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R&D ORGANIZATIONS AND RESEARCH CATEGORIES

> ***Clockmakers were the first consciously to apply the theories of mechanics and physics to the making of machines. Progress came from the collaboration of scientists—Galileo, Huygens, Hooke, and others—with craftsmen and mechanics.***
>
> **DANIEL J. BOORSTIN**
> ***The Discoverers***

The historic collaboration between scientists and craftsmen to create the clock, which Boorstin calls "the mother of machines," represents a rudimentary R&D organization.

Today the complexity of the technology has created correspondingly complex organizations, with sometimes hundreds of employees. Many disciplines have to be coordinated and it's the manager who brings the many components together so they can function smoothly, each making an optimal contribution to the R&D organization. Thus, today, as in the past, progress requires collaboration.

Managing a research and development (R&D) organization is, to a great degree, the art of integrating the efforts of its many participants. Beyond this, the manager has to provide order, purpose, and foresight and do this while dealing intelligently with the uncertainty inherent in an R&D enterprise. Considering the important role R&D plays in the economic well-being of a nation, the profitability of a business enterprise, the effectiveness of a technology-based governmental agency (e.g., the Department of Defense), and the enormous investment nations make in R&D activities ($355 billion in the Unites States in 2007), effective R&D management can have profound and far-reaching consequences. Effective management, coupled with a vigorous research and science policy, is necessary for a nation to sustain economic growth, provide a strong national defense at an affordable cost, and maintain a position of leadership in the international community. It is therefore important to understand R&D organizations and their relationship to society. For this reason, the first chapter provides some basic definitions of research categories and research organizations and chapter 17 covers macro issues related to R&D and science policy. This information should be useful to those who conduct and manage research, and especially to those who seek funding support for research and who want to develop allies in influencing science policy.

This chapter first provides a perspective on R&D management and then discusses research and development definitions and categories. Sections that follow examine the question: What to research? This is in some respects a key question for an R&D manager. To what extent, for instance, should the manager allow basic research to be done in addition to the applied research needed by the organization? What is the best way to establish priorities among competing research projects? There are numerous suggestions in the literature on how to do that. Since a question is often raised as to what is so unique about an R&D organization management, a discussion of this issue is included in this introductory chapter.

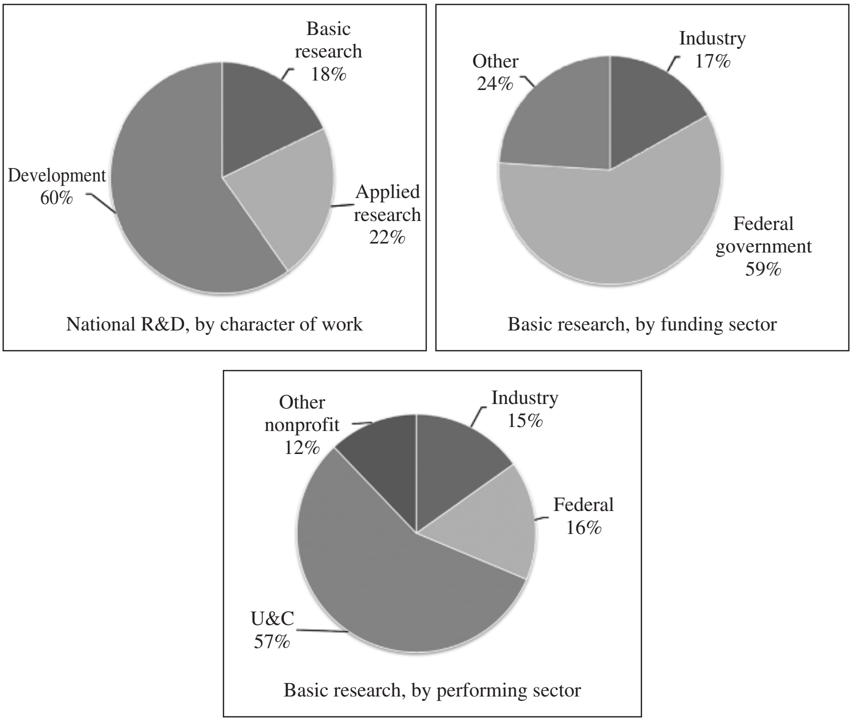
1.1 HOW INFORMATION CAN BE USED

Some readers may want to take a cursory look at the information presented in this chapter and keep in mind how some of it may help them. In addition to having important implications for R&D management, this information has other possible uses as well. Some examples follow.

As a principal investigator (PI), if you are interested in being involved primarily in basic research, in what kind of an organization should you be seeking employment? If you're working in industry you should not be too surprised if you are required to focus your efforts on "products and profits." As shown in Figure 1.1, on the average, 60 percent R&D is focused on product development, 22 percent on applied research, and only about 18 percent R&D is devoted to basic research. Expenditures on R&D by source and performer are shown in Figure 1.2.

