THE POLITICAL RATIONALITY OF AMERICAN SCIENCE

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ABSTRACT

Government support for science in the United States has been primarily a pragmatic response to perceived needs. Since World War II, as American science has undergone an explosive growth fueled by federal dollars, the support provided by the American people has carried with it expectations not just for theoretical advances, but for tangible developments to help preserve life, protect the blessings of liberty and aid in the pursuit of happiness.

Given the importance of pragmatic rationales for science, one might expect those fields which promise the greatest and most certain practical benefits to receive the strongest support, and one might seek a corresponding concern of politicians for a demonstrable link between means and ends, for a close accounting of the payoffs for resources expended, and for efficient allocation of those resources among competing scientific ventures.

In reality, areas of science which seem to offer little payoff gain large-scale support, initiatives which have never delivered on initial promises escape critical reassessment, and fields which seem to promise great benefits remain shut out of the big leagues of science.

In order to understand observed science policy one must understand how that policy was forged within the political process. What interests were involved, what needs were addressed, to what perceptions did the actors within the executive and legislative branches respond? From this perspective, I have examined three branches of science whose fates seem to confound naive, "rational" expectations.

The first and most detailed of these case studies deals with high-energy physics. Progress in high-energy physics depends on huge particle accelerators, which are among the costliest scientific tools. Yet these devices serve only a relatively small community of scientists in one of the most esoteric branches of pure science, and seem to promise little in the way of useful spinoffs. Yet high-energy physics has, from the start, maintained strong support.

I review the history of the American high-energy physics program in seven chapters, focusing on its financial relationship with the broader society which supports it, and emphasizing the key developments in that relationship and the factors which account for them. An additional chapter
reviews and categorizes the rationales cited on behalf of the program. In the aggregate, these rationales constitute both a claim about the relationship between high-energy physics and society as a whole, and the conceptual backdrop against which political decisions have been made.

Two additional, single-chapter case studies follow, dealing with cancer research and oceanography. The "war on cancer," formally launched in the early 1970's, has roots within the federal government dating back over fifty years. The conquest of cancer is a goal universally understood and shared, and every American potentially stands to benefit from progress in the field. After decades and billions of dollars expended on research, however, the threat of cancer looms as large as ever. Each year, developments in basic biomedical science advance our understanding of cancer biology, but their application in the form of widely successful treatments seems always tantalizingly out of reach. Yet support for cancer research remains robust.

Oceanography seemed poised to join the ranks of big science in the 1960's when, emboldened by the Great Society, ocean advocates envisioned the seas as key to solving environmental problems, eradicating global hunger, and surmounting limits to land-based mineral and fuel resources. Despite worthwhile goals, supporters of ocean science were unable to advance into the major leagues of government-sponsored research. Significant sums have been spent on ocean-related projects. Yet today, as environmental problems have renewed the focus on the oceans as a critical part of the global ecosystem, attempts to launch large-scale initiatives in ocean science remain stalled.

The final chapter examines the differences among the three programs, identifying factors shaping federal support for science, including the workings of Congress and the legislative process; the interface between Congress, the Administration and the scientific community, and the nature and impact of science advice within the political process; the role of organizational factors in federal budgeting; and the role of chance developments and historical precedent.

The key finding of this study is that the success of different areas of science in the competition for funding depends largely on institutional and political factors only indirectly or not at all related to the substance of science and its practical applications; the incentive structure of American politics works against substantive evaluation and oversight of science programs. But the present system of allocating money for science in America is a success in political terms, offering substantial benefits at relatively low cost to those politicians associated with it.

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Harvey Sapolsky has been an extraordinarily patient mentor throughout the long and difficult gestation of this thesis, alternating between activity and reactivity in a delicate balance that served to guide my work through some difficult passages, keeping it alive and well and seeing it through to its successful completion. I consider myself fortunate to have had the benefit of his wise counsel.

I am fortunate as well to have had Carl Kaysen as a member of my committee. At every stage of this project, he has challenged me to clarify and to justify my ideas and their expression in the form of this thesis. I have had to work harder — and I have been richly rewarded not only with
a better thesis, but with greater understanding as well.

From that long-ago day when I first appeared in his class as a senior medical student, Stan Finkelstein has been a teacher, colleague, advisor, consultant, tour guide and friend. As director of the program in health policy and management, he has dealt with problems, both expected and unexpected, with resilience, grace and good humor. I am grateful for his support.

I have learned much from my friends and fellow students in the Laboratory for Health Care Studies. The centrifugal forces generated by such a motley group have rendered the boundaries of our "laboratory" increasingly tenuous over time - but that same vitality has helped make this journey such a memorable experience. Thanks, too, to Kathie Eisenhaure for cheerful and efficient administrative and logistical support, and for always being there to lend a sympathetic ear.

*   *   *

I wish that I could turn back the clock, so that I could deliver a copy of this thesis to my mother. To the very end of a long and difficult illness, she continued to fuss about how her own difficulties were standing in the way of my thesis. Given the magnitude of her own pain and suffering, that was both an extraordinary and a ridiculous thing for her to be doing - and a profound indication of how much I owe to her.
But it will be with great pleasure that I will deliver it to my father, who has been awaiting it patiently for almost three decades. I won't take any stand here on the question of nature vs. nurture. I think, though, that in these pages my father will see how much I have inherited from him, one way or another, and how much I owe to him as well.

Thanks to Johanna Perlmutter for her encouragement, and for absorbing an unreasonable dose of orneriness without complaint. Thanks, too, to my brothers for their encouragement, to Roni for proving that there is life after school and to Jonathan for traveling the long and winding road to a PhD in perfect sync with me.

Finally, I owe a special debt of gratitude to Bill and Susan Klein, who reached out to set things right when all seemed quite wrong. Their support, and their generous sponsorship of a somewhat unorthodox physics experiment, are what made this thesis possible.
1. INTRODUCTION

... Since health, well-being, and security are proper concerns of Government, scientific progress is, and must be, of vital interest to Government. Without scientific progress the national health would deteriorate; without scientific progress we could not hope for improvement in our standard of living or for an increased number of jobs for our citizens; and without scientific progress we could not have maintained our liberties against tyranny...

Vannevar Bush(1)

Merely to agree that basic research is a good thing does not necessarily justify Federal support for it and in particular gives no basis for determining how much support should be provided for what kind of basic research.

It is inevitable that primary support should be given to those fields of natural science where potential payoffs in national security, health, and economic growth are obviously high even if uncertain in location and character.

Lyndon Johnson(2)

The rhetoric of politics cannot always be taken at face value, for public speech is itself a political tool, deployed where needed to gain advantage in the behind-the-scenes confrontation and negotiation which ultimately determine the direction of government. But when the substance of the rhetoric associated with a particular realm of policy is strikingly consistent over a long period we may be justified in concluding that it reflects some underlying reality of American politics.

The underlying reality of science policy in America is
a widely-shared faith in the practical benefits of basic scientific research. Whether speaking from the White House, from the floor or the committee rooms of Congress, or before gatherings of constituents across the nation, American politicians articulate a clear and consistent vision of what they believe they are buying with the taxpayer’s dollar: a better life for all Americans. In the years since the second world war, as the scientific community has undergone an explosive growth fueled largely by federal dollars, the generous support provided by the American people through their elected representatives has carried with it high expectations not just for theoretical advances, but for tangible developments that help preserve life, protect the blessings of liberty and facilitate the pursuit of happiness.

This political reality is not lost on the American scientific community, as evidenced by the rhetoric of scientists and administrators before government when money for science is at stake. The overwhelmingly pragmatic thrust of the rationales proposed by scientists is underlined by the occasional dissenter who laments the departure from a rhetorical high road on which basic science would be justified solely by its intellectual virtue.

Given the persistent and fundamental importance of pragmatic rationales in the support of science, one might expect that those branches of science which promise the
greatest and most certain practical benefits to the nation would receive the strongest support, and one might reasonably look for a corresponding concern of politicians for a demonstrable link between means and ends, for a close accounting of the payoffs for resources expended, and for efficient allocation of those resources among competing scientific ventures.

In reality, we observe that areas of science which seem to promise little practical payoff have gained large-scale support, initiatives which have never delivered on initial promises have escaped critical reassessment, and fields which seem to promise great practical benefits remain shut out of the big leagues of science. Clearly there is more to the process of resource allocation for science than meets the eye.

What are we to make, for example, of the government’s expenditures on that branch of science known as high-energy physics? High-energy physics, or elementary-particle physics, has been described as "the study of the basic nature of matter, of force, of energy, of time, and of space."(3) High-energy physicists seek to understand matter, energy, and the forces that govern their interactions by studying the creation and destruction of the unimaginably small subatomic particles which are thought to constitute the basic building blocks of the macroscopic universe. The primary experimental apparatus used in high-
energy physics is the particle accelerator, in which beams of elementary particles are generated, boosted to high energies, and collided with fixed targets or other beams before sophisticated detectors so that the creation, destruction and interactions of such particles can be studied in a controlled fashion.

At its present stage of development, high-energy physics is so advanced and esoteric that only a few thousand individuals are capable of arguing its subtleties, while perhaps a fraction of one percent of the American population understands enough about it to discuss intelligently its place in the context of scientific thought and practice. The physical phenomena which are the present focus of the field occur at energies orders of magnitude beyond the most intense that man has ever attempted to harness, let alone those energies characteristic of the processes of our daily existence and of life itself.

Yet federal government obligations for high-energy physics in fiscal year 1988 totaled over half a billion dollars. The proposed FY90 budget, which exceeded $850 million, included initial construction funding for a next-generation particle accelerator, the Superconducting Super Collider, which was projected to cost $6 billion or more by the time of its completion, and to require $250 million or more annually for its operation. In 1988, even before the projected expansion took effect, high-energy physics
received 6.6% of all federal basic science dollars to support the work of a community which accounted for only about 3 percent of all active basic research scientists and only 0.6% of PhD students in science. (4)

This commitment to the continuation and expansion of a high level of funding for high-energy physics has been made at a time when many other branches of basic and applied science, and the improved scientific education widely believed to be critical to the nation's future in an increasingly competitive world, are seen as at risk in the face of the federal government's struggle to contain a deficit in its annual budget which has exceeded $100 billion for several years running. When virtually all existing programs face a budget squeeze, and new programs are believed to be sustainable only at the expense of existing ones, the apparent affluence of the high-energy physics program raises questions about the government's ability to chart a judicious course for its program of basic science.

Consider, as well, the government's "war on cancer," which was launched with a formal declaration amid great fanfare in the early 1970's, but has roots within the federal government dating back more than fifty years. The conquest of cancer is a goal which is universally understood and shared, and every American potentially stands to benefit directly from progress in the field. After several decades and billions of dollars expended on research, however, the
threat of cancer looms as large as ever. Each year, new developments in basic biomedical science make significant contributions to our understanding of the biology of cancer, but the application of new concepts in the form of widely successful treatments seems always tantalizingly out of reach. Yet support for the National Cancer Institute and its mission remains robust.

Finally, how are we to account for the fate of oceanography? The science of the seas seemed poised to join the ranks of big science in the 1960's when, emboldened by Johnson's Great Society initiatives, supporters of ocean science envisioned the seas as key to solving environmental problems, eradicating global hunger, and surmounting limits to land-based mineral and fuel resources. Despite a long list of worthwhile goals, advocates of oceanographic research were unable to carry their science into the major leagues of government-sponsored research. Significant sums have been spent on ocean-related projects. Yet today, as once again environmental problems have renewed the focus on the oceans as a critical part of the global ecosystem, attempts to launch large-scale initiatives in ocean science remain stalled.

In this study I seek to account for the seemingly paradoxical fates of these three branches of American science, and in doing so to understand more generally the factors that shape American government science policy. I
will try to explain the apparent gap between the simple rhetoric and the complex reality of that policy, seeking in the development of these three programs traces of a complex tangle of interests and expectations that will guide us to a more realistic understanding of the way science policy is made.

I emphasize at the outset that it is not my purpose here to pass judgment on the programs themselves, although in the course of this work I will necessarily report facts and interpretations which are relevant to such judgments. However, I will review arguments on behalf of and in opposition to the programs as I seek to understand the role that such advocacy plays in setting the direction of policy. In doing so, I adopt the attitude that what is true of political rhetoric in general is equally true of advocacy in science policy: while the substance of such arguments is not irrelevant to an understanding of policy, by itself it is incomplete and potentially misleading, for in the real world there is no way of balancing arguments in order to yield an "objectively determined" direction for policy.

To understand why this is so, it is instructive to recall two efforts to bring some "rational" order to the setting of priorities in science. During the early 1960's, as the Sputnik-induced boom in funding for science neared its end under the fiscal pressure of the growing involvement in Vietnam, the Committee on Science and Astronautics of the
House of Representatives posed two questions to the National Academy of Sciences:

I. What level of Federal support is needed to maintain for the United States a position of leadership through basic research in the advancement of science and technology and their economic, cultural, and military applications?

II. What judgment can be reached on the balance of support now being given by the Federal Government to various fields of scientific endeavor, and on adjustments that should be considered, either within existing levels of overall support or under conditions of increased or decreased overall support?(5)

In response to these questions, an ad hoc panel organized by the Academy's Committee on Science and Public Policy presented a series of individually-written essays on various aspects of the problem. As a summary essay reported,

Nobody on the ad hoc panel challenge[d] the proposition that the purposes of Government, as opposed to the techniques of Government, are nonscientific. Thus, the question to which question I naturally leads: Why should our society support basic science at all, and the corollary question: how much basic science should we support? must be answered in terms that generally lie outside science.(6)

Instead, panel members attempted to justify public support for science on the basis of its contributions to more or less concretely defined goals of society, such as national defense, public health, economic well-being, and education.

As panelist Harry Johnson pointed out,

From the point of view of economic analysis, research is conceived of as one form of investment of resources, the investment involving the use of human and material resources to acquire knowledge and the return resulting from the application of that knowledge to increase human welfare in one
way or another.

Granting that basic research plays an important role in the American economy, Johnson wrote,

This fact by itself, however, does not constitute a case for Government support of basic scientific research, though scientists frequently write as if it did... In order to establish a case for Government support, it must be shown that basic research yields a social return over its cost that exceeds the return on alternative types of investment of resources.(7)

Or, more precisely,

The economic argument... would require allocating resources among scientific fields so as to equalize the prospective social rates of return from marginal expenditure on each field, and fixing the total of resources allocated to basic research at the level yielding a marginal rate of return on all investment in basic research comparable to what is earned on other forms of investment, or else equal to the rate of interest at which the community is willing to forego the alternative of consuming the requisite resources.(8)

Even if one accepts economists’ market failure arguments as establishing a presumption of inadequate private funding for research and justifying public investment as a necessary supplement, one still must determine the extent of the deficit and the amount of the investment needed to rectify it; the information necessary to make such calculations was, and still is, simply unavailable. Not surprisingly, then, the other panelists did not make them, most resorting instead to arbitrary, rule-of-thumb procedures which tended either to endorse the existing level of government support or to recommend that it be increased. While several of the
panelists complemented Johnson's analysis with insightful observations about the difficulties inherent in tracing the linkages between basic research and societal benefits and the factors which were relevant to policy decisions, when it came to specific recommendations the panel's report to Congress was, in effect, simply more of what politicians are used to hearing: subjective pleading on behalf of a (possibly meritorious) special interest, poorly disguised as objective analysis.

Johnson, along with other economists before and since, was content to observe that more economic research is needed. However, it is difficult to conceive of a feasible program of research which could extract that which the economic approach appears to demand: a rigorous set of causal relationships among the countless tangible and intangible factors that bear on the productivity and impact of basic research.

If a rigorous neoclassical demonstration of the route to Pareto-optimality was not within our grasp, however, perhaps a more informal approach to reasoning about resource allocation could lead to suitably impartial decisions. As part of a debate on problems of scientific choice which ran in the pages of the journal Minerva during the early 1960's, Alvin Weinberg proposed several criteria to guide resource allocation for science. Within a given scientific field, "internal criteria," or professional judgments about the
quality and importance of research, were important guides. But broader questions of resource allocation required evaluation of science with respect to "external criteria," which captured the interaction between science and society. "Technological merit" referred to the contribution of science to desired technological ends; "scientific merit" involved the contribution of science to the strengthening and unification of the broader network of knowledge of which all of science is a part; and "social merit" addressed the relevance of science to human values and the welfare of man.(9)

As Jean-Jacques Salomon has observed in a detailed analysis of Weinberg’s approach, however, these criteria are "non-operational:" while they categorize and make explicit some of the conceptual issues relevant to the problem, they give no help in matching the presumed scale of values with allocations of actual dollars in the budget. Nor are the proposed criteria so "objective" as Weinberg hopes, reasonable though they seem at first glance. Weinberg’s own application of these criteria to the assessment of several different scientific and technical fields is thoughtful and instructive but also so obviously a product of one individual’s outlook as to remind us that the criteria themselves arose from a strong set of personal values.(10)

As Salomon points out, referring to the claimed linkages between basic research and societal goals,
... as soon as we try to delve deep into the nature of these goals, it becomes apparent that none of them lends itself to rigorous demonstration; value judgments inevitably outnumber findings of fact and even further outnumber unchallengeable conclusions. This is not only because any discussion of the goals of society sparks off an ideological debate, it is also because in this debate the postulates 'it is just as if...' or 'all other things being equal...' refer both to such a multitude of heterogeneous facts that it is idle to look for any rational link between them, and to an irreversible evolution of the research system which has its repercussion on the whole definition and orientations of modern society....

... It is clear that, owing to the nature of research activities, the choice among them at government level cannot represent a rational calculation in the sense of expressing the logic of scientific thought. The terms of the relation between science and power remain conditioned by the objectives of power rather than by a rationally established conjunction between the objectives of the one and the objectives of the other; the scientific debate on the overall orientation of the research effort is doomed to be one political debate among many others.(11)

If a scientistic approach offers only the illusion of an objective derivation of means from ends, there nonetheless remains for the advocate of science one escape from the messy, slippery task of justifying basic research in reference to societal needs. This is to argue for science as a "cultural good," or, in economic terms, a consumption good, in its own right. For the scientist, this argument can be a natural extension of personal values which find their expression through a fulfilling career. But it, too, is ultimately political, for it is a debatable assertion about the values which should form the basis for
public policy.

Thus, my approach in this study is explicitly political. Despite the mystique of science, public spending for its support, like public spending on anything else, can be understood only within the context of the overall process by which public priorities are established and expressed in the form of the government budget. The rhetoric of government policy conceals a complex and ambiguous reality; things are not always as they seem. But while the process of resource allocation appears messy and irrational, it often embodies more order than is commonly realized as it captures a chaotic jumble of conflicting goals and perceptions in a concrete and manageable form which allows the government to function.

To understand science policy one must understand how that policy was forged within the constraints of the political process: what interests were involved? what needs were addressed? to what perceptions did the actors within the executive and legislative branches respond?

The purpose of this study is to answer two questions, one specific and the other general. What accounts for the differing fates of high-energy physics, the war on cancer, and oceanography? And is the supposed pragmatism of American science policy myth or reality? I seek to answer these questions, and thereby to contribute to our understanding of the "political rationality" behind American
science policy, through a comparative study of the development of the three programs introduced here.

The approach of this study is descriptive and explanatory, and not prescriptive. Its goal is to understand American science policy on its own terms. I believe that such an understanding is essential to any attempt to improve policy, and I hope that the findings reported here will contribute toward that end. However, to draw normative conclusions would require an analysis of certain more fundamental issues, not related to science per se, which extend far beyond the scope of the present work.

I devote the greatest attention to the high-energy physics program, which as the most esoteric and most basic of the three research programs cited, offers perhaps the strongest test of the power and relevance of pragmatic rationales.

I begin by examining the history of the high-energy physics program, which I tell in seven chapters following this introduction. In my review I focus on the financial relationship between high-energy physics and the broader society which supports it, with an emphasis on the key developments in that relationship and the factors which account for them.

Following the history I devote a chapter to a more detailed review and categorization of the rationales which have been cited on behalf of the high-energy physics
program. In the aggregate, these rationales constitute both a claim about the nature of the relationship between high-energy physics and society as a whole, and the conceptual backdrop against which political decisions have been made.

The chapters on the high-energy physics program are followed by single chapters presenting condensed histories of the cancer research and ocean science programs in order to provide the basis for a comparative analysis.

That analysis is presented in the final chapter, in which I summarize the factors which account for the relative success of the three programs and describe their implications for the role of pragmatism in American science policy. I conclude by suggesting possible extensions of this study to allow testing and generalization of its conclusions.

The development of the so-called standard model which is at the heart of elementary-particle theory today is a fascinating and important episode in the history of science, filled with the twists and turns, dead ends and surprises which make basic research such a challenging and fulfilling pursuit.(12) The story of the institutional and political arrangements which made this great achievement possible is remarkable in its own way, encompassing brilliant and ambitious scientists and politicians, questions of high national purpose and petty political horse-trading, and the conflicts which inevitably arise over the disposition of
huge sums of money.

Although the origins of the American high-energy physics program predate the involvement of the federal government, the evolution of the enterprise during its earliest years was to affect the relationship with the federal patron in ways which persist to the present day. Accordingly, it is with a bit of prehistory that I begin.
Notes


2. ORIGINS

... A quite different tradition was established with the building of the first cyclotron by Ernest O. Lawrence in 1930. It has spread from his laboratory at the University of California and has come to dominate experimental nuclear physics in this country....

This tradition, called "berkelitis" by its detractors, is a true departure in experimental physics....

Robert R. Wilson(1)

It all began with Ernest Orlando Lawrence.

True, as particle physicists take pride in reminding us, their field can in an important sense be considered simply a continuation of a distinguished tradition of inquiries into the nature of matter and energy, a tradition which can be traced back to the ancient Greeks. But this point of view is an abstraction which does not begin to account for the emergence of the specific institutions and the professional, political and financial relationships by which the ancient quest manifests itself in our own time.

And true, it was J. D. Cockcroft and E. T. S. Walton, not Lawrence, who first achieved the artificial disintegration of atomic nuclei by bombardment with accelerated particles. But Cockcroft and Walton were working within the string-and-sealing-wax tradition of Ernest Rutherford's Cavendish Laboratory in Cambridge, England.
Rather, it was Lawrence, an American, who, combining scientific innovation with a flair for promotion and a free-wheeling approach to organization building and fund raising, set particle physics on a course which it still follows today.

As early as 1919, Rutherford had called for the development of a particle accelerator as a way of overcoming the limitations of the scarce, naturally radioactive substances then used for nuclear disintegration experiments. During the 1920's, the quantum revolution in theoretical physics made the need for experimental observations of the properties of atomic nuclei and their constituent particles ever more acute, and by the end of the decade, many experimental physicists were attacking the accelerator problem through a variety of approaches.

As one of the most promising young physicists in America, Lawrence was well aware of the importance of the results which might be obtained from a powerful accelerator. In 1929, while browsing in the library, he came across a paper in which the Norwegian engineer Rolf Widerøe described a method for accelerating potassium ions via resonance with a high frequency oscillating voltage. Working out the implications of the scheme, Lawrence realized that charged particles could be accelerated to disproportionately high energies with a given alternating voltage signal if the path of the particles were bent with a magnet into a circle so
that the gap between a single pair of electrodes could be used to boost the same particles repeatedly. With the help of a graduate student, Lawrence built a crude 4-inch device with properties which appeared to be consistent with his theoretical conception. In September, 1930, he discussed and demonstrated the device before a meeting in Berkeley of the National Academy of Sciences. Eager to continue building a first-rate physics department, aware of Lawrence's growing reputation and of attractive offers he was receiving from other institutions, the incoming president of the University of California at Berkeley, Robert G. Sproul, overrode opposition from senior faculty in other departments to promote Lawrence, then only twenty-nine, to a full professorship at a salary of $5000.

Lawrence and his students worked to refine the 4-inch device, but with his mind already set on the idea of accelerating particles to the magic goal of a million electron volts (MeV) of energy, he was already looking ahead to the design of a larger machine. A $1000 grant, extracted "with little trouble" from the National Research Council, allowed his team to build an 11-inch device which by February, 1932, reached and passed the 1 MeV mark.(2) When, shortly thereafter, word was received of the achievement of Cockcroft and Walton in disintegrating lithium with a proton beam, recognition that the energy required was well within the reach of their machine brought only momentary
disappointment for Lawrence and his group. Offering
congratulations to the Cavendish Laboratory, Lawrence
quickly set up experiments to confirm and extend the
observations of Cockcroft and Walton, while pressinward
in the quest for higher energies.

Achieving higher energies required bigger and more
elaborate "cyclofrons," however, and as the nation slipped
into the Great Depression, the expenses involved represented
a significant obstacle. As his machines grew bigger,
Lawrence was forced to devote increasing time and energy to
seeking support for his work. To save money, he scavenged
parts wherever he could. When he learned that a huge magnet
core intended for a now-obsolescent radio wave generator was
gathering dust in a nearby warehouse, he talked its owner,
the Federal Telegraph Company, into donating it. The fact
that the expense of hauling it across the bay to Berkeley
would be large and that he as yet had no place to put it
when it arrived he considered only a minor complication.(3)
When the group began to realize the full extent of the
danger from unshielded X-ray tubes in the laboratory,
Lawrence obtained a loan of 350 tons of quarter-inch lead
sheeting from the American Smelting and Refining Company in
San Francisco.(4)

He traveled extensively in search of funds. Following
a demonstration in Berkeley of the production of million-
volt protons by the 11-inch device, Dr. Frederick Cottrell
of the Research Corporation advised Lawrence to think big: "If you want a large sum, ask for it. It is easier to get five thousand dollars than five hundred." (5) Cottrell introduced Lawrence to corporation president Howard Poillon, personally pleaded his case before the corporation board, and arranged for Lawrence to meet the board himself. Even though the corporation was in the red as a result of the depression, its board was sufficiently impressed by Cottrell's backing and by Lawrence's enthusiasm to borrow funds sufficient for a $5000 pledge. Poillon introduced Lawrence to William Buffum of the Chemical Foundation, who pledged another $2500. (6) Later, both organizations were to give additional sums, though Poillon suggested that Lawrence would be in a better position to request help if the University were to demonstrate its own commitment. (7)

Back on campus, President Sproul provided breathing room for Lawrence's growing operation by transferring an old wooden building that was being vacated by an engineering group, appropriating $3000 in University funds for the necessary renovations, and authorizing an official appeal to the local electric company to contribute the power.

The new Radiation Laboratory continued to grow, attracting graduate students and visiting faculty and research fellows eager to participate. Increasingly, it drew public attention as well. Lawrence made for good newspaper copy, because of both his achievements and his
Again, when Irène and Frédéric Joliot-Curie announced the discovery of induced radioactivity, Lawrence found himself in the position of racing off to the laboratory and arranging for the rapid duplication and confirmation of a major discovery which could easily have been his. Consistently, he was in too much of a hurry to develop and promote his accelerator ideas, to be able to take the more reflective approach necessary to obtain the full scientific return from each stage of the cyclotron's development.

