commercial users. One view held that technology transfer or utilization could best be promoted by creating an office with direct responsibility for technology utilization, and distinct from the line organization. This was created, but was never quite able to overcome the obstacles to transferring technology that most agencies encounter at some point. The other view held that a program of Apollo's magnitude would inevitably spill over into the commercial sector, stimulating (or some would say, forcing) the growth of data-processing, integrated-circuit, and materials technologies. And this is precisely what happened.

Commercial and government diversification are similar, in that both expand outward from what Peter Drucker has called a "core of unity." The industrial executive, every bit as much as the Federal executive, needs to ask, What business are we really in? (What a company does and what its executives think it does do not always coincide. For decades AT&T executives believed that AT&T was in the business of providing universal phone service. In the 1970s, under pressure from Justice Department, AT&T decided that as the boundaries between telecommunications and data processing blurred, AT&T had to get into nonregulated areas in order to gain a market share in a major industry. It was this which led AT&T to acquiesce in the largest corporate divestiture in history.) As Drucker points out, companies may and do answer this question in very different ways. Sears, Roebuck "is willing to buy anything which the American family needs, whether it be fabric, underwear, life insurance, or garden furniture. As long as the family buys it, it is Sears, Roebuck's business, because Sears, Roebuck understands what the family is as an economic unit, and is the expert buyer for the family . . . At the other extreme, Corning Glass is willing to go into any market, as long as it is based on glass technology. It is in the customer market, it is the largest producer of television tubes — any market, as long as it is glass —

because they understand their technology." (ref. 237.)

We can extend this idea. A successful company's "mission" will normally be greater than any single product or service. It may involve concentrating on *packaged consumer goods* — say, cigarettes, liquor, cookies, hand lotions, staplers (American Brands). Or it may lead to providing a variety of *financial services*, as with American Express or the insurance, realty, and brokerage arms of Sears, Roebuck. For our purposes, the really interesting kind of diversification occurs when a company moves into those *high-growth, advanced technology* areas where it already has, or can gain, a commanding market share. Some companies which have recently gone this route are the General Electric, Gould, and Harris corporations. In each case, these companies sold divisions in which they either had substantial investments or which were their original reasons for being, because they did not fit their chief executives' visions of the future. Thus General Electric, Gould, and Harris sold off, respectively, their natural resources, small appliances, battery, and printing press divisions.*

But the reader may still wonder how a company chooses a particular route toward diversification. Here are two examples. The Eastman Kodak Company is the world's largest producer of photographic products; it was established by George Eastman in the 1880s to bring photography within reach of a mass public, and this is still its primary "mission." All of the company's other product lines are, as it were, branches from this trunk. Because the company needed a reliable source of photo film base, Tennessee Eastman, now a major chemical manufacturer in its own right, was founded. Building on its capabilities in optics, Eastman Kodak began to manufacture scientific instruments; it has built the camera system for Lunar Orbiter and the windows of the Space Shuttle, and ground the main mirror of the Space Telescope. (Rochester, New York, where Eastman Kodak is located, has been for a century one of the centers of the American optical industry.) Eastman Kodak has used its expertise in optics and chemically-coated paper to move, with great success, into high-speed copiers; and it has recently combined its film chemistry and electronics skills to produce very advanced blood-analysis systems. The history of Eastman Kodak is literally a case of one thing leading to another.

Another example of successful diversification within the basic mission pertains to the data-processing units of certain large aerospace

* But note that one company's divestiture is another company's diversification. When General Electric sold its small appliances division to Black and Decker in 1983, it enabled the latter company to expand its manufacturing base from power tools to other lines of household goods.

firms — Boeing, Martin Marietta, McDonnell Douglas, and General Electric, although that company is involved in other areas as well. These units were originally created to handle their parent companies' internal data-processing needs. As excess computer time became available, the companies began to sell some of it to commercial and government users; and through internal growth, acquisitions, and the development of new products, these units have become among the most profitable and fastest-growing within their parent companies.

At the beginning of this section, we said that diversification should have a particular relevance to a laboratory's (or company's) mission. So stated, this advice may appear obvious. But diversification has many turnings and byways; the logic of successful diversification often appears obvious only in retrospect. There is something a little mysterious about the process; a laboratory director may choose one of several courses of action, when others might have appeared, at the time, to be as logical. All we have done here has been to list *preconditions* for successful diversification and give some examples of how the process occurs.

4. *The principal role of technology development laboratories is to strengthen the research and engineering base of new technologies, rather than to serve as managers of large systems or to develop new products.*

Because of the difficulty in distinguishing between technology development and product development, we would stress that the principal role of the technology development laboratories is to strengthen the technological base rather than to do projects or to develop products. But it is also important to understand that occasionally a technology development laboratory should undertake a project — such as Voyager in the case of NASA's Jet Propulsion Laboratory — just as it should conduct basic research. At any given point, there are a great many projects that can be undertaken. The function of the technology development laboratories is to provide technology capability to undertake one or another of these projects. Thus, the Apollo program stemmed from well over half a century of technology development carried out in the laboratories of the National Advisory Committee for Aeronautics and the U.S. Army's Ballistic Missile Agency.

Note, however, that the existence of technology is necessary but not sufficient for carrying through successful projects. It is also necessary to have the proper organization to carry the projects through. The execution of the Apollo program required entirely new approaches to organization and this is, of course, where the Washington headquarters has to play a leading part. As James Webb has written, "It is the new and different way of doing things — of organizing the use of knowledge and technology and human and material resources — rather than the new things themselves that is of most importance in the large-scale endeavor." (ref. 238.) While all of this is true, it is still the creation of new technology that

constitutes the central role of the laboratories. If this is not carried out on a continuing basis, there will be no projects.

The history of the Shuttle program also shows how the laboratories can provide the technology base on which such systems are built. The Shuttle was possible because NASA and its predecessor agency had fifty years' experience of research on the boundaries of flight. The Shuttle was, so to speak, at the interface of aeronautics and space technology. The NASA centers had the test facilities for studying reentry systems, launch vehicle design, the feasibility of an unpowered landing, the separation of the orbiter from the external fuel tank — indeed, all the parameters that defined the Shuttle as an operating system (ref. 239). The design and test of the Shuttle preceded, ran concurrently with, and followed on the manufacture of its components. The experience that centers like Langley, Ames, and Marshall could bring to bear on defining the system was absolutely essential, and it could not have been obtained elsewhere. And this, we believe, is precisely what justifies both the continued existence of large multidisciplinary government research establishments and their commitment to basic and applied research.*

5. The Federal laboratory is best thought of and best administered, as a number of loosely coupled units rather than a classical hierarchical system.

This is probably the least controversial of our conclusions. A top-down style is incompatible with an organization's task when that task is advancing our knowledge of physical processes or applying that knowledge to complex technological systems. Success in mission-oriented work requires that professional staff accept the responsibility for defining their own goals, that there be mobility between basic and applied units, and that (within limits) laboratory executives give division chiefs the freedom to strike out on their own. Here, if anywhere, those laboratories operated under contract are marginally superior to those operated by government employees. Brooks has called attention to the paradox of those civil service laboratories where “. . . the high level of scientific performance of individuals [is] contrasted with the often disappointing results from the organization. A good scientific performance often does not add up to an effective overall performance, partly because of the cumbersomeness of the decision-making process, and partly because of poor communication between the working scientists and

* There also comes a time when a laboratory and its parent agency withdraw from a particular kind of technology development. Under pressure from Congress, NASA, in the early 1970s, withdrew from communications satellite technology. The technology — so many Congressmen thought — was sufficiently advanced for private corporations to develop it still further. In fact, all of the satellites built for INTELSAT have been manufactured by two companies — Hughes Aircraft and Ford Aerospace and Communications.

the headquarters organization that supports and administers his work . . . These faults are by no means confined to government laboratories, but the strongly hierarchical nature of government tends to aggravate the problem." (ref. 240.)

Recall our analysis of the Federal research executive in Chapter IX. We said there that the executive's task is to make a mechanistic organization behave like a living organism. The research executive's role becomes one of sensing or setting the direction in which the organization should move and then bringing the rest of the organization with him. The great laboratory directors have had very different management styles, but they have all shown an ability to set goals which they persuaded their subordinates first to understand, then to accept, and finally to make their own. From this viewpoint, too close a coupling of science and engineering or of basic and applied work might be almost as bad as too little. The organization's mission is compromised when every division has its marching orders or when a tentative move into new areas requires level on level of approval or finally, when Headquarters insists that such moves be embodied in a formal long-range plan.

6. The multifaceted nature of the Federal and private-sector technology establishment is a necessary feature of the system described in this book.

In the United States, research is a function attached to an agency or a company or a laboratory, not a separate institution in its own right. Although the National Science Foundation was created with the mission of fostering basic research, it is well to recall that it has no laboratories of its own and that its mission did not and does not preclude other agencies from conducting their own research. This decentralized system owes its effectiveness to several features: the complex interchanges of personnel among universities, industry, and government laboratories; the parallel investigations of similar problems by different agencies, which is not to be confused with wasteful duplication; the public accountability of presidential advisors; and the high turnover in scientific advisors to agencies and the Executive Office of the President. This decentralized style is different, not only from the way scientific research is sponsored in countries like Soviet Russia, where centralization of science has been the rule since the Russian Academy of Science was established by Peter the Great over two and a half centuries ago, but even from Britain, where a tradition of quasi-permanent scientific administrators has been the rule. This distinction has been neatly drawn by Rose and Rose: "One has only to contrast the two decades and more of science policy-making by Sir Solly Zuckerman with the rapid turnover of scientific advisors to the . . . President, all coming into government science administration from outside and returning to the outside when their term of office is over." (ref. 241.)

From time to time there have been proposals to centralize American science policy, either by establishing a Cabinet-level Department of Science and Technology or by strengthening the authority of the President's Science Advisor. We believe that neither measure is necessary. The reasons for separate research installations at NASA, the Defense Department, the Energy Department, the National Institutes of Health, and the National Bureau of Standards are as different as the missions of those agencies are. A decentralized scheme requires much less information than a centralized scheme would require (ref. 242). The budgetary and authorization cycles provide a bargaining mechanism by which tradeoffs within and between programs can be made.

