Energy Research and Development (1985)

Pages
34

Size
9 x 10

ISBN
0309322022

Ahearne, John F.; Committee on Science, Engineering, and Public Policy; Academy Industry Program

ENERGY RESEARCH AND DEVELOPMENT

John F. Ahearne*
Resources for the Future

To address the economic value of federal investment in energy research and development (R&D), this paper will cover three general points: First, what are the stages in progressing from a glimmer of an idea to a commercial product? Second, why should the federal government fund any of these stages? Third, what can we learn from past efforts at federal funding?

Several stages can be included in "research and development." For example, the Department of Defense (DOD) includes five categories: basic research, exploratory development, advanced development, engineering development, and production. Sometimes a subcategory of test and evaluation is included, but it is really a part of engineering development. As another example, the Congressional Budget Office has described the stages of energy R&D as research, development, demonstration, and commercialization.[1] These stages are analogous to the last four DOD categories; thus, the Congressional Budget Office excludes basic research. There are no clearly defined boundaries between each stage of R&D, overlap is common, and the stages are closer to being part of a continuum rather than being distinct, well-defined categories. However, each stage does have certain characteristics. Specific application is not a necessary justification at the basic research end, whereas it should be considered as a program moves toward demonstration. The funds spent at each R&D stage increase substantially: Total basic energy science funding is less than 10 percent of the total allocated to energy research, development, and demonstration. For a given project, the funds required as the project moves from concept to commercial plant increase by a factor of about ten at each stage.[2] Economic analysis is justified increasingly as a program progresses through the spectrum of research, development, demonstration, and commercialization, although often it is not applied. Finally, because project size increases significantly at the demonstration end, political pressures--to locate the project in a specific region, to maintain or add funding, or to reduce or end funding--also increase.

---

* With grateful acknowledgement for significant assistance from Joel Darmstadter; valuable advice from John Deutch, Norman Metzger, Joel Snow, and Karl Willenbrock; and data collection by Caroline Bouhdili. Responsibility for the conclusions is the author's alone.

BACKGROUND

Although warning signs had appeared earlier, the 1973 oil embargo was a shock to the economic systems of many countries. In the United States, energy became a major topic, driven initially by long gas lines and later by the political process. The executive branch and Congress focused on energy research and development. President Nixon announced a $10 billion, five-year funding program with the goal of achieving energy independence by 1980. Senator Henry ("Scoop") Jackson counterproposed $20 billion over ten years. Many sources suggested ways to spend those monies. Some observers questioned whether rational analysis had been applied to the amounts proposed or to the processes by which funds would be distributed. In particular, questions were raised about whether economic analysis had been used in choosing what would be appropriate energy R&D programs.[3]

A decade has passed. The energy crisis has abated, oil prices have fallen, and energy supplies (of gas and oil in particular) appear plentiful, at least for the near term. In the United States, the nuclear industry is staggering or dying, the Synthetic Fuels Corporation is being closed down by Congress, and many initiatives for encouraging small generators of electricity have been cut back. But the United States will remain an energy-consuming country, and innovative energy technologies could make substantial improvements in the use of energy and in the national economy. A technology-driven United States must rely for future growth on developing new ideas and transferring those ideas into commercial operation successfully. Usually, this role is ascribed to industry, and, in particular, to entrepreneurs. Nevertheless, the federal government has had substantial involvement in energy-related programs, for example, the fission and fusion programs of the national laboratories, and has supported programs in energy conservation and development of new techniques for housing insulation. The federal government has funded development of windmills, low-head hydrodynamic generation systems, and other energy systems. Unfortunately, a retrospective look finds several instances in which large funding has led to little obvious benefit.

Between fiscal years 1971 and 1982, the federal government spent $34.7 billion (1985 dollars[4]) in research and development to improve energy supply or reduce energy use (for example, through energy conservation).[5] (This is less than the amount calculated in some summaries of energy-related items in the federal budget, because the $34.7 billion does not include nuclear weapons activities of the Department of Energy (DOE) and its predecessors, the Energy Research and Development Administration (ERDA) and the Atomic Energy Commission (AEC); energy R&D funding through the National Science Foundation (NSF); or funding for basic energy science.) Federal funding for energy R&D has matched, approximately, that of nonfederal funding since the early 1970's.[6]

In the years 1971 to 1982, the nuclear research and development programs (nuclear fission, fusion, uranium enrichment, and those of the Nuclear Regulatory Commission) received $14.7 billion (1985 dollars), or 42 percent of the total. What has the nation achieved through these funds? A review of the past ten years shows such projects as the Clinch River Breeder Reactor (CRBR), canceled after $1.5 billion were expended; the gas centrifuge uranium enrichment plant (GCEP), canceled after $2.6 billion were expended and before operation was achieved;[7] and improvements in gaseous diffusion uranium plants, one of which is to be closed, for which about $1.5 billion were spent.

Another major recipient of federal funding has been coal R&D. Fossil fuel projects, with coal projects predominating, received $6.1 billion (1985 dollars) from 1971 to 1982, or 18 percent of the total energy R&D funding. These programs funded some work that should have led to commercial use. The major federal effort at commercialization of novel fossil energy use has been the Synthetic Fuels Corporation--also not very successful. For example, the Great Plains synthetic fuel plant has been abandoned by its developers, apparently leaving the federal government with at least a $1 billion loss.[8] The government funded an unsuccessful magnetohydrodynamic (MHD) project for over eight years at a cost of $450 million. Careful review of past history would uncover additional projects that were canceled after being partially under way or closed after completion because they were ineffective or uneconomical.

Some federal funding has been fruitful. Proving a hypothesis wrong or a promising avenue to be a blind alley is not a failure in basic research--both advance knowledge. Failure is funding poor experimental design, mediocre work, publication for the sake of publication. Depending on the stage of R&D, finding a technology uneconomical or infeasible may or may not represent a misuse of federal funds. The stages of research and early engineering development are for exploration of new concepts and for preliminary determination of economic and technological feasibility. If these stages are done well, some proposals will fail. The failures represent appropriate uses of federal funds, as will the successes. Imprudent use of federal funds occurs when the early stages are not implemented carefully or examined rigorously, and projects advance that should not.

BASIC RESEARCH

Usually labeled as basic energy sciences within the DOE budget, basic research develops fundamental knowledge, and (usually) does not have a specific application as an explicit goal. For example, DOE and NSF help support the application of high-speed computers to quantum mechanical calculations for predicting the behavior of chemical systems. Writing about one such application, William Goddard described the theoretical prediction and experimental verification of the use of one surface of molybdenum trioxide for the selective catalysis of methanol with oxygen to produce formaldehyde, a step that may lead eventually to the design of new catalytic processes.[9]

Basic research need not be a small project. The Department of Energy funds the nation's largest accelerator, at Fermilab. That facility has produced many significant results and is, by any scientific standards, a successful example of "big science." Fermilab's success stems in large part from the ability of its two directors, R. R. Wilson, known widely for his magical ability to get accelerators running, and his successor, Leon Lederman, who before taking over at Fermilab had built an international reputation for large, well-executed, and innovative experiments.