There were some applications of the cyclotron which did capture his sustained attention, however. Foremost among them was the medical use of radioactive substances produced using the cyclotron and of direct irradiation using neutron beams generated by the cyclotron. His interest was genuine, and he pursued it vigorously in collaboration with his brother John, a Harvard-trained physician. Very quickly, though, he found medical applications a powerful stimulus to the generosity of potential benefactors. In 1935 he wrote to the great theorist Niels Bohr:

In addition to the nuclear investigations, we are carrying on investigations of the biological effects of the neutrons and various radioactive substances and are finding interesting things in this direction. I must confess that one reason we have undertaken this biological work is that we thereby have been able to get financial support for all of the work in the laboratory. As you well know, it is so much easier to get funds for medical research. (8)

Similarly, from a letter to Rutherford around the same time:
... I was in the East about two months, engaged in my annual task of raising money for the support of our work in the radiation laboratory. I rather expected considerable difficulty in raising needed funds this year, and indeed was rather worried that we might have to restrict our work a great deal, but fortunately matters turned out otherwise. In this country medical research receives generous support, and it was the possible medical applications of the artificial radioactive substances and neutron radiation that made it possible for me to obtain adequate financial support....(9)

The successful treatment of their mother for a malignant growth in 1937, using equipment designed at the Radiation Laboratory for the University of California Medical School in San Francisco, certainly reinforced the Lawrence brothers’ belief in the importance of medical applications of work in nuclear physics.

Lawrence was pleased when Radiation Lab alumni started cyclotron programs at other institutions, and was always delighted to share his expertise with interested colleagues in America and Europe. The diffusion of the cyclotron across the United States was assisted by funding offered in the name of medical research; at the Rockefeller Foundation, Warren Weaver made grants from the biology program in support of nuclear research and the construction of cyclotrons, as did the medically-oriented Macy and Markle foundations. Lawrence himself received much support from individuals and foundations interested in medical progress, including one of the first three grants awarded by the National Advisory Cancer Council in 1937.(10)
In 1936 an attractive offer from Harvard gave Lawrence the leverage to extract substantially increased support from the University of California for his work. To President Sproul he presented a shopping list which included an entirely new laboratory with a larger cyclotron intended primarily for medical and biological research, a small permanent staff with a continuing budget for support, and an increased salary for himself. During a continuing economic depression, he submitted to the president's office a budget including $25,000 for a new cyclotron magnet weighing approximately 200 tons and $40,000 a year to support medical work with the new machine. Within a few days Sproul announced that the Radiation Laboratory would officially become an independent entity within the physics department, with Lawrence as its director; a full-time assistant director would be provided along with a secretary, a machinist and $20,000 a year in the university budget. Sproul promised as well to discuss the proposal for a new cyclotron and laboratory with William Crocker, one of the Regents of the University of California, who had previously provided funds for medical research in the Radiation Laboratory. (11) Lawrence decided to stay.

Naturally, some of the Regents were suspicious of the young professor with the expensive tastes. John Francis Neylan decided to investigate for himself:

I found out where his laboratory was. It was like a secondhand tin shop - a dinky little place over
there on the campus, just a cul-de-sac. That’s where he had his little old cyclotron, which was insulated, incidentally, with five-gallon gasoline tins filled with water - that was the insulation wall of the cyclotron. I went in there. I met him. Then I was introduced to Dr. So-and-So from Cornell, and Dr. So-and-So from Japan, and Dr. So-and-So from this place and that place - and I want to tell you I was flabbergasted.... That day Ernest got talking, explaining to me the principle of the cyclotron. And of course, after he got started, after the first minute and a half, he was so far out beyond me I didn’t know where he was going. His face lighted up - he was lost in this subject. I was tremendously impressed - attracted to him.(12)

Ultimately, Regent Crocker agreed to provide funds for the new laboratory, to contain a 60-inch cyclotron and to be named after him.

The 60-inch cyclotron was not fully functional until mid-1939, but by then Lawrence was already devoting much attention to yet another, still larger machine, particularly after the stunning announcement of the fission of uranium atoms with a neutron beam by Hahn and Strassmann in Germany. When a model cyclotron was set up at the Golden Gate International Exposition in San Francisco, Lawrence spoke publicly for the first time of his determination to build a cyclotron ten times as large as the new 60-inch device. He had already discussed the idea with Warren Weaver of the Rockefeller Foundation, which by this time had granted tens of thousands of dollars to the Radiation Laboratory.(13)

A memorandum to President Sproul dated October 10, 1939, mentions a 2000-ton magnet - although this size was to grow further as the design evolved - and energies of between
100 and 200 MeV. In the memo, Lawrence referred to the "completely successful performance of the 60-inch," and went on to describe "compelling reasons for going forward without delay on a cyclotron ten times larger." To the same reasons used to justify the earlier machines he added, "The richness of the field opened up by the cyclotron is evidenced by the fact that practically every laboratory of any importance in the world now either has a cyclotron in operation or has one under construction. There are more than 35 cyclotron projects underway... we at California have a unique opportunity to pioneer a new domain... until we cross the frontier of a hundred million volts, we will not know what riches lie ahead, but that there are great riches there can be no doubt... evidence of the cosmic rays convinces every physicist that this is so... may be able to tap the unlimited store of energy in the atom..."(14)

As plans for the new device became ever more elaborate, Lawrence's promotional juggernaut moved into high gear, energized by his receipt of the 1939 Nobel Prize in physics. Sproul, momentarily fazed by the audacity of Lawrence's plans, regained sufficient composure to ask the regents for $85,000 a year for ten years - as much as all other departments combined spent on research. Lawrence himself made a presentation to the regents, and then to the National Academy of Sciences; the 60-inch cyclotron was featured prominently in Life magazine, while endorsements of the
project were collected from scientists across the country and in Europe.

When the dust had settled, the Rockefeller Foundation trustees had voted $1,150,000 for the project. The university regents followed with $250,000 and the Research Corporation with another $50,000, while the United States Steel Corporation and Phelps Dodge Products Corporation assured that adequate supplies of increasingly hard-to-get steel and copper would be reserved for the project. (15) The Rockefeller Foundation had previously helped finance the great 200-inch telescope atop Mt. Palomar; now Warren Weaver described the new cyclotron as "the definitive instrument for the investigation of the nucleus - the infinitesimally small - just as the two hundred inch telescope is viewed as the definitive instrument for the investigation of the universe - the infinitely great." (16) Raymond B. Fosdick, president of the Rockefeller Foundation, felt that this "titanic tool of science... might bring into fresh focus the enigma of the universe, its apparent order, its beauty, its power." As the darkness of Nazism descended upon Europe, Fosdick viewed plans for the huge cyclotron as an affirmation of internationalism in science, and of democratic, scientific civilization. (17)

The launching of the project for the 184-inch cyclotron marked the maturity of the Radiation Laboratory at Berkeley and the coming of what was later called "big physics,"
distinguished not only by expensive machines but by the large, interdisciplinary organizations needed to build and run them. No doubt, the advance of science and of engineering would ultimately have led to devices like Lawrence's cyclotrons in any case. But Lawrence's distinctive personality and talents left their mark on this new field.

As time passed, and the demands of planning and institution-building grew, Lawrence gradually gave up personal participation in research, though he kept a close watch on the full range of activities in the Radiation Laboratory. Lawrence was an extrovert, and was good company at social events. As his fame and fortune increased with the passing years, he came to enjoy the good living that came with them, and the many contacts and friendships that developed with prominent industrialists, politicians, lawyers, physicians and civil servants. His direct approach, his self-confidence, his own genuine achievements in physics and his ability to bring out the best in students and colleagues never failed to impress, and his able handling of high officials of government, industry and academia contributed greatly in the difficult task of raising money during a depression. Without malice towards other departments, but with no regrets, he demanded and got an increasing share of the resources of the University of California at Berkeley until his Radiation Laboratory "tail"
was clearly wagging the university "dog" of which it was nominally a subservient part. It was all worth it for the advancement of physics, he felt; from a position of strength he was able and delighted to share ideas and offer assistance to those who wished to spread the gospel of the cyclotron. (18)

Before the 184-inch cyclotron could be completed, however, the attention of Lawrence and the rest of the American physics community was diverted to more pressing matters. The experience of World War II was to bring profound changes to American physics and its relationship with government and society, changes that would help sustain the growth of the then-nascent field of high energy physics, cultivated so assiduously by Lawrence, long after he was to pass from the scene.
Notes


5. Quoted in Childs, *An American Genius*, p. 166. Cottrell, himself a Berkeley chemist, had invented a process to recover chemicals from industrial smokestack emissions and transferred the patent to the Research Corporation, which was a non-profit entity set up to manage the patent rights and distribute the profits in support of research. See also Daniel Kevles, *The Physicists* (Cambridge, MA: Harvard University Press, 1987), p. 268.


18. These aspects of Lawrence's achievement are discussed in Oliphant, "The Two Erinests - I", and Childs, *An American Genius*. The latter contains much anecdotal detail on Lawrence and the Radiation Laboratory, including an interesting passage on relations between the laboratory and the rest of the university (see pp. 248-250). Another good review of the early development of the Radiation Laboratory can be found in the first two chapters of J.L. Heilbron, Robert W. Seidel and Bruce R. Wheaton, *Lawrence and his Laboratory: Nuclear Science at Berkeley* (Berkeley, CA: Lawrence Berkeley Laboratory and Office for History of Science and Technology, University of California, Berkeley, 1981).
3. WAR AND ITS AFTERMATH

... There is, of course, a fashion, which prescribes that everybody who can talk the Office of Naval Research out of enough money builds a big machine to make very high energy particles....

... The Navy may be said almost without hyperbole to own all of nuclear physics which is not owned by the Manhattan District....

Philip Morrison(1)

What would we think if the Army should start to spend its funds to improve the health of our youth? We would not criticize the Army.... We should, however, direct our criticism against the civilian authorities for neglecting public health to the point where the Army finds it necessary to do something about it.

The present situation of the laboratories is similar....

Edward Teller(2)

If World War I, during which key scientific efforts had revolved around developments in explosives, poison gases and production of high-quality optical glasses, had been in the view of many observers a "chemist's war," by World War II it was the physicists' turn to take center stage. For the duration the American physics community set aside its usual work to participate in several huge, elaborate, yet highly secret research and development programs which resulted in advanced radar systems, the proximity fuse and the atomic bomb.

As Vannevar Bush was assembling what was to become the
Office of Scientific Research and Development, a number of physicists scattered across the country were pondering the potentially explosive implications of nuclear fission. Ernest Lawrence was a central participant in these behind-the-scenes deliberations. In 1941, Glenn T. Seaborg, working in the Radiation Laboratory at Berkeley, created the element plutonium by bombardment of uranium with neutrons from one of the cyclotrons. Reacting to news that British scientists had estimated that the quantity of the uranium isotope U-235 required for a fission bomb could be as little as ten kilograms, Lawrence himself developed the idea of converting a cyclotron into a large mass spectrometer for the separation of uranium isotopes by electromagnetic means. As the government's own inquiry into the defense implications of nuclear fission stumbled and sputtered along, Lawrence joined other prominent physicists in pressing for a stepped-up program of atomic bomb research.

Ultimately, Lawrence was to play a key role in the war effort, both as a scientific advisor and consultant and as director of the electromagnetic separation isotope production program within the Manhattan Engineer District atomic bomb project. Unlike most of the other physicists involved in the Manhattan Project, who chafed at the security restrictions and other side effects of working on a program dominated by military concerns, Lawrence got along well with General Leslie Groves, who directed the project.
with a clear strategic vision, a firm administrative hand, and little patience for anything he felt distracted attention from the urgent task at hand. (4) The rest of Lawrence's team at the Radiation Laboratory joined in the war research as well, either as part of the electromagnetic separation project or on assignment to other projects where their expertise proved useful.

The Radiation Laboratory at Berkeley expanded considerably to accommodate the demands of wartime research. Until as late as mid-1944, Lawrence expected that after the war the laboratory would return rapidly to its prewar size and scope. (5) However, as the atomic bomb project neared its successful conclusion and the work in some parts of the vast Manhattan Project began to wind down, some scientists began to entertain new ideas about the funding of basic research in the postwar era. Scientists at the Metallurgical Laboratory in Chicago, a center for nuclear reactor research which had completed most of its contribution to the bomb project by early 1944, appealed for a continued effort in basic research to run beyond the end of the war. Groves believed that attention must be focused on the pressing task of completing an atomic bomb, and was wary in any case of overstepping the bounds of his authority, which was to carry out a limited task for a specific, military purpose. In response to agitation by the scientists he placed strict limits on the amount of
extracurricular work which could be carried out within the Metallurgical Laboratory, and proposed placing the lab on standby status by the fall of 1944. (6) Vannevar Bush and Arthur Compton, director of the Chicago lab, channeled the scientists’ discontent into the preparation of a pair of reports advocating national support of a comprehensive program of nuclear research after the war. But with efforts focused on the demands of the moment, the reports had no immediate impact, disappearing into Bush’s own behind-the-scenes efforts to lay the groundwork for later planning of postwar research.

A Scientific Panel formed in May, 1945 to advise the Interim Committee on Atomic Energy included Lawrence, who stressed the need for the United States to stay ahead in both atomic bomb and nuclear energy research. Bush, as head of the Interim Committee, agreed, but taking into account the unsettled state of plans for the postwar organization of research, asked the Panel not to plan research, but to prepare proposals which could be considered by the appropriate agency when the situation was resolved. (7)

Lawrence and his colleagues had no trouble coming up with ideas. The 184-inch cyclotron remained unfinished, its huge magnet having been diverted during the war for research on electromagnetic separation. During his work on radar at the MIT Radiation Laboratory, Luis Alvarez developed ideas for the application of radio-frequency generators to a
linear accelerator for electrons. Edwin McMillan, working during the war at Los Alamos, had conceived the phase-stability or synchrotron principle, which opened the way to accelerators of much higher energies than had been achieved before.

But as a member of the Scientific Panel, Lawrence saw as well the reams of proposals that were pouring in from around the country, including many plans for basic research with accelerators. Unwilling to let his Radiation Laboratory get lost in the shuffle as plans for demobilization of the Manhattan Engineer District were sorted out, Lawrence went directly to Groves with his proposed program.(8) Still wary of setting an unwarranted precedent, but appreciative of Lawrence's contributions during the war, Groves transferred $250,000 worth of Army surplus radar sets for Alvarez's linear accelerator and $203,000 worth of capacitors for McMillan's synchrotron, with the understanding that these were to be used for "pilot plant" development. In December, 1945, he authorized construction of the electron synchrotron at an estimated cost of $203,000 under the contract then in force, and allowed $170,000 for completion of the 184-inch cyclotron after Lawrence documented excess expenses due to the war.(9)

When the May-Johnson bill to set up an Atomic Energy Commission encountered a storm of controversy around the issue of civilian vs. military control of atomic energy
affairs, hopes for a rapid resolution of the postwar research situation faded. Complicating matters further, Vannevar Bush's proposal for a National Research Foundation, to accept responsibility for funding basic research, was itself to be swallowed in Congressional politics; creation of the National Science Foundation was to be delayed until 1950, with meaningful funding not to follow until even later.

Now Groves faced the imminent disintegration of the Army's nuclear research organization. In early 1946, he appointed an Advisory Committee on Research and Development to help create plans to preserve the research program until it could be reestablished under proper authority. The committee proposed a broad program including continuation of essential weapons-related research, support of fundamental research of an unclassified nature in universities and private laboratories, and creation of several national laboratories, including one in the Chicago area as successor to the Metallurgical Laboratory - what was to become the Argonne National Laboratory, and one in the northeast - the future Brookhaven National Laboratory, and proposed offering Lawrence's Radiation Laboratory at Berkeley status as a special type of national laboratory. Groves responded by scheduling $72.4 million in research expenditures, with the bulk going for construction of facilities at existing Manhattan District installations and at the Argonne,
Brookhaven and Berkeley labs, while substantial funds were provided as well to other institutions including the University of Washington, the University of Rochester, Iowa State, Columbia, the Massachusetts Institute of Technology, and the Battelle Memorial Institute. The Military Appropriations Act of July 16, 1946, was the first specific appropriation of federal government funds for atomic research and development. (10) While most of the money was intended to keep the weapons program going, fundamental nuclear research, including accelerator work, was generously supported as well.

While Groves in the Army was trying to keep the Manhattan Engineer District program alive, the Navy was maneuvering into position as a patron of research as well. A wartime proposal for a future peacetime central office of naval research, advanced by a group of resourceful young naval officers, the "Bird Dogs," travelled a long, tortuous route up and down the Navy bureaucracy until it surfaced unexpectedly as the program for Admiral Harold Bowen's new Office of Research and Inventions. Bowen, not previously known for cordial relations with the scientific community, had been fighting his own battles within the Navy hierarchy, his power and influence fluctuating considerably over the course of the war. Now once again in a position of some authority, he began to marshal his forces in support of his interest in nuclear propulsion for the Navy.
Bowen observed that many civilian scientists, alienated by Groves's strict discipline, and ready in any case to return to their prewar pursuits, were quitting the Manhattan District and returning to the universities. Seeing potential allies for his cause, Bowen sent Captain Robert Conrad across the country in the fall of 1945, selling university presidents on his new program of research funding, calming their fears of military domination by promising an administrative structure which minimized red tape and allowed scientists virtually complete freedom in the conduct and publication of their research.

Ironically, the ups and downs of Navy politics soon caught up with Bowen, and the Navy's interest in nuclear propulsion was removed from his jurisdiction. By the time the Office of Research and Inventions had been converted by act of Congress into the Office of Naval Research, in August, 1946, Bowen was finished in the Navy, retiring three months later. Yet he left behind a functioning organization with 177 contracts in force at 81 universities or private and industrial laboratories, totaling $24,000,000. (11) Funding for nuclear physics, including extensive accelerator-based research, was an important part of the ONR program: for fiscal 1947, out of a total research budget of slightly over $13,000,000, ONR allocated $4,500,000 to high-energy nuclear physics. (12)

During the war, physicists learned how to think big,
and how much they could accomplish with free-flowing money. Even Ernest Lawrence, himself the pioneer of Big Science before the war, had his horizons expanded considerably. But prewar sources of support were grossly inadequate to support the professional style to which the physicists had become accustomed. Only the federal government could conceivably command the resources to finance such an ambitious program of science in the postwar era.

With plans for a National Science Foundation and an Atomic Energy Commission bogged down in Congressional politics, the military stepped in to fill the vacuum. The support they offered was surprisingly generous, and liberal in its terms. Many scientists objected to military domination of funding for basic research. Philip Morrison echoed the thoughts of many when he spoke before the New York Herald-Tribune Forum on October 29, 1946:

Some of the apprehension that workers in science feel about this war-borne inflation comes from their fear of its collapse. They fear these things: the backers - Army and Navy - will go along for a while. Results, in the shape of new and fearful weapons, will not justify the expenses, and their own funds will begin to dwindle. Then now-amicable contracts will tighten up and the fine print will start to contain talk about results and specific weapon problems. And science itself will have been bought by war, on the installment plan. The easy money for the fashionable problems will lead to an unbalanced development within science itself. The teaching of students - right now a desperately pressing problem if we are to have any science in the years to come - will suffer because of the urgency of research demands. The practice of patenting scientific results and techniques will spread from the industrial laboratories where it was built and
the government laboratories, where its value is doubtful, to the laboratories everywhere, where its effect will be to destroy the traditional free cooperation of science. The armed forces are always sooner or later concerned with secrecy, and with the restrictions such concerns imply on the travels, publications, and even the characters and background of their research workers. Such restrictions will greatly harm our science. It will become narrow, national, and secret. Above all, and in spite of every protestation, American science will appear to the world as the armorer of a new and more frightful war. We are not far from giving that appearance today.\(13\)

Like many others, Morrison promoted the concept of a National Science Foundation as the solution to the dilemma facing scientists. Yet the NSF was not to become reality until several years later, and in the meantime scientists faced the hard choice: military money or no money.

For nuclear physicists, whose elaborate equipment made the most extravagant demands on the budget, the choice amounted to physics or no physics, particularly at a time when their field was bursting at the seams with ideas for expensive new machines capable of responding to the pressing demands of state-of-the-art theory. The situation was captured in a song written by physicist Arthur Roberts in 1946, which made the rounds of the physics community and ultimately appeared, complete with music, in Physics Today:

\textbf{TAKE AWAY YOUR BILLION DOLLARS}

\begin{quote}
Upon the lawns of Washington the physicists assemble,
From all the land are men at hand, their wisdom to exchange.
A great man stands to speak, and with applause the rafters tremble.
"My friends," says he, "you all can see that
\end{quote}
physics now must change.
Now in my lab we had our plans, but these we'll now expand,
Research right now is useless, we have come to understand.
We now propose constructing at an ancient Army base,
The best electronuclear machine in any place, Oh

It will cost a billion dollars, ten billion volts 'twill give,
It will take five thousand scholars seven years to make it live.
All the generals approve it, all the money's now in hand,
And to help advance our program, teaching students now we've banned.
We have chartered transportation, we'll provide a weekly dance,
Our motto's integration, there is nothing left to chance.
This machine is just a model for a bigger one, of course,
That's the future road for physics, as I hope you'll all endorse."

And as the halls with cheers resound and praises fill the air,
One single man remains aloof and silent in his chair.
And when the room is quiet and the crowd has ceased to cheer,
He rises up and thunders forth an answer loud and clear:
"It seems that I'm a failure, just a piddling dilettante,
Within six months a mere ten thousand bucks is all I've spent.
With love and string and sealing wax was physics kept alive,
Let not the wealth of Midas hide the goal for which we strive. Oh

Take away your billion dollars, take away your tainted gold,
You can keep your damn ten billion volts, my soul will not be sold.
Take away your army gen'rals; their kiss is death, I'm sure.
Ev'rything I build is mine, and ev'ry volt I make is pure.
Take away your integration; let us learn and let
Oh, beware this epidemic Berkelitis I beseech.
Oh, dammit! Engineering isn’t physics, is that plain?
Take, oh take, your billion dollars, let’s be physicists again."(14)

Physicists sang the song, but in the end they took the billion dollars, too. Certainly, some physicists, their viewpoint represented by the Bulletin of the Atomic Scientists, kept their distance from the military and continued to sound the alarm about the hazards of military domination. But with the rise of the Soviet Union as a military power and a perceived threat, many scientists made a smooth transition from World War II to the Cold War. Not all believed that the military connections were inherently evil, and some, notably a group centered on Lawrence in Berkeley, and including such luminaries as Alvarez and Edward Teller, were committed Cold Warriors. As Lawrence’s old friend Merle Tuve put it, scientists had a "definite, irreducible and nontransferable responsibility" to shoulder their fair share of the nation’s defense burden.(15)

Why did the Army and Navy step in to fill the gap in funding for basic science after the war? In part, they were acting out of a public-spirited recognition, widely shared in light of the experience of the war, that "scientific strength is national strength and the best preparedness for any eventuality."(16) Too, much of the money did go for obviously defense-related work - for example, Groves’s investments in maintaining the Manhattan Project
infrastructure, crucial to the production of atomic weapons. There was an element of chance involved — no one could have predicted that the maneuverings of the Bird Dogs and of Admiral Bowen were to result in the ONR, a free-spirited patron of pure research which set its own course for quite some time until, inevitably, it was reined in and restricted to programs of more direct interest to the Navy.

But there was a deeper reason as well. Support from the military for basic research was concentrated in the physical sciences, and especially in nuclear physics. World War II had been the physicist's war; radar, the proximity fuse, and above all the atomic bomb were widely seen as miracles of physics. And indeed, plutonium had been created in one of Lawrence's accelerators and his 184-inch machine had served as the foundation for the electromagnetic isotope separation program, and was a key to the success of the atomic bomb program. Of course, the unfinished 184-inch machine in truth served only as a huge magnet, not as an accelerator. And the electromagnetic separation approach was ultimately superseded by more efficient methods. But was it not also true that accelerators were used to reveal the principles which governed nuclear forces, principles which were essential to understanding and controlling nuclear energy for both peaceful and military purposes?

There was considerable confusion among non-physicists at the time as to the relationships among the different
branches of the science and the development of weaponry. An incident which occurred in November, 1945 is revealing on this score. American occupation forces in Japan, operating under orders from Washington, confiscated and destroyed the cyclotrons at three Japanese universities. Many American scientists were furious, seeing the incident as an indication of the Army's intent to retain a stifling degree of control over all aspects of nuclear science. Yet the destruction appears to have been the consequence of an innocent but telling misunderstanding. The General Staff order instructing that all enemy war equipment be destroyed was explicit in requiring that enemy equipment not essentially or exclusively for war, which was suitable for peacetime, civilian use, was to be spared. But cyclotrons were widely perceived as instruments of war, as a draft statement prepared for Secretary of War Patterson made clear:

...the action taken was in implementation of the established policy of the United States that the Japanese should be prevented from engaging in any activity related in any way to war making.

In order to insure peace for generations to come we desire to eliminate to the maximum extent possible, the Japanese war-making potential. While it is recognized that a cyclotron may be used for scientific research in other fields, it is essential to the carrying out of atomic bomb research which our government believes should be prohibited to naturally belligerent and dishonest nations. (17)

Groves, who had a more sophisticated understanding of the situation, stepped in and convinced Patterson to issue
instead a statement which described the destruction as a mistake. But the damage, to both the cyclotrons and the Army's reputation among scientists, was done.(18)

Physicists themselves, for all their protestations that their work in basic research had no military relevance, somehow managed to avoid relieving those who held the purse strings of their supposed illusions. The following remarkable passage appeared in an article about accelerators written by Edward Teller:

This great building program in the physics laboratories may look to the outsider like work on new mysterious weapons. The development started immediately after the power of atomic bombs was demonstrated. The laboratories will produce particles carrying more concentrated energy than was encountered in the atom bomb....

Even more specific arguments are heard. Matter can be transformed into energy according to the, now notorious, relation of Einstein: \( E = mc^2 \). Yet in the atomic bomb only a small part, less than one per cent, of the energy was so transformed. Clearly, the physicists are searching for complete transformation of energy into matter. The search is carried into regions of increasingly high energies. The result, if the search is successful, will be new and vastly more dangerous weapons of destruction.

Actually the preceding arguments are misleading. In spite of appearances the construction of big machines marks a return of physicists to their peacetime pursuit: the search for the laws of nature. This search goes forward with little expectation of practical results and very little probability that the high energy machines will be particularly helpful in the construction of bigger bombs.(19)

Teller couldn't have expressed a justification for military involvement any better - and he wrote in the Bulletin of the
Atomic Scientists, no less. Under the circumstances, an outsider could be forgiven for dismissing the last paragraph as lip service to the prevailing mythology of science.

Physicists learned as well as anyone the art of selling a program of research to a potential patron. In a published debate about whether military support constituted a threat to science, Louis Ridenour related the following anecdote:

...I have a friend who is a band spectroscopist on the faculty of a large state university. He has been particularly interested in the band spectrum of the element nitrogen. He once said to me: "When the representatives of the state legislature visit me, I always tell then I am trying to make better fertilizer." There is to be sure nitrogen in fertilizer and knowledge is power. It is just conceivable that my friend's investigations of the band spectrum of nitrogen may some day affect the fertilizer industry in some unexpected way. But it is undeniable that his interest is in spectroscopy, in and of itself.(20)

Physicists had a well-developed sense of what kind of fertilizer the armed forces were interested in purchasing.