The majority of American scientific and technical opinion opposes the degree of centralization inherent in the organizational changes mentioned above. Were American science and technology to be concentrated in one Federal department, it would be a reversion to much of what is done in Soviet Russia. The Soviet Academy of Sciences exercises a much tighter degree of control over Russian technology development than any government or non-governmental unit exercises in this country. The great danger in centralization is, of course, that the "innovators" and the "mavericks" will have real trouble in promoting their ideas. One need only compare the fates of Andrei Sakharov in the Soviet Union and J. Robert Oppenheimer in the United States. Both individuals took positions opposed to the orthodox lines of their governments. Sakharov, although he is a member of the Soviet Academy, lives in disgrace and cannot move freely within the Soviet Union or abroad. Oppenheimer, on the other hand, retained his full citizenship rights, was permitted to travel, and was perfectly free to hold high and honored positions in the country's decentralized scientific establishment. He suffered in that he lost his security clearance, but this is by no means a loss which crippled his activities as a scientist, scientific administrator, and public figure. Our decentralized system, therefore, has the virtue that a person can disagree with the established government line but still maintain his standing in the scientific community.

There is one final point that should be emphasized. In research and in technology development we are dealing with that most precious of all commodities, the human imagination. While it is true that the very best people, such as Sakharov, will not be intimidated by any kind of organization structure, we cannot arrange our science and technology around behavior patterns that require genuine heroism. We must see to it that even people who are not in the heroic mold can exercise their talents to make important technical contributions. These people can be more easily stifled, and it is most important that whatever organization is adopted for the conduct of science and technology in the United States, that the imagination be left free to work. Most people involved in

scientific and technical matters in the United States believe that this objective is best accomplished under a decentralized system, where decision-making is not monolithic but yet is well enough organized to make the importance of science and technology felt.

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BIBLIOGRAPHIC ESSAY

There is no book that corresponds precisely to what we have attempted here—namely, to provide an up-to-date account of what a certain kind of Federal laboratory does. In the course of our research, we have drawn heavily on the internal documents of many agencies, our own experience in Federal research establishments, and the printed sources cited below. Our aim in this essay is perhaps less to indicate the scope of our investigations than to encourage readers to explore the ramifications of science and technology policies on their own. Our approach is to go from the general to the more particular. We begin by citing the more accessible works in organization theory, list works dealing with the theory of scientific method and the history of science and technology, and conclude with contemporary accounts of technology development sponsored by the Federal Government.

Theories of Organization. The work of Herbert Simon is indispensable to an understanding of modern organization theory, particularly *Administrative Behavior*, 2nd ed. (New York: Free Press, 1957) and (with James March) *Organizations* (New York: John Wiley & Sons, 1958). Also important is Chester Barnard, *The Functions of the Executive* (Cambridge, Mass.: Harvard University Press, 1938) and its emphasis on informal organization. Peter Blau and Marshall Meyer, *Bureaucracy in Modern Society*, 2nd ed. (New York: Random House, 1971), is a useful introduction to its subject, and it may be supplemented by D.S. Pugh, ed., *Organization Theory* (New York: Penguin Books, 1971) and Hans Gerth and C. Wright Mills, eds., *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1947), which contains essays by the classic theorist of bureaucracy. A work that discusses both organizations and the relations between them is Theodore Caplow, *Principles of Organization* (New York: Harcourt Brace Jovanovich, 1964).

Among the empirical studies of organizations, the following are noteworthy: Philip Selznick, *TVA and the Grass Roots* (Berkeley: University of California Press, 1949); Alfred Chandler, *Strategy and Structure* (Cambridge, Mass.: MIT Press, 1962), uses case studies of DuPont, Sears, Roebuck, General Motors, and Standard Oil to illustrate the transformation of huge centralized organizations into

more decentralized ones; while Thomas Peters and Robert Waterman, *In Search of Excellence* (New York: Warner Books, 1982), have discussed (with a wealth of illustrations) the lessons to be learned from "the best-run American companies." Finally, Herbert Kaufman has contributed three well-written, frequently witty, studies in public administration: *The Forest Ranger* (Baltimore: The Johns Hopkins Press, 1967), which examines the ways in which a large and decentralized organization gains the cooperation of its field officers by means of "preformed" decisions; *Administrative Feedback* (written with Michael Couzens) (Washington, DC: The Brookings Institution, 1973); and *Are Government Organizations Immortal?* (Washington, DC: The Brookings Institution, 1976).

Scientific Method. The most influential philosopher of science in the English-speaking world in this century is probably Karl Popper. His *Logic of Scientific Discovery* (New York: Harper & Row 1968), originally published in Vienna in 1934 as *Logik der Forschung*, and his *Conjectures and Refutations: The Growth of Scientific Knowledge* (New York: Harper & Row, 1965) have as their aim the demarcation of science from metaphysics. According to Popper, a theory is scientific insofar as it is testable or falsifiable or refutable; put differently, "repeated observations and experiments function in science as *tests* of our conjectures or hypotheses, i.e., as attempted refutations" (*Conjectures and Refutations*, p. 53).

Some of the most interesting work in the philosophy of scientific method blends history with analyses of what working scientists do. There is a tradition, dating from the last quarter of the nineteenth century, of scientists examining philosophical problems growing out of their work. The most important figures are Ernst Mach (1838-1916), Heinrich Hertz (1857-1894), Henri Poincaré (1854-1912), and Pierre Duhem (1861-1916), the last-named also being an eminent historian of medieval science. The work of these men is discussed in Peter Alexander, "The Philosophy of Science, 1850-1910," in D.J. O'Connor, ed., *A Critical History of Western Philosophy* (New York: Free Press, 1964), pp. 402-425. More recently, Alfred North Whitehead's *Science and the Modern World* (New York: Free Press, 1967, originally published in 1925) is a beautifully-written account of the evolution of the scientific world view. Thomas Kuhn's *The Copernican Revolution* (Cambridge, Mass.: Harvard University Press, 1957) deals with the role of planetary astronomy in the development of western thought; his *Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970), is the most influential recent analysis of how scientific world views originate, become accepted, and are ultimately supplanted as "normal science."

On the sociology of science and scientific institutions, the following are recommended: Peter Medawar, *The Hope of Progress* (Garden City, N.Y.: Doubleday, 1973) and Joseph Ben-David, *The Scientist's Role in Society* (Englewood Cliffs, N.J.: Prentice-Hall, 1971). Two works dealing with the exponential growth of science and its effects on scientists are Derek de Solla Price, *Little Science, Big Science* (New York: Columbia University Press, 1971) and Henry Menard, *Science: Growth and Change* (Cambridge, Mass.: Harvard University Press, 1971).

On the relation between technology, science, and economic development, Nathan Rosenberg, *Inside the Black Box: Technology and Economics* (New York: Cambridge University Press, 1982), is illuminating. John Jewkes, David Sawers, and Richard Stillerman, *The Sources of Invention*, 2nd ed. (New York: W.W. Norton, 1969), is the most exhaustive review of the causes and consequences of industrial innovation, as well as its relation to applied science. Richard Nelson, Merton Peck, and Edward Kalachek, *Technology, Economic Growth and Public Policy* (Washington, DC: The Brookings Institution, 1967), analyze the way in which technical advances occur, the impact of new technology on the economy, and the changes in governmental policy needed to stimulate technical advances. Finally, David Billington, *The Bridge and the Tower* (New York: Basic Books, 1983), shows how the structural art of the engineer is distinct from both architecture and modern design.

History of Science and Technology (General). In the history of science and technology, there are two basic works in English: George Sarton, *Introduction to the History of Science*, 5 vols. (Baltimore: The Johns Hopkins Press, 1927-1948), and Charles Singer, E.J. Holmyard, and A.R. Hall, eds., *A History of Technology*, 5 vols. (New York: Oxford University Press, 1954-1958). A particularly useful collection of short pieces is Melvin Kranzberg and William Davenport, *Technology and Culture, An Anthology* (New York: New American Library, 1972).

Useful shorter histories of science include: Herbert Butterfield, *The Origins of Modern Science* (New York: Free Press, 1965); Stephen Toulmin and June Goodfield, *The Fabric of the Heavens: The Development of Astronomy and Dynamics* (New York: Harper & Row, 1965); Alexander Koyre, *From the Closed World to the Infinite Universe* (New York: Harper & Row, 1958); A.R. Hall, *The Scientific Revolution 1500-1800: The Formation of the Modern Scientific Attitude*, 2nd ed. (Boston: Beacon Press, 1962); and Robert Merton, *Science, Technology and Society in Seventeenth-Century England* (New York: Harper & Row, 1970).

Finally, we would cite three quite unclassifiable works in the history of science and technology. Lynn White, Jr., *Medieval Technology and Social Change* (New York: Oxford University Press, 1966), is one of the great works in the history of technology. Siegfried Giedion, *Mechanization Takes Command* (New York: W.W. Norton, 1969), is a contribution to what the author calls "anonymous history," a study of the evolution of mechanization and its effects on modern civilization. Pierre Duhem, *Le Systeme du Monde*, 8 vols. (Paris, 1913-1958), is a monumental account of scientific concepts from classical Greece to the Renaissance.

History of Science and Technology in the United States. A. Hunter Dupree, *Science in the Federal Government* (Cambridge, Mass.: Belknap Press of Harvard University Press, 1957), is an indispensable survey of the subject, from Revolutionary times to 1940. H.J. Habakkuk, *American and British Technology in the Nineteenth Century: The Search for Labour-Saving Inventions* (New York: Cambridge University Press, 1967), is an important essay in comparative history. Daniel Kevles, *The Physicists* (New York: Random House, 1979), is, in the words of its subtitle, a "history of a scientific community in modern America." It is an outstanding work of scholarship. In industrial research and technology development, Bell Laboratories has sponsored a monumental history of the organization. To date, three volumes have appeared, all published by Bell Laboratories: M. D. Fagen, ed., *A History of Engineering and Science in the Bell System: The Early Years, 1875-1925* (1975); Members of the Technical Staff, Bell Telephone Laboratories, *A History of Engineering and Science in the Bell System: National Service in War and Peace, 1925-1975* (1978); and A.E. Joel, et al., *A History of Engineering and Science in the Bell System: Switching Technology, 1925-1975* (1982). On General Electric, see Kendall Birr, *Pioneering in Industrial Research: The Story of the General Electric Research Laboratory* (Washington, DC: Public Affairs Press, 1957).