Basic research can be analyzed, criticized, and critiqued. Monies can be allocated wisely or poorly, but how much, and for what, are not subjects for economic analysis. This position is similar to that of a former president of E. I. du Pont de Nemours (Dupont) who wrote, "We defined fundamental research as inquiry into the fundamentals of nature without specific commercial objective."[10] He went on to comment on the value of research, but also wrote, "Research is not, of course, a cash-and-carry activity in which a given expenditure can guarantee a given result."[11] Similarly, in a book devoted to exploring the government's role in funding R&D, a commentator described "basic research,...research whose strategy is governed by the logic of science rather than potential applicability,...is thus of such a nature that the area of ultimate payoff cannot be more than vaguely foreseen."[12]

This lack of tight links to products often has concerned critics of research. Several attempts have been made to examine the evidence, if any, that supports direct links. *Project Hindsight*,[13] a report prepared for the Department of Defense in the 1960's, attempted to trace 20 military products to their origins. Some conclusions were that basic research pays off more than 20 years later; applied research results from hundreds of sources were incorporated in the final products; and no simple relationship could be found between the cost of research and the value of the results linked to that research. There is some evidence that industrial basic research can be linked to productivity.[14] However, the author of this paper knows of no studies providing similar analytic support for federal funding of basic research, although Harvey Brooks has provided many examples of valuable science performed with federal support, science that can be connected, at least plausibly, to important commercial applications.[15]

Basic research usually has been supported across a spectrum of political philosophies:

Prudence would also dictate that the federal basic research budget should grow each year at a rate that matches inflation--and, where possible, allows for a small real increase. This proposal logically follows from an agreement that has developed over several decades--and has been strongly reaffirmed by the Ford, Carter, and Reagan Administrations--that the private sector will not support basic research at an adequate level.[16]

Support is not unanimous, and Milton Friedman is a notable opponent:

> What ethical justification do you have for extracting tax money from people for purposes that do not yield them some greater benefit? I challenge you to find a single study justifying the amount of money now being spent on government support of research.[17]

A major problem in trying to provide quantitative support for basic research is the time lag between such research and commercial application of the resulting knowledge. Typically, this lag is more than 20 years (as concluded by *Project Hindsight*), when both congressional memory and explicit links connecting the basic research to subsequent applications have become blurred. The director of DOE research noted recently that 20-30 years are required from the start of basic research on a new technology to reasonable market penetration.[18] For example, research on glassy, or amorphous, metals first was funded 25 years ago, leading eventually to new methods for forming metallic glasses. Glassy metals now are used in recording and playback heads in tape recorders and in the starting circuits of fluorescent lights, and new forming techniques may lead to more substantial uses.[19]

Basic research cannot be valued directly. Perhaps like some aspects of environmental quality, such as clear skies in the Rockies or virgin forests in the Pacific Northwest, it is only through secondary measures that one can measure the value of basic research. Such techniques as contingent valuation (for example, willingness to pay) are used for economic analysis of environmental quality.[20] Basic research might be measured economically by such measures. But whereas most citizens can understand, and may appreciate, clear skies, few can understand the areas of basic research. Superstring theory is of bubbling interest today, and debates are strong within the science community on funding the superconducting super collider--but it is doubtful that the public ever will have the level of knowledge to enter into such debates. Fundamentally, support for basic research is subjective, based on the conclusion that basic research leads to important public benefits--in ideas generated, people trained, and intellectually stimulating environments provided.[21]

Critics of basic research find support from a public skeptical of the value of federal funds being used for "research," often because of well-publicized failures of costly programs in the later stages of the R&D cycle. Consequently, the next sections will focus attention on those stages.

WHY FEDERALLY FUNDED R&D?

Why should the federal government be involved in nonbasic research and development? Although specific objectives of national administrations obviously differ substantially, the fundamental goals for research and development have not changed much over the last ten years. A 1984 description states,

> The Federal Government funds R&D activities to serve two broad purposes:
>
> - To meet specific Federal Government needs--where the principal user of the R&D is the government itself--for example, to insure a strong national defense;
> - To meet broad national needs--where the Federal Government supports R&D that the private sector lacks incentive to invest in adequately, in the national interest--to help insure the strength of the economy and the quality of life for all people.[22]

This statement is not substantially different from that of the Carter Administration in 1979:

> Research, development and demonstration programs figure prominently in fulfilling energy supply. The Federal Government subsidizes research and development when circumstances such as the uncertainty or the inability to capture future benefits inhibit private industry efforts.[23]

Many writers have described and discussed reasons for the federal government to fund research and development that could lead to national use. Table 1 outlines reasons given by five sources, two from the early 1970's and three from the 1980's. Both the similarities and the differences are important.

Tilton[24] is an economist; the Research and Development Act is a political creation; Deutch[25] is a former director of energy research and undersecretary of DOE, and a chemist; the members of the Energy Research Advisory Board[26] are technologists primarily, but writing within political constraints; and Brooks[27] is a technologist writing on public policy.

Four of the five sources agree that one appropriate role for government funding of R&D is where there is no private developer (or

TABLE 1 Reasons for Federally Funded Research and Development

TILTON (1974)	FEDERAL NON-NUCLEAR ENERGY R&D ACT OF 1974	DEUTCH (1982)	ERAB (1983)	BROOKS (1985)
Incomplete Appropriability of Benefits	Limited potential for sufficient non-Federal benefits	Private sector does not internalize some benefits to this nation and its allies		Generic applied research No one user has sufficient stake in benefits to sponsor research
Public Goods Environmental quality National defense Balance of payments Economic prosperity	National problems	To address serious issues facing the nation	To support Federal regulatory, environmental, policy responsibilities	Exceptional social return Narrow markets Key industries Where government has the responsibility to regulate
Imperfect Knowledge		Uncertainty with regard to future price and availability of crude oil		
Regulation (leading to market distortion)	Limited Federal mechanism to encourage, other than dollars	Uncertainty with respect to Federal policy and regulation		Mixed goods: could be either public goods or market goods. Social benefit justifies public development.

Disagrees with (unprovable)				
Availability of funds	Costs more than private can fund	Uncertainty in performance make multi-billion dollar projects too risky for private funding		Fragmented industries can not afford necessary investment
Risk Pooling and Discounting	High risk	Definite private advantage to being second--and for government to take the risks of being first	High risk: technical, market, or governmental policy	High risk
Time Discounting and Welfare of Future Generations			Long time to fruition	
Income Distribution				
Employment of Scientists and Engineers				
Competition				
Government Purchases				
	Urgent and not likely to be done without Federal dollars		High payoff	

developers as a group within the antitrust laws[28]) that can foresee sufficient benefits to that developer to justify investment in the research. Tilton gives examples across a broad spectrum of industries within and outside the energy sector, such as earth moving, catalysis, combustion, and construction technologies.[29] Brooks gives as a classic example the aeronautical research conducted by the National Advisory Committee on Aeronautics (NACA) after World War I and continued by the National Aeronautics and Space Administration (NASA) (also cited by Mowery[30]). These examples are at the research end of the research, development, demonstration, and commercialization continuum. Such projects tend to take a long time to fruition, but also are not particularly expensive (although construction of some of NASA's wind tunnels was). Bell Laboratories performed this type of work for decades, in fields focused on communications, but including such diverse efforts as measuring the cosmic 3-degree-Kelvin background noise, which is related to the "Big Bang" theory of cosmic creation. (This research began with the goal of eliminating noise in commercial long distance circuits. The researchers were led to examine a persistent noise source, which they found came from outside the earth. Bell Labs allowed them to follow this research, which led eventually to the Nobel Prize. It is not clear that an unregulated industry, in which the Bell Laboratories parent now lives, can afford such public services.) Four of the five sources agree that this generic area, in which, in the economist's terms, there is incomplete appropriability of benefits, is appropriate for government funding.