In this chapter, indeed in this book, we argue that, in a productive and effective research organization, a researcher should have a mix of activities including basic, applied, and product development research. Examples of successful organizations and results of studies conducted are provided to support this assertion. For a manager of an R&D organization interested in productivity and effectiveness, understanding this issue is crucial and has important managerial implications. If we are successful in persuading you to include basic research in your mix of activities, even if your organization focuses on product development, would you not use the information in this chapter to persuade corporate decision-makers to allow this flexibility?

Is there any R&D manager who has not been accused of being unresponsive to customer needs and of focusing on esoteric, nonproductive research activities? Throughout this book a strong case has been made for customer



U&C = universities and colleges
Notes: National R&D expenditures projected at $340 billion in 2006. Federal performers include federal agencies and federally funded research and development centers. Figures rounded to the nearest whole number. Due to rounding detail may not sum to totals.

Figure 1.1. Relative Distribution of U.S. R&D Expenditures by Source, Performer, and Character of R&D: 2006
(Source: *Science and Engineering Indicators 2008*, p. 4-15.)

participation in needs assessment and in the innovation process. The issue is much broader.

Let us consider an R&D organization that works only on those research needs identified by the customer. Would such an organization not be working on yesterday's, or, at best, today's problems in a very narrow framework? Using this approach, during World War II, would researchers have been working on bigger and better binoculars to detect incoming airplanes rather than on developing radar?

We propose a two-tier model, which includes an economic index model and a portfolio model that should overcome some of these difficulties. Further, a systematic and a conceptual approach for prioritizing potential projects is presented. Depending on the organizational setting and the decision-makers involved, this approach provides a crucial mechanism for research project selection and

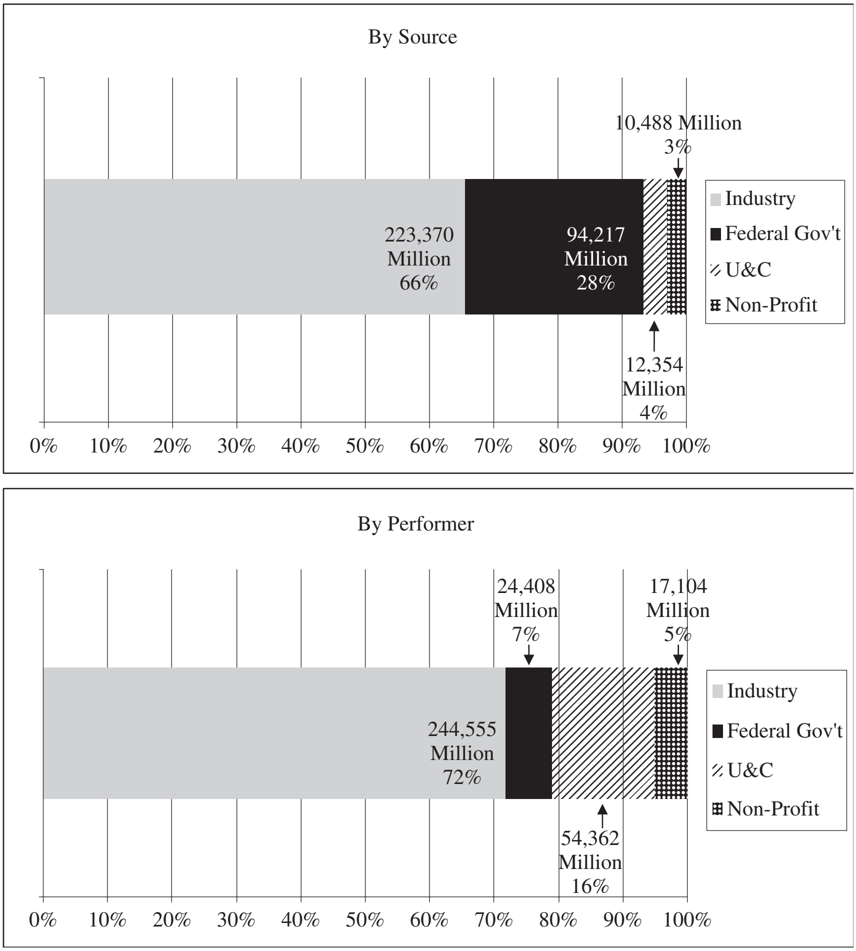


Figure 1.2. The National R&D Effort
(Source: http://www.nsf.gov/statistics/seind04/c4/tt04-01.htm (table 4-1). [*Science and Engineering Indicators, 2004*.])

effective decision-making. By being systematic, it also gives psychological comfort to the decision-makers.

Oh yes, how about these mundane definitions! Anyone involved in research knows them, or should know them. Maybe so. Careful reading would show that there are some key points brought out that are not commonly appreciated. For example, what really differentiates basic research from applied research? Basic research is not inevitably unapplied. Differences lie elsewhere. If nothing else, these definitions may facilitate communication among the various actors involved in conducting and sponsoring research.