Finally, the hard-to-gauge factor of personal influence should not be overlooked. Lawrence maintained good relations with Groves at a time when many Manhattan District physicists were highly critical of the general's leadership, and did not join in the chorus of condemnation at the time of the Japanese cyclotron incident. Lawrence is also said to have arranged the honorary degree awarded Groves by the University of California in recognition of his achievements.(21) Groves trusted Lawrence and respected him for his genuine achievements. When Groves felt that
circumstances required him to take action to preserve the
great research system that was still in his trust, it is not
surprising that Lawrence, and the branches of science that
he favored, were key beneficiaries.

By 1947, accelerator physics found a secure
institutional home within the federal government. But
federal support was not to follow the National Science
Foundation model supported by most scientists, in which the
government was to make a substantial commitment to a
vigorous, independent basic science for its own sake.
Instead, particle physics found itself under the umbrella of
the Atomic Energy Commission, and particle physicists were
to become an establishment more prominent and secure than
would ever have been possible under an NSF.
Notes


Mr. RABAUT. What is this thing you are talking about?

Dr. JOHNSON [Director, AEC Division of Research]. What I would like to bring out is that as a result of these experiments with the high-energy machines there has been a major advance in scientific thinking which will pervade the whole of science in the future. . . . It is occurring because we do have these large facilities which cost a lot of money. . . .

. . .

Mr. RABAUT. Is this related to a breakdown after you get through with the atom? Is it an electrical field? What is it? I realize you know something about it, but you have not imparted it very well to me.

Dr. JOHNSON. With these large accelerators you bombard a target with high-energy particles. Out of the target come effects, radiation, which are studied. Now, the nature of these rays that come out of a target that has been bombarded has been quite a puzzle. There are new kinds of particles which no one has ever seen before. . . .

These particles are of very short life. They decay and they produce other things. There is a tremendous variety of them. The whole study involves many, many experimentalists and many, many people, theorists as well as experimentalists. It is a whole new field of science that is building up for the first time around these large machines. Our progress is very encouraging. It is very fundamental. It is the most fundamental thing you can think of in the structure of matter. Just what its application will be, no one can tell at this time.

. . .

Mr. RABAUT. Specifically, how would advancements
in bringing the benefits of this new science to mankind be set back if the program were held to the 1957 level?

Dr. JOHNSON. That is a question I would hate to have to answer....(1)

With the signing of the Atomic Energy Act of 1946 and the creation of the Atomic Energy Commission, a long and bitter battle over the issue of civilian vs. military control of the nation's atomic energy efforts appeared to have been resolved in favor of civilian control. On January 1, 1947, the operations of the Manhattan Engineer District were transferred from the Army to the new AEC.

According to the Act, the new commission was, among its other responsibilities,

... authorized and directed to make contracts, agreements, arrangements, grants-in-aid, and loans -

(1) for the conduct of research and developmental activities relating to (a) nuclear processes; (b) the theory and production of atomic energy, including processes and devices related to such production; (c) utilization of fissionable and radioactive materials for medical or health purposes; (d) utilization of fissionable and radioactive materials for all other purposes, including industrial uses; and (e) the protection of health during research and production activities; and

(2) for studies of the social, political, and economic effects of the availability and utilization of atomic energy....(2)

For the first few months of its existence, however, the AEC was preoccupied with an overwhelming array of other problems demanding immediate attention: taking administrative

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control of the vast but rapidly decaying research and production empire of the Manhattan District, finding competent staff, and establishing goals and priorities for its first years of work.

But proposals began flowing in from universities, other Government agencies, private companies, and the AEC's own national laboratories, as scientists who had waited impatiently for a resolution of the postwar uncertainty moved to press their claims under the new agency's mandate for research. The situation was brought to a head in the spring of 1947 when the Office of Naval Research, running out of its own allocation, asked the AEC for $4.1 million to support its program in high-energy physics. The director of research for the AEC, James B. Fisk, was wary of making any commitments. In the absence of a clearly defined AEC policy interpreting the provisions of the Act of 1946, he took a skeptical stance, evaluating proposals in terms of their relevance to a fairly narrow and literal interpretation of the AEC's mission.(3)

Along with the AEC itself, the Atomic Energy Act of 1946 created a General Advisory Committee to "advise the Commission on scientific and technical matters relating to materials, production, and research and development."(4) The first GAC consisted of J. Robert Oppenheimer, the brilliant theoretical physicist who had directed the atomic bomb design work at Los Alamos; James B. Conant, chemist,
president of Harvard, and close associate of Vannevar Bush on the wartime Office of Scientific Research and Development; Lee A. DuBridge, president of Caltech and a key figure at the MIT Radiation Laboratory during the war; Enrico Fermi, the physicist who had designed and constructed the first nuclear reactor; I.I. Rabi, another physicist alumnus of the MIT Radiation Laboratory; Glenn Seaborg, known for his research on transuranium elements at Berkeley; Cyril Smith, a metallurgist who had been a division leader at Los Alamos; Hood Worthington, a chemical engineer from DuPont, who had supervised construction of the Hanford atomic fuels production plant during the war; and Hartley Rowe, an electrical engineer for the United Fruit Company who had held a variety of responsible positions during the war. In all, it was a formidable group of individuals, including distinguished scientists and engineers, several Nobel Prize winners, and above all, men who had made essential contributions to the wartime research and development program. AEC chairman David Lilienthal, a remarkable individual in his own right, was clearly dazzled by the collective brilliance of the GAC.(5) In matters of science, the expertise of the GAC clearly dominated that of the Atomic Energy Commission itself, which, besides Lilienthal, consisted of three other lay people and one physicist.

And the GAC had strong opinions on the matter of
support for basic research. At the May, 1947, meeting of the GAC, Oppenheimer expressed the committee's point of view in what Lilienthal called "as brilliant, lively, and accurate a statement as I believe I have ever heard."(6) Oppenheimer stated that the AEC must support a broad range of fundamental research in the nuclear sciences, and not be limited by a narrow interpretation of relevance to the immediate practical tasks facing the commission. Rather than asking what proportion of its budget should properly go to research, or how many accelerators it should support, the AEC should ask how many accelerators were necessary in view of the needs of well-qualified research groups already in existence.

Fisk held stubbornly to his literalist interpretation of the AEC's research mission, but the cards were stacked against him. An August, 1947, trip took the commissioners to the exclusive Bohemian Grove resort near San Francisco for several days of wining and dining with Ernest Lawrence and the directors of the national laboratories, and they emerged favorably inclined toward a broad interpretation of the AEC's responsibilities in the area of basic research.(7) And before long, with the perceived threat from the Soviet Union growing and the new Congressional Joint Committee on Atomic Energy moving to exercise its oversight responsibilities, the commissioners, and especially Lilienthal, found their energies absorbed by a seemingly
endless series of controversial congressional hearings focusing primarily on issues of security. The approach of the GAC prevailed. The greatest pressures on Fisk, and the first crack in his position, were in the area of high-energy physics. In October, 1947, Fisk and the commissioners agreed to set aside $15 million for the field, and within a few months the ONR had gotten its $4 million along with an agreement to establish a cooperative program with the AEC for the support of high-energy physics. (8)

Both Groves and Fisk had tried to resist precedent-setting commitments to expensive high-energy physics projects in order to allow the development of an explicit, carefully planned policy for the field. Both had failed, and the extent of that failure was evident in the events of the first half of 1948, when both Lawrence's group in Berkeley and the new Brookhaven National Laboratory submitted proposals for new proton accelerators in the billion electron volt (BeV or GeV) range. Rabi and the rest of the Brookhaven group were well aware of Lawrence's aggressive moves to build on the existing strengths of the Berkeley lab, and they did not want to be left behind. M. Stanley Livingston, who had collaborated with Lawrence on the earliest generations of accelerators, was sent to Berkeley to learn the state of the art and figure out what was the highest-energy machine that Brookhaven might hope to build. Rabi responded to his report by urging him to think
even bigger, and the result was Brookhaven’s proposal for what was to become the Cosmotron, counterpart to Berkeley’s proposed Bevatron.(9) With both Rabi, representing Brookhaven, and Seaborg, representing Berkeley, on board, the GAC had a hard time refereeing the contest. An initial sentiment that the AEC should commit itself to only one major new machine at a time soon gave way, amid concern for morale at the laboratories, to a consensus that both labs should get their machines, and the problem of which lab should get the higher-energy machine in this round of construction was left to Fisk, to be resolved in consultation with the labs themselves. In the end, the Brookhaven Cosmotron was approved for an energy of 2.5 GeV, and Lawrence’s Bevatron for an energy of 6-7 GeV.(10) In the process, an aggressive, expansionist approach to high-energy physics became the de facto policy for the AEC, and Berkeley and Brookhaven were firmly established as the strongholds of American activity in the field for the next decade. The replacement of Fisk as AEC research director by Kenneth Pitzer of the chemistry department at Berkeley late in 1948 only confirmed the obvious.

The Congressional Joint Committee on Atomic Energy, whose members brought no technical expertise to their new assignment, paid little attention to the AEC’s basic science research budget, which represented in any case a fairly small fraction of the agency’s expenditures. They focused
instead on pressing matters that were more easily understood: security concerns, conditions in the AEC-managed communities associated with the great laboratories at Los Alamos, Oak Ridge, and Hanford, allegations of mismanagement, and the progress of the weapons programs. To the extent that they were aware of the high-energy physics program, they probably understood it as essential to the future of the weapons program. When Senator Tydings, chairman of the Senate Armed Services Committee and a member of the JCAE, complained during an open hearing about the public release of sensitive information in the AEC's fifth semiannual report, he pointed to a photograph of a particle accelerator as an example. (11)

The JCAE made clear its understanding of the purpose of the AEC:

It is the considered conviction of the committee that, until such time as an effective, enforceable and reliable program for the international control of atomic energy is in successful operation, the most vital business of the Atomic Energy Commission must be the meeting of the atomic requirements of national defense. (12)

This was widely understood; the AEC was "essentially an agency for making bombs," noted one critical observer, who went on to observe that "its scientific facilities and expenditures are but parasitic appendages to that central purpose." (13) Whether the high-energy physics program was "parasitic," an enterprise worth pursuing for its own sake which merely supported itself out of the deepest available
pocket, or was in fact an essential part of the achievement of the purpose of the AEC, remained a point of ambiguity for most non-scientists, and the scientific community continued to do its best to keep it that way. The AEC itself played the game in its semiannual reports. Introducing a section on the AEC research program in its seventh semiannual report, the agency quoted from a speech by Lee DuBridge:

The chief goal of science is not to develop weapons of war; it is not to develop new or improved industrial products; not even to find cures for human disease. The primary goal of science is to understand nature.... Its most valuable and important products are not atomic bombs, radar, or penicillin, but new facts and new laws concerning the behavior of the natural world....

The future strength and progress of technology depends on the present strength of science.... Even if we disregard - which we should not - the intrinsic value of science itself, the essential intellectual and spiritual values of understanding nature; even if we are solely interested in building a better industrial or military technology of tomorrow, still the most practical, indeed the only way of assuring that aim is to maintain a strong basic science today.(14)

And lest the message to Congress be misinterpreted, DuBridge's remarks were followed by excerpts from a letter from Commissioner Henry D. Smyth to Senator Brien McMahon, chairman of the JCAE:

The weapons whose production now concerns us have developed from discoveries made in the realm of abstract science in 1939 - just 10 years ago. I do not believe that anyone 11 or 12 years ago could possibly have foreseen this development. Similarly, we cannot foresee what may happen in the next decade, but we can start from certain obvious facts.
The weapons which we are producing involve principles of nuclear physics. This is a subject on which our knowledge is still extremely fragmentary.... It is hardly conceivable that further study of nuclear physics... will not produce information that would be of fundamental value in the design and manufacture of future weapons.(15)

Away from public scrutiny, the AEC could be even more explicit. When the discovery of the strong-focusing principle at Brookhaven in 1952 led to serious investigation of the possible application of particle accelerators as particle-beam weapons, the AEC director of research suggested that the "military usefulness of particle accelerators should be included as an additional justification in further defense of ultra-high energy particle accelerator construction items before the Bureau of Budget and the Congress."(16)

In the early years of the AEC, the GAC wielded broad influence over virtually the full range of AEC programs. More than a decade later, Frederick Seitz, president of the National Academy of Sciences, commented that in the early days, "it wasn't clear whether the Commission or the General Advisory Committee ran the organization."(17) However, the virtually unquestioned acceptance of GAC authority over the full range of substantive technical issues facing the AEC came to an abrupt end during the controversy which erupted in late 1949 over the decision to proceed with development of the hydrogen or thermonuclear bomb.(18) Reacting to the
GAC's arguments against proceeding, Senator McMahon of the JCAE objected that the members of the GAC had trespassed far beyond their area of competence in opposing the "Super" bomb on moral and political grounds. (19) At this time the JCAE was itself reaching a new stage of maturity, moving beyond its early, primarily investigative role to take an increasingly aggressive stance in promoting expansion of the AEC and its program. For more than a decade after 1950, the JCAE became what two informed observers called "probably the most powerful Congressional committee in the history of the nation." (20) During most of the decade, however, basic research in general and high-energy physics in particular remained low on the list of JCAE interests. Whether the Committee was actively suspicious of the basic research programs or simply couldn't be bothered with the still relatively small fractions of the AEC budget involved, is not entirely clear. (21) In any case, high-energy physics was largely left to the GAC, which despite its generally narrowed sphere of influence remained the focus of policy development for that field, among others. (22) And since the rest of Congress, and even the Executive Branch itself in large measure, generally deferred to the expertise of the JCAE in atomic matters, the high-energy physics budget was virtually home free once it had slipped through the JCAE.

Virtually home free - but not quite. AEC officials still had to face questioning from the House Committee on

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Appropriations, which continued to pursue its traditional role as guardian of the public purse from the depredations of spendthrift administrations and senators. While the AEC’s physical research programs often escaped detailed scrutiny because of the pressures of other parts of the AEC budget, they were not entirely immune to the House’s familiar crusade against "waste" and "duplication" in federal programs. The following exchange from 1953 is illustrative:

[Rep.] Mr. PHILLIPS. ...You are getting considerable money for research and have you not, in the past year, gone outside the original field of what I would call applied research and gone into pure research, more so than you originally justified before this committee?

Mr. [Gordon] DEAN [AEC Chairman]. I think that it has been the tendency, in the AEC, Mr. Phillips, for the last 3 years, to try to have a sensible balance between basic research and applied research. We have always recognized in all our submissions that there is an obligation on the part of the Commission to encourage technically competent people in the basic research fields, and one reason the Commission gets into it rather than turning it over to some university is because of the cost of the instruments which you now use in physical research; particularly the large accelerator.

Mr. PHILLIPS. I do not know that the committee is in accord with you on that at all, but I think this has been brought out in all of our investigations and in our discussions with other agencies and that is that there is now a duplication in the field of research, beyond ordinarily desirable duplication.... as the chairman of this committee, with some little background experience in this, I think you are exhausting the field of scientists who can produce the results which justify the expenditure of that much money...
Mr. DEAN. I think that I would have to question that as to whether we are exhausting the group. I think most of our physical research for which we would ordinarily want some increase in 1954 grows out of the work with the laboratory particles accelerator, such as the cosmotron and the Betatron [sic], which will come in at the University of California. Those are costly...

May I respectfully request the committee to consider where this country would be with the present thermonuclear-weapon program if there had not been available a few years back the fundamental knowledge of nuclear theory, the nuclear constants of the isotopes, and all the other data needed to cope with this extraordinarily difficult problem. Without that accumulated knowledge it is inconceivable that we could have planned and executed the tests and other accomplishments in this effort in the 3 years since the Presidential directive to accelerate development. This is of course a spectacular example of research's contribution. Others can be mentioned which will also be significant in future years.

Among these may be the progress that has been made in the design and operation of high-energy particle accelerators. The higher particle energies now foreseeable—perhaps as high as 100 billion electron volts—are opening extremely important new avenues to our understanding of the nature of forces within the nucleus. (23)

And later in the same session:

[Rep.] Mr. COTTON. On research construction, you referred to this particle reactor or particle accelerator.... Could you, either on or off the record, give us an idea of the importance of this?

Dr. SMYTH [Commissioner, AEC]. [Responds with a short discourse on the need to be able to study the high-energy particles found in cosmic rays under more controlled conditions.]

Mr. COTTON. You are liable to get far over my head.

Dr. SMYTH. I am sorry.

Mr. COTTON. I do not blame you a bit. But is it
not a fact that this particular kind of basic research constitutes the real frontier in your particular field which might lead to the development of new weapons?

Dr. SMYTH. That is correct.

Mr. COTTON. And is it or is it not a fact that other nations abroad are perhaps already in this field?

Dr. SMYTH. That is correct.

Mr. COTTON. Either on or off the record, do you know that to be true — whether there are or not — and, if so, to what extent?

Dr. SMYTH. I know it is true in the cosmic-ray field. Many of the basic discoveries in the cosmic-ray field since the war have been made in European laboratories. In the field of reproducing these same events artificially, we at present hold the advantage because of the cosmotron at Brookhaven and the bevatron which is coming along at Berkeley. There is now in process of organization a West European laboratory, and the scientific people in that laboratory plan to build a machine that will be more powerful than anything we now have.

Mr. COTTON. In your original budget, what did you have for it and what did you want to do with that amount of money?

Dr. SMYTH. We put $5 million in the original budget. That was put in many months ago when some ideas for building one of those big machines had just been brought forward, and with the usual over-optimism about new ideas the thought was they were so good, perhaps, that it would be possible to build a bigger machine for $5 million. Further examination has shown that, although these ideas are good and will be very useful and will make it practically possible to build a big machine such as the West European laboratory wants to build, the cost will still be greater than $5 million to go to anything that would be worthwhile. And we do not know how much the cost will be, because that depends on how big a step forward you want to make.(24)

No doubt Rep. Cotton believed progress in high-energy
physics to be a sensitive topic, for twice he suggested to Commissioner Smyth the possibility of taking the discussion off the record. For his part, Smyth took the opportunity to raise, in addition to the weapons-development rationale, the specter of European competition, referring to what was in the process of becoming the European Organization for Nuclear Research (CERN). Strictly speaking, he did state that to date European progress was largely in the less-useful cosmic-ray specialty. Yet merely dropping a hint that the Europeans were hoping to get into the field was enough to feed the congressman's anxieties - and Smyth did not confuse him with details about the long and difficult gestation of CERN amid the problems of postwar Europe. In any case, it appears that scrutiny by the House Appropriations Committee did not materially retard plans in high-energy physics during this period.

Interestingly, even as Americans raised the possibility of European competition as a rationale for support from their own government, they were extending considerable assistance to European scientists trying to launch CERN. (25) I. I. Rabi was considered by many to be the "father" of CERN on the basis of his resolution and supporting remarks at a UNESCO conference in Florence, Italy, in June, 1950. (26) Later, American encouragement was a factor in the CERN decision to build a machine larger than anything in the United States, and there was close American collaboration,
particularly involving Brookhaven, in the development of the alternating gradient synchrotron design.(27) This collaboration was interrupted only for a period during the first half of 1954, when the American Department of Defense imposed security restrictions because of renewed interest in the possibilities of particle beam weapons.(28)

Through the first half of the fifties, federal government support for high-energy physics proceeded steadily along a gradually expansionary course, with occasional fluctuations largely attributable to beginnings and endings of large construction projects. This status quo was to be jolted in mid-decade. Paradoxically, the precipitating event was one which on the surface seemed to represent a thawing in the Cold War. Yet the 1955 Geneva Conference on the Peaceful Uses of Atomic Energy was to result in an intensification of the Cold War on the high-energy physics front, fueled by a sudden acceleration in government support for the field.

The Geneva Conference was a rare opportunity for American particle physicists to confer at length with their Soviet counterparts, and what they reported on their return to the States was alarming indeed. The Russians were said to be putting the finishing touches on a 10 GeV synchrotron at Dubna, which would be the world's most powerful accelerator until at least 1960 or 1961, when a new machine currently being designed at Brookhaven might be ready.(29)
AEC Commissioner Willard Libby, who had previously been a member of the GAC and was himself a chemist with a distinguished record in nuclear research, had described the largest known Russian machine to the JCAE as "just a bit too weak to compete in this fantastic ball game on which so much of our understanding of the nature of nuclei depends."(30) Now he raised the issue anew at JCAE hearings early in 1956:

[Transcript begins with a discussion of plans to support theoretical development by the Midwestern Universities Research Association (MURA) of designs for a future, very-high-energy accelerator, and of plans for an interim machine in the range of 10-12 GeV, to be built at the Argonne National Laboratory in Illinois.]

[Rep.] Mr. COLE. What is the difference between the big machine and this [Argonne machine]? Commissioner LIBBY. This is to hold forth and furnish facilities for experiments in this field during the time the big machine - well, frankly, Mr. Cole, to get down to brass tacks, the thing we saw in Geneva, which the Russians showed us, is exactly in the energy range in which we think this is going to operate. And when their machine starts the Russians are going to take over the show for several years....

On this accelerator the particular aim is to get the lead back from the Russians as quickly as we can. It isn't only a matter of competition with the Russians. We know that the new and important things in nuclear physics lie in this range. We know that and we want to get those discoveries as soon as possible and draw on the skills and techniques involved in making those discoveries. At the present time we have only 2 machines, the 1 at Berkeley and the 3 billion electron volt machine at Brookhaven. These, of course, are leading the whole world at the moment and we have a real corner on things, but we won't have it very long.

[Rep.] Mr. VAN ZANDT. What is the span of time there that the Russians will enjoy leadership?

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Commissioner LIBBY. The best we can do, I think, is to cut it down to 2 or 3 years.

Mr. VAN ZANDT. 2 or 3 years.

Commissioner LIBBY. Whatever we do, and it depends on the breaks, even then. I am afraid they have got the jump on us a little bit. I am afraid that is it. They were very quiet about that machine. The first I ever heard of it was at the Geneva Conference.

Mr. COLE. I recall your concern, Dr. Libby, and I am curious to know if you have verified their claims to the point that there isn't any doubt in your mind that their claim was based on fact?

Commissioner LIBBY. There is no doubt in my mind, but it is an interesting fact - (31)

At this point the transcript switches abruptly to another topic, leaving us to wonder where Rep. Cole's skepticism may have led.

In May, 1956, fourteen Americans were among a group of foreign physicists invited to the Soviet Union as guests of the Soviet Academy of Sciences. Beyond the pleasures of an "apparently unlimited entertainment budget," the foreigners were "freely shown practically everything worth seeing in connection with Soviet high-energy physics." The Americans returned impressed by

... the urgency with which high-energy physics research is now being pursued in the Soviet Union. The atmosphere at the meetings and in the laboratories reminded them of Los Alamos in the Manhattan District days. There was the same personal dedication to the work, the same emphasis on speed rather than cost, the same lavish financial support for facilities. There was also, in the U.S.S.R., a strong sense of competition - of trying to outstrip a rival.(32)
The National Science Foundation, which had finally become involved in high-energy physics, albeit on a scale representing only a tiny fraction of the AEC's support, had convened an advisory panel on the field in 1954. Reflecting the atmosphere at the time, the panel had recommended a moderate, business-as-usual program. Now the panel reconvened, with virtually the same membership, to sound the alarm:

During recent years high energy physics has become more of an international activity... The number of high energy installations abroad has increased significantly, notably in the U.S.S.R. Although at present the United States is leading in the number and diversity of its facilities and in the productivity of its research programs, the rate of Soviet progress has been such that this situation may not persist through the next decade. The peak energy of the high energy machines will seesaw between the Soviet and Western installations; more importantly, the apparently unlimited Government support and the high rate of production of qualified technical people of the Soviet educational system will contribute to the relative enhancement of the Soviet position.(33)

The panel went on to recommend a rapid expansion of support for particle physics to a level of between $60 and $90 million (expressed in 1956 dollars) by 1962. (For comparison, actual combined expenditures for all government agencies involved in high energy physics in 1956 came to just under $16 million.)

The JCAE moved with no dissent to authorize the expenditure of $15 million for the Argonne machine.(34) A year later, in 1957, before any construction had begun, the AEC returned to the JCAE to ask that the project be
superseded by a new design at a cost of $27 million. In response to questions from the Committee, K. E. Fields, general manager of the AEC, and T. H. Johnson, director of research, were forced to admit that the feared Russian machine was still not in operation, and that their new proposal was more clearly thought through from a physics perspective than the $15 million proposal had been. The almost comical transcript of the hearing makes clear that there was considerable confusion on the part of the JCAE with respect to every important aspect of the proposed new machine - what was the meaning and significance of the new design, what was its relationship to the previous year’s proposal, to existing machines and to the theoretical work being done by MURA, what accounted for the increase in cost, and what was the extent of Soviet progress and where the new machine would leave the US compared with the Russians.(35) But the Joint Committee went along anyway, and the Argonne Zero Gradient Synchrotron was launched.

The AEC plunged ahead with an expanding high-energy program. Unusually sharp questioning from a suspicious congressman in the House Appropriations Committee later in 1957 proved to be only a temporary irritation, soon forgotten in the wake of the bombshell which stunned the world on October 4 of that year, courtesy of the Soviet Union.
Notes:


5. See the references to the GAC and its members throughout the second volume of his published journals, David Lilienthal, The Atomic Energy Years: 1945-1950 (New York: Harper & Row, 1964). For example, "Lunched in a terrible little cafeteria in the War Department building with Enrico Fermi and I. I. Rabi, two Nobel Prize winners in physics. To have spent the day with Fermi is like saying that one spent the day with Copernicus or Galileo or the primitive who discovered fire." (p. 128) "Met with the General Advisory Committee — a really distinguished group of men.... (A thought: if the President’s Cabinet were made up of men who in general philosophy of administration and policies had the distinction and brain power of this group!"

6. Quoted in Hewlett and Duncan, Atomic Shield, p. 82.


8. Hewlett and Duncan, Atomic Shield, p. 246.


20. Harold P. Green and Alan Rosenthal trace the development of the JCAE and its unique role in Government of the Atom (New York: Atherton Press, 1963); the quote is from p. 266.


25. The early history of CERN is explored in detail in Armin Hermann, John Krige, Ulrike Mersits, and Dominique


5. SPUTNIK SPARKS AMERICAN SCIENCE:
PARTICLE PHYSICS GOES PRESIDENTIAL

As it beeped in the sky, Sputnik I created a crisis of confidence that swept the country like a windblown forest fire. Overnight there developed a widespread fear that the country lay at the mercy of the Russian military machine and that our own government and military arm had abruptly lost the power to defend the homeland itself, much less to maintain U.S. prestige and leadership in the international arena. Confidence in American science, technology, and education suddenly evaporated. Today it is hard to believe that such a panicky reaction could have occurred, but there were few Americans who were not caught up in a mood of chagrin and concern, with a desire to see prompt action to ensure the nation's security.

James R. Killian, Jr.(1)

As the shock of Sputnik swept America, the many parts of the nation's political apparatus moved, each in its own way, to fashion a response. President Eisenhower had been briefed on progress in the American programs in missile and military satellite technology and on intelligence estimates of the Soviet position, and was in any event temperamentally disinclined to react rashly; he took the event itself in stride. Yet even he was startled at the apparent psychological vulnerability of the American people. Under pressure to take bold steps to reassure the public, he moved quickly to reorganize and upgrade the Office of Defense Mobilization's Science Advisory Committee to a President's Science Advisory Committee, and appointed MIT president
James Killian as the first Special Assistant to the President for Science and Technology.