All five sources agree that federal funding is appropriate for research on national problems, including R&D programs for goods that cannot be quantitatively economically justified. Deutch describes this as due to a "security premium."[31] Examples of national benefits include a cleaner environment, a more stable economy, and the availability of orphan drugs. Another benefit may be maintaining industries or technologies that are described as being essential for some public purpose, usually national security. Linking some of these benefits directly to research projects is difficult. For example, how best to improve U.S. energy security and how to stabilize the economy are matters that have been debated extensively for at least the last 12 years with no substantial consensus among either the public or the "experts."

Four of the five sources also agree that it is appropriate for the government to fund research and development when risk is high. The economist, Tilton, believes that this case has not been proven. He argues that there would be only a smaller number of high-risk projects qualifying for public support, "because the more risky projects are concentrated at the basic research end of the research and development spectrum, and the more expensive projects are at the development end." He also writes, "Whether firms on average actually have higher discount rates for risks than society as a whole, however, is uncertain. Indeed, it is not entirely clear why they should."[32] Deutch counters with several points that, although made in the context of synthetic fuels, apply to the full range of energy R&D. He notes, "The most persuasive reason for federal action is the evidence of inaction by the private sector....The evidence is quite unambiguous that the private sector will not undertake the massive initial investments without federal assistance.[33]

The time horizon for industry almost always is much closer than a societal benefit analysis would support (forestry is an exception). Consequently, even though low present-value benefits are calculated, the government should give a higher weight to future benefits than industry does--a form of Deutch's security premium. In addition, research and development, by its very nature, whether for energy or other fields, has a pace set primarily by the ability of the researchers. This has been described as the pace of science or the pace of technology, as opposed to the pace of economics. It is true that additional funding can solve problems more rapidly--NASA has demonstrated this in some of the early space programs. On the other hand, a substantial infusion of funds does not buy an equivalently increased level of competence. There are a finite number of good researchers and a limited number of good research institutions. Increased funding may bring in only lower quality people and groups, and not add significantly to the rate at which useful work is done.

Other observers have given arguments similar to those in Table 1 in support of federal funding of R&D. The Office of Technology Assessment proposed: "One approach to accelerating development would be to increase or concentrate Fedsral R&D efforts on technologies... where cost and performance are of greatest concern."[34] An earlier report by the Energy Research Advisory Board concluded that federal research support is required "where there is reasonable certainty that timely and adequate response by industry and commerce is an unrealistic assumption."[35] These conclusions are not restricted to the United States. A recent report from the International Energy Agency noted that market forces may be inadequate to develop appropriate solutions in a timely manner when national interests--such as energy and economic security, health and safety research and regulation, environmental concerns, development of technology infrastructure for future industry, employment, industrial and regional development, and defense--are concerned.[36]

Over the past 15 years, each major energy-related company, such as Westinghouse and General Electric, has invested substantial funds in research and development. As was noted by a participant in a 1974 conference, Public Policy and Energy R&D, hosted by Resources for the Future, it is likely that industry-related R&D has more direct applicability than government-related R&D. The participant commented that a review of defense and NASA funding showed that the number of patents resulting per dollar of government R&D was only 10 percent of those resulting from private industry R&D.[37] Of course, this comparison may not be completely fair, since fedsral agency funding does not have getting a patent as an objective.

A major development was the recognition by the electric and gas industries that R&D can be a substantial aid if not an absolute necessity. This led to the founding of the Electric Power Research Institute (EPRI) and the Gas Research Institute (GRI). However, as shown in Table 2, government funding dominates that of both EPRI and GRI. This is due partially to the fact that both industrial organizations are funded by regulated utilities. The Energy Research Advisory Board has pointed out that "[r]egulators are often unwilling to authorize risky ventures. Furthermore, the benefits of successful risk taking are unlikely to accrue to the utility, while the penalties of unsuccessful risk taking have often been borne by its stockholders."[38]

There will be some R&D that the United States will need that industry will not fund because sufficient benefits will not accrue to an industrial developer or because the cost will be too large for anyone but the federal government.[39] A second area of gradually increasing importance is where government regulation or responsibility is involved. Traditionally, national defense R&D was seen to be an appropriate, if not necessary, area for federal funds. With increased technological demands on social regulatory agencies (the Nuclear Regulatory Commission and the Environmental Protection Agency the foremost), federal R&D also seems to be necessary where government action could lead to increased costs for industry, for example, research on acid rain, pollutant hazards, and nuclear safety. Finally, federal funding may be necessary where successful R&D could undermine significantly the profits of existing industries, for example, research on cogeneration technologies and end-use efficiencies.

APPLIED RESEARCH

Applied research retains some of the basic research flavor of advancing knowledge, but applied research projects should be connected to specific identifiable applications.

The Department of Energy and its predecessors have funded a variety of applied research programs. "More applied research is carried out in Federal laboratories than anywhere else."[40] For example, recognizing that improved transmission of electricity can be a major factor in developing the U.S. electrical power network, the government has funded a superconducting cable project at the Brookhaven National Laboratory, research at the Los Alamos National Laboratory on superconducting magnetic techniques to improve transmission efficiency, and test programs to evaluate the effects on animals of the high-voltage electrical fields around transmission lines.

In the 1970's, as use of solar energy became a national goal, DOE developed programs on photovoltaic properties of thin films, concentrators, and advanced materials. The eventual payoff from

TABLE 2 Federal and Industry Funding for Selected Energy Research and Development

	1982 ($ Million)		
R&D Funding Area	Department of Energy	Electric Power Research Institute	Gas Research Institute
Electric-related supply	2,189	141	---
Liquid- and gas-related supply	447	39	36
Conservation and improved end use utilization	392	64	38

Source: *The Federal Role in Research and Development, A Report of the Energy Research Advisory Board to the United States Department of Energy*. DOE/S-0016. Washington, DC: Department of Energy, February 1983.

those programs could be in economically useful solar systems. Since some of the programs have been funded for nearly ten years, perhaps it is time for a careful survey of what results have been transferred to demonstration or commercialization.

Much applied research consists of small, unglamorous projects, which may, however, have a significant effect on the national economy or quality of life. A good area to study is conservation, in which DOE has funded many projects, including the following: a method for trapping and then burning paint solvent fumes in metal coating processes; a method for using foam rather than water in textile finishing; a study of heat transfer between buildings and the ground; studies of insulating materials; development of ceramic materials for use in combustion engines; and development of electric storage batteries.[41] Whether any of these programs has led to usable results should be part of the evaluation. Positive DOE descriptions should be accepted as the views of program advocates and treated with neutral skepticism.