1.2 A PERSPECTIVE ON R&D MANAGEMENT

The ideas presented in this book focus on ways to improve the productivity of R&D organizations and foster excellence in such organizations. The book is primarily aimed at principal investigators, their colleagues, and supervisors. As indicated, others may also find the information presented here interesting.

In mathematics or physics, most concepts can be readily judged as useful or worthless. Management concepts, on the other hand, are more difficult to evaluate. The following example might illustrate the case.

One well-known scientist was recruited to be vice president of a biotechnology company. In trying to prepare for this important new position he took a course at the California Institute of Technology on "Managing Research and Development." After completing the course the scientist felt that it had failed to teach him how to prioritize and manage research projects. On his evaluation he stated that the course had been "expensive and worthless." In response to this criticism, the course program director pointed out that the scientist had "completely misunderstood the goals of the course." According to the director, the course was geared toward planning research and development activities rather than managing scientists (*Wall Street Journal*, November 10, 1986).

Managing researchers is one of the most daunting tasks a manager can undertake. It's not clear how one plans or anticipates a "scientific breakthrough." If this is the case, is there any point in undertaking extensive efforts in strategic planning or doing any planning at all? Scientists are thought to be dedicated to ideas and research. However, as shown in Figure 1.1, a majority of the R&D is devoted to product development and applied research, and less to basic research. The challenge then is to provide a mix of activities to achieve organizational goals and sustain the researcher's motivation and curiosity, which are essential to scientific breakthroughs and product development.

The effect public policy and management decisions have on the resources available for R&D is well understood; one also needs to consider and understand the important role engineers and scientists can and should play in developing science policy. Of the approximately 595,000 doctoral scientists and engineers employed in the United States as of 2003, approximately 372,000 work in R&D. Of the 372,000, it is estimated that about 60,000 work in management of R&D. The remaining doctoral scientists and engineers are involved in many forms of professional practice, in addition to the substantial number who teach (184,000). Those involved in professional services and consulting number nearly 96,000. Consulting engineers and scientists undertake creative activities that are, in many ways, responsible for closing the loop between research and development and application. Table 1.1 shows the primary and secondary work activities of doctoral scientists and engineers (note that respondents could choose more than one category).

A doctorate is a research degree, and the majority of scientists and engineers with PhDs work in research, development, and teaching. It is significant that

TABLE 1.1 Employed Doctoral Scientists and Engineers, by Primary or Secondary Work Activity: 2003

	All Fields	Science	Engineering	Health
Total	593,300	468,570	101,500	23,230
Any R&D	371,830	283,660	75,080	13,133
Management, Sales, Administration	241,190	191,540	39,320	10,330
Applied Research	194,380	145,260	39,480	9,640
Teaching	183,650	154,230	20,050	9,370
Basic Research	141,240	127,470	10,660	3,110
Professional Services	95,630	85,750	4,810	5,060
Development	86,330	52,050	32,450	1,830
Computer Applications	56,280	38,380	16,980	910
Design	38,060	20,410	16,990	660
Other	35,700	28,020	6,370	1,310

Source: Characteristics of Doctoral Scientists and Engineers in the United States, Table 16, 2004.

relatively few engineers, as compared to scientists, hold doctoral degrees. In 2003, 101,500 engineers held doctorates but this represented only about 7 percent of all employed engineers. Among scientists, however, about 23 percent hold doctorates (*Science and Engineering Indicators, 2003*).

We favor managers of R&D organizations with high-level technical skills, because studies have clearly shown that where supervisors were rated highest in technical skills the research groups were most innovative. And where supervisors did not possess excellent technical skills (but had high-level administrative skills), the research groups were least innovative (Farris, 1982, p. 340). These findings in no way minimize the importance of administrative skills, but rather point to a fundamental need for a supervisor in an R&D organization who possesses excellent technical skills. Ideally, both kinds of skills should be available to a manager. Consequently, the role of a scientist* in managing R&D organizations has been and will continue to be an important one.

To make sure we communicate effectively, we must first define some basic terms. We will do this in the next section.

1.3 WHAT IS RESEARCH AND DEVELOPMENT?

The National Science Foundation (NSF) classifies and defines research as follows (*Science and Engineering Indicators*, 2008):

Basic Research. Basic research has as its objective "a more complete knowledge or understanding of the subject under study, without specific

*Whenever we are considering engineering, technology, or pure science for the purpose of this book, the word scientist is used to apply to a person (engineer or scientist) who possesses the technical knowledge and skills that are essential to the work of an R&D organization.

applications in mind." To take into account industrial goals, NSF modifies this definition for the industry sector to indicate that basic research advances scientific knowledge "but does not have specific immediate commercial objectives, although it may be in fields of present or potential commercial interest."

Applied Research. Applied research is directed toward gaining "knowledge or understanding to determine the means by which a specific, recognized need may be met." In industry, applied research includes investigations directed "to discovering new scientific knowledge that has specific commercial objectives with respect to products, processes, or services."