A very useful source book for the post-World War II period is James Penick et al., *The Politics of American Science, 1939 to the Present*, revised ed. (Cambridge, Mass.: MIT Press, 1972). On the wartime mobilization of scientists in general and for the Manhattan Project in particular, the following are important: James Phinney Baxter, *Scientists Against Time* (Cambridge, Mass.: MIT Press, 1968); Irvin Stewart, *Organizing Scientific Research for War: The Administrative History of the Office of Scientific Research and Development* (Boston: Little, Brown, 1948); and Richard Hewlett and Oscar Anderson, Jr., *The New World, 1939-1946* (University Park, Pa.: Pennsylvania State University Press, 1962). The *New*

World book is the first volume of a history of the United States Atomic Energy Commission. The Atomic Energy Commission has since published volumes 2 and 3 of its official history: Richard Hewlett and Francis Duncan, *Atomic Shield, 1947-1952* (University Park, Pa.; Pennsylvania State University Press, 1969) and by the same authors, *Nuclear Navy, 1946-1962* (Chicago: University of Chicago Press, 1974). On the early history of NASA, Arnold Levine, *Managing NASA in the Apollo Era* (Washington, DC: NASA Scientific & Technical Information Branch, 1982) and Homer Newell, *Beyond the Atmosphere: Early Years of Space Science* (Washington, DC: NASA Scientific & Technical Information Branch, 1980), may be consulted. On the National Bureau of Standards, see Rexmond Cochrane, *Measures for Progress: A History of the National Bureau of Standards* (Washington, DC: U.S. Department of Commerce, 1966). Milton Lomask, *A Minor Miracle: An Informal History of the National Science Foundation* (Washington, DC: National Science Foundation, 1976), is a lively history of that agency. In a special category is Harvey Sapolsky, *The Polaris System Development* (Cambridge, Mass.: Harvard University Press, 1972), for its searching analysis of the bureaucratic politics underlying the development of a major weapons system.

Federal Technology Development. Although incidentally concerned with Federal technology development, David Allison, ed., *The R&D Game: Technical Men, Technical Managers and Research Productivity* (Cambridge, Mass.: MIT Press, 1969), is, of all the books listed here, the one closest in purpose to our work. It consists of nineteen essays by the editor and industrial research managers on freedom in research, diversity in research, designing a technical company, and how the U.S. buys research. This book is highly recommended for anyone interested in the nature of industrial technology development. Peter Drucker, *Technology, Management and Society* (New York: Harper & Row, 1977), examines the social implications of the large technology-based corporation.

Don Price has written with an insider's knowledge of science policy in the Federal Government. His *Government and Science* (New York: New York University Press, 1954) and *The Scientific Estate* (Cambridge, Mass.: Harvard University Press, 1965) were among the first books to analyze the political implications of government by contract. The implications of large-scale contracting are further explored in different ways by Clarence Danhof, *Government Contracting and Technological Change* (Washington, DC: The Brookings Institution, 1968), Richard Nelson, *The Moon and the Ghetto* (New York: W.W. Norton, 1977) and Harold Orlans, *Con-*

tracting for Atoms (Washington, DC.: The Brookings Institution, 1967).

On the mechanisms of science policymaking, the following are recommended: Alvin Weinberg, *Reflections on Big Science* (Cambridge, Mass.: MIT Press, 1967); Daniel Greenberg, *The Politics of Pure Science* (New York: New American Library, 1967); Harvey Brooks, *The Government of Science* (Cambridge, Mass.: MIT Press, 1968); Joel Primack and Frank von Hippel, *Advice and Dissent* (New York: New American Library, 1974), and W. Henry Lambright, *Governing Science and Technology* (New York: Oxford University Press, 1976).

Much of the recent policy literature on the role of Federal laboratories has appeared in government publications. The report of the Congressional Research Service, *The National Bureau of Standards: A Review of Its Organization and Operations, 1971-1980*, which was published by the House Committee on Science and Technology in May 1981, is an exhaustive analysis of its subject. Or the multiprogram laboratories of the Energy Department, see the September 1982 report of the Energy Advisory Board, *The Department of Energy Multiprogram Laboratories*, reprinted (along with two volumes of supporting studies) in U.S. House of Representatives, Committee on Science and Technology, Subcommittees on Energy Development and Applications and Energy Research and Production, *The Future of the Department of Energy's Multiprogram Laboratories*, 97th Congress, 1st Session, December 2, 1982. On NASA, see the hearings held by the House Committee on Science and Technology, Subcommittee on Space Science and Applications, *Review of the National Aeronautics and Space Act of 1958*, 98th Congress, 1st Session, October 18-19 and 25-26, 1983. Or the Department of Defense, see Jacques Gansler, *The Defense Industry* (Cambridge, Mass.: MIT Press, 1980).

For statistics on Federal technology development, the official sources to consult are the annual reports of the National Science Foundation on Federal funds for research and development, and the *Statistical Abstract of the United States*, published annually by the U.S. Bureau of the Census.

ABBREVIATIONS AND ACRONYMS FREQUENTLY CITED IN TEXT

AEC	Atomic Energy Commission
DOD	Department of Defense
ERDA	Energy Research and Development Administration
GE	General Electric Company
JPL	Jet Propulsion Laboratory
NACA	National Advisory Committee for Aeronautics
NASA	National Aeronautics and Space Administration
NBS	National Bureau of Standards
OAO	Orbiting Astronomical Observatory
OMB	Office of Management and Budget
PERT	Program Evaluation and Review Technique
R&D	Research and Development
RTOP	Research and Technology Operating Plan
SPO	Special Projects Office

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APPENDIX I

Glossary of Science and Technology Terms*

Development—The systematic use of the knowledge and understanding gained from scientific research directed toward the production of useful materials, devices or methods, including design and construction of prototypes and demonstration of processes.

Engineering—The profession in which a knowledge of the mathematical and natural sciences gained by experience, study, and practice is applied to develop ways to utilize economically the materials and forces of nature for the benefit of mankind.

Innovation—A term used to signify either the product of a complex series of activities, or the process itself. It includes (1) a perception of a problem or opportunity; (2) a “first conception” or invention of an original idea; (3) a succession of interwoven steps of research, development, engineering, design, market analysis, and management decisionmaking; and (4) a “first realization” of “culmination” when an industrially successful thing—a product, industrial procedure, or technique—is first used in an economic, industrial, or social context, and perhaps also the adoption of the process or manufacture of the product by others in competition.

Lead Time—The time between two designated events, the second one generally being an objective or goal. In research and technology development, usually refers to the time between the beginning of a project, like the commitment of funds to develop an airplane, and the project’s successful completion, which may be when

*Source: This glossary is extracted from Congressional Research Service, Library of Congress, *Science Policy, A Working Glossary*, prepared for the Subcommittee on Science, Research and Development, Committee on Science and Astronautics, U.S. House of Representatives, 93rd Congress, 1st Session, July 1973.

a successful prototype flies or when new planes are in mass production. However, the term has come to be applied widely to any preparatory period, decision sequence, or time lag between signal and response.

Mission—A single large operation or task, or a continuing specific function. Examples of missions might include the construction of a number of housing units, capture of a hill, development of a prototype breeder reactor, or achievement of improved pollution control. A distinction may be made between an agency of government performing a continuous or repetitive function such as budgetary control or revenue administration, and an agency responsible for carrying out some one of the missions listed. The latter might be called a “Mission Agency,” but probably not the former.

Operations Research—Defined by Lord Rothschild as “the application of objective and quantitative criteria to decision making previously tackled by experience, intuition, or prejudice.”

PERT—Acronym for Program Evaluation and Review Technique. The concept involves identifying significant actions or accomplishments, identifying actions that must precede these, estimating the time required to accomplish each, and presenting this information graphically (PERT chart) and as a computer printout. The scheme strengthens management by enabling flexible scheduling, identifying long leadtime tasks, and calling attention to problems needing correction. It speeds the process by showing the “critical path” to completion, and identifying the sequence of events that must take place so that management attention can be focused on them.

Program (noun)—A set of actions to implement an agency’s mission, or a major part of the mission; also, a pattern of instructions to a computer.

RDT&E—Research, Development, Testing, and Engineering (or Evaluation). An abbreviation used primarily in the management of military hardware; it covers the spectrum of basic research, applied research and development—including the design and development of prototypes. It extends from initial determination of a strategic requirement for a system with defined performance capabilities to the operational deployment of the system.

Research—Loosely, any gathering of information. More precisely, the gathering, ordering, and analysis of information systematically and according to predetermined criteria. Scientific research—research in accordance with scientific method—is defined by the National Science Foundation as “systematic, intensive study directed toward fuller scientific knowledge of the subject studied.”

Research, Applied—Systematic application of information, systematically acquired and validated. The National Science Foundation defines it as research directed “toward practical application of knowledge—it covers ‘research projects’ which represent investigations directed to discovery of new scientific knowledge and which have specific commercial objectives with respect to either products or processes. By this definition, applied research in industry differs from basic research chiefly in terms of objectives of the reporting company.”

Research, Basic—The systematic acquisition and validation of structured information or knowledge about the universe, employing for the purpose the methods and assumptions of science. In particular, basic research is directed toward a fuller knowledge or understanding of the subject under study, rather than toward the practical application of the knowledge or understanding. One view of this activity stresses that its motivation is curiosity about nature, leading the practitioner “to proceed along sophisticated disciplinary lines as delineated by peer judgment as to the frontier problem areas.” Moreover, “open and free dissemination of the results of such inquiries is an international tradition of the scientific community.”

Basic research is sometimes distinguished from fundamental research, which is “the search for new knowledge in a broad but definite scientific field without reference to specific applications.” Fundamental research is not the pursuit of knowledge for its own sake. It seeks knowledge which is intended to benefit someone someday. But the specific nature of its eventual application is not known at the time the research is performed.

Research, Exploratory—This category of investigation may be thought of as an intermediate stage between basic and applied research. Administratively, exploratory research is defined by the National Science Foundation as “the early stages of research in areas not yet well enough defined or understood to merit full programmatic support.”

Satisfice (verb) — This is an activity of “administrative man” who looks for a course of action that is adequate, reasonably satisfactory, or “good enough.” It may be contrasted with “maximize,” an activity of “economic man” who selects the best alternative from among those available. A significant aspect of satisficing behavior is that administrative man, because he satisfices rather than maximizes, can make his choices without first examining all possible behavioral alternatives and without ascertaining that these are in fact all the alternatives. Herbert Simon introduced the term in his *Administrative Behavior*, 2nd edition (New York: Free Press, 1957), pp. XXIV-XXV.

Science — A term for a broad area of human activity based on the unifying assumption of the universal relationship of effects to causes. It is aimed at discovering, characterizing, organizing, and explaining facts and relations according to principles of systematic and logical thought. Characteristic of science is the method of developing and testing hypotheses through empirical observation, the validation of findings through replication, the construction of orderly taxonomies of related information, and reliance on quantitative measurements employing accepted standards.