Applied research can have influence beyond that planned originally. For example, the Atomic Energy Commission and its regulatory successor, the Nuclear Regulatory Commission, funded research in basic geoscience and in seismic design and analysis. The purpose was to develop seismic criteria for nuclear power plant location and design. One recipient of this funding was Nathan Newmark, professor of civil engineering at the University of Illinois.[42] Professor Newmark trained scores of graduate students in seismic design, and those students went on to design buildings around the world, including many of the buildings that did not fall in the recent Mexican earthquake.[43] As another example, the use of radioisotopes in diagnostic and therapeutic medicine has spread throughout the world, based upon techniques and products developed through earlier AEC and ERDA funding. The total funding for those projects was small, but the results were substantial.

Basic and applied research are nibbling processes--they are not done in great leaps, easily seen and costed, whose effects on the products and services in the economy can be predicted and measured. The president of Dupont wrote, "The average research man pays his way by the 'bit-by-bit' research, the day-to-day effort that produces results which over a short period of time seem inconsequential, but which over the long run are extraordinarily important."[44]

Nevertheless, criticism of federal research funding will not disappear, nor should it. Therefore, the government would be wise to invest some effort in studying at least selected areas of applied research to determine what, if any, national value has been added. Solar energy and conservation programs would be appropriate areas to examine for the effectiveness of funding over the last decade, and seismic programs for the last two decades. All three are areas in which large commercial firms (such as General Electric, AT&T, IBM) are not dominant, so the threads from federal funding may be more apparent.

LARGE PROJECTS

The large projects that begin to appear at the end of applied research dominate federal R&D funding and political visibility. Whether the projects are called R&D or demonstration often depends on political decisions. Nevertheless, they are included in the federal R&D budget. Analysis of federal R&D funding cannot avoid addressing why such projects are supported, by what criteria their contributions should be judged, and what can be learned from those already funded. Reiss has given an elegant description of the current state of economic analysis in this general area and concludes, "Current economic measures of returns to federal R&D at most provide crude historical statements about the contributions of federal R&D....[C]urrent methodologies simply do not allow us to say that because the returns to energy R&D in the late 1970's were miserable, we should expect them to be miserable in the future."[45] Mowery[46] and Hertzfeld[47] demonstrate the accuracy of Reiss's first statement. Examination of the large energy projects may lead to an understanding of why they failed and, perhaps, enable a change to be made in the underlying features of the U.S. approach to energy projects, so that the future will not be like the past.

As federal projects get large, political pressures increase,[48] particularly pressures by those who do not understand the technology involved. Susceptibility to such pressures is increased by the relatively small size of the projects in relation to the total federal budget. Although $50 or $100 million in a year can be overwhelming to those in the fields affected, amid the debate on a $1 trillion budget, a $100 million item may go unnoticed. Therefore, pressures often are applied outside the notice of news media and other political observers. As Deutch notes, "Understanding the balance sheet of who gains and who pays is important to understanding the political fate of a program."[49]

As discussed earlier, nuclear programs have dominated federal energy R&D funding, and several of the programs cited most frequently as problems are in this category. However, federal nuclear R&D also can be linked to national benefits. Nuclear power now provides approximately 15 percent of all electric generation in the United States and is approaching 100 gigawatts (GW) of capacity. During the period 1971 to 1982, in which some $15 billion (in 1985 dollars) were expended on nuclear research, many nuclear power plants were under construction or beginning operation.

In the period 1973 to 1984 (a two-year lag has been taken to allow some diffusion of results), 58 nuclear power plants received operating licenses, with a combined capacity of 50.7 GW.[50] (Three Mile Island-2 has been excluded.) An underestimate of the construction cost per plant in 1985 dollars would be $2,000 per kilowatt. (Palo Verde, a successful plant completed recently, is estimated to have a cost of $2,245 per kilowatt.[51]) Using $2,000 per kilowatt for 50.7 GW gives approximately $101 billion in capital investment. A crude comparison to the results expected in industry from R&D can be made using the fact that from 1921 to 1955, for each $1 spent on research, Dupont spent $3 for new plants, products, and processes.[52] Of course, the nuclear industry also spent a substantial amount in developing products during the period just described. Taking one half of the $100 billion, or $50 billion, to relate to government R&D, the Dupont 3-to-1, application-to-research ratio gives approximately $17 billion as being appropriate, compared to the $15 billion that was spent. Of course, much of the R&D on which the nuclear plants are based occurred in the 1950's and 1960's. However, the nuclear safety research was directly applicable, and the attempt here is to provide a comparison based on research as a percent of capital investment.

This comparison does not justify the spending, but does indicate that the amount of dollars spent in nuclear energy research has not been out of line with the related industry's capital investment. However, several features of that spending deserve examination. The first is represented by the criticisms addressed to several of the large projects. It is not clear that the federal government has a process to identify and manage large energy R&D projects. The radioisotope technology development and most, if not all, of the research that led to improvements in nuclear power were small projects, small at least by the many hundreds of millions or billion dollar scale of the large projects criticized above. Second, there appears to be an inherent bias against some types of research. This is reflected not only in nuclear versus non-nuclear arguments, since for many years there has been criticism of the United States' choice of the light water reactor, and then the liquid metal breeder reactor, instead of gas reactors. In an understatement, Deutch notes, "There is evidence to suggest that the federal government, partially as a result of poor judgment and partially as a result of political pressure, will not always adopt sound projects."[53]

There also is growing criticism from supporters of solar energy--both political and technical--who argue that the government has subsidized nuclear energy unfairly. Although nuclear support has been productive, the large dollar amounts spent on such projects as the Clinch River Breeder Reactor could have been spent much better elsewhere. As far back as 1955, Greenewalt wrote, "I wonder what our position would have been today had amounts of money and effort equivalent to those expended on atomic energy been devoted to the utilization of solar energy."[54] That same statement could have been made in 1965 and in 1975, and it can be made today.

A principal difficulty facing any large government project is maintaining support over many years. Instability of political support and shifting world conditions weaken continuity. In reviewing alternative energy programs, Teich has noted, "Most were never given a chance to succeed or fail on technical grounds. As in so many other instances of federal civilian R&D, the programs were cut off before a real assessment could be made."[55]

A former director of the EPRI fossil fuel and advanced systems division has said that the time required to get the first commercial plant on line is about ten years from the time the decision is made to design and build it.[56] Industry also has difficulty investing in R&D programs that take ten years to complete. Tilton noted that in the 1960's, manufacturing firms expected about 90 percent of their R&D to pay off in less than five years.[57] Industry can invest in projects that take a long time to complete, but these usually are at the commercial stage. For example, utilities have done this consistently on large base-load plants, although most utilities, at least in the past, have been guaranteed a return on such investments. In recent years, as utility commissions have raised the prudence argument, many utilities have stopped investing in expansions that will take a decade to complete. This regulatory questioning is similar to some congressional criticism of energy projects.

The federal government attempts to identify projects in which the private incentive to fund the research or the development is insufficient to give reasonable confidence that the project will be developed. These projects should be aimed at meeting a foreseeable national need. In the energy area, projects may relate to increasing the supply of energy, to increasing the absolute amount available or its distribution, or to decreasing the demand for energy through load management, conservation, and improved efficiency. In some cases, the risk is too great for industry to see a project as a worthwhile investment. In other cases, the return on the investment is likely to be spread across many companies and, therefore, not able to be captured for profitable return by a single company interested in investing in the research. Finally, in some cases, the need is seen to be so far in the future that company decisionmakers are unable to weight the need great enough to support current investment.