Development. Development is the "systematic use of the knowledge or understanding gained from research, directed toward the production of useful materials, devices, systems or methods, including design and development of prototypes and processes."

In its publication *The Measurement of Scientific and Technical Activities* (1993), the Organization for Economic Co-operation and Development (OECD) defines some research activities as follows:

> *Basic research* is experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundations of phenomena and observable facts, without any particular application or use in view. Basic research analyzes properties, structures, and relationships with a view to formulating and testing hypotheses, theories or laws. The results of basic research are not generally sold but are usually published in scientific journals or circulated to interested colleagues. *Pure basic research* is carried out for the advancement of knowledge, without working for long-term economic or social benefits and with no positive efforts being made to apply the results to practical problems or to transfer the results to sectors responsible for its applications. *Oriented basic research* is carried out with the expectation that it will produce a broad base of knowledge likely to form the background to the solution of recognized or expected current or future problems or possibilities. *Applied research* is also original investigation undertaken in order to acquire new knowledge. It is, however, directed primarily towards a specific practical aim or objective. Applied research develops ideas into operational form. *Experimental development* is systematic work, drawing on existing knowledge gained from research and practical experience that is directed to producing new materials, products and devices; to installing new processes, systems and services; or to improving substantially those already produced or installed.

Research and development covers many of these activities. The OECD defines R&D as "creative work undertaken on a systematic basis in order to increase the stock of knowledge of man, culture and society, and the use of this stock of knowledge to devise new applications."

In order to provide functional and understandable definitions for various research activities, *Science Indicators* categorizes R&D activities as efforts in science and engineering as follows:

- Producing significant advances across the broad front of understanding of natural and social phenomena—*basic research*
- Fostering inventive activity to produce technological advances—*applied research and development*
- Combining understanding and invention in the form of socially useful and affordable products and processes—*innovation*

Many United States governmental agencies have categorized research and development activities to provide a better focus on these activities and, ostensibly, to facilitate technology transfer. One such categorization for the U.S. Department of Defense (DOD) is depicted in Table 1.2. Since DOD accounts for approximately 60 percent of the federal government's R&D expenditures, some understanding of its research program categorization would be helpful to those seeking research support from the DOD.

1.4 RESEARCH CATEGORIES

Harvey Brooks (1968, p. 46) has suggested a general set of dimensions and categories of research:

- The degree to which the research is fundamental or applied—for example, basic research versus applied research and development. The term "fundamental" refers to an intellectual structure, a hierarchy of generality, while the term "applied" refers to a practical objective. It is true that fundamental research is generally less closely related to practical application, but not inevitably so.
- The scientific discipline—for example, physics, chemistry, or biology
- The function of the research, or its primary focus—for example, defense, health, or environment
- The institutional character of research—for example, academic (university), governmental laboratory, or industrial
- The scale of research or style of research—for example, big science versus little science
- The extent to which the research is multidisciplinary focusing on a single class of objects—for example, environment, space science, oceanography, or requiring multiple disciplines

For planning purposes, Brooks (1968, p. 57) has suggested three broad categories of research organizations: mission-oriented research, scientific institutional research, and academic research.

TABLE 1.2 U.S. Department of Defense Research Program Categorization

6.1	*Research: Directed to the Development of Fundamental Knowledge*. Includes scientific study and experimentation directed toward increasing knowledge and understanding in those fields of the physical, engineering, environmental, biological—medical, and behavioral—social sciences related to long-term national security needs. It provides fundamental knowledge for the solution of identified military problems. It also provides part of the base for subsequent exploratory and advanced developments in defense-related technologies and of new or improved military functional capabilities in areas such as communications, detection, tracking, surveillance, propulsion, mobility, guidance and control, navigation, energy conversion, materials and structures, and personnel support.
6.2	*Exploratory Development: Directed to the Development of New Techniques, Methodologies, and Criteria*. Includes all effort directed toward the solution of specific military problems, short of major development projects. This type of effort may vary from fairly fundamental applied research to quite sophisticated breadboard hardware, study, programming, and planning efforts. It would thus include studies, investigations, and minor development effort. The dominant characteristic of this category of effort is that it be pointed toward specific military problem areas with a view to developing and evaluating the feasibility and practicability of proposed solutions and determining their parameters.
6.3	*Advanced Development: Concerned with Design and Development and Hardware (Material) Items for Experimentation*. Includes all projects that have moved into the development of hardware for experimental or operational test. It is characterized by line item projects and program control is exercised on a project basis. A further descriptive characteristic lies in the design of such items being directed toward hardware for test or experimentation as opposed to items designed and engineered for eventual service use.
6.4	*Engineering Development: Directed to Testing and Demonstration of New Techniques or Methodologies, and to Technical Systems Equipment*. Includes those development programs being engineered for service use but that have not yet been approved for procurement or operation. This area is characterized by major line item projects and program control will be exercised by review of individual projects.
6.5	*Management and Support: Directed to the Support of Installations for Their Operations and Maintenance and for the Procurement of Special Purpose Equipment*. Includes research and development effort directed toward support of installations or operations required for general research and development use. Included would be test ranges, military construction, maintenance support of laboratories, operation and maintenance of test aircraft and ships, and studies and analyses in support of the R&D program. Costs of laboratory personnel, either in-house or contract operated, would be assigned to appropriate projects or as a line item in the research, exploratory development, or advanced development program areas, as appropriate. Military construction costs directly related to a major development program will be included in the appropriate element.