In the months after Sputnik, a variety of Congressional hearings addressed various aspects of the state of science, technology and defense preparedness in the Unites States. The JCAE conducted a detailed review of the progress of the AEC’s physical research program, bringing many prominent scientists to Washington for show-and-tell sessions on their activities. House and Senate members in attendance struggled to understand the scientists’ presentations and to relate them to their own perceptions of national needs. For example, following his review of the concepts of particle physics, Caltech theorist Murray Gell-Mann found himself explaining in response to committee members’ questions that particle physics was unlikely to make any direct contribution to the cure of disease, that photons of light were of no immediate use in space propulsion, and that space expeditions to retrieve antimatter from distant galaxies were not soon to be a practical source of energy. \(2\)

Throughout the hearings the scientists were continually pressed to estimate the standing of the Russians in their respective fields and to assess the impact of funding limitations and shortages of scientific manpower on their own progress.

In the field of high-energy physics, a careful listener at the hearings would have found reason to question the
hyste-ria about Soviet technological domination:

Representative HOSMER. Did you pick up any information from the Russians during their visit as to what they are doing?

Dr. [Luis] ALVAREZ. Yes; they told us in great detail about what they were doing....

Representative HOSMER. Did they tell you about the difficulties they were having?

Dr. ALVAREZ. Yes. They said they were having difficulties making their 10-billion-volt machine work. They said the engineers working on it would not allow any physicists in the laboratory yet, because it was not working well.

Chairman DURHAM. Doctor, what do you think of their work, after listening to them?

Mr. [sic] ALVAREZ. Well, I learned more about their work, sir, when I was in Russia last year, than I did by talking with them here. I was very much impressed with the work that was done at their cyclotron. It was very competent work. It was very well done. The only thing that seemed missing in it was that there was nothing new that had not been done in the outside world....

So then was there no reason to fear the Russians after all?

Alvarez was not quite ready to leave it at that:

... They have to learn the techniques and repeat the old experiments. I expect that in the next few years we will see them branching out into new areas of physics, that we have not been in yet. As of a year and a half ago, when I was there, I did not see anything new in the way of physics that had not been done in the outside world. But I think this is just part of their process of growing up scientifically.(3)

In hearings before the House Committee on Government Operations, NSF Director Alan Waterman was less cautious in his remarks. Comparing the (still-malfunctioning) 10 GeV machine with the 6 GeV Bevatron, and a 50 GeV Russian
machine supposedly under construction with the 25 GeV alternating-gradient machine in its early stages at Brookhaven, he described the Soviet Union as "a step ahead of us all the way."(4)

Senator Alexander Wiley of Wisconsin pitched in as well with a statement before the JCAE. Though not himself a member of the committee, he had been granted the usual Congressional courtesy of being allowed to testify in favor of a project which would benefit his constituency, in this case funding for construction in Wisconsin of a MURA-designed accelerator. In an impassioned speech, he emphasized the dangers of falling behind the Soviet Union in what he called "high-speed physics," stressing that there was not a moment to lose, and implying that complacency on the part of Congress could result in another Pearl Harbor. While his remarks can be understood as simply another colorful chapter in the long-running quest by senators and representatives for a slice of federal pork, his choice of themes was a striking reflection of perceptions at the time.(5)

American physicists, warning of the threat of an "accelerator Sputnik," were feeding the fears of laypeople like Wiley. Yet there is a tantalizing hint that Ernest Lawrence himself, the confirmed Cold Warrior and the father of the quest for ever-larger accelerators, felt otherwise. Writing three decades later, Herbert York, a former student
of Lawrence's who had become chief scientist of the Defense Department's new Advanced Research Projects Agency in the wake of Sputnik, described their last meeting, which occurred at around that time. According to York, Lawrence, while still interested in seeing bigger machines built, expressed the opinion that the rates of expenditure then being discussed could not be justified by the potential economic and social value of the likely results. While maintaining his belief in the value of pure science as an activity in its own right, Lawrence had not lost his early concern about its benefits to humanity as a whole. One cannot but wonder whether he would have found the occasion to express such views in public, and what their impact would have been. But Lawrence took sick while serving as a delegate to a nuclear test ban conference in Europe that summer, and died shortly thereafter.(6)

It was in the midst of this atmosphere of acute concern over the state of the nation's efforts in science and technology that high-energy physics became for the first time a matter of direct Presidential concern. The field came to the President's attention not so much because of its substantive implications as because of its rapidly escalating cost: the proposed Stanford Linear Accelerator, or SLAC, would at its projected cost of $100 million be the most expensive basic research project ever undertaken up to that time.
In August, 1958, Killian and AEC Chairman John McConem met and concluded that evaluation of the SLAC proposal required a high-level review of policy for high-energy physics. The NSF advisory panel was reconstituted for another review, to be followed by a joint PSAC-GAC evaluation. In its report, the PSAC-GAC panel, headed by Emanuel Piore, recommended proceeding with the Stanford accelerator as part of a general expansion of high-energy physics to a projected funding of $125 million in fiscal year 1963. Beyond its specific recommendations, however, the panel staked out an extraordinary position on the matter of the government's responsibilities in funding basic science in general and high-energy physics in particular:

Concern has been expressed that increased financial support for high energy physics may have an adverse effect on the support of other areas of science. In our judgment it should not.

It is not possible to assign relative priorities to various fields of basic science nor should they be placed in competition. Each science, at any given time, faces a set of critical problems that require solutions for continued growth. Sometimes these solutions can be acquired at little cost; sometimes large expenditures of funds are needed. Hence, the cost may not reflect the relative value but rather the need.

Each area must be funded according to these needs. The peculiarly high cost of high energy accelerator physics is due to the size and complexity of the research tools required to attack the fundamental problems of the field. The tools required are proposed by active leaders in the field anxious to devote their talent and energy to developing these tools. Hence, in high-energy physics the necessary combination of qualified people with sound ideas for meeting
widely recognized scientific needs does exist and the field should be supported.(7)

What the government owed science was a blank check to be filled out at the scientists’ discretion.

Eisenhower was never one to buy such an audacious claim on the taxpayer’s dollar in the name of science. For one thing, he was well aware of the voracious appetite of scientists (and others as well, to be sure) for federal funds. Referring to the scientific side of the American rocket programs, he is said to have told an aide, "First these science boys come to me and want 22 million dollars — and I say ‘sure.’ After awhile they want 60-odd million more, and I say ‘Fine.’ So they pack some trickier instruments into the things — and want 80 million or so more. And I say ‘Okay.’ But — finally — when they say they need another 150 million dollars, I have to say: ‘Just a minute, fellows. Where does all this end?’"(8) He believed in "civilian control over military expenditures, a low degree of government intervention in science and technology activities, and steadfast opposition to crash programs."(9) Years after Eisenhower had left office, Killian was prominent among those who argued that Eisenhower was more sympathetic to science than he had been given credit for. Yet his was a sympathy that was chary of amorphous, open-ended commitments, and aware of the need for a coherent policy. As Killian himself reported, Eisenhower "was always pretty much impressed by the fact that the scientific
community was always, in his judgment, seeking greater support, more funds, and he didn’t see any way in which decisions could be made on any objective basis as to what the budget should be. He kept asking when we were arguing for more basic research, more research funds, ‘How can we tell? Give me some basis of policy to deal with these problems.’" (10)

In his own memoir, Killian described the approach to Eisenhower on the SLAC proposal after the submission of the NSF and PSAC-GAC reports. Referring to the projected cost of $100 million, he noted that:

... At that time this was an unprecedented program for the federal government, especially the Eisenhower administration, to undertake in behalf of pure science. There were no military applications involved in the development of such an accelerator but certainly great potential for nuclear physics, as it impressively demonstrated when completed. After we had discussed the recommendation with the president’s key staff, General Wilton B. (Jerry) Parsons, for example, who had succeeded Sherman Adams, and with General Goodpaster, it was decided that a group of us would go to see the president and present the report favoring this project. This group was made up of Professor Edward [sic] McMillan from the University of California at Berkeley, Dr. Piore, then director of research at IBM, Dr. [Herbert] York, and myself. We asked Dr. McMillan to join us because his institution was not involved in this proposal. He was head of the Radiation Laboratory at Berkeley where so much brilliant work had been done in the building and use of large accelerators. During the course of our presentation Eisenhower became interested in what these machines would do and how they operated. We had expected to spend a half hour with him, but we spent longer, with McMillan giving the president insights into high-energy physics, and Ike probing and probing. We spoke of the possibility that the Russians might get ahead of us in high-energy
physics, but this was not the central argument for the machine.(11)

The implication of Killian's account is that Eisenhower was sold SLAC essentially on the basis of the merits of pure science, although there is no direct evidence that the President understood the presentation in just that way.

Nevertheless, buy it he did, and at Killian's suggestion, he announced his decision in a speech delivered at a New York symposium on basic research sponsored by the Alfred P. Sloan Foundation, the National Academy of Sciences, and the American Association for the Advancement of Science. The text of the speech capture. Eisenhower's ambivalence concerning the government's involvement in big science:

... Too often we have tended to look unduly to the Federal Government for initiative and support in a multitude of activities, among them scientific research. We must recognize the possibility that the Federal Government, with its vast resources and its increasing dependence upon science, could largely preempt the field or blunt private initiative and individual opportunity. This we must never permit.

Too much dependence upon the Federal Government may be easy, but too long practiced it can become a dangerous habit....

... I am recommending to the Congress that the Federal Government finance the construction as a national facility of a large new electron linear accelerator. Physicists consider the project, which has been sponsored by Stanford University, to be of vital importance. Moreover, they believe it promises to make valuable contributions to our understanding in a field in which the United States already is strong, and in which we must maintain our progress. Because of the cost, such a project must become a Federal
Presidential involvement was not the only change that occurred in the political milieu of high-energy physics during the SLAC episode. The Bureau of the Budget became actively involved, taking the initiative to steer responsibility for SLAC from ONR, which had supported previous accelerator work at Stanford, to the AEC. Efforts by the NSF to carve out a piece of the SLAC project were shut out as well by opposition from the AEC and the BOB, marking an effective end to NSF hopes of gaining a leading role in particle physics despite its late start. These efforts to consolidate high-energy physics under one agency ran counter to the repeatedly expressed wishes of the scientific advisory panels on the field, and presumably represented an attempt to bring the government's program under more effective control.

In addition, the rise of the PSAC marked the end of the GAC's exclusive domination, in collaboration with the various NSF advisory panels, of policy making for high-energy physics. With the field achieving the status of a national research program, no longer a minor vestige of AEC program responsibility, it became too important to be left to the GAC alone. By the middle of the next decade, the GAC was to lose most of its remaining influence on the high-energy physics program as other institutional structures arose to supersede it.
As for SLAC, even after Eisenhower's endorsement it faced a rough course through the Congress. The Democrat-controlled JCAE, which had been at odds with the Eisenhower White House over many issues, was not prepared to be seen as acquiescing in an unprecedentedly large new research venture for which the administration had taken the initiative and claimed the credit. The committee proceeded to examine the SLAC proposal critically in every detail, and threw in a variety of parliamentary stalling maneuvers for good measure. In the end SLAC was not approved until the early days of the Kennedy administration, when it no longer carried the rap of being a "Republican accelerator."

Despite Eisenhower's endorsement, the position of high-energy physics on his list of priorities is indicated by his unwillingness to spend the political capital which might have pushed SLAC through during his own administration.

The high-energy physics community responded in traditional fashion to the delay in SLAC, convening yet another advisory panel late in 1960 to prod Congress on the eve of a new administration. This second Piore panel set out a program which implied expenditures of up to $350 to $400 million annually by 1970. What was noteworthy about this report was not the now-familiar claim on an increased portion of the Federal budget, but the appearance of a dissenting opinion by Princeton physicist Eugene Wigner. Wigner took great pains not to appear to be straying outside
of the narrow mandate of the panel or to be repudiating the conclusions of the panel. Nevertheless, he ventured the comment that "the present writer considers the emphasis of the majority report on the significance of high-energy physics exaggerated and indeed some statements may even be misleading." Choosing his words carefully, he set aside the issue of the potential impact of high-energy physics' voracious appetite for money on other branches of science, and limited himself to questioning whether the panel had adequately accounted for the impact of the proposed high-energy program on scientific manpower.(15) This public dissent, unusual for such a panel, was a restrained forerunner of much louder debates yet to come.

On the surface, the Kennedy administration, short as it was, seemed a relatively uneventful period in the development of high-energy physics after the approval of SLAC. Yet forces were gathering which would shortly result in drastic changes in the environment for basic science in general and particle physics in particular.

Kennedy himself was quite sympathetic to science, more so than Eisenhower. During his administration, the science advisory apparatus, headed by Jerome Wiesner, drew the elite councils of the American scientific establishment into close integration with the White House, to apparent mutual benefit. But the arrangement raised doubts about the position of Kennedy's science advisors: were they impartial
advisors to the President, or lobbyists for the interests of the scientific community? When Johnson became President, bringing with him a wholly different set of attitudes about science, its relationship to national goals and the influence of its east- and west-coast elites on national policy, this ambiguity became anything but an advantage, fueling Johnson's mistrust of the scientific community.

After fifteen years of rapid growth in federal support for basic research, many in Congress were ready to pause and consider more carefully just what it was that they were buying. At the same time, the escalation of the space race and the beginnings of the war in Vietnam were but the first of several staggering new demands on the Federal budget which would increasingly put pressure on all existing programs, especially those deemed "nonessential." The introduction of new techniques of cost-benefit analysis and new approaches to program budgeting was beginning to have an impact within the administration, making the task of justifying major research and development programs harder than ever. The favored programs of the JCAE did not escape the squeeze, and the committee awakened to find that, however it felt about high-energy physics, the field was beginning to take an uncomfortably large piece of a static AEC budget.

In discussions with Wiesner about high-energy physics, Kennedy is said to have given an informal assurance that his
administration would support the construction of one major new accelerator about every five years. Physicists would have to be content with that, for chances of getting anything more were slim.(16) In this context, the PSAC-GAC high-energy physics advisory panel of 1963, headed by Norman Ramsey, faced the task of setting explicit priorities for the field. In its recommendations, the panel, as usual, projected an ever-increasing expenditure, out to a level of about $600 million annually by 1975. It adhered to the traditional institutional pattern by recommending a 200 GeV proton accelerator for the Lawrence Radiation Laboratory at Berkeley, to be followed in five or six years by an accelerator in the 600 to 1000 GeV range for Brookhaven. By its faint praise for an accelerator planned by MURA, the panel provided the ammunition needed to kill the nearly decade-long effort by that group to gain an accelerator for the midwest, independent of the Argonne National Laboratory, where, because of a long-simmering political dispute, the MURA scientists did not feel welcome.(17)

Reading the handwriting on the wall, MURA finally broke ranks with the rest of the high-energy physics community. While up to that point the MURA scientists had played the team game, regularly testifying in favor of other accelerator proposals in the faith that their turn would eventually come, now they launched an open, last-ditch lobbying effort on behalf of their project. Kennedy came
under heavy pressure to approve MURA's $170 million machine. Wisconsin Senator William Proxmire, known as a long-time foe of unnecessary government R & D spending and as originator of the "Golden Fleece" awards, given with great fanfare for what he considered particularly egregious examples of such largess, was an active lobbyist for the machine, which was expected to be built in his state. Senator Hubert Humphrey of Minnesota was a strong proponent as well, and believed later that Kennedy had promised him that funds for MURA would be included in the budget for fiscal year 1965.

As Johnson took office following the death of Kennedy in November, 1963, MURA advocates saw no reason for added concern. However, their project soon ran afoul of higher-stakes political issues. Johnson came into office determined to enact a tax cut. But upon becoming president, he found a new budget topping the then psychologically significant $100 billion mark. The symbolism of being able to prove his own fiscal responsibility became an overriding concern. When Budget Director Kermit Gordon prepared a list of programs that could be cut to keep the budget under the magic $100 billion level, MURA was an obvious candidate both because BOB did not regard it highly and because, as a new start, it had few sunk costs to reckon with. To make matters worse for MURA, Wiesner and AEC Chairman Glenn Seaborg, while sympathetic, felt that the priorities set in the Ramsey report made the most sense on scientific grounds.
Johnson was scheduled to meet separately with MURA representatives and with a congressional delegation from the midwest on December 20, 1963. However, a scheduling snafu brought both groups to the President's office at the same time, along with Wiesner and Seaborg. The result was a chaotic meeting in which Johnson killed MURA and both delegations left feeling wronged, and believing (incorrectly) that Wiesner, representing the east-coast scientific establishment, was responsible for yet another denial of midwestern scientific aspirations.(18)

It is hard to imagine a clumsier way of introducing a new President to the world of high-energy physics. As Johnson later wrote to Humphrey, "The [MURA] decision was a difficult one. I devoted more personal time to this problem than to any nondefense question that came up during the budget process."(19) And this was at a time when Johnson faced the unexpected burdens of a sudden transition to the Presidency after the assassination of Kennedy.

For MURA, it appeared that the battle had been lost, once and for all. Yet in a twist of fate reflecting the drastically changed circumstances in which the particle physics community found itself, the high-energy physicists of the midwest were ultimately to find that while they had lost a battle, they would win their war, albeit in unexpected fashion.
Notes


13. See the testimony of McConie before the JCAE, Stanford Linear Electron Accelerator, Hearings before the Joint Committee on Atomic Energy, p. 10.

14. The NSF had been supporting the MURA team since its inception in the hope of earning a share in the eventual construction of its proposed accelerator, but opposition from AEC and BOB prevented MURA from progressing beyond design studies. After its exclusion from a leading role in SLAC as well, the NSF eventually resigned itself to a secondary role in accelerator construction, focusing its support on a series of relatively small but productive machines at Cornell University. See J. Merton England, A Patron for Pure Science (Washington, DC: National Science Foundation, 1982), pp. 292-297.


6. LESSON IN SURVIVAL, PART I:
HIGH-ENERGY PHYSICS IN THE GREAT SOCIETY

A.M. Weinberg: I hardly relish my role as the bête noire of the elementary-particle physicists....

Yet, the fundamental view I espouse cannot be ignored: that any branch of basic science that requires very large public support must be justified in terms which transcend the narrow viewpoint of the few scientists who can participate in, or even appreciate, that branch of science. It is not enough that a basic science be ripe for exploitation, or that every one of its practitioners be a genius of high order. Science which commands great public support must be justified on grounds that originate outside the particular branch of science demanding the support; it must rate high in social, technological, or scientific merit, preferably in all three....

Owen Chamberlain: It is clear that the reason we are having this... discussion is that some Congressmen are somewhat skeptical of the investment in high-energy physics...(1)

When Lyndon Johnson came to the Presidency, he was not entirely new to issues of science and technology. As a prominent member of Congress, he had dealt with a wide range of such matters; he had even served briefly on the JCAE, and had been a key player in the drive to build a strong U.S. space program. Yet while he had faith in the potential of science and technology, he viewed it from a populist perspective, primarily in terms of its short-term, pragmatic payoffs. And as an experienced politician, wise in the ways of Congress, he was willing to cut back on federal
scientific programs when the needs of his budget, considered as a whole, called for such a step.

Johnson's strong focus on the tangible benefits to be expected from science reflected a broader trend in American society. Increasing recognition of unsolved social problems led to widespread questioning of government research and development priorities. As the example par excellence of expensive pure science, high-energy physics was an obvious target for reexamination.

Within the scientific community, too, questioning became more pointed and more open. Perhaps the most prominent scientific critic of business-as-usual for high-energy physics was the physicist Alvin Weinberg, director of the Oak Ridge National Laboratory. Like the leaders of the particle physics community, Weinberg had solid credentials as a spokesman for science: he, too, had been a participant in the great war effort, and he, too, had pursued a distinguished career as a scientist and scientific administrator after the war. But Weinberg was given to thinking deeply and carefully about the role of science in society, and by the early 1960's he began to share his thoughts with the public with increasing frequency. In a series of articles which began in Minerva and continued in Physics Today, he explored the dimensions of what he called "criteria for scientific choice." With respect to high-energy physics in particular, he questioned not only the
practical utility of the field, but one of the its fundamental articles of faith as well: the claim that, as the most fundamental of all sciences, dealing as it does with the most basic aspects of matter and energy, high-energy physics constitutes the foundation on which all other natural sciences depend. (2)

Another aspect of President Johnson’s populism which struck a popular chord was his sensitivity to the geographical distribution of federal government expenditures. The problem was not a new one; as one commentator noted, "It is hardly an exaggeration to say that the main stuff of American history has been the recurring sense of regional deprivation as a deliberate infliction by favored regions upon the rest of the country."(3) But a variety of factors conspired to bring it to the fore once again. Even as the defense budget was expanding with the escalation of the war in Vietnam, the need to rationalize expenditures led to increasing talk about closing down unneeded military installations - installations that had long represented one of the most important ways for the government to spread its money around. At the same time, the obviously disproportionate share of research and development money going to a small number of geographically-concentrated, elite research universities and the high-technology companies that grew up around them inexorably led to the widespread belief that federally-funded academic
science was the key to the economic vitality of a region. Sympathetic to a broader distribution from the start, Johnson quickly realized the implications of his MURA decision and hastened to reassure Humphrey that he "share[d] fully your strong desire to support the development of centers of scientific strength in the Midwest..."(4) The principle of a broader distribution of federal resources became a keystone of Johnson's policy.

By now, the JCAE was acutely concerned about the impact of the rapidly increasing high-energy physics budget on an essentially static overall AEC budget, with special concern for the resulting pressure on development projects favored by the committee. During AEC authorization hearings early in 1964, Representative Chet Holifield, vice-chairman and a long-time member of the JCAE, called Glenn Seaborg and newly-installed presidential science advisor Donald Hornig before the committee and told them in no uncertain terms that the administration had better prepare a clear national policy for high-energy physics, similar to that which guided the space program. Holifield placed in the record a detailed set of cost projections for the AEC's high-energy physics budget, and, addressing Hornig, left no doubt as to what was on his mind:

There is no doubt, as you said in your statement, I think, that high-energy physics is the most exciting scientific field we are now working in, and that it is not limited by ideas. We concede that there is no limit to the scientists' ideas or their ambitions to explore
these ideas. This is not a deplorable thing. I think it is perfectly natural. But in exploring these ideas and implementing them, it seems to some of us that we are getting to the point where we are squeezing to death many other fields of science. Very frankly, the Congress is becoming alarmed at it....

... Do you intend to increase the emphasis on unending studies? I use that word advisedly, because, as you move into new fields and into higher and higher energies, there is no end to the projects and ideas which scientists may have. There is an end, however, to the public purse....

... Are you planning to add to this program which is already set up and which is moving toward the half billion dollar mark per year? Are you planning on adding some more accelerators, - the 600 to 1,000 billion electron volts that you talked about or a 200-billion electron volt, and so forth, and if so, where is the money going to come from?...

What is your attitude toward this? What are you going to do about it?

Hornig responded that high-energy physics was unique in its capacity to expand limitlessly, and that it would not be possible to lay out a firm long-term plan. Holifield was not satisfied:

I can understand that you cannot lay out a rigid plan, but in my opinion the administration is going to have to lay out a general plan with a forecast of expenditures as a basic national policy... and I am not talking about a study, because the Ramsey study made a number of recommendations which have already been turned down... This policy must be fitted in with the overall ambition of the scientists and this ambition is going to have to be curbed somewhat because it cannot be allowed to run wild in every direction that opens up for investigation....

... This policy should... get the acquiescence of the scientific community to a considered step-by-step program in this field. It is also important to obtain the acquiescence of the President in
general to this... Otherwise we are going to find ourselves in trouble in the Congress in getting this projected program authorized and funded. (5)

The JCAE drove home its point by cutting $4 million from the AEC's FY 1965 request for physical research, including $1.8 million from high-energy physics. The House Appropriations Committee followed suit, cutting an additional $8 million from the physical research budget, stating that "the committee would like to reiterate the admonition that there be a tightening up in the process of selecting areas and subjects of research undertakings with greater emphasis on overall usefulness of the potential results." (6)

Both Hornig and the AEC under Seaborg responded with policy statements which were, in effect, warmed-over versions of the Ramsey report. Early in 1965, the JCAE followed up with a series of hearings in which the high-energy physics program was examined in exhaustive detail. Chairman Holifield opened the hearings with a call for the high-energy physics community to justify its work in terms of its benefits to the broader society which was footing the bill. Acknowledging that benefits could sometimes be intangible, and that crude cost-effectiveness arguments could be seriously misleading, he nonetheless emphasized that the public had a right to expect an understandable explanation of why its money should be spent on such endeavors:

... Laymen are sometimes accused of not seeing the forest for the trees with regard to
research. Presumably the scientists making such an accusation have in mind a forest of better understanding with the trees representing specific technological or developmental goals.

This may be true. However, scientists should not be surprised if laymen are equally puzzled when eminent scientists engaged in research, and concerned with the forest of understanding, do not also recognize the need for identifying some fruit-bearing trees in their forest. Scientists should not forget that if society pays for the research, there must be adequate repayment to society....

... If, because of the complexity of this field, you must talk to us in parables, do so. However, I cannot emphasize too strongly that since your support depends on public funds, the public must be able to understand the purpose of high-energy physics research and the reasons why expensive tools are required in this research. Relating your research to our national needs, goals and aspirations is equally essential.(7)

How did the high-energy physicists respond in the hearings to this direct challenge at a time when virtually all of science was being called into question? The old claims, explicit or implied, about the direct contributions of particle physics to the development of new weapons were hardly mentioned, save for a wildly speculative conjecture by Luke C.L. Yuan of Brookhaven, who proposed that if the postulated but as yet undiscovered "quarks" and "antiquarks" could be separated they might be brought together to annihilate, with a release of energy vastly greater than that produced by the fusion of hydrogen. The Red Scare no longer seemed so scary when Robert Marshak of the University of Rochester reviewed Soviet progress in high-energy physics. Evaluated in light of the glowing reports of the
American visitors of 1956, the Russian program had in fact been an utter failure, though Marshak himself would not use those words. Marshak concluded his presentation with the traditional platitude about how the Russians would surely make great progress now that they had worked out their problems - though expressed with perhaps a shade less conviction than usual:

While we have all learned the danger of prophecy regarding Soviet developments, I would hazard a prediction that a decade from now will find the Soviet Union much more of a coequal with the United States and Western Europe in high-energy physics than it is at present.

With a certain amount of luck, the [new 70 GeV] Serpukhov machine may set a fast pace with exciting new experimental results and the Novosibirsk laboratory may surprise the international high-energy physics community with important innovations in accelerator design.(8)

Frederick Seitz, president of the National Academy of Sciences, presided over a roundtable discussion staged for the benefit of the JCAE. In his own brief historical introduction, Seitz acknowledged that high-energy physics had effectively become a separate field from nuclear physics:

As the 1950’s advanced, and many new particles came to light, the main focus of interest of the scientific community began to pull away from the applied matters related to fission and fusion which had provided the impetus for much of the Federal support in the 1940’s, and the region of high-energy physics became once again a good field of pure science concerned in the main with the fundamental structure of matter.(9)

Eugene Wigner and geophysicist Philip Abelson, who had begun
his career with a PhD earned under Ernest Lawrence, argued that high-energy physics had indeed become separated from the other sciences, to the extent that its results were irrelevant to the progress of other sciences, citing such examples as chemistry and molecular biology. Yet their testimony was undercut by W.D. McElroy, chairman of the biology department at Johns Hopkins and, perhaps not incidentally, a trustee at Brookhaven. McElroy professed his faith in the particle-physics-is-fundamental-to-everything line, arguing that the health of even such fields as biology and medicine depended on a vigorous high-energy physics program. The rest of the panel, all physical scientists, echoed McElroy's position. When it came to citing concrete benefits from particle physics, though, the examples given ranged from the indirect to the tenuous, including developments in hardware and instrumentation, the training of many people who later go into other fields, and the effects of the work on scientific and general culture.