Brooks notes that the government, as it moves into the demonstration or engineering development phases of a project, often neglects to analyze future markets objectively.[58] In the past, the government frequently has attempted to use project sharing with industry as a mechanism to ensure sound market analysis. The underlying theory is that industry will not put up its own funds for projects for which it does not see a long-term market need. There are hazards associated with this approach, particularly in estimating the amount of industry contribution necessary to ensure that the project has undergone industrial scrutiny for potential markets. (Of course, there is always the problem that merely having the market analysis conducted by industry does not guarantee that it is sound. Many segments of American industry today are suffering from the lack of sound forecasts.)

It was this approach--that industry was heavily involved--that the government used for many years to justify continuing the Clinch River Breeder Reactor. However, of the $258 million pledged, 24 utility companies accounted for 53 percent of the funds by pledging an average of $5.7 million each. The other 699 utilities pledged an average of $174,000 each.[59] Furthermore, the utilities' original commitment was fixed. As the cost of the project rose from approximately $500 million to estimates of close to $3 billion, the utilities' contribution remained at $258 million.

Both liberal and conservative administrations have seen a need for federal government investment in energy research and development. A review of the failure rate of federal R&D projects does not differentiate noticeably between political philosophies of administrations. The failures, however, do not deny the validity of a need for nationally funded R&D projects. In general, the time horizon for private industry investment is too short to support the high-cost R&D needed to develop major new energy technologies for the late 1990's or early 2000's. There is sufficiently high probability that the United States will need such technologies to warrant an examination of the reasons that previous efforts have failed and, in particular, what new approaches can be taken to improve the probability of success.

REASONS FOR FAILURE

The failures have had many causes. These include:

- Ideologically chosen projects, or continued support based on ideological grounds far past the point at which prudent management would have canceled the project (CRBR is an example);
- Political pressure based primarily on geographical interests (for example, the MHD project);
- A poor understanding of the current state of technology on the part of government managers (for example, ISABELLE, the ill-fated Brookhaven accelerator project);
- An inadequate objective examination of future possibilities for both energy supply and demand (for example, GCEP);
- Imprecise or erroneous criteria for success: promising economic feasibility, which depends on world economic conditions at the time of project completion, instead of technological feasibility, which should be based on previous R&D programs;
- Control of funds by program advocates, with limited oversight by any review board;
- Instability of funding; and
- Instability of research direction, related to frequent changing of principal policymakers.

Although not addressing failed projects, Tilton, in 1974, noted several problems with federal R&D funding that are related to those listed above:

- Distortion in motivation,
- Inflexibility,
- Centralization of decisionmaking,
- Bias in project selection,
- Instability of government R&D funding, and
- Reduction of private R&D.[60]

More than ten years later, Brooks identified several similar problems:

- A tendency for government to hang on too long, or to distort the commercial judgment of the private sector by overpromotion;
- The more attractive the social benefits, the greater the likelihood that the private sector will see a commercial opportunity, but, also, the more political pressure there will be for the government to ensure the realization of the benefits in the shortest possible time; and
- Inadequate analysis of the market.[61]

Deutch has identified another problem: incompetent management. He notes that the conventional government practice is direct federal R&D contract support, which "always requires a good deal of governmental involvement in project management which at best obscures and sometimes ruins performance."[62]

The projects listed previously as failures suffered from several of these faults. The Clinch River Breeder Reactor was uneconomical when first proposed. James R. Schlesinger, then Assistant to the President for Energy, testified:

> At the time I was Chairman of the AEC I told the staff to go away and to bring me a cost/benefit-study on the demo plant by itself, and one could not emerge from such a study with a positive benefit/cost ratio, simply looking at the demo plant in isolation as an R&D experiment. It had to be embedded in an entire program of commercialization. So the Clinch River plant turned out to be integral to the program of commercialization. In order, in that study, to get favorable cost/benefit ratios, one had to assume perfect foresight [for example, a very high rate of growth of electric power demand and the breeder being the only technology

> option for the future]. Perfect foresight is not readily available to mankind, but it was embodied in that particular study.[63]

World events and U.S. reactions made the project even less economical, but, by the mid 1970's, it had become a symbol, as well as a large project. Thus, inflexible and distorted motivation factors prevented addressing the economic analysis. A member of the Council of Economic Advisers, who had developed a detailed familiarity with CRBR economics as a member of the National Academy of Sciences Committee on Nuclear and Alternative Energy Systems (CONAES) study, wrote in 1977 about "a growing body of evidence which supports that *the case for the LMFBR* [liquid metal fast breeder reactor] *program cannot be supported on economic grounds*."[64] (Emphasis in original.) Lest anyone now argue that the Clinch River Breeder Reactor was not viewed as an R&D program, it should be noted that in 1977 the Comptroller General of the United States reaffirmed that "the LMFBR program should be clearly identified and recognized for what it is: a research and development program." At the same time, the Comptroller General, perhaps unconsciously, identified the symbolic character of the program as he wrote:

> The President's decision to defer indefinitely the Clinch River Breeder Reactor...does not coincide with those positions we have taken in the past. In our view, this country should not now abandon the nuclear fission option nor should it abandon the LMFBR research and development effort.[65]

The causes for the other failures vary. The magnetohydrodynamic facility received its major impetus from its location--Montana--and was seen by many backers as support both for Western coal interests and for an extremely well-respected senator. The diffusion plant improvement projects were in support of government commercial operations. But they did fit Brooks' description:

> In the United States itself there are many examples of government technical initiatives in housing, transportation, [and] energy...that have failed in the market, largely because they were primarily motivated by the recognition of the technological opportunity without adequate analysis of the market.[66]

In the 1970's, the government supported three enrichment improvement programs simultaneously. While funding the $1.5 billion cascade improvement and operating programs, the government started the gas centrifuge plant (canceled after expenditures of over $2 billion), which was a combination demonstration project and government commercial venture, and also funded three advanced isotope separation projects at about $50 million per year. None of the programs was subject to an economic analysis to determine what would be the real market for uranium enrichment services. Local politics (in Tennessee and Ohio), international issues, nonproliferation concerns, and the power of the nuclear option block in Congress kept all enrichment programs alive.

The influence of Congress can be seen in Table 3, which shows the funding for breeder and coal research and development projects. During the Carter years, coal was favored and the breeder disdained. Congress supported increasing the coal budget, but resisted strongly the Carter Administration's attempt to cut back the breeder program. In the Reagan years, the roles have reversed. The White House has attempted to reduce coal funding sharply. Congress has gone along reluctantly, but has kept funding substantially above the Reagan Administration's request. On the breeder, however, the opposite is true. Finally, Congress concluded that breeder funding should be cut back drastically, but the Reagan Administration tried to maintain it. Table 3 also illustrates Tilton's point that federal program funding has high instability. For R&D programs, which by their very nature have a time to fruition of five to ten years, the substantial changes indicated by Table 3 can be devastating. Even when funding is not cut as drastically as the administration or Congress would like, staff in the program offices that face funding cutbacks realize clearly that they cannot make long-term commitments wisely, and, frequently, they leave for other opportunities.