Source: AR70–9 Army Research Information Systems and Reports, May 1981, NTIS, Springfield, VA.

Mission-Oriented Research Organizations

The term "mission" refers to an objective defined in terms of the long-range goals of the organization rather than a specific technical objective. Examples of such organizations include Department of Defense research laboratories and industrial research laboratories. Such research laboratories are vertically integrated organizations that conduct both basic and applied research and may provide technical support for operation or manufacturing. While their research may be of the most sophisticated and fundamental type, it is directed to fulfilling the objectives and the mission of the organization rather than to the development of science per se.

Scientific Institutional Research Organizations

This covers organizations whose mission is defined primarily in scientific terms—for example, advancement of high-energy physics or molecular biology. Such research organizations follow some sort of a coherent program adapted to changing frontiers in their area of interest.

Academic Research Organizations

Academic research is usually small-scale basic research carried out in academic departments of universities by students or research associates under the direction of university professors who also teach.

1.5 WHAT TO RESEARCH

There are few discussions of research funding, research program planning, and execution that do not include comments about what really ought to be researched. Governmental agency and industry management hierarchies constantly talk about the need for a better focus on research programs so that research will meet agency and organization needs. Users in production departments, operational personnel in agencies, and consumers often complain about the lack of relevance of the research program and about the lack of timeliness of research results.

Let us take the case of a research laboratory where sponsors, though quite satisfied with the research output of the laboratory, nonetheless provided these kinds of comments about the research program:

- Research takes too long.
- Our need to solve the alternative fuel problem is now, not three years from now. We just can't wait for years for researchers to study the problem.
- We need answers more quickly than researchers provide them.
- The research program is too esoteric. We need solutions that are practical.
- Researchers study the problem to death to find a 100 percent solution. What is wrong with a quicker solution that is not quite 100 percent?

- This problem seems to go on forever. Five years ago I worked at the Department of the Interior. We thoroughly studied the problem of land disposal of hazardous toxic waste. I thought we solved the problem or at least put the issues to bed. When asked whose bed and what the results were, the sponsor did not know.
- We always hear about your previous accomplishments. How about the future? What can we expect from you next year and the year after? Be specific.

First and foremost, R&D managers need to understand the sponsor's perspective and then develop a strategy for effective communication. Consequently, the focus of such research is rather "specific," "commercial," and "product-oriented." For the sponsors to raise questions, as exemplified in the preceding quotes, is to some degree understandable. Consequently, the response of the R&D manager or the PI need not be defensive. For basic research, however, issues are likely to be of a different nature.

How, then, should one respond? One could take each question and provide extensive documentation to refute the sponsor's assertion. For example, one could prove that studying and solving the alternate fuel problem, which was created through decades of neglect, would take some time. Solutions, especially cost-effective and environmentally safe solutions, may well take three years, or even longer, to find. One could also ignore sponsor assertions and go on with the research activity since the sponsor is not likely to find any other researcher who could do the work any faster anyway.

Another approach that an R&D manager could utilize would be a two-part strategy:

- First, empathize with the sponsor's needs and be responsive in a genuine manner. This would translate to providing interim solutions, to the degree possible, for critical problems. Explain to the sponsor the limitations and uncertainties involved.
- Second, educate the sponsor regarding the nature of the research enterprise. Focus on why it is in his or her best interest to follow a systematic, though time-consuming, process of research and development so that solutions developed are scientifically valid, are appropriate to the problem at hand, and truly provide a more advantageous solution to the problem than the existing technology does. This could involve undertaking a mix of research activities ranging from basic research that might take three to five years, to applied research that might provide some solutions within one to two years.

What to research is also affected by what our adversaries or competitors are doing. Some governmental agencies (for example, the Department of Defense) and some industries (for example, high technology) often are concerned about being surprised by a technological development by an adversary or competitor. This is simply because the payoff or effectiveness of the defense establishment of a nation, or profitability of an industry, depends on its own capabilities and also on the capabilities of its adversaries or competitors. New technological developments of an adversary or a competitor can have a profound effect on the security of a nation and on the competitive success of an enterprise.

Other questions and issues related to the issue of what to research often include the following:

- How should user needs be considered?
- Who are the real users?
- How should a comprehensive and responsive research program be formulated?
- How should the tradeoffs between long-range research needs and short-range or immediate requirements be made?