A statement prepared for the record of these hearings by Edwin McMillan, director of the Lawrence Radiation Laboratory, is worth quoting in full, because it captures in a pure, extreme form the attitude of many in the high-energy physics community:

There is sometimes a tendency, which seems wrong to me, to isolate high energy accelerator physics as a separate field of inquiry that started early in 1948. This field, which is better described as particle physics, is the latest chapter in the ancient search for an
understanding of the ultimate forces and structures of the universe. This search has been, and continues to be - nowadays in the form of particle physics - the spearhead of all science. Some argue that particle physics bears little relationship to and provides little spinoff for other sciences or for technology. This may be true, but only in the short term. It has been true all through the history of the central search for an understanding of the ultimates. It was true when Becquerel discovered radioactivity, when Thomson discovered the electron, when Rutherford discovered the nucleus, when Chadwick discovered the neutron, and when Hahn discovered nuclear fission.

However, when we take the long-range view of the relationship of the search for ultimates to other sciences and to society, we find an entirely different story. We see the birth of modern theoretical chemistry and solid-state physics rising from quantum theory, which was based on information gathered in the scientifically remote studies in atomic physics. Molecular biology, the fascinating story of the "molecular tape" on which the information to construct and operate a living being is recorded, and by which it is transmitted to future generations, arose from experiments using X-rays diffraction. This technique originated in an experiment to demonstrate the wave nature of X-rays, and at the time of its discovery, an application to fundamental biological problems would have seemed to be in the realm of fantasy. As to the consequences of fundamental discoveries to technology, I can point out that this committee was established as a result of one of those discoveries.

I do not mean to make any comparison between particle physics and other sciences. I believe no comparison can be made. There are no criteria for comparison. Particle physics is admittedly remote, as the exploration of the far frontier has always been. It does not pretend to yield daily bits of input to other sciences or to technology. Basic research never has, and it never will. What high-energy physics may do at any time, if it is allowed to continue in a forward-looking way, is to create new sciences, to revolutionize old ones, and perhaps to revolutionize our lives. These things are more likely in this field, where we are dealing with the elemental and the primordial,
than in other fields of science.

This reasoning - which I believe to be true and which I believe history bears out - leads me back to my central conclusion; the step to the next energy range must be taken. There is no other way to do this exploration. If there were a cheaper, quicker way, physicists would be aggressively exploiting it, as they have been exploiting accelerators for the last 35 years. If we do not proceed to higher energy, we will eliminate the frontier and kill the field or badly cripple it.

The long-range results would be many:

1. The discouragement of advances in particle physics would put boundaries around the central search for knowledge for the first time in U.S. history. We would lock the door on future discoveries at the heart of matter.

2. The vitality and dynamism that have characterized American science in the last 30 years would be adversely affected. The long-range feedback from this kind of frontier research will cease. The other sciences have much to explore. There is much to do in expanding understanding of the phenomena at the nuclear, atomic, and molecular level. But the important facts will gradually become known, and the present sciences would grow increasingly technological. There will be no prospect of going deeper; of understanding phenomena in fundamental terms, unless we get our feedback from Europe or Russia.

There is also an intangible factor in this effect on American science. Even though he may not have a present awareness of its significance, every good scientist must be affected by the knowledge that someone is asking the ultimate questions of nature. We have never had a closed horizon in science in this country. We cannot measure the potential effect of the elimination of the horizon, but I am convinced it will be important.

3. The impact on technology is potentially disastrous. We cannot predict what, if any, new technologies may rise from particle physics, but we can look at the past and the present to see how our present technology has derived from the basic
research of the past.

4. Closing the horizon would affect American prestige. Particle physics is a field in which the United States has clear world leadership. For 35 years this country has attracted some of the world's best scientists through its strength in the search for an understanding of matter, and this immigration has greatly strengthened American science and education.

5. Europe presently has under study a machine of 300 billion electron volts. Russia is building an accelerator of 70 million [sic, actually billion] electron volts. In view of these projects American leadership could not be expected to continue. We might expect an exodus of some of our best brains to Europe.

6. American science might be expected to return, over a period of time, to the secondary position it occupied until the late 1920's, when a vigorous broadly based American science began to develop. Until that time, the United States relied primarily on Europe for new discoveries from the frontiers of science.

In today's world, when scientific discoveries are sometimes quickly translated into technology, this could have tragic consequences for the United States.(10)

But this was 1965, not 1955 or 1950. While it remained unlikely that any of the members of the JCAE would ever fully understand the details of the science they were being asked to support, several of them had had a long tenure on the committee, and through years of hard work had gained a meaningful understanding of the place of science in the context of American government and society. As their increasingly sophisticated questioning made clear, they could no longer be swayed merely by the aura of authority of a distinguished academician. The arrogance and
condescension, directed toward both skeptical laypeople and scientists in other, "less fundamental" fields, implicit in McMillan's statement, represented precisely the attitude which had begun to irritate, rather than impress, the JCAE.

This developing dissatisfaction became even more apparent when the committee's questions reached beyond details of the scientific program to such touchy subjects as the high degree of autonomy with which the high-energy physics community managed such huge sums of federal money and the relatively small, closed circle of scientists that seemed to be behind all of the ostensibly different advisory panels and policy reports. As Rep. Craig Hosmer put it, "it seems to me that the high-energy fraternity has created a model of the best of all possible worlds with everything in it that they would like. It is kind of like a bunch of business tycoons sitting down and figuring out what taxes they are going to pay and writing the laws to fit them." And, "as one scientist said of this situation, it is one in which the division of the pork barrel is being taken care of by the pigs themselves."(11)

It was against this backdrop that the particle physics community's quest for the giant 200 GeV accelerator, endorsed in the Ramsey panel's report, was played out. Strictly speaking, the Ramsey panel recommended building the MURA machine as well, but only so long as it did not get in the way of the "200 BeV," as it was called at the time.
When Johnson used the panel's recommendation as part of the rationale for rejecting the MURA proposal, however, he implicitly gave a push to the 200 BeV, whether he intended to or not. As the OST Administrative History noted, "This was the first implicit commitment to build the 200-BEV because it would be hard to give for the reason not to build a cheaper, low priority accelerator that a higher priority or was more important, if you did not intend to build the higher priority machine." (12)

The first hard evidence that the MURA episode was to have a lasting impact came less than a month after Johnson's decision on the project, when the AEC issued a press release indicating that while design work on Berkeley's 200 GeV machine and Brookhaven's 600-1000 GeV machine was continuing, "no decisions have been made for the construction of either of these large national accelerators, nor have site locations been selected." (13) [emphasis added]

This hint that the 200 BeV, with its projected construction cost of nearly $300 million and operating budget of $60-100 million, was potentially up for grabs, was not missed by scientists, congressmen, governors, various development groups and many others sensitive to the new imperatives of regional technological development. Before long, a chaotic site selection process had erupted, in which Congress, the Administration, the AEC and the NAS invented the rules as they went along, responding to political pressures when and
where they appeared.

The remarkable but complicated story of this site selection derby is beyond the scope of this study. (14) While it was underway, however, the Bureau of the Budget was exerting a substantial effort to keep the President from making a commitment to construction of the 200 BeV, so as to preserve its own flexibility in dealing with a major budget item. When the policy statement and subsequent report requested of Hornig by the JCAE were prepared, the BOB made sure that Johnson would not be seen as endorsing a specific plan of action. In the case of the full report, submitted early in 1965, the transmittal letter prepared for Johnson by the Office of Science and Technology and the AEC concluded, "I endorse the AEC report...", but the final, BOB-influenced version was changed to "I believe that the AEC report provides a useful guideline for decision making in the development of high energy physics." (15)

By the time of the JCAE hearings early in 1965, however, the BOB was fighting a losing battle. The opening of a formal site selection competition resulted in tremendous lobbying pressure on Congress, on the Administration, and from Congress on the Administration in favor of specific sites, and the matter of approval of the accelerator itself was soon accepted by almost all involved as a given. But the BOB continued to try to hold back the inevitable. On August 31, 1965, Charles Schultze of BOB
wrote to Johnson, "The site location has been scheduled with the hope that you would approve this for the 1967 budget review. We have talked with Chairman Seaborg and urged him to avoid any implication in the press release or otherwise that this program has been approved for the 1967 budget. In fact, we plan to raise this as one of the major issues with AEC in our fall budget review."(16)

When Weston, Illinois was announced as the site for the 200 BeV on December 16, 1966, it was widely believed that Johnson had a direct role in the choice, both in support of his general policy of spreading federal funding more widely and in response to lobbying by Illinois Senator Paul Douglas. David Z. Robinson, who worked in OST at the time, later asserted that Johnson "personally picked" the site.(17) Catherine Westfall argues otherwise.(18) In any case, the resultant of the powerful political forces at work at the time clearly pointed away from a traditional siting at Berkeley or Brookhaven, or, for that matter, anywhere on the east or west coasts.

As planned, funding for the 200 BeV was included in Johnson’s FY 1968 budget request submitted shortly after the approval of the Illinois site. But before 1967 was out, the President cut back the request and proposed stretching out the design phase and delaying the beginning of construction for the project. On July 16, 1967, the New York Times editorialized against the project, citing its
"irrelevance... to any real present national problem," at a time when there were riots in the streets and "a bloody war in Vietnam," and calling it an "interesting but unnecessary scientific luxury."(19) An intense debate over open housing in Illinois further complicated the approval of design funding, but ultimately Illinois Senator Everett Dirksen and his supporters prevailed on that point.

When construction funds for the 200 BeV again became an issue during the FY 1969 budget debate, BOB Director Schultze again squared off with Seaborg and Hornig, and again Johnson made the final decision in favor of proceeding with a $25 million item to start construction, a request which ultimately received Congressional approval.

Lambright describes the politics surrounding the launching of the 200 BeV project as a sort of partnership between the President and the Congress, with neither side taking a firm position of leadership, but each side taking incremental actions which elicited a response from the other. Within the administration, the BOB and the AEC and OST took opposite stands on the issue, and ultimately, the dominant pressure on Johnson to proceed came from Congress, which itself took its cue primarily from its midwestern delegation. Lambright argues that there were many proponents of the project, and few strong opponents, who in any case entered the battle late.(20)

With construction of the 200 BeV underway, the high-
energy physics community could have been forgiven a huge, collective sigh of relief. But the years of plenty were gone, though few could have foreseen just how difficult the road ahead was to be.
Notes


15. Quoted in Lambright, _Presidential Management of Science and Technology_, p. 60.


18. In "The Site Contest for Fermilab", Westfall reviews the comments of Robinson and the analyses of Lowi, Ginsberg _et al._ and Lambright in light of her own research and argues that Johnson did not intervene in the final site selection.


20. Lambright, _Presidential Management of Science and Technology_, pp. 56-64.
7. LESSON IN SURVIVAL, PART II:
HIGH-ENERGY PHYSICS IN AN AGE OF MALAISE

Projection of Present Trends into the Future

The simplest approach to projection of present operating funds would be a straight line fit to the data... from FY 1969 through FY 1975. Since the extrapolation of this line into the future would indicate complete elimination of funding in about 20 years, a somewhat more optimistic assumption... was made.

From the report of the HEPAP Subpanel on the Health of High Energy Physics, October 1974(1)

Lowi, Ginsberg et al. argue that from the perspective of the physics community, the site selection competition for the 200 BeV was irrelevant - that the physicists had gotten everything that really mattered, namely the accelerator they wanted, located in a reasonably congenial place.(2) However, this reading of the situation overlooks the fact that the site selection process was merely one of many symptoms of a drastically changed underlying reality. Gaining funding for the 200 BeV project each year became increasingly difficult. For FY 1970, the AEC requested $102 million for construction and got $70 million; for 1971 it wanted $112 million and got $65 million in the budget.(3) The demands of the 200 BeV were made against a total high-energy physics budget which was not only no longer rising rapidly, but which was nearly static in dollars in an environment of persistent inflation. The discussion of the
high-energy physics program before the JCAE during AEC authorization hearings early in 1970 was filled with tales of woe, as scientists and AEC administrators pleaded for reconsideration of decisions to drastically curtail operations at existing accelerators or to shut them down entirely. Even SLAC, which following its opening in 1966 had become one of the shining lights of the American program, was limping along at less than full capacity. Word from the AEC’s director of research that American work on the important new technology of storage ring based, colliding beam accelerators was severely constrained by budget limitations, while CERN plowed ahead in exploiting this American-born conceptual breakthrough, elicited not an endorsement of additional funding from the JCAE, but merely a question from member Craig Hosmer: "What is the price of admission to CERN, if any?"(4)

The High Energy Physics Advisory Panel, established as a permanent standing committee in 1967, thus found itself facing a different task from that faced by the earlier advisory panels. Now the task was not to draw up an ideal wish list, but to make hard decisions about terminating existing programs while trying to build new machines on a budget that was effectively declining, and above all, to plead against further cuts. The strategy of using multiple funding sources to buffer changes in any single source, used so effectively in a number of other branches of science, had
never caught on in particle physics, despite years of lip service, because of the overwhelming dominance of the AEC's support. Now, as if to drive home the point, the military, represented by the Office of Naval Research, withdrew entirely from the field as part of a broader reevaluation of its support of basic research, leaving only the small NSF program, representing about 10% of the total high-energy physics expenditure, as an alternative. The NSF continued to sponsor important work by university-based groups, and was represented on the hardware side by a series of relatively small but still productive electron accelerators at Cornell University. But despite the long-standing theory of NSF as a "balance wheel" for American science, it was in no position to pick up significant slack in the AEC particle physics program.

The arrival of Richard Nixon in Washington was no help. The relationship between Nixon and the academic scientific community was one of mutual suspicion and disdain. Academic scientists, many of whom had long been antipathetic to Nixon, were in the forefront of protests against administration handling of the war in Vietnam. PSAC members and former members caused the administration considerable embarrassment through public disagreement on controversial matters such as the space shuttle, the antiballistic missile and the supersonic transport. Never one to be tolerant of much dissent even under the best of circumstances, and
finding the White House science advisory apparatus worse
than useless in meeting his political needs, Nixon disbanded
the science offices entirely in 1973. Like many other
advocacy groups, representatives of the scientific community
found themselves increasingly sealed off from the President
by his powerful inner circle of staff and advisors.

"Nixon as president was attracted to sophisticated
technology because it represented strength, the triumph over
adversity," observed James Everett Katz. However, "he
understood poorly the limitations of high technology and the
dynamics of scientific research."(5) As a consequence, he
supported scientific and technological programs selectively
for their perceived political advantages. According to
Katz, this strategy clashed with the continuing reality of
constrained budgets, resulting in a periodic "gush of
funding" for newly declared priority areas but turmoil and
retrenchment in scientific research taken as a whole. In
the face of Nixon's sporadic and political interest in
science and technology, esoteric programs in pure science
were at a disadvantage.

High-energy physics funding did reach a new high in FY
1970 as construction activity at the new National
Accelerator Laboratory (the 200 BeV project) reached a peak,
but thereafter annual funding began a sharp decline in real
dollars which continued through the end of the Nixon
administration.
When Gerald Ford took office in 1984, he faced the problem of healing the political wounds left by the Watergate scandal while dealing with continuing economic crisis. Both by natural inclination and in response to his unprecedented role as an unelected president at a time of crisis, Ford took a conciliatory approach, seeking broad consensus as the basis for his policies.

Basic science fared better under Ford than under Nixon, both because Ford had a more realistic understanding of the impact of the budgetary process on science and because Ford himself, Office of Management and Budget Director James Lynn, and Lynn's associate Jim Mitchell shared a belief in the importance of basic research to the economy and to the long-term solution of social problems. Before leaving office at the end of his brief term, Ford reestablished a formal science advisory apparatus in the White House.

The key bureaucratic development affecting the high-energy physics program during the Ford administration, however, was the breakup of the Atomic Energy Commission, with the AEC's research responsibilities assumed by the new Energy Research and Development Administration. After years of debate about the status of the AEC as atomic power reached maturity and about the federal government's broader responsibilities for energy policy, the change was no surprise. Yet in the crisis atmosphere surrounding energy policy, the high-energy physics community feared that its
funding could get lost in the confused bureaucratic shuffle toward a renewed and broadened set of pragmatic goals. The initial placement of the AEC's physical research program under ERDA's division for solar, geothermal, and advanced energy systems raised fears of a substantial shift in funding priorities. ERDA administrator Robert C. Seamans, Jr., spoke of substantial reorientation of the physical research program to better serve the agency's energy-related mandate, and speculated about diverting part of the high-energy physics budget toward more applied areas.(7)

The message about public priorities was clear, and the physicists wasted no time in adapting traditional arguments to the new situation. Dr. John M. Teem, ERDA Acting Deputy Assistant Administrator for Solar, Geothermal, and Advanced Energy Systems, described the high-energy physics program for the JCAE in March, 1975:

The [ERDA] physical research program supports about 90 percent of the Nation's basic research in high energy physics, which is concerned with subnuclear phenomena, the fundamental nature and relationships of the four basic forces of nature and the transformations of matter and energy from one form to another. Since all forms of energy are ultimately based on the four fundamental forces of nature, if any new source of energy is to be discovered, it probably will be based on one or more of these forces.

High energy physics research is widely believed to be on the frontier of importance to the long-term progress of science and continues to provide technological spinoffs important to future energy technology [for example, superconducting applications, instrumentation, on-line data processing].
However, the primary objective of the high energy physics program is to increase our fundamental knowledge of nature, that is, of the structure of matter and of energy transformations. We cannot foresee exactly how such information will be utilized; but if the past is any guide to the future, we can be confident that mankind will benefit substantially from such knowledge. (8)

At the National Accelerator Laboratory, renamed Fermilab upon its official dedication, the team headed by director Robert R. Wilson had managed to complete a 400 GeV accelerator rather than the initially planned 200 GeV device, on schedule and within a tight budget. The Fermilab scientists soon developed a proposal, the "Energy Doubler," to install superconducting magnets in the existing main accelerator ring as a cost-effective way of increasing its energy still further, to as much as 1000 GeV (1 TeV). Now, however, with the cost of electric power to run Fermilab soaring and Congressional attention focused on the energy crisis, the energy-conserving properties of the proposed superconducting upgrade became equally important. By the time Wilson testified before the JCAE in 1975, the "Energy Doubler" had become the "Energy Doubler/Saver."

During the 1975 hearings, Teem, like many of his predecessors in prior years, was forced to walk a fine line in responding to questions from the JCAE. Having argued the importance of high-energy physics for the nation's energy program, he nevertheless had to defend the administration's budget, which did not include funds for projects the scientists had labeled "high priority," in the face of
strong questioning by Senators John Tunney and Joseph Montoya:

Senator TUNNEY. I don’t really understand in any way the discipline of high energy physics and what goes into making decisions as to how much money ought to be spent on high energy physics programs, how much ought to be spent on new construction or maintaining existing facilities. I am completely over my head just the way that a high energy physicist would probably be over his head if he wanted to talk contract law with me.

Dr. TEEM. I am sure you are right, sir.

Senator TUNNEY. The thing is you are a high energy physicist, and if you want to contract to buy a new house you go to a lawyer.

If you are in the Senate and want to learn something about high energy physics you go to the scientists who are specialists in the field. I think that it is constructive [sic; instructive?] that the Government scientists felt that there ought to be a new funding authorization of $72 million for the PEP program [at SLAC] with funding of $23 million, and the nonphysics economists at OMB felt there should be no funding.

What I am trying to ask is: Are we jeopardizing the future of this field of research by not including construction authorization for fiscal year 1976?

...

Senator MONTOYA. Let me ask you, Dr. Teem, and I want you to be very frank and you divorce yourself from the OMB mentality here, do you feel that we are doing enough and providing enough money for energy research at this time and for the immediate future?

Dr. TEEM. Mr. Chairman, I have had some –

Senator MONTOYA. Now, be frank with me.

Dr. TEEM. I am trying to be frank. I have had some experience in managing research and development projects and I have never seen any that couldn’t use somewhat more money than was
available....

Within the constraints that we have for fiscal responsibility across the whole budget, I believe that this is a balanced program that we have before you.

Senator MONTOYA. We are in a period where the need for new technologies and new approaches for the development of energy are so urgent that I don't know whether it is in order in this type of atmosphere or syndrome to use the words "reasonably constrained level for new facilities construction."(9)

Of course, Teem had several distinguished physicists on hand, including Wilson, who could speak freely where he could not. At the same hearings, however, Teem also had to respond to a grilling by Representative Andrew Hinshaw, who took a skeptical position in response to Teem's and the physicists' reluctance to be specific about energy-related payoffs from high-energy physics:

One of the basic reasons why ERDA was created, and I formerly was on the House Government Operations Committee which had a part in it, the only reason I believe it got created at this time was because of our concerns for the immediate future of our energy sources. I, for one, would not sacrifice the immediate, applied goals of trying to get sources of energy now to solve not only our own problems, but the worldwide shortage of energy so that we are not dependent upon foreign sources of energy.

I am a strong believer, probably second to none, in basic research and development. I will recognize that to get to the point where we can provide the money for research and development we need to have some immediate payoffs.

Quite frankly, in your statement... when you said the primary objective of high-energy physics is to increase our fundamental knowledge of nature, not being a scientist that statement turns me off, particularly when you go on to say we
cannot foresee exactly - maybe that is a qualifying word - how such information would be utilized.

I believe that in today's climate - scarcity of money - that if you can't predict with a reasonable degree of certainty how such basic research information can be used in the future for the energy program, then that basic research ought to take a second priority to the immediate goals.(10)

With the coming of the Carter administration, the high-energy physics community faced yet another bureaucratic reorganization, as ERDA was transformed into the new Department of Energy. This transition was less of a shock than the abolition of the AEC, because it did not represent a qualitative change in focus in the way that the earlier reorganization had. Perhaps of greater immediate significance was the end of the Joint Committee on Atomic Energy. Once one of the most powerful committees in Congress, the JCAE had outlived its usefulness. As the 95th Congress opened for business in 1977, it voted to parcel out the jurisdictions of the JCAE to a range of other committees; effective jurisdiction over ERDA (and the forthcoming new DOE) and all its nuclear research was handed to the House Committee on Science and Technology. For ERDA/DOE's physical research program, this change symbolized the transition to a new generation of Congressional leaders which had already been well underway within the JCAE. The Congressional experts on atomic energy - the Holifields, the Hosmers, the Prices - were gone. A new generation of public
representatives, most of whom were not present to participate in the construction of the government's research program during the postwar years, had to be educated in the meaning and significance of the physical research program by a new generation of physicists, most of whom did not share the special distinction of having participated in the great war effort.

One of the new generation in Congress was Mike McCormack of Washington, who was beginning his fourth term in the House as President Carter took office. McCormack held a master's degree in chemistry from Washington State University, and had worked as a research scientist at the AEC's Hanford installation from 1950 to 1970, giving him a technical background rare in Congress. McCormack had served on the JCAE, and in 1977 became chairman of the Subcommittee on Advanced Energy Technologies and Energy Conservation Research, Development and Demonstration of the House Committee on Science and Technology. That long-winded title gave him a platform from which to express his support for the physical research program. During the final authorization hearings for ERDA early in 1977, McCormack took up the high-energy physicists' cause, arguing along with the scientists that the American high-energy physics program was living off past investments, and that the now significantly greater European funding for particle physics threatened to leave the United States in a disadvantaged
position in basic research in the future. (11)

As the Carter administration and the new Department of Energy moved into action, the situation in high-energy physics appeared reasonably clear. Near the end of a painful decade of shutting down older, smaller accelerators, the particle physics community envisioned a future in which cutting-edge research was concentrated in three facilities: SLAC, in California, with its electron accelerator projects; Fermilab, in Illinois, which would focus on further development of its high-energy, fixed-target accelerator; and Brookhaven, on Long Island, which would be the site of Isabelle, a new project which was to bring America into the major leagues of colliding beam accelerators. Isabelle was to play a role similar to that played by the 200 BeV project in the 60's and 70's — it was to be the big accelerator project of the decade, and was intended to keep American particle physics in the lead through the 80's.

But money for development of these three centers, as well as ongoing support for the remaining older machines and the university-based user groups, had to come from an overall budget which remained tight by the high-energy physics community's standards, even after three years of moderate increases begun during the Ford administration. There was little maneuvering room in the budget, particularly after October 3, 1977, when Congress authorized $10.5 million to begin construction of Isabelle,
establishing a formal commitment to the estimated $275 million project. And behind the scenes, trouble was brewing.

Within the physics community, rumors had already started to circulate that Brookhaven scientists were having a harder time than anticipated developing the new generation of superconducting magnets on which the success of Isabelle depended. A proposal had been floated at Fermilab to upgrade its machine into a colliding beam machine on a timetable competitive with that of Isabelle, but Robert Wilson of Fermilab was having a hard time financing even the Energy Doubler/Saver upgrade, keeping it alive by calling it an R&D project and shuffling funds from other parts of the laboratory. Within a few months after Isabelle construction was launched, OMB decided that it was no longer willing to tolerate the subterfuge and insisted that the Energy Doubler/Saver must be funded up front as a construction project, or it would be stopped. Ultimately, a compromise was worked out within DOE to keep the Fermilab upgrade alive. When the extent of the Carter administration’s commitment to Isabelle became clear, however, Wilson threatened to resign as director of Fermilab unless additional funding was forthcoming. Wilson was bluffing, but his bluff was called, and when no additional funds were made available, he resigned. Some particle physicists felt that Wilson, who had begun his studies under Ernest Lawrence
himself, had guided Fermilab along the traditional path on which bigger machines and higher energies took precedence over all else, even the physics which was the ostensible purpose of accelerators. But he had proven to be an effective manager under difficult circumstances. And when Harvard physicist Carlo Rubbia, a key player behind the colliding-beam proposal at Fermilab, successfully transferred his lobbying efforts to CERN, the United States was left with essentially its entire bet for the future placed on one horse, Isabelle, at a time when the Europeans were making a renewed, well-funded push to finally grab the lead from the Americans after decades of also-ran status.(12)

The increases in real funding resumed during the Ford years were not to continue past FY 1978. Secretary of Energy James Schlesinger, concerned with more pressing matters, was indifferent at best with respect to the high-energy physics program. When the new director of the Office of Energy Research at DOE, physical chemist John Deutch from MIT, worked out a plan with OMB under which the high-energy physics program would be level funded for the foreseeable future, thus assuring that Isabelle would not cut into other DOE programs, Schlesinger approved the budget and moved on to other issues of greater concern. Including all operating and construction costs for both DOE and the much smaller NSF program, the projected level of funding for the program was
to be approximately $325 million in FY79 dollars. Deutch defended the plan before the House Appropriations Committee in March, 1978:

... The plan... provides programmatic stability and the completion of needed new facilities at each of the three laboratories [SLAC, Fermilab, Brookhaven] on a reasonable schedule. In this manner we can expect continued world leadership of the U.S. in High Energy Physics. If it is government policy to provide for real growth in basic research in future years I would expect High Energy Physics to share in that growth.(13)

Over the succeeding years, considerable confusion developed over whether the "Deutch guideline" had been intended as merely a guideline, as a ceiling, or as a floor.(14) Deutch himself recalls it as a ceiling, and in fact, following FY 1978, funding in real dollars started to drift downward yet again.(15)

As the budget slowly tightened, the rumors coming from Brookhaven became increasingly worrisome. By 1981 the now widely-acknowledged technical problems in the Brookhaven magnet program became the focus of a major news article in Physics Today.(16) Not surprisingly, the problems drew increasing attention from Congress as well. Year after year, however, the particle physics community rallied behind the project, at least in public. Repeatedly, the High Energy Physics Advisory Panel convened to consider the state of the American program, and repeatedly it endorsed Isabelle as "essential" to the health of American high-energy physics in the next decade. A seemingly endless parade of
physicists and DOE bureaucrats marched to Capitol Hill to reassure skeptical congressmen again and again that yes, there had been problems, but all was now well, and that the future depended on continuing support from the Congress.