Size may be an additional reason for the cited failures. The projects were simply too large for government control. Although often thought of as an overwhelming force, the federal government is more liks a large collection of single-celled creatures--it moves almost blindly, in slow, disorganized motion. The government succeeds in small projects--but so does private industry. The president of Control Data Corporation reported that small companies produce 24 times more innovations per dollar invested and 2-1/2 times more innovations per employee than do large companies.[67] This is consistent with Mansfield's findings that although the largest firms do most of the research, "they generally seem to carry out a disproportionately small share of the R&D aimed at entirely new products and processes."[68] When government funding gets large, political pressures or incompetence may lead to failure, usually in attempts to develop commercial products. Brooks has concluded that much of the influence of government on the development of commercial technology has been inadvertent,[69] and even a report from the Energy Research Advisory Board has observed, "Overall, few benefits of federal technology transfer programs have been noted in the past...."[70]

Given all these problems, to what extent can economic analysis be brought to bear on future decisionmaking in energy research and development, recognizing that energy R&D may not be much different from any other R&D? If one accepts the argument presented earlier that economic analysis cannot be applied effectively to basic research, what standards can be used for the later stages of R&D? If the past approach has done poorly, are there options that can do better in the future?

TABLE 3 Federal Funding for Coal and Breeder Research and Development; budget authority (current $, millions)

FISCAL YEAR	COAL ADMINISTRATION REQUEST	COAL CONGRESSIONAL APPROPRIATIONS	COAL CONGRESSIONAL ACTION (%)	BREEDER ADMINISTRATION REQUEST	BREEDER CONGRESSIONAL APPROPRIATIONS	BREEDER CONGRESSIONAL ACTION (%)
1977		409			749	
1978	527	579	+10	564[1]	575	+ 2
1979	618	681	+10	462	742	+61
1980	663	779	+17	590	762	+29
1981	1047	736	-30	384	681	+77
1982	381	513	+35	737	647	-12
1983	91	267	+193	577	550	- 5
1984	110	234	+113	603	423	-30
1985	178	253	+ 42	308[2]	198	-36
1986	149			161		

1/ The original request was for $714 M, but this included $150 M for CRBR, which the Administration subsequently attempted to cancel.

2/ The Administration's 1985 request dropped significantly from that of 1984 because the Congress terminated the Clinch River Breeder Reactor in 1984.

Data Sources: Narrative Highlights, Revised FY 78 Budget, ERDA, Feb. 1977, pp. 6, 14, 33.
U.S. Department of Energy, FY 1979 Budget to Congress, Budget Highlights, DOE/CR-0003, Jan. 1978, pp. 3, 8.
U.S. Department of Energy, FY 1980 Budget to Congress, Budget Highlights, DOE/CR-0004, pp. 4, 10.
U.S. Department of Energy, Budget in Brief 1981, DOE/CR-0012, pp. 8, 15.
U.S. Department of Energy, FY 1982 Budget in Brief, DOE/CR-0022, March 1981, pp. 8, 14.
Federal Energy Programs, FY 1983 Budget Highlights, DOE/MA-0062, Feb. 1982, pp. 9, 16.
U.S. Department of Energy, FY 1984 Budget Highlights, DOE/MA-0064/1, Jan. 1983, pp. 8, 14.
U.S. Department of Energy, FY 1985 Budget Highlights, DOE/MA-0062/2, Feb. 1984, pp. 10, 12, 16.
U.S. Department of Energy, FY 1986 Budget Highlights, DOE/MA-0062/3, Feb. 1985, pp. 10, 15.

ALTERNATIVE APPROACHES

The three stages suitable for government involvement in a project are exploration of concepts (basic and applied research), proof of concepts (development), and test of industrial application (demonstration). The fourth stage, commercialization, is inappropriate for the U.S. government. (If government is the final user, as it is of military products, then a different set of national goods is involved, with different, or at least additional, criteria.) In the three suitable stages, government funding can range from little to all, and federal involvement can vary from hands-off to direct project management.

Four questions should be addressed by the executive branch before entering each project stage (and congressional oversight should check that these questions have been asked):

(1) What are the potential benefits from the program for the United States? If federal funds are involved, national benefits should be the goal. For example, national benefits might be energy security, an improved economy, or meeting national defense needs.

(2) What are the likely costs? (This simple question is the bete noire of program advocates.)

Uncertainties should be considered in answering both (1) and (2), and there should be a realization that the future may be similar to the last 60 years, that is, a series of "surprises."

(3) Has the assessment screened out political factors, symbolism, and prior-commitment bias? If not, are these acknowledged explicitly, so their influence can be understood?

(4) What criteria will be used to judge success, and on what are those criteria based?

As projects progress into the later stages of development, and before they enter into demonstration, the following also should be answered:

(5) What are the performance characteristics and market conditions at project completion that are necessary for economic feasibility?

(6) What level of industry participation should be required? (Is a large amount needed to guarantee commercial usefulness of the project?)

(7) What should be the relative share of the risk for government and industry?[71] For example, should it be equal, so that if the project costs rise, the industry contribution also rises? If

industry has the principal role at the demonstration stage, particularly if the government is only a funder with no management role, should industry's share increase if the project overruns?

(8) Is the proposed management capable of handling the project? Normal managers can handle "human size" projects. Extraordinary managers are required for billion-dollar projects. (Wilson and Lederman of Fermilab, Low and Glennan of Apollo, and Groves and Oppenheimer of the Manhattan Project fit the latter description.)

These questions are simple; in fact, they are obvious. However, to paraphrase Chesterton, it is not that they have been tried and found wanting, it is rather that they have been found difficult and left untried. It is easy to ask questions. To answer them usefully requires objective analysis by knowledgeable people--a rare commodity. To use this list effectively will require unusual objectivity by the executive branch and rare restraint by Congress.

Answering the questions also would address a research need suggested by Brooks: "more effective and systematic ways of estimating the likely social returns...and comparing...the appropriable private return." He also suggested research on "institutional arrangements capable of blending market oriented and socially oriented decisions about strategy and technological development."[72]

Currently, federal energy research and development is funded almost entirely by the Department of Energy. Alternatives have been suggested, including the following:

(1) An elimination of most, if not all, federal R&D funding for large projects. This would take a step toward reducing federal expenditures at a time of great budget pressure, and it would recognize the failures of the past and assess the future as being not likely to be different from the past. It would retain some small projects, which both supporters and critics of federal R&D see as federal successes. Accepting the high-risk argument of Table 1, this approach would not bring new energy technology into being when needed.

(2) An expanded federal research and development agency, which would attempt to develop and retain or to bring in more competent program managers. However, such an agency would be even more subject to congressional pressure and targeted projects, and inherently would have the problems associated with centralized management, which often include an unwillingness to be objective.

(3) An expanded national laboratory system, in which energy research and development would be conducted essentially by full-time government (or government contract) employees. This arrangement could provide greater program stability, but it would suffer from the same conservatism and bias toward continuation of existing programs

that afflict the national laboratories currently. As a recent review of federal laboratories stated, "There are many opportunities for low quality research in pedestrian subjects...."[73]

(4) An independent but federally funded board, which would review projects proposed to it for possible, partial, or complete funding. The board could be similar to one set up recently to study the health effects of diesel emissions (the Health Effects Institute).[74] However, examination of the Synthetic Fuels Corporation (SFC) experience should provide insight into the pitfalls of such a board.