Many approaches to formulating research programs have been proposed. For example, Merten and Ryu (1983, pp. 24–25) have proposed dividing an industrial laboratory's research activities into five categories:

- Background research
- Exploratory research
- Development of new commercial activities
- Development of existing commercial activities
- Technical services

Schmitt (1985) has discussed generic versus targeted research and market-driven versus technology-driven research. Shanklin and Ryans (1984) contend that high-technology companies can make a successful transition from being innovation-driven to being market-driven by linking R&D and marketing efforts.

A considerable literature is available related to R&D project selection. The proper approach applicable to an organization would clearly vary depending on the needs of an organization.

Two criteria seem most important in deciding what to research: (1) What will advance the science? (2) What do the customers of our research need? Once we have answered those questions, we need to ask: What are the prospects for a solution?

There are other considerations that may override them. Other criteria may apply in the solution of very specific problems. For example, in oil exploration, safety considerations may be a top research priority. Such problems may have to be solved regardless of cost because the organization would be wrong to ignore them. Research needed to protect human health and the environment from improper disposal of hazardous waste falls in the same category.

One of the most difficult problems is deciding when to abandon a problem that does not seem to be solvable. There is always the hope that with a few more months of work the problem will be solved. Yet, one usually has some sense of what is likely to happen. If one researcher is sure that the problem can be solved and no one else is so convinced, it is necessary to determine whether the one researcher is a "genius" or a "neurotic." People do get attached to hopeless causes, and when that happens they exhibit a variety of such symptoms as extreme tension and the inability to be self-critical. Managers must be sensitive to clues that indicate that the optimism about a project is unjustified. Since stopping such a project without destroying the motivation of the scientist is important, some suggested approaches to achieve this follow.

A manager may agree to give the scientist short deadlines and establish mutually agreed-upon milestones to ascertain whether tangible progress toward the goal is being made. If the project indeed is hopeless, lack of project progress during the milestone review would reveal the problem. In most cases, the scientist would, on his or her own initiative, agree to drop the project.

Should the scientist still request to continue the project, the manager should consider allowing the scientist to spend some time (say 20 percent) on the project and again establish agreed-upon milestones to review progress. If results again are not very promising and the scientist still perseveres and wants to continue, two options are possible. One, the manager may direct that the project be stopped. The other possibility is to still allow the scientist to spend some time on the project but strip away all support, such as for laboratory equipment, computer expenses, and technicians. In time the project will fade away.

The manager, however, should not be too surprised when some researchers supposedly pursuing unpromising theories or projects thought to be nonproductive in their early stages end up producing promising results. It is good for all concerned, especially for the manager, to keep in mind that predictions about the success or failure of research projects are most unreliable. Two examples come to mind, one dealing with fundamental research and the other with applied research.

Astrophysicist S. Chandrasekhar was working on the theory of black holes and white dwarfs. He sought to calculate what would happen in the collapse of larger stars when they burn out. He theorized that if the mass of a star was more than 1.4 times that of the sun, the dense matter resulting from the collapse could not withstand the pressure and thus would keep on shrinking. He wrote that such a star "cannot pass into the white dwarf stage." His paper on this theory was rejected by the *Astrophysical Journal*, of which he was later to become a well-respected editor.

As reported in the *New York Times* (October 20, 1983), Sir Arthur Edington, rejecting Dr. Chandrasekhar's theory, stated that "there should be a law of nature to prevent the star from behaving in this absurd way." Chandrasekhar was urged by other scientists to drop his research project because it did not seem very promising. Dr. Chandrasekhar persisted and in 1983 won the Nobel Prize for his discovery. His research led to the recognition of a state even denser than that of a white dwarf: the neutron star. The so-called Chandrasekhar limit has now become one of the foundations of modern astrophysics.

As another example, a group of researchers developing a complex environmental impact analysis system and associated relational databases chose to pursue this research project by using a higher-order computer language instead of the traditional FORTRAN. They also wanted to experiment using an operating system developed by the Bell Laboratories. Management attitudes ranged from enthusiastic support to tepid support, opposition, and downright hostility. The less technically knowledgeable managers were opposed; and the further removed they were from the research group; the more opposed they were to the continuation of this research project. Because of the creativity of the researchers and with some degree of support and acquiescence of the management, the project was allowed to continue in parallel with other activities. On completion the project was one of the most successful and one of the most widely used systems in the agency. It received the agency's highest R&D achievement award and became an archetype for future systems development research activities.

No one approach for categorizing or organizing research and for identifying the research needs of an agency or an industrial enterprise may satisfy the complex and, at times, unique needs of an organization. We propose a two-tier model for identifying "what to research," in an effort to develop an approach that provides a flexible, systematic framework for integrating various requirements that at times seem in conflict with each other. The model includes an economic index model and a portfolio model. This two-tier model may apply more readily to mission-oriented research than to scientific institutional or academic research. Further discussions of this model follow.

Economic Index Model

Under this model, research needs are defined as those needs designed to improve the operation or manufacturing efficiency of the organization or the enterprise. The emphasis is on building a "better mousetrap" to reduce the cost of doing things. Inputs for such needs come from the users, operation units, and scientists, as well as from looking at competitive products and operations.