The 1978 DOE/OMD guideline had been intended to be sufficient to support Isabelle along with the other, ongoing programs. But the superconducting magnet problems at Brookhaven began to eat into the money available for the rest of the program. In a report in late 1981, HEPAP recommended that Isabelle be completed, subject to Administration commitment to a significantly higher level of ongoing support. (17) By 1982, the Reagan administration was proposing holding back on further construction money for Isabelle. In March, 1982, Sidney Drell, deputy director of SLAC and former HEPAP chairman, testified before the House Science and Technology Committee that Isabelle could not be completed if the high-energy physics budget were to remain within the DOE/OMB guideline. (18)

The high-energy physics community simmered with dissension over the future of the program. On January 27, 1983, Alvin Trivelpiece, director of DOE’s Office of Energy Research, wrote to Jack Sandweiss of Yale, chairman of HEPAP, calling for the physicists to get their act together, and quickly:

... I am concerned that the present debate on directions and distribution of resources within the high energy physics community is taking on some unhealthy characteristics that have the
potential for making the situation worse for all concerned. I am only too well aware of the budgetary problems that this and other science programs are facing, and I am sympathetic with the frustrations that have led to the acrimony and quasi-lobbying efforts of some of the high energy physics community. However, these efforts are not helping to resolve the problems, and I would ask you, through HEPAP, to encourage a more restrained and statesmanlike approach by all members of the community.

... I am requesting that you work together with Jim Leiss, the Associate Director for the Office of High Energy and Nuclear Physics, to develop a charge letter for the 1983 [HEPAP] New Facilities Subpanel that makes it clear that what is needed at this time are unequivocal recommendations on the research programs that will, in their opinion, make the most effective progress. I then expect HEPAP to debate these recommendations and to endorse, qualify, or reject them, as might be appropriate.

Whatever the outcome of your deliberations, I hope that you will not deliver up "soft" recommendations at the end of this process. By that I mean that it will not be particularly useful to have recommendations that we could do thus and so if we just had so much funding. What are the most important high energy physics facilities for the United States to have, regardless of where they might go or how much they might cost? What are the elements of a balanced high energy physics research program that will ensure continued progress in experiment, theory, and the associated advanced technology? These are the kinds of questions that the Subpanel and HEPAP will have to consider in making their recommendations.(19)

The growing impatience within the Reagan administration was further underlined by the blunt comments of Reagan science adviser George Keyworth in a speech before the American Physical Society in Baltimore that April. Keyworth told the physicists that "our world leadership in high-energy physics has been dissipated" by "a pork-barrel
squabble" over which laboratories should be allowed to build new accelerators, while the Europeans were moving "boldly ahead."(20)

Meanwhile, the scientific rationale for Isabelle was evaporating quickly. A key goal of the Isabelle program from the start had been the search for the postulated carriers of one of the four fundamental forces of nature, the weak nuclear force. In January, 1983, Carlo Rubbia announced the discovery by his team at CERN of two of the three anticipated particles, the $W^+$ and $W^-$. By June, Rubbia had announced discovery of the last of the three, the $Z_0$. The discoveries were made using CERN's Super Proton Synchrotron operating in colliding beam mode - an upgrade constructed as a result of Rubbia's lobbying in Europe following the indifferent response to his Fermilab proposal. The discoveries were to lead to the Nobel Prize for Rubbia and his colleague Simon van der Meer.

Within days, the New York Times, which more than a decade and a half before had been quick to condemn spending on the 200 BeV project as an extravagance, published an editorial entitled "Europe 3, U.S. Not Even Z-Zero," demanding "earnest revenge." "The physics team needs to try harder," opined the Times, adding that "coach Keyworth should reward any sensible new strategy with management's full support."(21) The HEPAP New Facilities Subpanel was already scheduled to meet during June; it reached its
conclusions at the beginning of July, and on July 12 HEPAP itself endorsed all of the subpanel’s recommendations and forwarded them to Trivelpiece. The panel’s key recommendation was to terminate Isabelle and initiate immediately a research and development program to lead to the construction of the Superconducting Super Collider, a completely new machine with an energy far greater than that of any other accelerator then operational or planned, and with an estimated cost in the billions of dollars. (22)

Expressed in terms of the gambling metaphors so commonly used to characterize the international rivalry in high energy physics, it was as though the Americans had just folded their hand after throwing a hundred million dollars into the pot, in favor of joining yet another game with stakes at least twenty times as high.
Notes


2. Theodore J. Lowi, Benjamin Ginsberg *et al.*, *Poliscide* (New York: Macmillan, 1976); see, for example, p. 102.


15. Personal communication, October 4, 1989.


What is at issue is the viable pursuit of a concept set forth most clearly in the Greek colony of Miletus in 650 B.C.: The universe is beholden to a rational order and the human mind is capable of comprehending this order. Consider that a scientific instrument, devised by inhabitants of a minor planet, may serve to illuminate the issues of creation, evolution and the mechanisms of the entire physical universe. The supercollider must be one of the most spectacular bargains ever offered to the American public!

Leon M. Lederman(1)

The purpose of the SSC is to force basic particles to collide so we can better understand the four basic forces of the universe - the weak force, the strong force, gravity, and electromagnetism. The debate over the SSC serves a similar function. Our funding priorities collide, and that shows the forces at work in the Congress.

We have the weak force: apathy and ignorance. It says the SSC is too big to really understand, so let’s just go with it. Then we have the strong force, interested constituents and lobbyists that stand to benefit from the SSC and who are calling members to advance their own special interests through support of a science project. I urge my colleagues to resist that force. Then we have gravity, the arguments that pull us back to earth and make us realize there are serious policy questions at stake here. I believe that force argues against putting money into site work when there are still open questions about the project and more urgent science priorities. And finally, there’s the equivalent of the electromagnetic force, that sees science as a single unit, not as a series of programs scattered throughout appropriations and budget accounts. And I think that, when we look at science as a whole, the SSC does not jump out as the most urgent item.

Rep. Sherwood Boehlert(2)
If this project is so good, why are [foreign nations] not doing it themselves? Because the fact of the matter is that the development of the SSC will have America eating its science seed corn.

Rep. Dennis Eckart(3)

The idea of an ultra-high energy collider was not new; indeed, high-energy physicists had a long tradition of looking beyond current machines to speculate about what ought to come next. However, the idea of an SSC is reported to have gained widespread enthusiasm for the first time at an American Physical Society divisional conference held in Snowmass, Colorado, in the summer of 1982, when physicists began to realize that such a machine might be within the reach of current technology.(4) Thus, it was already on the agenda when matters reached a crisis point the following year.

The discoveries of the W and Z particles at CERN in 1983 were not really a surprise for the Americans. Nevertheless, the events served to drive home the changed situation in particle physics. In the early 1980's, the scientific content of leading edge experimental work in the field amounted to an effort to test the so-called "standard model" in which the theory outlined by Sheldon Glashow, Steven Weinberg and Abdus Salam described the unified electromagnetic and weak nuclear forces, and the theory of
"quantum chromodynamics" described the strong nuclear force. By mid-1983, though, the Europeans had twice beaten the Americans to key results in this effort. The West German electron-positron collider PETRA went into operation in 1978, about a year before SLAC's equivalent, PEP, and "skimmed the cream" of the interesting physics accessible to such machines, including the first experimental evidence for gluons, a central component of QCD. And then came the W and Z particle discoveries of 1983.

Pressure on the high-energy physicists came from another source as well. The scientific community tended to be suspicious of Reagan as they had earlier been of Nixon, seeing him as ignorant and "anti-science," with a science policy favoring budget cuts for non-defense-oriented, basic research. His selection of George Keyworth as his science advisor perturbed many still further. Keyworth, who had previously served as director of the physics division at the Los Alamos National Laboratory, came from outside the traditional science policy ranks. His early speeches in his new job dealt with the need for the federal government to make some hard choices about what to support. Keyworth argued that the sense of entitlement common within the scientific community was one of the causes of a creeping mediocrity that threatened American science.

For Keyworth, the Isabelle affair was a prime example of what could go wrong with science when the scientists
became too complacent. But he also believed in the need for selected, large-scale, high-visibility "big science" or "big technology" projects to play a symbolic role representing national optimism and expectations for the future, and in the power of such projects to serve as catalysts for the revitalization of science and technology across a broad front.(5) As he pressured the leaders of the high-energy physics community to confront reality and abandon Isabelle, he urged them to think big, and promised them in return that he would support a well thought-out proposal if the community united behind it. And given the decade or longer that any major new project would take, a total price tag in the billions of dollars was not out of the question, he suggested, even though this would require a substantially increased annual budgetary commitment.(6) In his speech to the APS meeting in Baltimore in April, 1983, Keyworth made a public statement that "a 20-TeV accelerator should be taken extremely seriously."(7)

Thus the HEPAP recommendation, bold as it was, had the tacit approval of the Reagan administration, and DOE accepted it as the basis for its own planning. In letters dated October 18, 1983, to the chairmen of four Senate and House committees dealing with science and energy research, Secretary of Energy Donald Hodel announced DOE's intent to terminate the Isabelle project, marking the formal opening shot in what was to be a long and difficult battle on behalf
of the SSC.(8)

The Congress reacted immediately, with Rep. Don Fuqua, chairman of the House Science and Technology Committee, calling a hearing at the request of New York Rep. William Carney, whose Brookhaven constituents were fuming at the decision. Carney began his remarks at the hearing with a slide show designed to drive home the extent of the investment at Brookhaven. He opened with a satellite picture of Long Island, pointing out that the only discernible man-made object in the entire picture was the huge circular tunnel built for Isabelle, and continued with closeups of the millions of dollars worth of gleaming new facilities already in place at Brookhaven. And then he began grilling the Administration witnesses, Trivelpiece, Sandweiss and Stanley Wojcicki of Stanford, chairman of the HEPAP New Facilities Subpanel:

Tell me, Dr. Sandweiss, how do I go back to the people in the First Congressional District in New York and tell them that it is prudent to abandon a $200 million concrete doughnut in that district to move on to build a machine that I can’t even describe.

I don’t know if it will be 50 kilometers. I don’t know if it will cost $8 billion. I don’t know where it is. I don’t know who heads up the team. I don’t know the design concept.

How do I get a consensus of the American taxpayer that this is a prudent step?(9)

Carney had obviously done his homework, for he hammered hard at the witnesses on almost every conceivable point of weakness: Why the radical change in plans after years of
asserting that Isabelle was "essential"? Why after years of struggling to get by within the "Deutsch limit" did they suddenly believe that substantially increased annual funding was to be available? Was Keyworth behind this? If the Z particle was already discovered at CERN, justifying the decision to kill Isabelle, what justified the decision to proceed with the new SLC project at SLAC, itself designed to produce Z particles? How did an almost evenly split HEPAP subpanel vote magically become a unanimous HEPAP recommendation? Was not the proposed SSC an unreasonably risky extrapolation beyond existing technology? What would happen to the high energy physics program if the R&D program for the SSC ran into trouble?

But the decision to terminate Isabelle was final, and before another year had passed, Carney himself had come to support the SSC. Meanwhile, as the decision began to sink in, all concerned began to ponder the implications of the SSC. So controversial an undertaking was ripe for satire, of course, and science journalist Gary Taubes did not let the opportunity slip by. The SSC had been referred to by some as the "Desertron", because it would be so huge that it might have to be built out in the desert somewhere. "Onward to the Dessertron," then, was the title of Taubes’s guest comment in Physics Today:

In July, the High Calorie Dessert Advisory Panel of the Food and Drug Administration recommended that the number-one priority in research for the next two decades should be the
ice-cream accelerator officially named the Superconducting Super Osterizer (SSO). The mammoth blender, as they have proposed it, would be as much as 120 miles in diameter with several different speeds from puree all the way through whip. It would take twelve years to build and cost $2.2 billion, but it would also chop, dice, slice and make moist icing. Among the desserts that scientists hope the machine will find are the raspberry quark, the Higgs Sundae... and several different flavors of antipastries...

...The state of Texas has promised that if the machine is built in Texas, it will pay for the tunnels and the refrigeration equipment needed to cool the ice cream down to a few degrees above absolute zero to save money on artificial preservatives...(10)

A concerned Texan physicist hastened to respond that not all shared this "Desertron" fever.(11) But Taubes's satire was on the mark. A group of four Texas universities had already convinced Governor Mark White to write to DOE in support of a generous bid for the megaproject. Other states followed suit, having learned well the lessons of the 200 BeV, and the gold rush was under way.

During hearings in February, 1984, before the House Energy Development and Applications Subcommittee, DOE's Trivelpiece addressed comments about SSC sitting by noting one of the most far-reaching suggestions: "arrange the SSC in such a way that its beams, which can be steered with magnets rather effectively, will be directed to pass through the physics departments of about 20 different universities on the eastern seaboard. In that way, a support base would be assured." Rep. Carney responded in good humor: "If that is the design plan, high-energy physics would be operating
like the Pentagon. Put it in 218 districts of Congress and any program will be funded."(12) Carney may have spoken in jest, but the Congressional dynamics of pork barrel were to prove even more critical to the progress of the SSC than had been the case for the 200 BeV, perhaps because the stakes were even higher this time around.

Through the mid-1980's, preliminary R&D work on the SSC was nursed along on a few tens of millions of dollars per year. Congress and the Administration grappled with the problem of whether and when to commit to actual construction against a backdrop of ever-increasing budget deficits, leading to the Gramm-Rudman deficit-reduction measures. But the Europeans, after triumphantly "scooping" the Americans, now faced the same problem. CERN was level funded, forcing its staff to cover both operating expenses and new construction from a fixed pool of money. In this environment of renewed fiscal stringency on both sides of the Atlantic, themes which had appeared previously in the evolution of high-energy physics reemerged as the focus of a chaotic, hard-fought battle.

For decades, particle physicists had speculated that a day would come when the next particle accelerator would be too expensive for any one nation - or regional consortium - to build, forcing genuine international collaboration as the price of continuing progress. Yet somehow that point kept receding into the future, as succeeding national and
regional machines grew ever larger. Discussions in the mid 1970's about a hypothetical "Very Big Accelerator" led to the establishment of an International Committee for Future Accelerators to further develop ideas for such a device. Less than a decade later, however, Robert Wilson, writing as a participant, was forced to admit that the still-hypothetical 10 TeV "VBA" had been bypassed by the very real American proposal for the SSC with its colliding 20 TeV beams. (13)

Thus, when in response to Congressional pressure the Administration began to seek international collaboration on the SSC, it was understood by all that, rhetoric and the continuing and very real international collaboration on experiments at all of the major accelerators notwithstanding, the United States had no intention of allowing the SSC to become a truly "international" machine. Purely and simply, the Americans needed the money in order to convince the Congress to buy into the project. But the Europeans now faced the same funding dilemma, and had no desire to let their governments send scarce money across the ocean to help restore American dominance in particle physics. (14) Even without a major European contribution, the construction of the SSC might provide the CERN member states with an excuse to squeeze the budget even more. "If the Americans build [the SSC], then we in Europe are in trouble," said Nobel Prize winner Simon van der Meer.
CERN director-general Herwig Schopper responded by floating a proposal to install an extra ring of superconducting magnets in the tunnel being built for CERN’s newest machine, the Large Electron Positron collider, thus creating a proton-proton collider capable of reaching center-of-mass energies of up to 18 TeV, compared with the SSC’s 40 TeV, at a small fraction of the cost of the SSC. He invited the Americans to collaborate on this proposed Large Hadron Collider. There were many unresolved questions about the LHC proposal, including whether the cost and technology projections behind it were realistic, and whether the LHC’s energy limit of 18 TeV would miss important physics that could be captured by the more powerful SSC. But it was a clever gambit designed to take advantage of the Congress’s acute concern with cost-effectiveness, and it complicated matters at home for the Americans. (15)

The Japanese were considered likelier candidates, because of their extensive experience with superconducting magnets, their desire to join the big leagues of particle physics, and the manageable size of their existing financial commitments in the field. (16) But the growing American concern over Japanese competition in high technology made this possibility controversial. Potential Canadian contributions, which could not have been huge in any case, were thrown into jeopardy when DOE ruled out any possibility of a cross-border site for the SSC, arguing that it would
give certain states an unfair advantage in the site selection competition.(17)

Dissent within the scientific community over the implications of the huge funding demands of high-energy physics had expressed itself twenty years before as a polite, measured debate confined primarily within scientific and science policy circles. Now, unusually bitter disputes between physicists and other scientists, and among practitioners of various specialties within physics itself, broke out at professional meetings and in professional journals. An article by Sheldon Glashow and Leon Lederman promoting the SSC, which appeared in the March, 1985, Physics Today, drew a letter from Penn State materials scientist and science policy analyst Rustum Roy, sharply questioning the wisdom of spending $5-6 billion on the SSC at a time when other branches of science and technology with far more direct connections to technological and economic well-being were starving for funds. Roy’s letter and Lederman’s somewhat flippant response, which repeated parts of the traditional particle physics catechism without seriously addressing the issues raised by Roy, elicited a cascade of letters which continued into 1986. Lederman had seriously misjudged the nature and extent of the public relations problem facing proponents of the SSC within the scientific community, as his remarks at the close of the exchange attest: "Wow! It would seem as if my letter was
not received with universal approbation...." Lederman responded to some of the issues raised in the letters, and concluded: "Finally, a rereading of Roy’s letter convinces me that his is an extremist view. In spite of the mail, this can’t be the consensus of Physics Today’s readers. To use the punch line of my favorite story, 'Is there anyone else out there?'" (18) More decorous debates appeared as well, notably in the pages of Science and the NAS journal Issues in Science and Technology. (19)

With all of the debate, and the continuing funding for R&D, the Administration had yet to commit itself to construction of the SSC. Responding to questions from members of the House Science and Technology Committee, Secretary of Energy John Herrington acknowledged in March, 1986, that a decision must be made soon, one way or another. In July, with a decision still pending, the House Appropriations Committee cut $20 million from DOE’s high-energy physics budget for 1987 to emphasize its impatience. But with OMB persisting in its opposition to committing to such an expensive item at a time of huge deficits, the draft FY 1988 budget approved in the early fall of 1986 showed no funds for construction or even for continuing R&D work by the SSC Central Design Group.

Behind the scenes, however, Herrington had come to favor proceeding with the SSC, reportedly under the influence of Alvin Trivelpiece. He joined Trivelpiece in a
concerted effort to sell the SSC. Top industrialists such as Roger Smith of General Motors, Douglas Danforth of Westinghouse Electric, Edward Jefferson of duPont and John Akers of IBM were enlisted to write letters of support to Reagan, Cabinet officers, and members of Congress. Trivelpiece met with delegations of scientists to calm fears about adverse impacts on other branches of science, and made the rounds of Congressmen and their staffs on Capitol Hill.

White House preoccupation with the Iran-Contra scandal slowed their progress. Not until December was the SSC was the focus of discussion at two meetings of the Domestic Policy Council. Although the results of the meetings were inconclusive, it appeared that a small majority of the Cabinet opposed the project, primarily on grounds of cost. On January 29, 1987, however, Herrington and Trivelpiece had an opportunity to make a pitch before Reagan himself. After listening to Trivelpiece's presentation of the scientific context of the project and Herrington's discussion of the budgetary implications, Reagan is reported to have told an anecdote about professional football quarterback Kenny Stabler. Stabler was once asked the meaning of a Jack London poem, which Reagan proceeded to read to the assembled Domestic Policy Council off a card pulled from his pocket:

I would rather be ashes than dust
I would rather that my spark
Should burn out in a brilliant blaze
Than it should be stifled in dry rot.

I would rather be a superb meteor
Every atom of me in magnificent glow
Than a sleepy and permanent planet.

The proper function of man
Is to live, not to exist
I shall not waste my days in trying to prolong
them
I shall use my time

As the President told the story, Stabler said that London's credo meant, "Throw deep." Reagan concluded the meeting by advising Herrington to "throw deep."

George Keyworth, who had enthusiastically promoted the SSC, had by then left the Administration. But he had played an essential part not only in launching the SSC proposal, but in laying the groundwork for its approval by Reagan. By his energetic promotion and defense of the Strategic Defense Initiative, Keyworth gained the President's appreciation, and as a result, "was able to bank IOUs that he could cash during budget season on behalf of the growth of fundamental research." Looking back on his term as science advisor, he was to write that, contrary to the early expectations of many in the scientific community, Reagan was a strong believer in the importance of science to the nation, but with a special twist:

... His goal was to find ways to encourage and to allow American industry to be stronger competitors in the international market - not by manipulating trading conditions, but by making American industry better. And one of the cornerstones would be building on American science and speeding its translation into technology.... But he also was referring to something grander as well. His vision of America was one of growth and of unlimited potential.... America's leadership in science and technology was among the most enduring
post-war symbols of confidence in the future and opportunity for individual advancement through ability and effort. He saw science as future-oriented and as a powerful beacon for a democratic people - and he was naturally drawn to scientific and technological projects that captured and communicated that spirit, much as the Apollo project did twenty years ago.

Keyworth reviewed his own involvement in the launching of the SSC, concluding that

... The SSC, somewhat like the space program, is a national effort, one that restates our determination to be the center for scientific thought and creativity. The payoff will come not so much from quarks as from inspiration that spreads widely and from a continuing flow of the brightest young people in America into pursuit of science and technology - because the nation will have made it such a high priority. For my fellow scientists, that justification may seem vague, yet I believe it is exactly the kind of thinking that won the SSC political support.(22)

Herrington's own comments at a January 30 news conference, hastily called to announce the decision after Texas Senator Phil Gramm had leaked the news, reflected Reagan's attitude as recalled by Keyworth:

In high-energy physics the development of the Super Collider is the equivalent of putting a man on the moon...

It will have spinoffs, discoveries and innovations that will profoundly touch every human being. This is a watershed for America's scientific and technological leadership and another clear sign that President Reagan is committed to keeping this nation on the cutting edge of world leadership and competitiveness....

[The decision is] of tremendous scientific significance and historical consequence... It is a tremendous leap forward for America and for science and technology. Once again, this nation has said there are no dreams too large, no innovation unimaginable and no frontiers beyond
our reach. By virtue of this decision we are embarking on an adventure of unlimited opportunity, tremendous promise and a new scientific world to be won.(23)

What is not clear from the record is whether the hyperbolic tone of Herrington's remarks was an accurate reflection of Reagan's understanding of the scientific implications of the SSC, or simply a natural side effect of the enthusiasm of the moment.

The New York Times, in a curious editorial a few days later, endorsed the decision:

"In building the supercollider our nation will be constructing yet another temple to the one-dimensional hierarchy of prestige that equates the most atomistic science with the most blessed." That's the grumble of an astronomer, Arno Penzias, at the soaring price of the equipment demanded by his colleagues in particle physics.

Last week, despite a current budget deficit of $173 billion, the Administration announced it would spend $6 billion by 1996 for the next particle-colliding machine on the physicists' wish list. Is that a sensible call? Yes, though not for the reasons cited by Energy Secretary John Herrington.

The new supercollider... is research at its purest, with few obvious practical applications. The spinoffs that Mr. Herrington avers will "profoundly touch every human being" may prove as elusive as the magnetic monopole.

The justification for the supercollider is much simpler.... In supporting particle physics, America contributes to the disinterested pursuit of knowledge whose only benefit may be better understanding of the natural world....

In principle, Mr. Penzias is right to fear that the supercollider will mean less money for other branches of science. But the particle physicists long ago had the foresight to choose the Atomic Energy Commission, now the Department
of Energy, as their sponsor. They dip into a different, larger pool than other scientists.

The superconducting supercollider will open new energy regions, but "We know almost nothing about what will be found there," wrote the physicists who recommended the machine in 1983. The leading quarry is the Higgs boson... Many doubt such bosons exist. If they do, the Europeans may nail them down first.

No matter; something else may turn up. "Accelerators have never been built for the right reasons," comments Wolfgang Panofsky, a Stanford physicist. Rejecting the gamble of building the supercollider would have been a sorry retreat from a leading frontier of research.(24)

The Times's nonchalant endorsement of a $6 billion dollar venture in admittedly esoteric science in the face of a $173 billion budget deficit was remarkable for an editorial board normally given to grave calls for careful weighing of priorities, as was the implicit characterization of DOE as a handy deep pocket, cleverly chosen by particle physicists.

With Presidential approval of SSC construction, debate over the proposal entered a new phase. The announcement of rules for the site selection process, modeled after those for the 200 BeV, weeded out a handful of less-than-serious applicants but left a large group of states competing for the prize. Thirty representatives, two senators, and more than twenty governors and lieutenant governors appeared before the House Committee on Science, Space, and Technology or submitted written statements for the record of hearings held on the SSC in April, 1987. Thirty-one governors signed a letter, dated June 19, to Chairman Robert Roe of the House
committee and Chairman J. Bennett Johnston of the Senate Energy and Natural Resources Committee, urging them to back the collider. (25) The competing states represented a powerful political force in favor of the SSC, but despite the many protestations of undying faith in the inherent merits of the collider, wherever it may be sited, it was clear to political veterans that the situation at that moment was artificial and temporary. Rep. Joel Hefley of Colorado captured the dilemma in his questioning of Trivelpiece at the SSC hearings:

... I think you, Mr. Chairman, have outlined really the dilemma that we're in here, because I think most of us are enthusiastic about this, but we're dealing with a finite size pie or at least it doesn't always look like Congress deals with a finite size pie, but I think there's one out there somewhere and there's not money for everything. So, we're going to have to set priorities.