The term is loosely applied to encompass not only the activity itself but also the community of practitioners of science, who are also governed by the rules and constraints of science. The term also embraces the products of science, in the form of discovered factual information, laws, concepts, inventions, and even novel artifacts relying on scientific discoveries for their inception.

Science Infrastructure — The institutions necessary for the support of scientific research but which neither perform research nor control it. They include the industries producing instruments, the institutions establishing scientific standards, the institutions and other arrangements for documentation, exchange of scientific communications, interpersonal contacts among scientists, and for the training of technical support personnel in skills required in the laboratory, such as glass-blowing, electronic circuitry, instrument calibration, and the like.

Standards(s) — Units, quantities, procedures, agreed to by consensus or imposed by decree, and available for reference in the reporting of scientific discoveries, in specifications and other procurement documents, and in international or other technical communications of all kinds.

State-of-the-Art—A general term of applied science, engineering, and systems engineering. It refers to the level of useful development in some category of technology; it carries the implication that if design should call for performance requirements or a level of sophistication that exceeds the present stage of development, it will invite a significantly increased level of engineering risk. In general, applied research has the purpose of advancing the state of the art in the subject to which it is addressed, to reduce the engineering risk that might otherwise be involved.

System—This term involves the idea of complex, interrelated elements or components working effectively together in harmony to yield a single desired result. Most systems also involve communications from a central control point, governing the operation of subsystems and reporting back to the control point, at which operating decisions are made (so that the system possesses the capability of self-correction).

The development of a system requires that a complete array of relevant analytical techniques be brought to bear, each contributing to the outcome. The actions resulting from the products of these different analyses are harmonized to produce a coherent structure possessing effectiveness. The sum total of the process described is signified by the term Systems Analysis.

Technology—In its earliest usage, technology signified mechanical tools and implied machinery of various kinds. However, it has come to signify tools and their development and use in the broadest possible sense. It encompasses any systematic employment by man of the cause-and-effect relationship or empirical methods to achieve some desired purpose. The purpose of all technology can be generalized as an attempt to modify in some intended and desired way the relationship of man and his environment.

Accordingly, technology encompasses all basic and applied research, all manufacture and use of products, all knowledge rationally applied to agriculture, biomedicine, applications of the behavioral sciences, and any other rational human actions toward intended results.

Technology, High—A loosely defined and imprecise term that appears to carry the implication that some kinds of technological innovation involve a higher content of scientific output than others. While it is true that some technologies have been developed empirically, it should also be recognized that all technological innovations are amenable to improvement by the systematic application of

science. Perhaps the nearest approach to precision in defining high technology would be: Hardware developments relying extensively or chiefly on recent discoveries of the physical sciences for their operational principle. An equivalent term might be "technology intensive."

APPENDIX II

Chronology of Federal Executive Branch Science Organization, 1787-1976*

1787: Science in the Constitution. The only specific reference to "science" in the Constitution is in Article I, Section 8: "The Congress shall have Power . . . To promote the Progress of Science and useful Arts, by securing for limited Times to Authors and Inventors the exclusive Right to their respective Writings and Discoveries."

July 16, 1798: Provision of medical care for merchant seamen by the Federal Government was authorized by Congress. The first marine hospital constructed with Federal funds was completed in 1800. The U.S. Public Health Service traces its beginning to these hospitals.

February 10, 1807: Coast Survey established under administrative direction of the Secretary of the Treasury by act of Congress.

February 19, 1818: Surgeon General's Office and the Army Medical Department established, with authority to prevent and treat disease and to collect weather data for processing and analysis.

July 4, 1836: Permanent office of commissioner of patents created.

August 31, 1842: By act of Congress a sum of \$25 000 was authorized for a building for the Navy Depot of Charts and Instruments, later the Naval Observatory.

*Sources: This chronology has been extracted from U.S. House of Representatives, Committee on Science and Technology, *A Proposed National Science Policy and Organization Act of 1975*. 94th Congress, 1st Session, 1975, pp. 33-55; and the *Astronautics and Aeronautics* chronologies published by NASA for each year from 1961 through 1976.

August 10, 1846: The Smithsonian Institution was chartered by Congress. Initial endowment came from gift of \$500 000 from James Smithson in 1829.

March 3, 1849: Department of the Interior established, taking over the General Land Office from the Treasury Department, the Office of Indian Affairs from the War Department, and the Pension Office and the Patent Office, which had been independent.

May 15, 1862: U.S. Department of Agriculture established. Among its missions was the systematic application of scientific methods to agriculture. The department was elevated to Cabinet status in 1889.

July 2, 1862: Morrill Act or Land Grant College Act passed providing for establishment in each state of at least one college to provide instruction in agriculture and the mechanic arts. The significance of the act was that it formally recognized the national need for trained manpower in selected fields, and established mechanisms for cooperative Federal and state government participation in financing academic activities related to science and research interests.

March 3, 1863: National Academy of Sciences was established by congressional charter.

June 20, 1878: Coast Survey redesignated Coast and Geodetic Survey.

March 3, 1879: President Hayes signed bill creating U.S. Geological Survey within Department of the Interior.

March 2, 1887: Hatch Act further encouraged scientific agriculture by providing for agricultural experiment stations in the land-grant colleges.

October 1, 1890: Weather Bureau established within the Department of Agriculture.

March 3, 1901: National Bureau of Standards established in Department of the Treasury. The new bureau was given full powers over custody, preparation, and testing of standards and responsibilities for "the solution of problems which arise in connection with standards. . . ." In addition to service to Federal, state, and

municipal governments, the bureau was to provide for a fee standards for nongovernmental units or individuals. The legislation indicated Congress's willingness to provide administrative means for dealing with government science needs. On February 14, 1903, the bureau became part of the new Department of Commerce and Labor.

March 6, 1902: Bureau of the Census was established, giving permanence to an organization for the census in preference to the previous temporary organizations set up every ten years and subsequently allowed to lapse.

April 28, 1904: An act to incorporate the Carnegie Institution of Washington. The objects of the corporation "shall be to encourage . . . investigation, research, and discovery, and the application of knowledge to the improvement of mankind."

August 14, 1912: Under an act, the name Public Health and Marine Hospital Service was changed to Public Health Service. The legislation also authorized the Public Health Service to conduct field investigations and studies and, in particular, investigations of the diseases of man and pollution of navigable streams. The significance of this legislation was that by opening the whole field of public health to research by the government, it was recognized as a legitimate area of Federal activity.

March 4, 1913: Department of Commerce and Labor separated by act of Congress, which created a new Department of Labor.

May 8, 1914: The Smith-Lever Act provided for cooperative agricultural extension work between the agricultural colleges receiving benefits under the Morrill Act. By this act the Extension Service of the Department of Agriculture was put on a separate and permanent basis.

March 3, 1915: The National Advisory Committee for Aeronautics was established by a rider to the Naval Appropriation Act ". . . to supervise and direct the scientific study of the problems of flight, with a view to their practical solution." The sum of \$5 000 a year was appropriated for five years. NACA was the first war research agency of the World War I period.

1916: A National Research Council of the National Academy of Sciences was established to permit a larger part of the scientific com-

munity to assist in research in connection with national preparedness. Approval of the Council by a letter of July 25, 1916 from President Woodrow Wilson to the President of the NAS was formalized by Executive Order 2859 of May 11, 1918.

October 1, 1917: Congress created the Aircraft Board to expand and coordinate the industrial activities relating to aircraft and to facilitate generally the development of air service.

June 10, 1921: Budget and Accounting Act, 1921. Established the Bureau of the Budget, provided for the annual submission of a consolidated Federal budget, and established a General Accounting Office. Henceforth, all Federal agency fund requests including research would have to receive central approval prior to transmission to Congress.

1923: Naval Research Laboratory established. Its legislative basis goes back to initial sums appropriated in 1916 for a laboratory for the Naval Consulting Board.

April 13, 1926: An act amending the Morrill Act of 1862 provided for investment of proceeds of public land sales, the establishment of a perpetual fund, and use of interest from the fund to be applied toward endowment or maintenance of colleges specializing in agriculture and mechanics, "without excluding other scientific and classical studies."

May 20, 1926: Air Commerce Act, 1926. This was the first Federal legislation regulating civil aeronautics. Gave the Department of Commerce wide powers over aviation, including fostering of research to improve air navigation.

March 10, 1928: Authorized \$900 000 to complete transfer of experimental and testing plant of Air Corps to a permanent site at Wright Field, Dayton, Ohio, and for construction of technical buildings.

May 26, 1930: The Ransell Act reorganized, expanded, and redesignated the Hygienic Laboratory as the National Institute of Health.

July 31, 1933: Science Advisory Board under the National Research Council was created by President Roosevelt by Executive Order 6238. The Executive Order authorized the Board, acting

through the machinery and under the jurisdiction of the NAS-NRC, "to appoint committees to deal with specific problems in the various departments."

June 19, 1934: Communications Act of 1934. Created a Federal Communications Commission to regulate interstate and foreign commerce communication by wire or radio.

January 22, 1935: Federal Aviation Commission, appointed by the President as provided in the Air Mail Act of June 12, 1934, submitted its report. It recommended strengthening of commercial and civil aviation, expansion of airport facilities, and expansion of experimental and development work in coordination with NACA.

June 29, 1935: Bankhead-Jones Act provided for the expansion of scientific, technical, economic, and other research into the principles underlying basic problems in agriculture. By appropriating funds for basic research, Congress recognized that its value may exceed that of research on specific problems. Department of Agriculture implementation of the program authorized by this act led to the establishment of regional laboratories located according to problems of that area.

August 9, 1939: Congress authorized construction of second NACA research station at Moffett Field, California, which became the Ames Aeronautical Laboratory.

June 26, 1940: Congress authorized construction of the third NACA laboratory near Cleveland, Ohio, which became Aircraft Engine Research Laboratory. In 1948, it was named for George W. Lewis, NACA Director of Aeronautical Research, 1924-1947.

June 28, 1941: Office of Scientific Research and Development (OSRD) in the Office of Emergency Management was created by President Roosevelt by Executive Order 8807.

July 5, 1945: Dr. Vannevar Bush, OSRD Director, submitted report, *Science the Endless Frontier* to President Truman. This report covered all aspects of the study of post-war science which President Roosevelt had requested him to make in November 1944. A principal recommendation of the report was for the establishment of a National Research Foundation, responsible to the President and to Congress "to develop and promote a national policy for scientific research and scientific education."