(5) A semi-autonomous agency, blending COMSAT and a scaled-down SFC, with--if it could be done--a carefully designated R&D mission. Deutch has described several advantages inherent in a nongovernment entity, particularly the ability to involve the private sector through the use of such indirect financial mechanisms as loan and price guarantees.[75] However, it is difficult to see this type of agency as anything more than another attempt at a Department of Energy.

Thus, not one of these alternatives has obvious success built in. However, there are features of several of them that would be beneficial to incorporate into the current DOE approach.

CONCLUSIONS

This paper has not examined in detail most of the research and development projects funded by the government in the past 20 years. Hence, these conclusions are tentative. However, based on what has been reviewed here, and on an undrstanding of both current U.S. energy neede and the status of energy technologies, the following conclusions are offered:

(1) Knowledge developed in basic research can take 20 to 30 years to become visible in successful commercial products or to have significant national impact.

(2) Basic research has strong anecdotal support; however, it is not amenable to standard quantitative economic analysis.

(3) Although the boundary between basic research and applied research is blurred, the latter in general has a more specific potential connection to application. Applied research also can be labeled exploratory or even advanced development.

(4) Many relatively small projects have been funded in applied research. However, little research has been done either to support or to criticize this generic category of R&D.

(5) Although basic research usually is inexpensive, applied research and advanced development can become expensive. A technical success can be an economic failure.

(6) The largest dollar amounts per program go to engineering development projects, which also may be called demonstration projects. These have the highest public visibility and have been studied more than any other energy R&D category.

(7) Large federal energy projects have a low success rate.

(8) Federal funding for research and development should be tested against several criteria:

(a) The project should be designed to meet national needs. A reasonable case must be made for how the project will help the nation. For example, projects supporting programs of federal responsibility such as environmental regulation are an acceptable class.

(b) Industry cannot capture enough of the benefits from the project, but a security premium makes it worthwhile for the nation.

(c) The risk involved is too high for private funders because the project is very costly, and there are large technical, demand, or regulatory uncertainties.

(9) There are several questions that should be addressed as a project moves through research, development, and demonstration.

(10) In assessing the answers to these questions, an independent panel of knowledgeable people would be useful as an objective buffer against bias and pressure. Individuals with industrial experience should be included. The Health Effects Institute model would be useful here.

(11) Demonstration programs must be subject to economic analysis to estimate possible markets objectively and cast the harsh light of reality on favorite concepts.

(12) Industry participation should be required for demonstration projects, with the percentage of participation increasing with project duration.

(13) Federal management should be restricted to the research and development stages, with industry management for the demonstration projects. The government is much better at establishing policy--what should be done--than in saying how it should be done, or doing it. Thus, the government should begin to step out during development, and certainly should not be involved in demonstration. The private sector is more likely to get together people who can handle large projects (not always, as the Edsel project and several nuclear plants

show--but failure there may be a pitfall of a regulated industry). If Congress (or an administration) deems the absence of government involvement during development and demonstration not to be prudent, then the government should at least contract out the management--and pay an appropriate salary. For example, if the government plans a several billion dollar project, then government should be willing to pay the manager $500,000 a year, five second-level managers $350,000 a year, and 20 third-level managers $200,000 a year. In constant dollars, given a ten-year project life for a successful large project, those salaries will cost $62.5 million--highly cost effective if a multibillion dollar project succeeds. (Variations might be included; for example, some percentage of the salary (such as 30 percent) could be held in a deferred account, payable upon successful completion of the project.)

(14) For the demonstration stage, government should explore further such indirect funding mechanisms as tax incentives, regulatory exemptions, and antitrust waivers.

(15) Finally, the government should invest in understanding the problems of the past, to learn how better to address the future. As the Energy Research Advisory Board has noted, "The technology base programs do not include any research in economics...which could be of considerable value in determining how and why decisions are made....A majority of the Board recommends that the Department [of Energy] initiate such research...."[76]

NOTES AND REFERENCES

1. *Energy Research, Development, Demonstration, and Commercialization*. Washington, DC: Congressional Budget Office, January 1977.

2. Using slightly different terminology for the stages--scientific feasibility, engineering development, engineering demonstration, and commercial plant--the costs have been estimated to be about $1 million, $10 million, $110 million, and $1 billion for a new process. See George R. Hill. "U.S. Fossil Fuel Industry: The Situation in the Year 2000: Coal." In *Technology and the Future of U.S. Industry*. Edited by James H. Gardner from a lecture series at the University of Utah. Indianapolis: White River Press, Inc. 1985, pp. 72-76.

3. John E. Tilton. *U.S. Energy R&D Policy: The Role of Economics*. Washington, DC: Resources for the Future, Inc., September 1974, pp. 3-7.

4. This paper uses the GNP deflator, as recommended by the National Science Foundation. This factor has been criticized by Mansfield. See Edwin Mansfield. "R&D and Innovation: Some Empirical Findings." In *R&D, Patents, and Productivity*. Edited by Zvi Griliches. Chicago: University of Chicago Press, 1984, pp. 129-130.

5. *Federal R&D Funding for Energy: Fiscal Years 1971-84*. NSF 83-301. Washington, DC: National Science Foundation, February 1983, Table 3 (modified), p. 3.

6. Ibid, p. 2, and *Science and Technology Data Book*. NSF 84-331. Washington, DC: National Science Foundation, October 1984, Figure 2.

7. Colin Norman. "Radical Surgery in Uranium Enrichment," *Science*, (June 21, 1985), p. 1407.

8. Stuart Diamond. "Synthetic Fuel Plant Scuttled," *New York Times*, (August 2, 1985), p. D1.

9. William A. Goddard III. "Theoretical Chemistry Comes Alive," *Engineering and Science*, (September 1985), pp. 3-7.

10. Crawford H. Greenewalt. "The Slow Steady Way of Progress." In *The Fabulous Future*. New York: E. P. Dutton, Inc., 1955, p. 108.

11. Ibid, p. 102.

12. Tilton, op. cit., p. 96.

13. Raymond S. Isenson. *Project Hindsight, Final Report*. Washington, DC: Office of the Director of Defense Research and Engineering, October 1969.

14. Mansfield, op. cit., pp. 128-129 (see Reference 4 above).

15. Harvey Brooks. "Returns on Federal Investments: The Physical Sciences," this conference.

16. Claude E. Barfield. *Science Policy from Ford to Reagan: Change and Continuity*. Washington, DC: American Enterprise Institute, 1982, p. 113.

17. Nicholas Wade. "Why Government Should Not Fund Science," *Science*, Vol. 210(1980), p. 33.

18. *The Role of the Department of Energy in Energy Research*. Unpublished speech, Cornell University Symposium for the Centennial of Electrical Engineering Education, Washington, DC, April 25, 1985, p. 3.

19. *DOE Annual Report to Congress*. DOE/S-0010/84. Washington, DC: U.S. Department of Energy, September 1984, p. 81.

20. *Valuing Environmental Goods: A State of the Art Assessment of the Contingent Valuation Method*. Edited by Ronald G. Cummings. New York: Rowman and Allanheld, forthcoming.

21. For a well articulated defense of basic research funding, as well as some thoughts on how to develop an economic rationale to support such, see Leon M. Lederman. "The Value of Fundamental Science," *Scientific American*, Vol. 251(November 1984), pp. 40-47.