Portfolio Model

Under this model, normative, comparative, and forecasted research needs are considered. *Normative needs* are those of the user (a user being the primary or follow-on beneficiary of the research product). *Comparative needs* relate to research needs derived from reviewing comparable organizations, competitive product lines, and related enterprises. *Forecasted research needs* focus on trend analysis in terms of consumer or organization needs derived from new requirements, changed consumer behavior, new technological developments, new regulations (e.g., environmental, health, and safety regulations), and new operational requirements. Often the effectiveness of a commercial enterprise or of a national defense effort depends not only on how well the organization itself does but also on how well the organization does in comparison with its competitor or adversary. Consequently, it is necessary to have effective intelligence concerning the portfolio of a competitor in order to focus properly on comparative and forecasted research needs.

After defining research needs using these two models, some research projects would be essentially modifying, adapting, or adopting existing scientific knowledge and would correspond to applied research and development; other research projects would fill technology gaps and would correspond to basic or fundamental research.

Inevitably, there are more projects to be researched than there are funds available. This is a normal and a healthy situation. A model derived from the work of Keeney and Raiffa (1993), which takes into account multiple objectives, preferences, and value tradeoffs, is suggested for deciding which projects to select among competing requirements. The main problem in using such an approach is the tendency on the part of many technical users to quantify items that do not lend themselves to quantification.

In developing a policy (at higher levels) or in making specific project choices among competing demands (at lower levels), the decision-maker can assign utility values to consequences associated with each path instead of using explicit quantification. The payoffs are captured conceptually by associating to each path of the tree a consequence that completely describes the implications of the path. It must be emphasized that not all payoffs are in common units and many are incommensurate. This can be mathematically described as follows (Keeney and Raiffa, 1993, p. 6):

$$a' \text{ is preferred to } a'' \Leftrightarrow \sum_{i=1} P_i' U_j' > \sum_{j=1} P_j'' U_j''$$

where a' and a'' represent choices, P probabilities, and U utilities; the symbol $\Leftrightarrow$ reads "such that."

Utility numbers are assigned to consequences, even though some aspects of a choice are not in common units or are subjective in nature. This, then, becomes a multiattribute value problem. This can be done informally or explicitly by mathematically formalizing the preference structure. This can be stated mathematically (Keeney and Raiffa, 1993, p. 68) as:

$$\begin{gathered} v(x_1, x_2, \ldots, x_n) \geq v(x_1', x_2', \ldots, x_n') \\ \Leftrightarrow (x_1, x_2, \ldots, x_n) \gtrsim (x_1', x_2', \ldots, x_n') \end{gathered}$$

where v is the value function that may be the objective of the decision-maker, x_i is a point in the consequence space, and the symbol $\gtrsim$ reads "preferred to" or "indifferent to."

After the decision-maker structures the problem and assigns probabilities and utilities, an optimal strategy that maximizes expected utility can be determined. When a comparison involves unquantifiable elements, or elements in different units, a value tradeoff approach can be used either informally, that is, based on the decision-maker's judgment, or explicitly, using mathematical formulation.

After the decision-maker has completed the individual analysis and has ranked various policy alternatives or projects, then a group analysis can further prioritize the policy alternatives or specific projects. A modified Delphi technique (Jain et al., 1980) is suggested as an approach for accomplishing this.

After research project selection and prioritization, an overall analysis of the research portfolio should be made. The research project portfolio should contain both basic and applied research. The mix would depend on the following:

- Technology of the organization
- Size of the organization
- Research staff capabilities
- Research facilities
- Access to different funding sources

It should be noted that the distinction between basic and applied research can become rather blurred. What is *basic* research to one organization can be *applied* to another and what is *basic* one year can be *applied* the next. Also, given the same general research project title, different emphases during project execution can affect the nature of research. As will be discussed below, to maximize R&D organizational effectiveness, scientists and work groups should be involved in a mix of basic and applied research.

1.6 EMPHASIS ON BASIC VERSUS APPLIED RESEARCH

We have discussed some research organization categorization and ways of developing an R&D portfolio. For planning purposes, three types of research organization categorization were presented. The emphasis on basic research versus applied research within each organization varies; consequently, there is a certain amount of conflict. The conflict is due to the fact that basic research is often dictated by the questions that science is asking. Such research may require activities that are not compatible with the mission-oriented research that a commercial or government organization is supposed to do. For example, a scientist while reading a scientific journal may have an insight that requires further experimentation. However, his supervisor may have already asked him to develop a particular product that meets particular specifications. Obviously the two activities are incompatible and some of the conflict that occurs within the scientist is due to the conflict between the need to discover and the requirements of the organization.

Some quite successful organizations—for example, 3M in Minnesota—have developed procedures that allow their scientists a certain amount of time to work on topics that are of interest to them. What percent of the scientist's time will be spent on such topics, and when such activities should take place, are matters of negotiation between the scientist and his or her supervisor. A successful scientist, who has had a better track record, may be given more time to discover other things by pursuing his or her own interest than one who does not have a good track record.