August 1, 1946: Atomic Energy Act established the Atomic Energy Commission to be the exclusive owner of all facilities for the production of fissionable materials and of all fissionable material produced. The Commission was made responsible for research and production of atomic energy for military purposes. The act also established the Joint Committee on Atomic Energy, the only joint congressional committee with substantive oversight powers.

August 1, 1946: An act to establish an Office of Naval Research in the Department of the Navy; to plan, foster, and encourage scientific research in recognition of its paramount importance as related to the maintenance of naval power and the preservation of national security; and to provide within the Department of the Navy a single office which, by contract and otherwise, should be able to coordinate and make available to all activities of the Department of the Navy worldwide scientific information and the necessary services for conducting specialized and imaginative research.

October 17, 1946: By Executive Order 9791 President Truman established a Presidential Scientific Research Board under Dr. John R. Steelman to investigate and report on the entire scientific program of the Federal Government, with recommendations for improving the efficiency of Federal research and development.

September-October 1947: The five-volume Steelman Report, *Science and Public Policy*, was issued. The report recommended that the President designate a member of the White House staff for scientific liaison, that the Bureau of the Budget set up a unit for reviewing Federal scientific research and development programs, and that an Interdepartmental Committee for Scientific Research be created. Interdepartmental Committee established by executive order on December 24.

December 31, 1947: OSRD was terminated and remaining personnel, records, and property were transferred to the National Military Establishment. OSRD had served as a high-level coordinating body for scientific research and medical problems related to national defense during World War II.

October 27, 1949: The Unitary Wind Tunnel Act authorized appropriations of \$136 million for new NACA facilities and \$100 million for the establishment of the Air Force Arnold Engineering Development Center at Tullahoma, Tennessee, in recognition of the

fact that industry could not subsidize expensive wind tunnels for research in transonic and supersonic flight.

May 10, 1950: Act established a Federal agency, the National Science Foundation, for the specific purpose of promoting the progress of science in the Nation. The Foundation was directed to carry out its mission by developing a national policy for promoting basic research and education in the sciences. The act was the culmination of a five-year effort to assure that the United States would continue to have a reservoir of research and trained manpower.

April 20, 1951: An eleven-member Science Advisory Committee in the Office of Defense Mobilization, within the Executive Office, was established by President Truman "to advise the President and Mobilization Director . . . in matters relating to scientific research and development for defense."

July 16, 1952: An act authorized the Secretaries of the three military departments to establish advisory committees and appoint part-time personnel necessary for research and development activities and to make five-year contracts to carry out this program. The object of this act was to facilitate the performance of research and development work in the armed forces.

March 17, 1954: President Eisenhower issued Executive Order 10521, which clarified Federal agencies' responsibilities for research and development and specified a broader role for the NSF than that in its 1950 charter by providing that the Foundation "shall from time to time recommend to the President policies for the Federal Government which will strengthen the national scientific effort and furnish guidance toward defining the responsibilities of the Federal Government in the conduct and support of scientific research."

August 30, 1954: The Atomic Energy Act of 1954 amended the Atomic Energy Act of 1946. It facilitated industrial uses of atomic energy, authorized exchange of information with friendly governments, and encouraged formation of an international atomic pool for peaceful purposes.

November 7, 1957: President Eisenhower announced creation of the Office of Special Assistant to the President for Science and Technology, and appointed James R. Killian, Jr. to be his first science advisor.

November 27, 1957: Science Advisory Committee of Office of Defense Mobilization was transferred to the Executive Office of the President and, enlarged and reconstituted, was redesignated the President's Science Advisory Committee. The action was taken to provide a more direct relation between the Committee, the President, and the Special Assistant for Science and Technology.

July 29, 1958: National Aeronautics and Space Act established the National Aeronautics and Space Administration, with NACA as its nucleus, and an advisory nine-member National Aeronautics and Space Council.

August 23, 1958: Federal Aviation Agency created with passage by Congress of Federal Aviation Act. FAA transferred to the Department of Transportation by act of October 15, 1966, which established the Department.

September 2, 1958: National Defense Act of 1958. This was the first general Federal aid-to-education legislation since the Morrill Act of 1862.

March 13, 1959: By Executive Order 10807 President Eisenhower established the Federal Council for Science and Technology, consisting of his Special Assistant for Science and Technology and representatives of the major science-oriented agencies, to promote interagency cooperation in the management of Federal scientific and technological programs. This executive order also specified that the National Science Foundation policy advisory role was to be limited to basic scientific research and education in sciences, rather than scientific research in general. Finally, this executive order abolished the Interdepartmental Committee on Scientific Research and Development.

July 7, 1960: An act authorized the Secretary of the Interior to establish an Office of Coal Research and contract for research to develop better methods of mining, preparing, and utilizing coal.

June 8, 1962: In the absence of congressional disapproval, Reorganization Plan No. 2 of 1962, establishing the Office of Science and Technology in the Executive Office of the President, became effective. The Plan transferred functions from the National Science Foundation to OST, relating to the coordination of Federal policies for the promotion of basic research and education in the sciences.

December 5, 1964: National Academy of Engineering of the NAS-NRC was established, with the adoption by the Council of the NAS of articles of organization making the new Academy a parallel organization.

July 13, 1965: Environmental Science Services Administration established, when Reorganization Plan No. 2 of 1965 became effective. Transferred to the new agency were the Weather Bureau and the Coast and Geodetic Survey.

October 15, 1966: Department of Transportation established by act of Congress. The Secretary of Transportation was authorized to undertake research and development in all modes of transportation.

July 11, 1968: An act authorized the Secretary of Commerce to arrange for the collection of standard reference data for the benefit of scientists and the general public. This act is administered by the National Bureau of Standards.

July 18, 1968: Amendments to the National Science Foundation Act of 1950 constituted the first major amendment of the enabling act. The act enabled NSF to support applied research relevant to its mission and it emphasized the Foundation's responsibility to report on the status of science in the Federal Government. The act also required NSF to obtain annual authorization for its funds, replacing the continuing authorization contained in the organic legislation.

January 1, 1970: President Nixon signed the National Environmental Policy Act of 1969. It established within the Executive Office a three-member Council on Environmental Quality and required the President to submit environmental quality reports to Congress annually.

July 1, 1970: By Reorganization Plan No. 2 of 1970 and Executive Order 11541, the Bureau of the Budget in the Executive Office of the President was redesignated as the Office of Management and Budget.

July 9, 1970: President Nixon sent to Congress Reorganization Plan No. 3 to establish Environmental Protection Agency and Reorganization Plan No. 4 to establish National Oceanic and Atmospheric Administration (NOAA) within Department of Commerce. NOAA would bring together functions of Environmental

Science Services Administration and its major elements with various oceanographic bureaus in several agencies. NOAA was to provide unified approach to problems of oceans and atmosphere and better conservation of marine resources. Both plans took effect on October 3.

October 26, 1970: Legislative Reorganization Act of 1970, approved this date, directed the first major congressional reorganization since 1946. Among the act's provisions were the assignment of review and analytical responsibilities to the General Accounting Office and the redesignation of the Legislative Reference Service of the Library of Congress as the Congressional Research Service.

January 26, 1973: Reorganization Plan No. 1 of 1973 transmitted to Congress. The plan provided for abolishing or transferring out of the Executive Office the Office of Science and Technology and the National Aeronautics and Space Council. The pro forma resignations of the President's Science Advisory Committee preceding the start of new presidential administration were accepted, and new members were not appointed. Plan went into effect on July 1.

May 7, 1974: Act established Federal Energy Administration as an independent executive agency, replacing the Federal Energy Office in the Executive Office of the President.

October 11, 1974: President Ford signed into law the Energy Reorganization Act of 1974, abolishing the Atomic Energy Commission and establishing three new entities in its place: the Energy Research and Development Administration, the Energy Resources Council, and the Nuclear Regulatory Commission. The Energy Research and Development Administration and the Nuclear Regulatory Commission were activated on January 19, 1975.

December 31, 1974: President Ford signed into law the Federal Nonnuclear Energy Research and Development Act of 1974, to establish and conduct a national program of basic and applied research and development, including but not limited to demonstrations of practical applications, of all potentially beneficial energy sources and utilization technologies, within the Energy Research and Development Administration.

May 11, 1976: The National Science and Technology Policy, Organization, and Priorities Act reestablished the post of White House science advisor, a job abolished by President Nixon in 1973.

The act also established four new executive-branch agencies: the Office of Science and Technology Policy, whose director would advise the National Security Council upon request, but would mainly assist the Office of Management and Budget in decisions on funding Federally-supported research and development and would prepare an annual science and technology report for Congress; the President's Committee on Science and Technology, consisting of 8 to 14 specialists in a wide range of fields who would study for two years and report on the nation's science and technology policies, and disband after submitting the report unless the President chose to continue it; a Federal Coordinating Council for Science, Engineering, and Technology, chaired by the Science Advisor; and an Intergovernmental Science, Engineering, and Technology Panel chaired by the Science Advisor to define civilian problems at state, regional, and local levels which could be resolved by science, engineering, and technology.

APPENDIX III

Summaries of Federal Development Laboratories

Department of Energy Multiprogram Laboratories*

Ames Laboratory, Ames, Iowa

The Ames Laboratory conducts basic research in materials and chemical sciences relying upon existing capabilities for preparing high purity metals, alloys, compounds, and single crystals, and conducts materials related research and development in areas such as nondestructive testing of materials. Smaller but unique and vigorous capabilities are maintained in high energy physics, nuclear physics, applied mathematics, coal preparation science, and solar technology. The laboratory enjoys extensive cooperations with Iowa State University through year-round faculty appointment, graduate student training, and facilities sharing programs. An effective summer research participation program for undergraduate students and faculty members (primarily from non-local institutions) is maintained by the laboratory to the mutual benefits of it, the participants, and their institutions. The Ames Laboratory actively disseminates the results of its work throughout the scientific and technological communities by the distribution of technical publications, the giving of invited presentations on technologically important topics, the consulting activities of laboratory staff members, the conduct of cooperative projects with industrial and other governmental concerns, and participation in the Federal Laboratory Consortium.

*Source: Energy Research Advisory Board, Multiprogram Laboratory Panel, *The Department of Energy Multiprogram Laboratories* (Washington, DC: Department of Energy, September 1982), pp. 37-48.