22. *Budget of the United States Government, Fiscal Year 1984, Special Analysis K*. Washington, DC: U.S. Office of Management and Budget, 1983.

23. *The United States Budget in Brief, Fiscal Year 1979*. Washington, DC: U.S. Office of Management and Budget, 1978, p. 36.

24. Tilton, op. cit.

25. John M. Deutch. *Prospects for Synthetic Fuels in the United States*. Boulder, CO: Colorado Associated University Press, 1982.

26. *The Federal Role in Energy Research and Development, A Report of the Energy Research Advisory Board to the United States Department of Energy*. DOE/S-0016. Washington, DC: Department of Energy, February 1983.

27. Harvey Brooks. "Science Policy and Commercial Innovation," *The Bridge*, Vol. XV(1985), pp. 7-13.

28. The U.S. antitrust laws often have been described as prohibiting collections of companies from joining together to fund research projects. Several years ago, 19 U.S. companies in the computer and semiconductor fields formed the Microelectronics and Computer Technology Corporation to work on computers of the future, specifically in competition with Japan. The National Cooperative Research Act of 1984 was written to ease the antitrust barriers to such projects. See William C. Norris. "Cooperative R&D: A New Strategy," *Issues in Science and Technology*, (Winter 1985), p. 95.

29. Tilton, op. cit., p. 29.

30. David C. Mowery. "Federal Funding of Research and Development in Transportation: The Case of Aviation," this conference.

31. In the context of synthetic fuel programs, the security premium "is the difference between the market price for oil (which is the basis for private sector decisions) and the value to this nation for reducing oil imports." See Deutch, op. cit., p. 12.

32. Tilton, op. cit., p. 126.

33. Deutch, op. cit., pp. 12-13.

34. *New Electric Technologies*. Washington, DC: Office of Technology Assessment, 1985, p. 3.

35. *Federal Energy R&D Priorities, A Report of the Energy Research Advisory Board to the United States Department of Energy*. Washington, DC: Department of Energy, December 1981, p. 7.

36. *Energy Technology Policy*. ISBN 92-64-1268880. Paris: International Energy Agency, 1985, p. 13.

37. Tilton, op cit., p. 117.

38. *The Federal Role in Energy Research and Development*, op. cit., p. 8.

39. All large projects need not be government funded. In the 1960's, IBM is reported to have invested $5 billion to produce its System 360 family of computers, including over $500 million on R&D programs. See T. A. Wise. "I.B.M.'s $5,000,000,000 Gamble," *Fortune*, (September 1966), pp. 116, 120.

40. *Federal Funds for Research and Development, Fiscal Years 1980, 1981, and 1982, Volume XXX*. NSF 82-321. Washington, DC: National Science Foundation, p. 4.

41. *Energy Conservation and the Federal Government: Research, Development, and Management*. DOE/S-0017. Washington, DC: Energy Research Advisory Board, January 1983.

42. David Okrent. "On the History of the Evolution of Light Water Reactor Safety in the United States." U.S. Nuclear Regulatory Commission, Washington, DC, pp. 5-19 - 5-21; published as *Nuclear Reactor Safety: The History of the Regulatory Process*. Madison: University of Wisconsin Press, 1981. See also N. M. Newmark. *A Study of Vertical and Horizontal Earthquake Spectra*. WASH-1255. Washington, DC: U.S. Atomic Energy Commission, April 1973.

43. Personal communication from Leopoldo Solis, Deputy Director, Banco de Mexico, S.A.

44. Greenewalt, op. cit, p. 101.

45. Peter C. Reiss, "Economic Measures of the Returns to Federal Research and Development," this conference, p. 21.

46. Mowery, op. cit.

47. Henry R. Hertzfeld. "Measuring the Economic Impact of Federal Research and Development Investments in Civilian Space Activities," this conference.

48. For a negative view of the political factors, see Roger G. Noll and Linda R. Cohen. "Economics, Politics, and Government Research and Development," this conference.

49. Deutch, op. cit., p. 18.

50. *Summary Information Report*. NUREG 0871. Washington, DC: U.S. Nuclear Regulatory Commission, June 1984, pp. 2.10-2.23.

51. "Palo Verde Cited as Relatively Cheap Despite Pump Problems," *Nucleonics Week*, (September 15, 1983), p. 5.

52. Greenewalt, op. cit., p. 100.

53. Deutch, op. cit., p. 9.

54. Greenewalt, op. cit., p. 106.

55. Albert H. Teich. "Federal Support of Applied Research: A Review of the United States Experience," this conference, p. 15.

56. George Hill, op. cit., p. 75.

57. Tilton, op. cit., p. 127.

58. Brooks, "Science Policy and Commercial Innovation," op. cit., p. 8.

59. "Breeder Reactor Corporation Member Systems." Memo from Eric S. Beckjord, Director, Division of Reactor Development and Demonstration, Energy Research and Development Administration, June 6, 1977.

60. Tilton, op. cit., p. 45.

61. Brooks, "Science Policy and Commercial Innovation," op. cit., pp. 7-13.

62. Deutch, op. cit., p. 14.

63. Testimony of Assistant to the President James R. Schlesinger before the House Committee on Science and Technology, June 7, 1977.

64. Letter to Senator Dale Bumpers from William D. Nordhaus, July 7, 1977.

65. Letter to Honorable Olin E. Teague, Chairman, House Committee on Science and Technology, from Elmer B. Staats, Comptroller General of the United States, June 14, 1977.

66. Brooks, "Science Policy and Commercial Innovation," op. cit., p. 8.

67. Norris, op. cit., p. 96 (see Reference 28 above).

68. Mansfield, op. cit., p. 129 (see Reference 4 above).

69. Brooks, "Science Policy and Commercial Innovation," op. cit., p. 9.

70. *Industrial Energy Research and Development, A Report to the Energy Research Advisory Board*. DOE/S-0029. Washington, DC: Department of Energy, May 1983, p. 5.

71. A 1976 report for the Department of Commerce noted the difference between cost-sharing and risk-sharing, and strongly recommended both for government-industry projects. See Walter S. Baer, Leland L. Johnson, and Edward W. Merrow. *Analysis of Federally Funded Demonstration Projects: Final Report*. Rand R-1926-DOC. Santa Monica, CA: Rand Corporation, April 1976, pp. 48, 56, 76.

72. Brooks, "Science Policy and Commercial Innovation," op. cit., p. 13.

73. *Report of the White House Science Council, Federal Laboratory Review Panel.* Washington, DC: Office of Science and Technology Policy, May 1983, p. 9.

74. The Health Effects Institute is funded by automobile manufacturers, fuel companies, and the Environmental Protection Agency. Research funds are allocated by a committee that decides what knowledge is needed and who can best do the necessary research. The committee issues requests for proposals (RFP's) and awards research contracts. When the research is completed, it is evaluated by a second committee, *independent* of the first. The evaluation committee decides if the work was state of the art, and what follow-on work is needed.

75. Deutch, op. cit., pp. 13-15. See also *Federal Financial Support for High-Technology Industries.*" Washington, DC: Congressional Budget Office, June 1985, pp. 57-80.

76. *The Federal Role in Energy Research and Development*, op. cit., p. 2.