I guess I would ask you, [Dr. Trivelpiece] how are you going to catch the imagination of the Congress and the American people for this project? Now, you've caught the imagination of the Congress now, because all of us want it in our District... There's 435 Districts out there that want this thing there, and they think it will be great for the District, and it would be great for any District or State or region of the country. But once the decision is made to put it in one District, in one place, then it comes back to, what is it really going to do for mankind, and how can we catch the imagination, so that it's something we have to do and it's more important than doing some other things? (26)

The task for proponents of the project was to strike while the iron was hot, and gain Congressional authorization and initial appropriations for construction before the political force behind the project was dissipated.
That was easier said than done, however. Budget and appropriations committees in both houses of Congress were wary of making even a small, first-year commitment to the SSC, recognizing that such a move would carry an implied commitment to massive future outlays—"the proverbial camel's nose under the tent," as Reps. Don Ritter of Pennsylvania and Buddy MacKay of Florida put it in "Dear Colleague" letters urging a go-slow approach. (27)

Ritter and MacKay staked out strong positions in opposition to the SSC. Ritter was a rarity in Congress, having earned an ScD in metallurgy from MIT and spent 1969-78 as a professor and research program manager at Lehigh University before coming to Congress. MacKay, an attorney, had been heavily involved in science policy matters since coming to Congress in 1979. Ritter, in particular, played a prominent role in the April SSC hearings, stating his position bluntly and driving it home repeatedly before witness after witness:

... America's technological position... as we all know, is being undermined. We're still winning Nobel Prizes like they're going out of style. That is not the problem with the American economy. The problem with the American economy is that we're taken to the cleaners in technology and the commercialization of technology, and fields like I have mentioned need to be funded at a far more substantial level. And if we fund $4.5 billion in a basic particle physics effort, that most certainly will detract from all these other areas which are going begging. (28)

Ritter waged an active campaign against the SSC, not only within the Congress but outside as well, collecting
criticisms from luminaries in science and industry and writing for public consumption.(29) He relied heavily on the positions of several prominent scientists, including Rustum Roy, physicist James Krumhansl of Cornell, and Nobel Laureate physicist Philip Anderson of Princeton. Roy's opposition to the SSC had not been restricted to the letters column of Physics Today; he continued to write regularly in opposition to the project, and had testified before Congress on this and other matters of research policy. Krumhansl had fired off a sharp letter to Herrington shortly after the January 30 press conference:

    ... in the last thirty years I have not seen that particle physics has made any substantive contribution to technology generally, nor energy science and technology specifically. The proposed project will not be different. This investment will do nothing, either, to improve our scientific, technological, or industrial competitiveness. In fact, unless the many other more important areas of engineering and science are restored to adequate funding levels the commitment of the amount of money contemplated for the SSC project will certainly have a damaging negative effect nationally.(30)

Krumhansl also testified during the April SSC Hearings. Anderson was unable to appear in person at the hearings, but submitted a hard-hitting written statement for the record in which he analyzed "the myths supporting the unique value of particle physics." While not opposing the SSC in principle, he argued strongly against the rush to build it under the present circumstances, concluding:

    It disturbs me to see accelerator physics seen as a nationalistic, competitive race;
science is too serious a matter for that. And if the lack of the right accelerator here at exactly the right time is really going to kill high energy physics, I must say it is better off dead, if only for the crippling lack of imagination that that reveals. (31)

Anderson, too, played the op-ed circuit. (32)

Ritter and the three scientists shared a common interest in materials science, broadly defined to include condensed matter physics. As it happened, an astonishing set of recent events in materials science provided them with plenty of ammunition for their fight against the SSC. The unexpected discovery of materials that become superconductors at temperatures far higher than previously known touched off a frenzy of speculation about magical new technologies as well as paranoia about the apparent Japanese determination to master the new materials and their applications before the United States. Ritter et al. were able to argue not only that the SSC threatened to starve potentially critical research on the new materials, but that perhaps the SSC itself could be radically redesigned and built at substantially reduced cost using the fruits of such research. (33) This latter possibility was soon recognized to be an unrealistic extrapolation from still meager experimental results, but it served for a time to confuse an ever cost-conscious Congress.

On October 15, 1987, the House Science, Space and Technology Committee finally approved an authorization for the SSC. Opponents, including Ritter and Claudine Schneider
of Rhode Island, turned what was to be a simple markup session into a five hour marathon as they offered amendment after amendment to try to block approval. In the end, however, the measure passed by an overwhelming 38-6 vote. (34) But it was a hollow victory for proponents of the collider, at least for the moment, because ultimately DOE’s fiscal year 1988 budget once again contained money for continuing SSC R&D only.

Fiscal year 1989 proved to be no better. Much of the political momentum behind the SSC was dissipated early in 1988 when DOE announced its list of seven finalists in the site competition, leaving most of the project’s erstwhile supporters in Congress with no direct incentive to continue, and a few as furious, sore losers. An incautious remark by Harrington on March 10, predicting that other countries would contribute up to 50% of the cost of the SSC, provoked a hearing before the House Science Subcommittee on International Affairs. Committee members, some of whom had themselves visited CERN and gotten their own sense of European intentions, reacted angrily, and talk of making SSC authorization subject to some minimum level of foreign participation flared up again. (35)

President Reagan was enlisted in an attempt to galvanize public opinion. At a media event in the White House Rose Garden, Reagan accepted a letter from six Nobel Prize-winning particle physicists endorsing the SSC and then
spoke on its behalf before a group of 39 students from DOE's High School Science Honors Program. He referred to the SSC in one of his regular radio addresses as well, though with a Freudian slip. He was particularly proud, he said, that "this year we'll begin work on the great grandchild of those particle accelerators that have spent - uh, meant so much to our economic growth."(36)

But with no end in sight to the budget crunch on the eve of the Presidential election, sentiment was growing in Congress to continue withholding construction money to force the new administration to either develop new sources of revenue for the SSC or make convincing arguments for terminating existing programs in favor of the new machine. After numerous rounds of sometimes bitter debate in the various committees and on the House and Senate floors, the budget for fiscal year 1989 was closed once more with money to keep SSC R&D alive, but nothing for construction.

In November, two days after George Bush won the election, Waxahachie, Texas, a town just south of Dallas, was announced as the site for the SSC. Although, despite bitter protests from the losing states, it appeared that the site had been selected on the merits, Texas was certainly a happy choice for SSC proponents from a political perspective. At the news conference called to announce the site, Secretary of Energy Herrington was flanked by influential Texans, including Rep. Jim Wright, Speaker of
the House, Sen. Lloyd Bentsen, chairman of the Senate Finance Committee, and Sen. Phil Gramm, who, ironically enough, was coauthor of the Gramm-Rudman-Hollings deficit reduction law. (37) But as with the earlier narrowing of the list of site candidates, the base of political support for the machine seemed to collapse still further from the moment Herrington gave the word.

The New York Times, which less than two years before had urged the nation to "Pursue the Boson, and Beyond," now editorialized about "The Unaffordable Atom Smasher." "America has no coherent policy for supporting scientific research," wrote the Times. In February, 1987, Arno Penzias was in practice mistaken to fear the impact of the SSC on other branches of science because DOE was "a different, larger pool" of funding; now the department was perceived as "the principal source of support for many kinds of physics research," and the Times itself believed that "the risk of damage to other branches of physics is too high."

"The supercollider epitomizes the luxuries that America is currently in no position to afford," concluded the editorial. (38) The comments of the Times's editorial board seemed to reflect the sentiment of many in Congress. And to most observers, the situation continued to deteriorate both on the political front, with the fall of Jim Wright, and on the fiscal front, as the full cost of cleaning up the newly appreciated safety and pollution problems at DOE's nuclear
weapons plants became clear.

Texas moved quickly to strengthen its hand, assembling a motley collection of politicians, bureaucrats, industrialists, physicists and labor leaders into a coalition designed to broaden the base of support for the SSC. Promoters emphasized the extent to which SSC funding would be spread among industrial contractors and university groups across the country, while the state itself sweetened the pot with a proposed grant of $100 million in research funds to supplement DOE money. (39)

On Capitol Hill, the pattern of previous years appeared to be repeating itself when Rep. Tom Bevill of Alabama, chairman of the House Appropriations Committee's energy and water subcommittee, threatened to withhold SSC construction funds again until firm commitments of support from foreign participants were secured. (40) Democrats David R. Obey of Wisconsin, Dennis E. Eckart of Ohio and Howard Wolpe of Michigan, and Republican Sherwood Boehlert of New York mounted a sharp rhetorical attack on the project.

But President Bush intervened, personally lobbying lawmakers on behalf of the SSC. Bevill and ranking Republican John T. Myers of Indiana were called to meet with Bush, DOE Secretary James Watkins and OMB Director Richard Darman. Watkins told the congressmen that he was confident that foreign governments such as Japan, Italy and India would participate, but only if Congress acted first, making
its commitment clear. Bevill was swayed. The Texas House delegation went to work as well. Robert Roe of New Jersey, the powerful chairman of the House Science Committee, had been a strong supporter of the SSC, but now hinted that the proposed appropriation was in jeopardy and that he could not be counted on to save it - unless he got something in return. An amendment was arranged to take $25.3 million from nuclear waste disposal and put it into magnetic fusion, with most of the money going to the Princeton Plasma Physics Laboratory. Rep. Lynn Martin of Illinois, one of the disappointed finalists, might have taken a public stand against the SSC on the House floor. She was accommodated with an amendment to spend $600,000 on an Illinois flood-control project. In fact, the 1990 Energy and Water spending bill included more than $200 million for 40 new water projects not requested by the administration, but proposed by individual members of Congress. When Bevill, who had a powerful say over the fate of those projects, changed his position on the collider, it sent a powerful message. (41)

As late as a day before the vote at the end of June, 1989, it appeared that the Texans were in serious danger of losing. But the final victory was overwhelming when the last amendment to eliminate SSC construction funds was rejected by a vote of 331-92. In the face of the SSC’s long and difficult history in Congress, the margin stunned both
friends and foes of the project. In the aftermath of the
House vote, Senate opposition melted away, and by early
September a House-Senate conference committee had settled on
an appropriation of $225 million for the SSC, of which more
than half was allocated to construction.(42) With approval
by the President expected shortly, the Superconducting Super
Collider was finally, officially underway.

But as the high-energy physics community prepared at
long last to break ground for the great accelerator, its
outlook for the future remained clouded by problems and
uncertainties. The high-energy physics budget of the
National Science Foundation, though a small fraction of the
total national program, played a critical role in the
support of university faculty and the training of graduate
students in the field. But NSF support of particle physics
had been declining in real terms since 1976, and in an
appearance before the American Physical Society in January,
1988, NSF director Erich Bloch suggested that the agency
might have to pull out of the field altogether. A report
delivered to HEPAP in February, 1989, criticized a
leadership vacuum in the foundation's program and warned of
a coming manpower crisis.(43) In the wake of the June House
action slashing the Administration's requested budget
increase for NSF even as it approved SSC construction,
Robert L. Park, director of the Washington office of the
American Physical Society, was left to wonder, "Are there

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going to be any high-energy physicists left in nine years to run this thing?" (44)

The potential impact of SSC construction on already tight operating funds for the existing laboratories fueled a general anxiety and left tempers frayed, especially at SLAC, which had been having serious problems of its own. While the first observation of the Z particle had been made at CERN in 1983, a detailed analysis of the particle's properties and tests of its full implications for particle theory had to await the construction of a "Z factory" which could produce copious quantities of the particle. The ambitious Large Electron Positron collider, under construction as CERN's next big project, was to be a gold-plated Z factory, producing a large, high-quality flux of Z particles for four huge detectors run by competing, high-powered teams of physicists. The Stanford Linear Collider had been approved and funded as a relatively cheap way of beating the Europeans by more than a year to the cream of the anticipated Z particle physics. Technically, the SLC was a kluge, an ad hoc contraption tacked on to the end of SLAC's existing linear accelerator in an attempt to get the job done in a hurry at minimum cost. But the SLC ran into a host of technical and management problems, many stemming from the attempt to do a difficult job in a hurry and on the cheap. Its scheduled startup in 1988, heralded by exuberant coverage in the national press, was a failure. After weeks
of intermittent operation marked by continual glitches, and without having produced a single Z, the SLC was shut down for an emergency overhaul and its project director was fired. By mid-1989, when the SLC was finally producing a trickle of Z's, CERN's LEP was about to come on line after a relatively smooth R&D and construction path, promising to swamp the SLC from the start with an expected production of thousands of Z particles per day. To make matters worse, the SLC was intended to demonstrate the feasibility of very large linear electron-positron colliders, a technology potentially applicable to a new generation of huge accelerators after the SSC, and a special strength of the Americans. But, as SLAC director Burton Richter pointed out at the height of the SLC troubles,

We are the experts in the whole world on linear colliders, and if we can't make one work, why should Congress believe that we can make a bigger one work? To say that we've proved the principle and that it's only technical problems plaguing us - that's a hard story to sell. It's true, but it's a hard sell.

It was indeed, for the ghost of Isabelle continued to haunt the Congress. (45)

And the ghost of Isabelle seemed to hover over the SSC itself. Shortly after DOE officials assured Congressional committees that rumors of problems with the SSC's superconducting magnets were overblown, and that all that remained in the magnet work was "fine tuning," an internal review, submitted on June 1, concluded that there were
indeed major problems in magnet development. SSC director Roy Schwitters found himself contemplating the possibility of a major redesign of the magnet systems - and an inevitable round of hearings before Congressional opponents sure to follow up on the report. By mid-November, projections of magnet-related cost overruns in the face of a tight ceiling clamped on the total cost of the project raised renewed doubts about the future of the SSC.(46)

As the sixtieth anniversary of Ernest Lawrence's first cyclotron approached, the future of high-energy physics remained uncertain. The one thing that was clear was that in looking to its survival the greatest strength of high-energy physics, as well as its greatest weakness, consisted in the fact that from a political perspective the race toward ever-larger accelerators was hardly a matter of physics at all - if, indeed, it had ever been.
Notes


3. Quoted in "SSC Debate..."


14. Some observers believe that European resentment over US-imposed restrictions on the export of high-technology goods to eastern-bloc nations and over clumsy attempts by the Americans to control the diffusion of information on new discoveries in superconductivity on grounds of national security would have scuttled any possibility of European involvement in any case.


22. Keyworth, "Science Advice During the Reagan Years."

23. Portions quoted in Goodwin, "Reagan Endorses the SSC..." and Franklin, "Reagan to Press for $6 Billion Atom Smasher."


28. Superconducting Super Collider, Hearings before the House Science, Space, and Technology Committee, p. 204.


[Rep.] Mr. RUDD. Mr. Wilson, I'm not a physicist, but what you were saying here earlier interests me a little bit, in that you are talking about neutrons and quarks being parts of particles that fell away from - I assume - fall away from protons and neutrons. There must be other particles, too, since you use it in the plural sense. This is all very nebulous, and I know it must be extremely interesting to physicists, but will this project eventually - or do you have hopes that eventually it will provide an energy propellant in any way at all for our future?

Dr. WILSON. I think it would be false of me to say that I could anticipate in any way that that can happen.

Mr. RUDD. I'm speaking of hope rather than knowledge.

Dr. WILSON. We always have hope. And I've never - as an old-timer in this business - I've never seen a time when this kind of knowledge didn't result in something of that kind. When we first started to study the neutron and proton, we had no idea that nuclear energy would result.

Mr. RUDD. I guess what I'm basically talking about - you don't seem to have any goals in mind, any perfect goals in mind, or even imperfect goals in mind. And are we -

Dr. WILSON. The knowledge itself.

Mr. RUDD. Just running experiments with the vague hope that we're going to achieve things that will be beneficial in the form of energy and energy propellants?

Dr. WILSON. I think in the first place the idea of an understandable world is something that's been pursued for thousands of years, and we - it's always been beneficial. There has been an increase in the stature of man that results from that kind of knowledge. And it has always turned out that there have been practical benefits. It's very hard to see the nature of those practical benefits when you start.
Mr. RUDD. Maybe I’m confused by the fact that it seems like a purely scientific endeavor, rather than – perhaps it should not be with ERDA. I don’t know.

Dr. WILSON. Yes. It is a purely scientific endeavor. That is correct.

Mr. RUDD. OK.

...

Mr. RUDD. Would you feel more comfortable under the National Science Foundation than where you are?

Dr. WILSON. There are many good arguments why we should be under the National Science Foundation, because of the pure motivation, the scientific motivation, of the physics. There are very good arguments for the research to be with ERDA, because the things that we are studying are fundamental to the production of energy, and because of the expensive nature of the projects.

Mr. RUDD. Thank you, sir.(1)

High-energy physics is among the purest of the pure sciences. It is far removed from application; it is subtle, abstract, highly refined and difficult in both its experimental and its theoretical aspects. Within a scientific culture which honors pure over applied science, generality and fundamentality over specificity, analysis over synthesis, and rigorous mathematics over messy, fallible heuristics, particle physics is an aristocrat.

But what counts for prestige in the race for Nobel prizes may serve as a handicap in the pursuit of tax dollars. In an egalitarian, democratic political culture,
the claim that government has a responsibility to serve as patron of a privileged elite devoted to essentially private intellectual pursuits is untenable. Public support depends on the ability of the elite to establish some means of contact with broadly shared values. (2) "When the public is asked to support science, it is, from its own scientifically illiterate perspective, being asked to support the production of incomprehensible intangibles," observes Daniel Greenberg. (3) As perhaps the most incomprehensible, intangible science of all, high-energy physics faces this problem in its most acute form.

The attempted solution to this problem can be seen in the rationales which have been invoked to justify support for the field. In this chapter, in order to better understand the nature of Presidential and Congressional decisions about support for high-energy physics, I shall abstract from the history of the debate the rationales which have been cited on behalf of high-energy physics. In doing so I will emphasize the appearance of these rationales in Congressional hearings, which represent one of two main foci of the decision-making process. The other, deliberations within the Administration itself, is unfortunately not so well represented on the record, although I have attempted to include such evidence where possible in the preceding chapters. I will, however, include some examples taken from other sources, both for reasons of rhetorical clarity and

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because, in the end, the battle of the scientist in seeking support for his field is never restricted to formal Washington channels alone, but in an important way goes on continually both behind the scenes and in the open arena of public opinion.

In order to clarify the significance of the various rationales and the roles that they have played over the years, I shall classify them into four categories. The first, which I call primary rationales, includes those compelling national purposes to which high-energy physics has been directly linked. The category of secondary, or supporting, rationales includes arguments which promote high-energy physics as a means of achieving the national purposes cited in the first category. The tertiary rationales are those which propose intangible values as justification for the pursuit of high-energy physics; these represent both expressions of the personal values of its practitioners and attempts to project such values as national purposes. Finally, recognizing the extent of the ambiguity and overlap among different rationales advanced in the heat of argument, I include a fourth category, which I call secondary/tertiary for lack of a more felicitous term to represent fractional dimension. As we shall see, the ambiguity in some of these rationales represents a conscious effort to, in effect, "have it both ways."
I. PRIMARY (NATIONAL PURPOSE) RATIONALES

BUILD A BETTER BOMB, OR, NATIONAL SECURITY IS AT STAKE

The weapons whose production now concerns us have developed from discoveries made in the realm of abstract science in 1939 - just 10 years ago. I do not believe that anyone 11 or 12 years ago could possibly have foreseen this development. Similarly, we cannot foresee what may happen in the next decade, but we can start from certain obvious facts.

The weapons which we are producing involve principles of nuclear physics. This is a subject on which our knowledge is still extremely fragmentary.... It is hardly conceivable that further study of nuclear physics... will not produce information that would be of fundamental value in the design and manufacture of future weapons.

AEC Commissioner Henry D. Smyth to Sen. Brien McMahon, 1949(4)

May I respectfully request the committee to consider where this country would be with the present thermonuclear-weapon program if there had not been available a few years back the fundamental knowledge of nuclear theory, the nuclear constants of the isotopes, and all the other data needed to cope with this extraordinarily difficult problem. Without that accumulated knowledge it is inconceivable that we could have planned and executed the tests and other accomplishments in this effort in the 3 years since the Presidential directive to accelerate development. This is of course a spectacular example of research's contribution. Others can be mentioned which will also be significant in future years.

Among these may be the progress that has been made in the design and operation of high-energy particle accelerators. The higher particle energies now foreseeable - perhaps as high as 100 billion electron volts - are opening extremely important new avenues to our understanding of the
nature of forces within the nucleus.

AEC Chairman Gordon Dean before the
House Appropriations Committee, 1953(5)

The reason for our great interest in high-
energy physics is that it is unique in concerning
itself with the most fundamental laws governing
the constitution of matter and the elementary
particles of which matter is constructed.... Our
ability in the past to exploit these fundamental
laws has contributed extensively to national
security and economic growth.

Presidential Science Adviser Donald
Hornig to Rep. John Pastore, 1964(6)

If we do not proceed to higher energy, we
will eliminate the frontier and kill the field or
badly cripple it....

In today's world, when scientific discoveries
are sometimes quickly translated into technology,
this could have tragic consequences for the United
States.

Dr. Edwin McMillan before the JCAE, 1965(7)

Defense of the nation from attack by foreign enemies is
one of the core functions of any government; the United
States is no exception. From the earliest days of the
Republic, the national defense has been a compelling
rationale, and until relatively recently, perhaps the most
compelling rationale for the expenditure of tax dollars.
But it was not until the Second World War that expenditures
on science in particular were perceived as not only relevant
but essential to an effective defense.

The national security issue was a mainstay in the
justification of high-energy physics in the early years
after World War II, although it can be difficult to track through Congressional hearings because discussions with the slightest hint of national security implications were commonly declared off the record during that period. With a boost from the launch of Sputnik, it remained at the top of the agenda through the end of the Eisenhower administration and into the Kennedy years.

By the mid-1960's, however, explicit national security claims were weaker and were expressed in more general terms. Although in succeeding years, curious Congressmen would continue to inquire from time to time about the contributions of high-energy physics to national security, the issue was always laid to rest quickly.

Today national security has apparently disappeared as a significant consideration in public debate about high-energy physics, except as a distant echo in discussions of amorphous concepts of "national strength" and "national prestige." But the effectiveness of the informal, behind-the-scenes network through which support and influence are communicated among high-energy physicists, their colleagues in the defense establishment, and members of Congress and Administration officials seeking advice persists, though the budgetary demands of the SSC are testing it as never before.

HIGH-ENERGY PHYSICS CONtributes TO ECONOMIC COMPETITIVENESS

There is a report, which I would like to have
introduced into the record, which is entitled "Economic Utility Resulting from CERN Contracts."...

This report illustrates a very interesting phenomenon, because in many cases people say, "Well, the benefits from research are 30, 40 years down the road," and I think this report clearly indicates that that's not the case. A study that they did talked about the amount of money that the European governments spent through CERN with European companies. They spent about 748 million Swiss francs over a period of about ten years in contracts with European companies who supplied goods and services to CERN. Those companies, then, were interviewed, some 160 of them, and they were asked what were the benefits to them and new business they booked over and above that which they did with CERN. Well it was about 3000 million Swiss francs; in other words, something like a return of 4 to 1 within ten years by virtue of activities that were done through CERN with European companies.

Now I don't want to make this a competition with Europe, but it is some of the products that were generated by virtue of that that is causing U.S. companies to be concerned about the lack of markets elsewhere. So in other words, the direct within-a-few-years benefits of having work done through CERN with European companies were very, very clear.

Dr. Alvin Trivelpiece before the House Science, Space and Technology Committee, 1987(8)

The advances in basic knowledge will contribute to the ability of future generations to compete economically and technologically through applications of SSC discoveries.

Rep. George Brown before the House Science, Space and Technology Committee, 1987(9)

... If the SSC is built and our scientists are retained, it will do more for U.S. competitiveness than most of the trade agreements devised by our most capable experts....
Rep. Ron Packard before the House Science, Space and Technology Committee, 1987(10)

We can use the SSC to create a Silicon Valley, a Route 28 [sic] - areas that contain the densest concentration of high-quality, fundamental research in this country are those parts of the country that are growing and prospering. And if there is anything that will concentrate knowledge, it is the SSC.

Rep. Terry Bruce before the House Science, Space and Technology Committee, 1987(11)

Construction of the Super Collider is important to maintaining American competitiveness in the increasingly challenging world economy. It would contribute significantly to America's scientific and technological leadership and give another clear sign that America is committed to keeping this nation on the cutting edge of world leadership and competitiveness.

John Herrington before National SSC Symposium, 1987(12)

"Economic competitiveness" is the 1980's successor to national security as the compelling national purpose linked to high-energy physics. A measure of the influence and appeal of the competitiveness issue is that the strongest opponents of the Superconducting Super Collider have also phrased their arguments in terms of the effect of the project on competitiveness.

Just as national security is an issue with not only national but international implications, with the security issue dominated in its heyday by the threat posed by the Soviet Union, the problem of economic competitiveness is perceived as one of vulnerability to more aggressive, better
managed foreign competitors. Within the context of high-energy physics, the Europeans, rather than the Russians, are today's primary competition. As an economic threat, however, the Europeans, while viewed with a new respect in light of the forthcoming economic unification of Western Europe, rank second behind the Japanese, who even as junior partners in the pursuit of high-energy physics are feared for their assumed ability to extract the maximum advantage from any exposure to advanced science and technology.

In referring to the widely-cited CERN "Economic Utility" study, DOE's Trivelpiece appeared to be making two claims - that investment in high-energy physics has a multiplier effect on the economy, and that it promotes competitiveness in high technology. It is worth noting that a later Congressional Budget Office review found the key assumptions on which the CERN study was built seriously flawed, and the results both overstated and not directly applicable to the United States.(13)

In the SSC debate, economic competitiveness arguments have often been stated in a way which blurs the distinction between national competitiveness and regional competitiveness, and between the presumed special contribution of the SSC to economic growth and the generic spread-the-wealth properties of any large public works project. However, it is the former argument which carries special weight under the present circumstances.
Ia. PRIMARY (NATIONAL PURPOSE) RATIONALE MANQUÉ

IT’S ESSENTIAL TO THE ENERGY PROGRAM

The [ERDA] physical research program supports about 90 percent of the Nation’s basic research in high energy physics, which is concerned with subnuclear phenomena, the fundamental nature and relationships of the four basic forces of nature and the transformations of matter and energy from one form to another. Since all forms of energy are ultimately based on the four fundamental forces of nature, if any new source of energy is to be discovered, it probably will be based on one or more of these forces.

High energy physics research is widely believed to be on the frontier of importance to the long-term progress of science and continues to provide technological spinoffs important to future energy technology [for example, superconducting applications, instrumentation, on-line data processing].

Dr. John Teem before the JCAE, 1975(14)

The "energy crisis" of the 1970’s thrust energy research into a role analogous in some ways to that of atomic energy research in the 1950’s, and ERDA bureaucrats responded accordingly. For better or for worse, however, there was no equivalent of the 1950’s Cold War to keep fueling the perception of a crisis, and while the nation’s energy difficulties have not disappeared, the hysteria has long since evaporated. While the energy crisis resulted in a substantial infusion of funds to ERDA and then DOE, thus indirectly shielding the high-energy physics program from
the most critical scrutiny, it never quite made it as a primary rationale for support of the field.

II. SECONDARY (SUPPORTING) RATIONALES

KEEP UP WITH THE RUSSIANS

Commissioner LIBBY. This is to hold forth and furnish facilities for experiments in this field during the time the big machine — well, frankly, Mr. Cole, to get down to brass tacks, the thing we saw in Geneva, which the Russians showed us, is exactly in the energy range in which we think this is going to operate. And when their machine starts the Russians are going to take over the show for several years....