Statistics (FY 1982 Estimated):

DOE Operating Budget Costs: \$15 M
Staffing (Full-Time Equivalent): 427¹
Capital Investment (Plant and Equipment): \$31 M²
Number of Acres: 56

Operating Contractor: Iowa State University

Responsible Operations Office: Chicago Operations Office

Principal Program Activities (FY 1982 Estimated):³

Materials Sciences (42%)
Chemical Sciences (19%)
Fossil Energy (11%)
Biological and Environmental Research (5%)
High Energy Physics (4%)
Nuclear Physics and Nuclear Sciences (2%)
Engineering, Mathematical, and Geosciences (2%)
University Research Support (2%)
Nuclear Energy (1%)
Miscellaneous DOE Programs (5%)⁴
Work for Others (6%)
 Nuclear Regulatory Commission (1%)
 Department of Defense (3%)
 Others (2%)

Argonne National Laboratory, Argonne, Illinois

The Argonne National Laboratory (ANL) provides advanced nuclear technology research and developmental support to the breeder reactor and other fission reactor programs, and maintains a substantial, diversified capability in physical and biological sciences in support of reactor efforts. Future efforts in the fossil area center on advanced process development, the materials technology, and instrumentation and controls system engineering. In basic research, the largest effort is in the area of general materials studies. High energy physics and nuclear physics will continue to play a significant role.

¹Total laboratory staff.

²Current book value.

³Percentages reflect direct staff (full-time equivalent).

⁴Programs less than 1% are aggregated in this category.

The consequences of the utilization of particular energy technologies are addressed through studies of health-related problems and the environment. The laboratory has a program to work closely with university professors on sabbatical leaves and sponsors a variety of programs providing opportunities for students through student-laboratory staff interaction.

Argonne is engaging in a number of activities aimed at improving industrial interactions, including involvement in the joint IRI/National Laboratory Task Force, as well as others.

Statistics (FY 1982 Estimated):

DOE Operating Budget Costs: \$210 M

Staffing (Full-Time Equivalent): 4 538

Capital Investment (Plant and Equipment): \$545 M¹

Number of Acres: 2 245

Operating Contractor: The University of Chicago in cooperation with the Argonne Universities Association

Responsible Operations Office: Chicago Operations Office

Principal Program Activities:

Nuclear Energy (36%)

Basic Energy Sciences (17%)

Conservation and Renewable Energy (9%)

Biology (7%)

Fossil Energy (6%)

High Energy Physics (3%)

Nuclear Physics (3%)

Environmental Protection, Safety, and Emergency Preparedness (2%)

Fusion (2%)

Defense Programs (1%)

Work for DOE (1%)

Miscellaneous DOE Programs (2%)

Work for Others (11%)

 Nuclear Regulatory Commission (5%)

 Department of Defense (1%)

 Others (5%)

¹Includes Argonne National Laboratory West, Idaho.

Major Active Facilities:

Experimental Breeder Reactor II
Hot Fuels Examination Facilities
TREAT Reactor
Zero Power Reactors
Zero Power Plutonium Reactor

Major User Facilities:

Intense Pulsed Neutron Source I
Facility for High Resolution Atomic Spectroscopy
Argonne Tandem Linac Accelerator

Brookhaven National Laboratory, Upton, New York

The Brookhaven National Laboratory (BNL) conceives, develops, constructs, and operates complex large research facilities which are used to study fundamental properties of matter and are shared with university and industrial scientific communities (e.g., the National Synchrotron Light Source and the Alternating Gradient Synchrotron). The laboratory conducts basic and applied research in technology base areas, in support of research facilities, and to discover important new directions for research. Major disciplinary strength is found in the areas of high energy, nuclear and solid state physics, chemistry, and biology. Biological and medical programs include studies of the effects of radiation and of chemical substances involved in the production and use of energy. Other research programs include meteorological dispersion, atmospheric chemistry, pulmonary physiology, and inhalation toxicology. The National Synchrotron Light Source gives the laboratory a unique position in the breadth of sophisticated facilities available to all areas of the physical and biological sciences. Over 1 000 faculty and 160 students have guest appointments enabling them to carry out research at BNL facilities. In addition, there are five different programs which provide 150 students with a variety of training opportunities. The National Synchrotron Light Source will greatly increase the number of faculty and students carrying out research programs at the laboratory. This facility will also be used extensively by industrial scientists; presently, nine of the 29 initial beam lines at the facility are partially or completely supported by the industrial community.

Statistics (FY 1982 Estimated):

DOE Operating Budget Costs: \$125 M
Staffing (Full-Time Equivalent): 3 300

Capital Investment (Plant and Equipment): \$496 M
Number of Acres: 5 265

Operating Contractor: Associated Universities, Inc.

Responsible Operations Office: Chicago Operations Office

Principal Program Activities (FY 1982 Estimated):

High Energy Physics (39%)
Basic Energy Sciences (19%)
Environmental Research and Development (6%)
Life Sciences Research and Nuclear Applications (5%)
Nuclear Physics (3%)
Miscellaneous DOE Programs (10%)
Work for Others (18%)
 Nuclear Regulatory Commission (9%)
 Others (9%)

Major Active Facilities:

National Synchrotron Light Source
Alternating Gradient Synchrotron
High Flux Beam Reactor
Tandem van de Graaff

Major User Facilities:¹

60" Cyclotron
Scanning Transmission Electron Microscope
Central Scientific Computing Facility
Research Hospital

Lawrence Berkeley Laboratory, Berkeley, California

The Lawrence Berkeley Laboratory (LBL) conceives, develops, constructs, and operates complex large research facilities which are shared with university and industrial scientific communities (e.g., Bevalac and Super HILAC). The laboratory capitalizes on its collocation with the University of California to provide a broad range and flexible pool of the scientific talent for research and advanced development with major research programs in high energy physics,

¹ In addition to those facilities listed under Major Active Facilities.

nuclear physics, basic energy sciences, conservation and solar energy, coal research, inertial confinement fusion, geochemistry, computer science, and mathematics. LBL has a role in the training of the next generation of scientists and engineers for careers in advanced energy research and technology. Over 500 resident graduate students conduct thesis research at the laboratory and many more use its facilities or perform collaborative research. Participation in energy research programs provides these students with skills in high demand by energy industries. The flow of Ph.D.s to industry is one of many forms of technology transfer that allows LBL to maintain close and supportive relationships with industry.

Statistics (FY 1982 Estimated):

DOE Operating Budget Costs: \$84 M

Staffing (Full-Time Equivalent): 2 524

Capital Investment (Plant and Equipment): \$195 M

Number of Acres: 70¹

Operating Contractor: The Regents of the University of California

Responsible Operations Office: San Francisco Operations Office

Principal Program Activities (FY 1982 Estimated):

Energy Research (67%)

Conservation and Renewable Energy (8%)

Fossil Energy (2%)

Miscellaneous DOE Programs (4%)

Work for Others (19%)

 Nuclear Regulatory Commission (4%)

 Department of Defense (2%)

 Others (13%)

Major Active and User Facilities:

Bevatron

Super HILAC

88" Cyclotron

High Speed Computers

¹Leased to the Federal Government.

Lawrence Livermore National Laboratory, Livermore, California

The primary role of Lawrence Livermore National Laboratory (LLNL) is the research, development, and test of nuclear weapons designs. Expertise developed in this area is also supplied to related areas of defense research such as inertial confinement fusion, non-nuclear ordnance, and particle-beam technology. LLNL also undertakes research and development technologies of national interest, primarily in the field of energy, where the skill or facilities requirements are unique and synergistic to the on-going weapons effort. As a result the Laboratory has active on-going programs in magnetic fusion energy, laser isotope separation, biomedical and environmental studies, *in-situ* coal gasification and oil shale retorting, energy storage systems, transportation energy conservation, and solar energy research and development. Within these programs, LLNL interacts with both universities and private industry through faculty/student training, consulting, and technology transfer activities.

Statistics (FY 1982 Estimated):

DOE Operating Budget Costs: \$450 M

Staffing (Full-Time Equivalent): 7 200

Capital Investment (Plant and Equipment): \$445 M¹

Number of Acres: 631

Operating Contractor: University of California

Responsible Operations Office: San Francisco Operations Office

Principal Program Activities (FY 1982 Estimated):²

Defense Programs (51%)

Magnetic Fusion (10%)

Laser Fusion Energy (10%)

Isotope Separation (6%)

Energy Programs (3%)

Biomedical/Environmental (3%)

Miscellaneous DOE Programs (3%)

Work for Others (14%)³

 Department of Defense (8%)

¹Current book value (including work in progress less accumulated depreciation).

²Percentages reflect Direct Full-Time Equivalent program effort (excluding Construction/Equipment).

Nuclear Regulatory Commission (3%)

Other Federal Agencies (3%)

Major Active Facilities:¹

Argus Laser Facility

Shiva Laser Facility

Nova Laser Facility (in progress)

100 MeV Accelerator Facility

National Defense Support Livermore Computer Center

Gaseous and Metallurgical Facility (Plutonium and Tritium)

Magnetic Fusion Energy Facility (in progress)

14 MeV Rotating Target Neutron Source

Mirror Fusion Test Facility (MFTF-B) (in progress)

Tandem Mirror Experiment (TMX)

National Magnetic Fusion Energy Computation Center

Fusion Target Development Facility (in progress)

High Explosive Flash Radiograph Facility

Weapons Materials R&D Facility (in progress)

Management Support Center—Core 1

High Explosive Application Facility (in progress)

Advanced Isotope Separation Facility (in progress)

Advanced Test Accelerator Facility (in progress)

Large Optics Diamond Turning Machine (in progress)

High Explosive Processing and Test Facility

Los Alamos National Laboratory, Los Alamos, New Mexico

The Los Alamos National Laboratory's (LANL) primary mission is the application of science and technology to problems of national security, that is, to the maintenance of a strong defense, the fulfillment of arms control commitments, and the guarantee of a secure energy supply for the future. In carrying out this mission, the laboratory serves as a liaison between research in academia and industry and complements the activities of both. Major programs include developing nuclear warheads, designing and testing advanced technology concepts, and maintaining an innovative weapons design program. In nuclear fission, programs include separating isotopes of uranium by laser induction, testing advanced reactor fuel elements, modeling reactor accident results for the Nuclear Regulatory Commission, evaluating biomedical consequences of nuclear energy pro-

¹ \$5 million or more acquisition cost.

duction, and developing effective nuclear waste disposal methods. In nuclear fusion, programs include magnetically confined plasma devices (especially high-beta devices), laser-induced fusion, and safe handling techniques for large quantities of tritium. Other programs include nonnuclear energy, basic energy science, and technology utilization.