Pelz and Andrews (1966a) did a study of 1300 scientists in 11 laboratories. They studied scientists in both industrial and government laboratories and they used five criteria to identify successful scientists: (1) the judgments of their peers, (2) the judgments of their boss, (3) the number of papers they published, (4) the number of patents they were awarded, and (5) the number of reports they issued. They then conducted intensive interviews to identify what distinguished the effective from the less effective scientists. One of the findings was that the more effective scientists did both basic and applied research.

We will return to the study of Pelz and Andrews throughout this book, but for the time being one basic point that we should keep in mind when thinking about how to structure research and development organizations is that both kinds of research are done by the more effective scientists. It is obvious that if a scientist has an insight while reading a journal that requires an experiment, the inability to do the experiment will be quite frustrating. It is exactly this point that indicates that some sort of freedom to experiment should be allowed by the organization. If reading scientific journals results in frequent frustration, it is very likely that the scientist will become obsolete by giving up such reading. Similarly, the organization should encourage its scientists to publish, since this provides an opportunity for the organization to acquire prestige in the eyes of the scientific community and also tests the capabilities of the individual scientist to become effective in relating to the wider scientific community.

It should be remembered that there are over 8,000 journal articles published every day in the sciences. Thus the output of any particular individual is a minute contribution to a very large pool of activity. However, the fact that a person has made a contribution essentially "buys" the ticket that allows him or her to interact with other scientists, to learn from them, and to discover what they are currently doing.

1.7 WHAT IS UNIQUE ABOUT MANAGING R&D ORGANIZATIONS?

R&D organizations are different from other organizations because of the people working in such organizations, the ideas that are generated, the funds or research support that are obtained, and the culture of the organization. These four elements—people, ideas, funds, and culture—are the basic elements of an R&D organization and are discussed in detail in the next chapter. A brief review of each element as related to an R&D organization's uniqueness follows.

People

People in R&D organizations normally would have graduate training and relatively high aptitude. They are socialized during their graduate training to work autonomously and show considerable initiative.

An anecdote will help convey more clearly what is special about R&D personnel. The famous German scientist Hermann Helmholtz put a sign up on his lab: "Do not disturb." This was all that his students and collaborators were able to see for a month. After some 30 days Helmholtz emerged with an important new theory that eventually led to the development of radio and television (related in Boring, 1957).

Ideas

Ideas in an R&D organization are generated through a unique communication network (discussed in the next chapter) and facilitated by the ethos of a scientific community (discussed in Chapter 3).

Funds

In general, funding sources for R&D organizations are different from those for any similar large enterprise. For example, in the United States about 28 percent (2006) of funds for R&D are provided by the federal government. The federal government spends over three times as much on basic research as does industry. Even for academic institutions, the majority of research funding support, 61 percent (2006), is derived from the federal government. This funding support, coupled with research productivity benefits that accrue to society at large rather than the individual or the sponsoring organization, gives R&D organizations a unique characteristic.

Culture

The culture of an organization relates to both objective and subjective elements. For an R&D organization, objective elements such as research laboratory facilities and equipment and office buildings are different from those of other organizations. Subjective elements such as rules, laws, standard operating procedures and unstated assumptions, values, and norms for an R&D organization are also different. For example, scientific discoveries, whatever their source, are subjected to impersonal judgments, and scientists often participate in organized skepticism and critically evaluate scientific ideas and discoveries. This permeates all aspects of an organization's function. Management decisions affecting individuals are thus critically evaluated and questioned by the researchers. After attending a senior management conference, a newly assigned deputy administrator of a federal research organization stated that he had never worked in an organization where people were so vocal and where management decisions were reviewed and discussed as openly and fully.

The culture and other elements vary from one R&D organization to another; however, as a group, R&D organizations generally possess unique characteristics.

1.8 SUMMARY

We first pointed out that the essence of R&D management is the coordination of the activities of many individuals. An effective R&D organization should have a mix of research activities that are both basic and applied. The chapter provided definitions of terms such as basic and applied research and development, and it reviewed proposals for a system of categories of research. One key issue is "What to research?" A model that deals with this question was presented. Finally, we examined what is unique about managing R&D organizations. One unique aspect is the need for the intricate coordination of people, ideas, funds, and culture. In the next chapter we discuss these elements and their coordination further, and the rest of the book is concerned with how a manager can be most effective and lead an organization that will be most productive.

1.9 QUESTIONS FOR CLASS DISCUSSION

1. How much R&D is too much for a corporation? When is it not enough?

2. How much R&D is too much for a country? When is it not enough?

3. Define and compare basic and applied research, and development.

4. How much basic research is desirable in what kind of an R&D lab?

5. Using an actual case of a government, industrial, or academic research laboratory, develop a systematic procedure and a short-term and long-term research plan.