Statistics (FY 1982 Estimated):

DOE Operating Budget Costs: \$420 M

Staffing: 6 770¹

Capital Investment (Plant and Equipment): \$322 M²

Number of Acres: 27 500

Operating Contractor: University of California

Responsible Operations Office: Albuquerque Operations Office

Principal Program Activities:

Weapons (48%)

Nuclear Physics (8%)

Magnetic Fusion (4%)

Advanced Isotope Separation Technology (4%)

Verification and Control (3%)

Breeder (3%)

Safeguards (2%)

Nuclear Materials Production (2%)

Miscellaneous DOE Programs (11%)

Other DOE (7%)

Work for Others (8%)

 Nuclear Regulatory Commission (2%)

 Department of Defense (3%)

 Other (3%)

Major Active Facilities:

800 Milton Electron-Volt Linear Proton Accelerator (LAMPF)

Weapons Neutron Research Facility

Stable Isotopes Production Facility

20 Terrawatt CO₂ Gas Laser Facility

8 Megawatt Nuclear Reactor

¹Figures are based on full-time regular employees.

²Current book value (excludes construction work in progress).

Plutonium Research Facility
Plutonium Heat Sources Fuel Production Facility
National Security Resources and Studies Center

Major User Facilities:

Clinton P. Anderson Meson Physics Facility (LAMPF)
Central Computing Facility
Hot Dry Rock Project (Fenton Hill)

Oak Ridge National Laboratory, Oak Ridge, Tennessee

The Oak Ridge National Laboratory (ORNL) primarily supports the fission nuclear fuel cycle and magnetic fusion energy development (advanced toroidal devices, such as Tokomak and Elmo Bumpy Torus) through scientific research and technology; identifies and solves generic research problems in energy technologies such as materials, separation techniques, chemical processes, and biotechnology; and is the major national source of production of stable and radioactive isotopes. Energy technology development at ORNL also includes other important efforts in residential and commercial energy conservation, renewable energy sources, and coal conversion and utilization. ORNL expends special efforts to involve industry in its programs and to encourage cooperative uses of facilities, both formally in users' groups and informally through professional contacts and participation. Similarly, the Laboratory provides universities with ready access to major research facilities, state-of-the-art research capabilities, training facilities for faculty and students, and an opportunity for collaborative research in areas where these sources are not available to universities.

Statistics (FY 1982 Estimated):

DOE Operating Budget Costs: \$278 M¹
Staffing (Full-Time Equivalent): 4 950
Capital Investment (Plant and Equipment): \$250 M²
Number of Acres: 10 270

Operating Contractor: Martin Marietta Corporation

Responsible Operations Office: Oak Ridge Operations Office

¹Costs based on the FY-82 column of Oak Ridge Operations Office's FY-83 budget.

²Current book value of plant and equipment including construction work in progress as of September 30, 1981.

Principal Program Activities:¹

Fission Energy Development (24%)
Basic Physical Science (21%)
Biomedical and Environmental Sciences (12%)
Fusion Energy (9%)
Conservation and Renewable Energy (5%)
Fossil Energy (4%)
Miscellaneous DOE Programs (6%)
Work for Others (19%)
 Nuclear Regulatory Commission (8%)
 Department of Defense (1%)
 Other (10%)

Major Active Facilities:

Isotopes Separation Facility
High Flux Isotope Reactor
Electron Linear Accelerator
Transuranium Processing Plant
Oak Ridge Research Reactor
Oak Ridge Isosynchronous Cyclotron
High Level Radiochemical Laboratory
Materials Processing Laboratory
Thorium Uranium Fuel Recycle Facility
High Radiation Level Examination Laboratory
Environmental Sciences Laboratory

Major User Facilities:

Holified Heavy Ion Research Facility
EN-Tandem Accelerator
Oak Ridge Electron Linear Accelerator (ORELA)
Neutron Scattering Facility
National Center for Small-Angle Scattering Research
Ion-Solid Interactions Laboratory
Shared Research Equipment Program (SHARE)

Pacific Northwest Laboratory, Richland, Washington

The mission of the Pacific Northwest Laboratory (PNL) is the advancement and use of multi-energy scientific and engineering

¹ Percentages reflect direct staff full-time equivalents.

technology for national security and the nation's energy needs. In accomplishing the mission, high priority is given to safe and reliable nuclear energy, and to promising new concepts and technologies for operations at Hanford. Areas of disciplinary strengths and concentration include nuclear waste process development, nuclear fuel development and evaluation, spent fuel storage reprocessing, inhalation toxicology, bioelectromagnetics, biology, ecology, atmospheric sciences, corrosion chemistry, biomass research, nondestructive testing and evaluation, materials characterization and research, chemical engineering, health physics technology, and energy economics and policy analyses. The laboratory provides a core capability for effectively utilizing the strengths of the universities and the private sector and has traditionally developed and maintained strong ties with the academic community. That resource is utilized through cooperative exploratory research efforts to accelerate application of new ideas, scientific research support to PNL efforts on team assignments, and exchange of staff and student research. Industry and utilities participate with PNL staff in efforts to develop advanced technology, to improve existing instruments or develop new ones, and to investigate and demonstrate energy conservation and conversion techniques.

Statistics (FY 1982 Estimated):

DOE Operating Budget Costs: \$98 M

Staffing (Full-Time Equivalent): 1 975

Capital Investment (Plant and Equipment): \$65 M

Number of Acres: Not applicable¹

Operating Contractor: Battelle Memorial Institute

Responsible Operations Office: Richland Operations Office

Principal Program Activities (FY 1982 Estimated):

Fission (25%)

Defense (17%)

Life Sciences (16%)

Conservation (7%)

Renewable (7%)

Basic Sciences (5%)

¹PNL facilities are in an area shared with Hanford Engineering Development Laboratory and UNC Nuclear Industries.

Miscellaneous DOE Programs (3%)
Work for Others (20%)
 Nuclear Regulatory Commission (15%)
 Department of Defense (3%)
 Others (2%)
Biomass Experimental Unit
Geophysical and Astronomical Observatory
National Environmental Research Park

Major Active Facilities:

Two Life Science Laboratories
Marine Research Laboratory
Meteorological Center
Critical Mass Laboratory
Nuclear Waste Vitrification Laboratory
Materials Reliability Center
Steam Generator Examination Facility

Major User Facilities:

Critical Mass Laboratory

Sandia Laboratories: Albuquerque, New Mexico; Livermore, California; Tonopah, Nevada

Sandia Laboratories' principal mission has been and remains research, development, and engineering of nuclear weapon systems, except for the nuclear explosive. The result of this effort is the existence of a national stockpile of nuclear weapons that are safe, secure, reliable, under strict control, and operationally ready. Sandia is also responsible for major energy programs in fossil, solar, fission, and basic energy sciences. Its strength lies in its ability to conduct large interdisciplinary engineering projects that are both technologically "risky" and sophisticated. Fundamental understanding and technological development are melded to create an environment conducive to generating new products and processes that are unlikely to be produced in either university or industrial laboratories. These new technologies are transmitted to the private sector through journals, workshops, and other information transfer mechanisms, and significantly contribute to the nation's scientific and economic prosperity.

Statistics (FY 1982 Estimated):

DOE Operating Budget Costs: \$610 M
Staffing (Full-Time Equivalent): 7 950
Capital Investment (Plant and Equipment): 208 M¹
Number of Acres: 3 140 (Albuquerque), 185 (Livermore)

Operating Contractor: Western Electric Company

Responsible Operations Office: Albuquerque Operations Office

Principal Program Activities:²

Defense Programs (67%)
Conservation and Renewable Energy (6%)
Nuclear Energy (5%)
Fossil Energy (3%)
Energy Research (3%)
Environmental Protection, Safety, and Emergency Preparedness
(1%)
Work for Others (15%)
 Department of Defense (8%)
 Nuclear Regulatory Commission (5%)
 Others (2%)

Major Active Facilities:³

Particle Beam Fusion Facility
Nuclear Safeguards and Security Facility
Combustion Research Facility
5 Megawatt Solar Central Power Test Facility
Center for Radiation-hardened Microelectronics
Tonopah Test Range
Radiation/Simulation Facilities

Major User Facilities:

Combustion Research Facility
Solar Central Power Test Facility
Tonopah Test Range
Radiation/Simulation Facilities

¹Current book value (excludes construction work in progress).

²Percentages reflect direct staff (full-time equivalent).

³Major construction line items only.

National Bureau of Standards: Gaithersburg, Maryland; Boulder, Colorado

National Measurement Laboratory

The Laboratory provides the national system of physical and chemical measurement, coordinating the system with measurement systems of other nations and furnishing essential services leading to accurate and uniform physical and chemical measurement throughout the Nation's scientific community, industry, and commerce. It conducts materials research leading to improved methods of measurement, standards and data on the properties of materials needed by industry, commerce, education institutions, and government; provides advisory and research services to other government agencies; and develops, produces, and distributes standard reference materials.

National Engineering Laboratory

The Laboratory provides technical service to promote development and use of technology and to facilitate technological innovation in industry and government; cooperates with public and private organizations in developing technological standards and test methods; and provides technical advice and services to government agencies upon request. It conducts research in support of the specific objectives of these activities; monitors NBS engineering standards activities; and provides liaison between NBS and national and international engineering standards bodies.

Institute for Computer Sciences and Technology

The Institute develops and recommends uniform Federal automatic data processing standards; provides automatic data processing scientific and technological advisory services to Federal agencies; and undertakes necessary research in computer science and technology.

Statistics (FY 1982 Estimated):*

Staffing (Full-Time Equivalent): 2 665 (2 280, Gaithersburg; 385, Boulder)

*Sources: General Accounting Office, *Information on Mission and Functions of the National Bureau of Standards* (CED-81-39, April 22, 1981), p. 61; and U.S. Department of Commerce, "National Bureau of Standards" (NBS Special Publication 643, February 1983), p. 21.

Total NBS Operating Budget Costs (in millions of dollars):	
Measurement and Engineering Research and Standards:	\$157.1
Measurement research and standards	77.3
Engineering measurements and standards	45.2
Computer sciences and technology	13.2
Core measurement research for new technologies	14.0
Fire research	7.4
Competence and Central Technical Support:	15.8
Technical competence fund	7.4
Central technical support	8.4
Total NBS	172.9

NASA Centers (including the Jet Propulsion Laboratory)*

Ames Research Center, Moffett Field, California

Established in 1940, Ames Research Center (ARC) operates in two locations. The Ames North location is on 423.5 acres at the southwest end of San Francisco Bay on land contiguous to the U.S. Naval Air Station, Moffett Field. Also housed at this location is the U.S. Army Research and Technology Laboratory. The Ames Dryden Flight Research Facility is 102 air kilometers northeast of Los Angeles. Dryden is located at the north end of Edwards Air Force Base on 521 acres of land under a permit from the Air Force.

The programs at ARC involve research and development in the fields of aeronautics, life sciences, space sciences and applications, and space technology. Specifically, the center's major program responsibilities are concentrated in: theoretical and experimental fluid mechanics and aerodynamics, rotorcraft technology, high-performance aircraft technology, flight simulation, flight testing, computational fluid dynamics, fluid and thermal physics, space sciences, airborne sciences and applications, human factors, space biology, and ground and flight projects in support of aeronautics and space technology. In addition to these major program responsibilities, the center provides major support for military programs.

Statistics (1983 Actual):

NASA Operating Budget Costs: \$107 M
 Staffing (Civil Service Workyears): 2 141
 Capital Investment (Plant and Equipment): \$797 M

*Source: National Aeronautics and Space Administration, *Budget Estimates, Fiscal Year 1985*, Vol. III.

Goddard Space Flight Center, Greenbelt, Maryland

The Goddard Space Flight Center (GSFC), located 24 kilometers northeast of Washington, DC, is situated on a 552-acre main site. Three additional nearby plots of 554 acres comprise the remote site area and contain the Goddard Antenna Test Range, the Goddard Optical Facility, the Propulsion Research Facility, the Laser Facility, the Magnetic Fields Component Test Facility, the Attitude Control Test Facility, and the Network Training and Test Facility. The center also utilizes an additional 6 165 acres at the Wallops facility located on the Atlantic coast of Virginia's eastern shore. The Wallops facility consists of 1 833 acres of the main base, 3 085 acres on Wallops Island launching site, 107 acres on the mainland tracking site, and 1 140 acres of marshland.

The majority of GSFC's personnel are located at Greenbelt. Other personnel are located at the Wallops facility, the Goddard Institute for Space Studies in New York, and tracking and communications network stations throughout the world.

GSFC, established in 1959 as the first major United States installation devoted to the investigation and exploration of space, conducts a wide-ranging program in space science and applications. GSFC has many capabilities: the management of complex projects; the development of wholly integrated spacecraft, ranging from systems engineering to development, integration, and testing; the development and operation of both the ground network of tracking and data acquisition facilities and the Tracking and Data Relay Satellite System; scientific research to include both theoretical studies and the development of experiments flown on satellites; and the operation of a research airport, located at Wallops, in support of NASA's aeronautics research programs.

Statistics (1983 Actual):

NASA Operating Budget Costs: \$181 M

Staffing (Civil Service Workyears): 3 709

Capital Investment (Plant and Equipment): \$881 M

Jet Propulsion Laboratory, Pasadena, California

The Jet Propulsion Laboratory (JPL) is located in Pasadena, approximately 32 kilometers north of downtown Los Angeles, with subsidiary facilities at Goldstone, California (tracking and data acquisition), Edwards Air Force Base, California (propellant formulation and testing), and Table Mountain, California (open-air testing and astronomy). The main facility, at Pasadena, occupies 176 acres, of which 156 acres are owned by NASA and 20 acres are leased.

JPL is a government-owned installation that is staffed and managed by the California Institute of Technology. Contract NAS7-918 between NASA and Caltech governs research, development, and related activities at the Laboratory, with facilities being provided under a separate contract.

JPL is primarily responsible for conducting NASA automated missions concerned with deep space scientific exploration; tracking, data acquisition, reduction, and analysis required by deep space flight; and developing advanced spacecraft propulsion, guidance, and control systems. The Laboratory is also responsible for selected automated Earth-orbital projects. In carrying out its mission, JPL operates the Deep Space Network, which provides tracking and data acquisition services for all NASA projects involving missions beyond near-Earth orbits.

Statistics (1983 Actual):

JPL Operating Budget Costs: \$208 M
Staffing (Permanent Workyears): 3 426
Capital Investment (Plant and Equipment): \$486 M

Lyndon B. Johnson Space Center, Houston, Texas

The Johnson Space Center (JSC) is located approximately 32 kilometers southeast of downtown Houston, Texas. Total NASA-owned land at the Houston site consists of 1 620 acres, with an additional 54 080 acres at the White Sands Test Facility, Las Cruces, New Mexico.

JSC was established in 1961 to manage the design, development, and manufacture of manned spacecraft; for selection and training of astronaut crews; and for the conduct of manned spaceflight missions. JSC's principal and supporting roles include the development of manned space vehicles and associated supporting technology; responsibility for systems definition and engineering of the Space Station; conducting medical research to solve space medicine problems; defining in-flight biomedical experiments; maintaining the technical discipline base for lunar and planetary sciences, as well as planetary materials handling techniques; and providing a discipline base for resource observations applications, including airborne experiments and space-based flight sensors.

Statistics (1983 Actual):

NASA Operating Budget Costs: \$195 M
Staffing (Civil Service Workyears): 3 411
Capital Investment (Plant and Equipment): \$982 M

John F. Kennedy Space Center, Cape Canaveral, Florida

The John F. Kennedy Space Center (KSC) is located 80 kilometers east of Orlando, Florida. The total land and water area occupied by KSC is 139 305 acres. NASA owns 82 943 of that total, with the rest consisting of land occupied under various easements and deeds of dedication.

Space Shuttle flights began at KSC in 1981 and will begin at Vandenberg Air Force Base, California in 1985. Expendable launch vehicle operations are conducted at both the Air Force's Eastern Space and Missile Center, at Cape Canaveral Air Force Station, Florida, and the Western Space and Missile Center at Vandenberg Air Force Base.

The principal roles of KSC are: (1) Space Transportation System Ground operations—this includes Space Shuttle launch preparation, launch, landing and refurbishment, Spacelab and Spacelab payloads ground processing, cargo/experiment integration and processing, upper stages ground processing, and operation and maintenance of ground support equipment; and (2) Expendable Launch Vehicle operations—includes launch preparation, checkout and launch for the current inventory of launch vehicles.

Statistics (1983 Actual):

NASA Operating Budget Costs: \$161 M

Staffing (Civil Service Workyears): 2 190

Capital Investment (Plant and Equipment): \$2 563 M

Langley Research Center, Hampton, Virginia

The Langley Research Center (LaRC) was established in 1917. LaRC, which is situated in the Tidewater area of Hampton Roads, utilizes 807 acres of government-owned land, divided into two areas by the runway facilities of Langley Air Force Base. Runways, some utilities, and certain other facilities are used jointly by NASA and the Air Force.

LaRC's principal roles and missions include the following: (1) developing a technology base for improving transport aircraft, as well as general aviation and commuter aircraft; (2) advancing the state of the art in fundamental aerodynamics; (3) conducting research related to reducing aircraft noise; (4) developing a technology base in aerospace vehicle structures and materials; (5) developing a technology base for advanced vehicle transportation systems, large space antennas, and space station systems; (6) developing space technology experiments in areas such as materials, struc-

tures, control, and dynamics of large space structures and advanced transportation systems; and (7) developing improved techniques for atmospheric sensing.

Statistics (1983 Actual):

NASA Operating Budget Costs: \$133 M
Staffing (Civil Service Workyears): 2 937
Capital Investment (Plant and Equipment): \$708 M

Lewis Research Center, Cleveland, Ohio

The Lewis Research Center (LeRC) occupies two sites in Ohio. The original site, established in 1941, adjacent to the Cleveland-Hopkins International Airport, includes 366 acres. There are over 170 buildings and structures, including wind tunnels, test chambers, laboratories, and other research facilities. The Plum Brook Station, established in 1956, is located south of Sandusky, about 80 kilometers west of Cleveland. There are 8 005 acres owned by NASA. During 1975, the principal facilities were placed on standby. Since then a number of Federal, state, and local government agencies have utilized office space and other facilities.

LeRC was established as an aircraft engine research laboratory to develop superior aircraft propulsion systems. Since then, LeRC has developed and constructed many unique facilities for testing full-scale aircraft engines and engine components, chemical rocket engines, electric propulsion systems, space and terrestrial power systems, and space communication systems. LeRC's principal roles include: (1) Maintaining a national capability in fundamental aeropropulsion disciplines (fluid dynamics, internal unsteady aerodynamics, fluid mechanics, propulsion and power transfer technologies, fuels and combustion chemistry, and kinetics); (2) managing the Atlas and Centaur launch vehicle systems; (3) developing and managing the cryogenic upper stage of the Space Shuttle; (4) developing the technology for advanced space propulsion systems; and (5) developing the technology for space power and energy conversion systems.

Statistics (1983 Actual):

NASA Operating Budget Costs: \$119 M
Staffing (Civil Service Workyears): 2 700
Capital Investment (Plant and Equipment): \$505 M

George C. Marshall Space Flight Center, Huntsville, Alabama

Operations at Marshall Space Flight Center (MSFC) are conducted at three locations.

The principal MSFC site is near Huntsville, on Army property at the Redstone Arsenal. The center occupies 1 841 acres under a non-revocable use permit from the Army. The Huntsville location is connected by deep-water access to its component Michoud Assembly Facility via the Tennessee, Ohio, and Mississippi Rivers.

The Michoud Assembly Facility is located 24 kilometers east of New Orleans, Louisiana, where the external tank for the Space Shuttle is being produced. The Michoud Facility occupies 832 acres and provides 3 634 344 gross square feet of space, including the main assembly plant. The Slidell Computer Complex, located at Slidell, Louisiana, 32 kilometers northeast of the Michoud Assembly Facility, occupies 14 acres and provides centralized computer services for MSFC, Michoud, other NASA centers, and associated contractors.

MSFC serves as one of NASA's primary centers for the design and development of Space Transportation Systems, orbital systems, scientific and applications payloads, and other systems for present and future space exploration. MSFC has the principal role within NASA for rocket propulsion systems. MSFC also has a principal role for designing and developing manned vehicle systems; for Spacelab mission management and payload definition; for designing and developing specialized automated spacecraft; and managing space processing activities. MSFC has a primary role for developing and processing space and applications experiments. Additionally, MSFC conducts a vigorous research and technology program and is involved in studying future programs in such areas as space propulsion systems, materials engineering, materials processing in space, and payload systems analysis and integration.

Statistics (1983 Actual):

NASA Operating Budget Costs: \$184 M

Staffing (Civil Service Workyears): 3 451

Capital Investment (Plant and Equipment): \$808 M

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