On this accelerator the particular aim is to get the lead back from the Russians as quickly as we can. It isn’t only a matter of competition with the Russians. We know that the new and important things in nuclear physics lie in this range. We know that and we want to get those discoveries as soon as possible and draw on the skills and techniques involved in making those discoveries. At the present time we have only 2 machines, the 1 at Berkeley and the 3 billion electron volt machine at Brookhaven. These, of course, are leading the whole world at the moment and we have a real corner on things, but we won’t have it very long.

[Rep.] Mr. VAN ZANDT. What is the span of time there that the Russians will enjoy leadership?

Commissioner LIBBY. The best we can do, I think, is to cut it down to 2 or 3 years.

Mr. VAN ZANDT. 2 or 3 years.

Commissioner LIBBY. Whatever we do, and it depends on the breaks, even then. I am afraid they have got the jump on us a little bit. I am afraid that is it. They were very quiet about that machine. The first I ever heard of it was at the Geneva Conference.
Why would this be a national tragedy, to sit on our haunches?

First, competent observers believe that the United States is now behind — I stop — behind the Soviet Union in high-speed [sic] physics experiments. That is dangerous business, Mr. Chairman.

Second, the outcome of our race with the Soviet Union for research in high-speed physics may well affect the future capacity of the United States to be supreme in nuclear warfare or defense....

Gentlemen of this committee, you have heard it many times before, but it is nevertheless true, time is of the essence. I remember back, and I have mentioned it many times, in 1941 when the Army and Navy and Air Force and America were asleep. Pearl Harbor came upon us like a thief in the night. Had we been alert and awake, we could have saved a hundred billion dollars in wealth, tens of thousands of lives, and 2 years of war.

Instead of that, we sat back complacent. In the opinion of men in the Senate and in the House and elsewhere, we are in a far more serious situation now than we were back in 1941.

Sen. Alexander Wiley before the JCAE, 1958(16)

[Rep.] Mr. LANIGAN. I have a question on the statement you made about the equipment being made available to the scientists in Russia. You mention such equipment as high-energy accelerators. Can you give us a little more information on the quality and quantity of equipment that is being made available to the scientists in Russia?

[NSF Director] Dr. WATERMAN. In the first place they make it all themselves. They will occasionally copy a foreign instrument, if it is just what they want. But in the construction of these big machines they do all the work themselves and, our scientists who have been there say it is
very high quality work in detail and every other way. It is not daring - rather conservative but very soundly constructed and complete. They are given every incentive to get on with this; there is no delay; if they want something they get it, provided it is on a high-priority job.

Mr. LANIGAN. Well how do Russia and this country compare in the development of high-energy accelerators and in the plans for the immediate future?

Dr. WATERMAN. That story is told very simply; the facts speak for themselves. We have at the University of California at Berkeley, a nuclear accelerator, the largest we have, which produces 6 billion electronic [sic] volt protons, and that is in good operation. The Russians have one which gives 10 billion, and that is in operation. In Brookhaven National Laboratories, under AEC contract to them, there is a machine under construction which is aimed at producing 25 billion electron volts. The Russians have one aimed at 50 billion under construction. So they are a step ahead of us all the way.

...

Mr. LANIGAN. You think this would indicate they are ahead in taking action to study nuclear physics?

Dr. WATERMAN. Yes.

Mr. LANIGAN. Ahead of this country at this time?

Dr. WATERMAN. As I will refer to a little later, they have a quicker system of making decisions than we do, and that is of great help. But at the present time they have not got the background, frankly, in this accelerator game as thoroughly as we. So from reports we get, while their work is very good and they have an extremely competent number of young people, thoroughly trained and thoroughly knowledgeable about the field, their output of research to date has not shown the significance that ours has and they seem to be having more trouble with their big machines than we are. For example, from what we hear the 10 billion electron volt machine is not producing as well as our 6; there is greater difficulty in its performing well enough to take good observations.
at the present time. Things like that where experience counts. But they will overcome that very soon because they have a very capable group.

Hearings before the House Government Operations Committee, 1958(17)

I know that the Soviet Union is installing a very high energy accelerator that perhaps should not go without competition for long.

Rep. Craig Hosmer, JCAE Hearing, 1964(18)

The "keep up with the Russians" argument was secondary to the national security rationale. In the early postwar years, with high-energy physics widely understood as a potential contributor to weapons development and the Soviet Union recognized as the obvious enemy, little or no attention was paid to the question of just where the Russians actually stood in particle physics. This changed in the mid-1950's with the first substantive contacts between American and Soviet physicists. For several years thereafter, with an added impetus provided by Sputnik, Congress never failed to inquire as to the position of the United States in the presumed accelerator race with the Russians.

However, those who would use the Red Scare as a tactic for gaining support had to deal with the inconvenient fact that all of the hard evidence, little as it was, that seeped out of the Soviet Union suggested that the Soviet accelerator program always seemed to be in trouble. Their nominally powerful machines either never materialized at all
or, if they were completed, never managed to turn out any physics worth mentioning. By the mid-1960's this had become clear to all, and the Russian threat in high-energy physics was essentially finished as a credible excuse for spending American money. To this day, the Soviet particle physics program remains an also ran. (19)

As an interesting and amusing sidelight, it is worth noting that the Congress was not necessarily entirely oblivious to the nature of the game being played by some high-energy physics advocates:

Representative PRICE. Doctor, you may be interested in the story about how the Russians look upon the capacity of these accelerators. One country gets one of 6 billion and the next one gets 10 billion. Then the other jumps a couple of more billion. The Dubna Laboratory asked our group when we were there 2 years ago how we got the money to build our accelerators. We told him the legislative process of getting money on our program. He said, "that is not the way I understand." He said, "I understand you get it by saying the Russians have a 10 billion electron volt synchrotron and we need a 20 billion electron [volt] synchrotron and that is how you get your money." I said, "there may be something to it." I said, "How do you get your money?" He said, "The same way."

Dr. WILLIAMS [of AEC]. That is certainly a very true story....

JCAE Hearings on SLAC, 1959(20)

KEEP UP WITH THE EUROPEANS

[Rep.] Mr. COTTON. ...Is it not a fact that this particular kind of basic research constitutes the real frontier in your particular field which might
lead to the development of new weapons?

Dr. SMYTH. That is correct.

Mr. COTTON. And is it or is it not a fact that other nations abroad are perhaps already in this field?

Dr. SMYTH. That is correct.

Mr. COTTON. Either on or off the record, do you know that to be true — whether there are or not — and, if so, to what extent?

Dr. SMYTH. I know it is true in the cosmic-ray field. Many of the basic discoveries in the cosmic-ray field since the war have been made in European laboratories. In the field of reproducing these same events artificially, we at present hold the advantage because of the cosmotron at Brookhaven and the bevatron which is coming along at Berkeley. There is now in process of organization a West European laboratory, and the scientific people in that laboratory plan to build a machine that will be more powerful than anything we now have.

House Appropriations Committee Hearing, 1953(21)

During a recent visit of Monsieur François Ortoli, French Minister of Industry and Science, we learned that the French have now decided to join with other European nations in financing the construction of a 300 billion electron volt accelerator for studies in high energy physics. In view of the fact that the European countries face difficult financial problems, this decision clearly re-enforces their conviction that high energy physics is a field of utmost importance to the future of science and technology. It is doubly important that the U.S. maintain its leadership in this field by proceeding without delay with the AEC’s Batavia accelerator. This can be in operation several years before the European machine can be built and with proper support the U.S. machine can equal or even exceed the 300 BEV goal set by the Europeans. I do hope that your Committee will give full support to the Administration’s program for moving ahead in this important area.

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Science Adviser Lee A. DuBridge to JCAE
Chairman Chet Holifield, 1969(22)

... I recently saw a graph showing the U.S.
investment in high-energy physics. This was in
absolute dollars, comparing the European community
to the United States. It showed the European
community went up in the mid-sixties and it has
leveled off. The American funding went up about
the same time and to the same \text{\&} int and it has
been dropping off ever since.

...

The European program is running at about $400
million flat across. What this means is that by
not holding up the funding levels of our programs
today, we are clearly putting the United States in
a disadvantageous position with respect to basic
research for the 1970's and 1980's.

Rep. Mike McCormack, House Science and
Technology Committee Hearing, 1977(23)

History records that Europe led the world in
science and learning and also in technology for
almost 500 years, from the time of the Renaissance
and the birth of modern science with Copernicus.
Europe's baton of leadership passed to the United
States as a result of two devastating world wars
and the flight to our shores of many leading
scholars escaping from Nazi persecution. It
saddens me greatly to see this Nation giving that
baton back without cause or reason after
possessing it a mere 50 years. Yet, this is the
inevitable consequence of a continued decrease in
our investment in basic research and learning.

Dr. Sidney Drell before the House Science
and Technology Committee, 1982(24)

For obvious reasons, the "keep up with the Europeans"
argument has never had the national security implications of
the corresponding view of the Russians. In recent years,
however, as economic competitiveness has become a major
concern, competition with the Europeans has taken on a significance beyond mere international sportsmanship.

The perceived significance of this justification has varied inversely with that of the Russian argument. While Congressional hearings from the mid-1950's onward show continuing traces of interest in and a sort of generic, polite respect for European progress in high-energy physics, it was not until the 1970's, when the Europeans were for the first time perceived as truly competitive in the quality of their physics, that this justification took on considerable weight.(25) Today's accelerator races are between the Americans and the Europeans, not the Americans and the Russians.

WE'RE NOT ASKING FOR SPECIAL TREATMENT - ALL FIELDS OF BASIC SCIENCE SHOULD GET MORE MONEY

Concern has been expressed that increased financial support for high energy physics may have an adverse effect on the support of other areas of science. In our judgment it should not.

It is not possible to assign relative priorities to various fields of basic science nor should they be placed in competition. Each science, at any given time, faces a set of critical problems that require solutions for continued growth. Sometimes these solutions can be acquired at little cost; sometimes large expenditures of funds are needed. Hence, the cost may not reflect the relative value but rather the need.

Each area must be funded according to these needs.
Piore panel report, 1958(26)

It seems to me that asking a scientist to make a choice of whether to have high-energy physics or to cut off something else is almost like telling a man he is getting too fat - will have to lose some weight - and asking whether he would choose to cut off the right or the left leg. This really does not quite seem to be the solution. In fact, what one wants to do is maintain a healthy development of each field, and to arrange for efficient use of the manpower interested in each. Furthermore, I believe the Nation can arrange for and will benefit from such support.

Dr. Charles Townes before the JCAE, 1965(27)

I believe that we should use SSC in conjunction with other proposers of expensive facilities in a unified, pro-science campaign. "SCIENCE, the sinew of society, should not cringe! If this nation is ever to overcome its economic problems, it will be through a greater investment in science, etc., etc."

Dr. Leon M. Lederman to Dr. Edward Knapp, 1985(28)

This argument first appeared with regularity in the early 1960's when the growth of the overall science budget was seriously threatened for the first time in the postwar era, and saw further use as a counter to accusations that high-energy physics, because of its disproportionate share of allocations, threatened progress in other branches of science. Such accusations became particularly sharp during the SSC debates.

During the years after national security had faded as a public rationale for high-energy physics and before economic (29)
competitiveness had become an acute concern, the concept of basic science as an essential part of the nation's infrastructure, with high-energy physics as an indispensable part of a "balanced" basic science program, directly carried much of the weight in justifying funding for high-energy physics.

IT'S ESSENTIAL TO THE ATOMIC ENERGY PROGRAM

Representative HOLIFIELD. To what extent is the Commission to continue in this program of building accelerators? Are we going to keep on building them, paying for larger ones?

Mr. [Dr. Willard] LIBBY. As of the moment, I would say the general situation is about as follows:

We have for the first time some opportunity to look into the intricacies of the nuclear forces from which atomic energy is derived. The only way you can get at these things is by bombarding with these tremendous energies and these machines are required to attain them.

JCAE Hearing, 1955(29)

The Commission's physical research program includes investigations in the fields of physics and mathematics, chemistry, metallurgy and materials, and controlled thermonuclear research. Such investigations are undertaken for the purpose of discovering natural laws relevant to the Commission's responsibilities for the development, use, and control of nuclear energy. The atomic energy program of today owes its very existence to a few basic scientific discoveries and theories which can be traced to investigations similar to those conducted under the physical research program....

AEC Annual Report, 1961(30)
... There may be unforeseeable, very practical applications connected with the further harnessing of the forces of the nucleus.

Dr. Glenn Seaborg before the JCAE, 1964(31)

The Department’s High Energy and Nuclear Physics programs constitute nearly 90 percent of all such research conducted in the United States. These programs, which are carried out as a national trust, differ from other departmental basic research in that the research itself and the knowledge it produces are the goals....

DOE Annual Report, 1981(32)

During the immediate postwar years, atomic energy was seen as an essential means in the pursuit of the end of national security, and as a branch of technology which showed great promise of future contributions to economic growth.

Both nuclear weapons and nuclear power remain important; what has changed is on the one hand, public perceptions of their development as a force for good, and on the other, the widespread belief in an intimate connection between state-of-the-art high-energy physics and such development.

During the 1950’s, high-energy physics was widely understood to be part of nuclear physics. In fact, the terms were frequently used interchangeably. Both fields were still sufficiently close historically to their origin in a unified stream of research that laypeople in general and Congressmen in particular were in no position to argue
subtle points of when the methods and goals of high-energy physics had made it a distinct field, no longer especially relevant to problems of applied science and technology encountered in the development of atomic energy. Many, perhaps most, were unaware that this was even an issue.

The Atomic Energy Commission itself took every side of the issue at one time or another, portraying particle physics as an essential part of the development of atomic energy, as a branch of fundamental science which at least in principle underlay all of the AEC's activities, or as an intellectual venture which just happened to fall into the Commission's bureaucratic lap. In the 1960's, when high-energy physics was more clearly identified as a separate field, and in any case the special aura of atomic energy had begun to fade, the ostensible link with atomic energy essentially disappeared.

By the time that the AEC gave way to its successors, ERDA and ultimately DOE, the official line was that the support of research in high-energy physics was a "national trust," a convenient designation which carried with it the implication that something worthy and valuable had been entrusted to that branch of the government for safekeeping.

HIGH-ENERGY PHYSICS CONTRIBUTES TO NATIONAL PRESTIGE

I think one of the most important things is that we have established a program in this country that is the envy of the world. I believe the
Soviets envy us. I believe the Europeans envy us. I believe all of the world feel we have leadership in this field. From that standpoint the high energy physics program does contribute to national security.

AEC Director of Research Paul W. McDaniel before the JCAE, 1964(33)

Closing the horizon would affect American prestige. Particle physics is a field in which the United States has had clear world leadership. For 35 years this country has attracted some of the world's best scientists through its strength in the search for the understanding of matter, and this immigration has greatly strengthened American science and education.

Dr. Edwin McMillan before the JCAE, 1965(34)

The "prestige" argument is frequently implicit in other rationales, and is naturally implicit in citations of Nobel Prize counts. However, in the context of high-energy physics it must be considered a secondary justification, for it has rarely turned up explicitly without being linked to some other benefit presumed to be a consequence of prestige.

IF WE DON'T SUPPORT HIGH-ENERGY PHYSICS WE'LL LOSE OUR BEST SCIENTISTS

Europe presently has under study a machine of 300 billion electron volts. Russia is building an accelerator of 70 million [sic, billion] electron volts. In view of these projects American leadership could not be expected to continue. We might expect an exodus of some of our best brains to Europe.

Dr. Edwin McMillan before the JCAE, 1965(35)
We must act now to prevent the brain drain from Europe and Japan. If Congress fails to fund this machine, we may as well begin preparing a couple of thousand visas for our most outstanding physicists.

Rep. Ron Packard before the House Science, Space and Technology Committee, 1987(36)

In principle, the "brain drain" issue is both real and serious. The United States is widely recognized to have benefited immensely from the exodus of scientists from Europe in response to the rise of Nazism and Fascism in the years preceding the second World War. More recently, many developing nations have lost promising talent to advanced Western countries because of the lack of opportunity at home, and limitations on scientific research stemming from economic conditions have had an impact even in such technologically advanced nations as Great Britain.

On the other hand, the reality of particle physics today is more complex than most "brain drain" arguments imply. Because of the limited number of leading-edge research facilities, high-energy physicists commonly travel freely between their institutional bases and the few sites where experiments can actually be run. Today, this implies a heavy traffic across the Atlantic in both directions. For example, physicist Carlo Rubbia, renowned for his Nobel Prize-winning discoveries of the W and Z particles at CERN in 1983, has collaborated extensively on experiments in the United States as well, and serves as a professor of physics.
at Harvard. Samuel Ting, a professor at MIT, won his Nobel Prize for work carried out at Brookhaven, but has also been for many years a leader of key experimental teams at CERN.

The "most outstanding" particle physicists, and many others as well, already have their visas. "Brain drain" arguments have routinely failed to spell out exactly how a change in the commuting patterns of high-energy physicists would affect national security, economic competitiveness or other values.

HIGH-ENERGY PHYSICS MAKES A MAJOR CONTRIBUTION TO EDUCATION AND TRAINING IN SCIENCE AND TECHNOLOGY

... High energy physics contributes to the technological strength of the country... Its challenging technical problems have engaged a group of most inventive and resourceful scientists, on a frontier where technology must be pushed to its limits. They are a reservoir of inventive energy and broadly based scientific and engineering skill from which leadership can be drawn for other scientific enterprises. It must be recognized that high energy physics is a unique training ground for some of our most creative people.

Ramsey panel report, 1963(37)

... Research in these fields of maximum intellectual effort and scope - involving constant reevaluation of basic assumptions about the structure of matter on both the submicroscopic and cosmic scales - is by far the best training for that bold use of ideas and materials which is one of our greatest sources of national strength.

AEC high-energy physics policy statement, 1965(38)
How can physics in general, and particle physics in particular, contribute to the indirect benefits of natural science to society? The influence will need to be on the one hand symbolic, mainly by setting a style for meeting new problems, and on the other, practical, by producing innovative people who are trained in that style....

In this context, one can ask whether elementary-particle physics has anything special to offer.... Elementary-particle physics provides a unique combination of high-flying abstract imagination with vigorous and exciting critical standards of validity. It also exemplifies the physicist's strong faith in the ultimate intelligibility of any situation and a belief in simple and elegant models. The challenge of the exploratory character of particle physics has traditionally served to develop the maverick mind, which can be highly useful in meeting other innovative needs. Furthermore, his exploration into the domain of the unknown frequently leads the particle physicist to false tentative conclusions, and he is brought face to face with his own fallibility often enough to be quite aware of it. Skepticism toward the insolubility of tough technical problems is another requirement for the successful experimentalist in this field...

NRC Physics Survey Committee, 1972(39)

The CHAIRMAN [Rep. Robert Roe]. Is it fair commentary when the political buzz words around this Capitol and around this country is "competitiveness," and our confrontations that are taking place with some of our foreign partners, our international partners, in other areas, coming back and saying, what is going to lead America into being more competitive? What is going to lead America into being up front, getting out in front?

Are we talking about the investment and setting priorities that we should be setting?

Dr. TRIVELPIECE. I think one of the things you touched on, and I touched on as my last remark, that is we need to train the next generation of talent in order to be competitive. In order to do
that, we need to find things that inspire and attract them into fields of science. I think things like the SSC constitute the attraction coefficient that persuades many young people to take up careers in science that might not otherwise do so. They may not end up pursuing a career in high energy physics or theoretical aspects thereof, but they end up being captured into a career in mathematics, physics, chemistry, whatever. I think that is an important ingredient here and not to be overlooked.

I am sure when Dr. Lederman testifies later, he would be willing to tell you about the numbers of high school students that visit Fermilab and get exposed to science education and whether or not they continue on.

I think we need to do these things across the country and find these means and reasons to inspire our young people to pursue careers in science. That will make us competitive, I believe.

House Science, Space and Technology Committee Hearing, 1987(40)

The "education and training" argument has always been popular as a component of the more general infrastructure argument, but has gained special weight with the rise of concern about competitiveness.

Quite a few cases of individuals who began their training in particle physics and went on to make exceptional contributions in other fields can be and frequently are cited anecdotally to bolster this justification. In the aggregate, the number of people trained in high-energy physics is minuscule, and of this number only some fraction disperse into other fields. (41) The profession does, of course, represent a reservoir of unusually talented people
who may be available for service in case of dire national emergency. Unlike during the immediate postwar era, however, scientific and engineering talent is now fairly widely dispersed across a variety of fields and institutions in both the public and private sectors.

**HIGH-ENERGY PHYSICS GENERATES VALUABLE TECHNOLOGY SPINOFFS**

For many years, one of the basic and most fruitful scientific studies has been the study of the nature of matter and of the particles of which matter consists. From these studies there have been immense practical applications whose value vastly exceeds the cost of the original research in the field.

Norman Ramsey before the JCAE, 1959(42)

There have been some particularly direct contributions of high-energy research to the national strength. These are the developments in particle and photon production and detection techniques which are necessary for both military and civilian nuclear technology, for the study of the upper atmosphere and of space, and for use in biology and medicine. Some examples of such developments have been Cerenkov counters, spark chambers, improved vacuum pumps, ion sources, magnet designs, photomultiplier and image-intensifier designs, improved fast electronics systems, and automated devices for pattern recognition and data encoding and analysis.

Accelerator technology, beginning with the earliest cyclotrons, has produced some ideas and stimulated inventions useful in other areas of science and industry. High-power transmitting tubes were developed in the late thirties in conjunction with the cyclotron. The early impetus to the development of the klystrons arose from the desire to build a linear accelerator. The Van de Graaff electrostatic generator, now widely used in radiology and radiography as well as nuclear physics, was developed by physicists as a particle
accelerator. More recently, the alternating gradient principle, discovered by high-energy accelerator physicists, has been applied in electron tubes in the communication industry.

Great contributions have also been made by the theoretical work in these fields. An outstanding example is that the quantum theory of fields was developed to its present advanced state in order to understand fundamental problems of elementary particle structure, yet this abstract theory found an important application in solid state and low-temperature liquid-state theories, leading to detailed predictions about transistors, superfluidity, and superconductivity.

AEC high-energy physics policy statement, 1965(43)

Past investments in studies of the interior of atoms have been repaid hundreds of times over in terms of new knowledge, new technologies, new jobs, national security, advances in medicine, and returns to the Treasury.

DOE SSC briefing book, 1987(44)

With the apparent effectiveness of the "national security" and "atomic energy" arguments through the 1950's, high-energy physicists usually saw no need to provide more specific lists of scientific and technological spinoffs generated by their work. That the situation had changed by the 1960's is attested to by the appearance of such lists in advisory panel reports and public statements starting around the time of the Ramsey panel report in 1963, and continuing to the present.

A detailed analysis of the nature and validity of spinoff claims is beyond the scope of this study.(45)
However, one example of such lists is quoted here as an illustration. The most important recent addition to the lists is the technology of large, high-field-strength superconducting magnets. The need for large numbers of these magnets, which have become essential to the construction of state-of-the-art accelerators over the past fifteen to twenty years, has spurred the development of design and mass-production techniques for these magnets and their components. The use of superconducting magnet technology in magnetic resonance imaging devices has been used to bolster always-appealing medical spinoff arguments.

WE CAN'T TELL WHAT WILL COME OF HIGH-ENERGY PHYSICS, BUT THERE WILL SURELY BE SOMETHING

The practical results that must derive from continued exploration with larger accelerators cannot be guessed. If the past is a guide they will be numerous and fantastic....

Piore panel, 1959(46)

... To degrade high-energy physics, because at present we cannot pinpoint the applications, in contrast to the materials sciences... and to state that the materials sciences should be supported at the same rate as high-energy nuclear physics is not a responsible analysis. I don't see how we dare gamble with our future with statements of this kind.

Dr. Emanuel Piore before the JCAE, 1965(47)

... The reason for this great interest is that high energy physics is unique and concerns itself with the most fundamental laws governing
the constitution of matter and the elementary particles of which matter is constructed. Although the consequences of the discovery and understanding of fundamental physical laws cannot be foreseen at the time they are made, it has been historically true that in the long run these understandings have had a very great impact on science and technology and all mankind.

Dr. Glenn Seaborg to Lyndon Johnson, 1965(48)

Rep. HINSHAW. Could you conjecture as to what benefit we might get in the energy field, itself, from this, recognizing that we are in a very embryonic state of research?

Dr. RICHTER. I don't really know how I can give you a very good answer to that.

Let me try to answer it by analogy. That is always a slippery way to answer such a question.

In 1933, the man who was probably the greatest of the nuclear physicists, and discoverer of the nucleus of the atom, Ernest Rutherford, was asked the same question about the nucleus. His reply at that time was that anyone who said a practical source of power can come from the nucleus of the atom is talking nonsense.

It was only 8 or 9 years later that the first chain reaction took place at the University of Chicago. From that has come nuclear weapons, which is not too pleasant and nuclear power, which could be very pleasant for mankind. It is too soon to say what these [newly-discovered J/psi] particles signify. It has only been since the 8th or 9th of November that we have known about these particles.

I have to confess not having sufficient vision on that short time scale to be able to say what they will do.

Again, you are trying to illuminate the strongest force of nature, to understand what holds the particles that make up the nucleus together.

I would be very surprised if nothing whatsoever came out of really having a fundamental
understanding of that.

... 

Dr. WILSON. ... The one thing we can say for sure is that if we don't know what practical applications will result from our research, we do know that if we do not do the research, then there will be no application.

JCAE Hearing, 1975(4?)

In a realm of public policy in which uncertainties are vast, yet consequences are perceived as profound, all decisions ultimately incorporate an important element of subjective judgment and faith. This argument, among all of the rationales, is the purest reflection of that reality.

III. TERTIARY RATIONALES

HIGH-ENERGY PHYSICS IS A MAJOR CONTRIBUTION TO CIVILIZATION

Mr. [Dr. John] DEUTCH. Let me be right out in front with you and say that I do not believe that in the case of high energy physics, that we can point to that having an impact on energy supply technology -

[Rep.] Mr. [Clair] BURGENER. It is not that far along?

Mr. DEUTCH. That is right; we can expect little impact during your lifetime and my lifetime, and even perhaps our children's lifetime.

Mr. BURGENER. It is that basic, is it?

Mr. DEUTCH. That is correct. It is one of the major enterprises that mankind is intellectually engaged in - high-energy physics.

Mr. BURGENER. But is it a precursor to anything
that might follow?

Mr. DEUTCH. That is right; it is concerned with the fundamental structure of matter, and it is not going to help us with coal gasification plants, or nuclear reactors, or breeder reactors for a long, long, long time. But it is fundamental to this society. As strong and as rich as we are, we have an obligation to do that kind of work to some extent.

Mr. BURGENER. Is it the sort of thing where, hopefully, in the distant future, some kind of breakthrough may occur of importance?

Mr. DEUTCH. A breakthrough could always occur, sir, but I am not here defending the budget on that basis.

Mr. BURGENER. Yes, but it is something that you think is in the public interest?

Mr. DEUTCH. Absolutely.

Mr. BURGENER. And not ought to be, but must be done?

Mr. DEUTCH. It is a major contribution that this nation makes to civilization. We are putting together the basic building blocks of knowledge forever, and it will stick forever and we ought to be very proud of it and we ought to be willing to support it.

House Appropriations Committee
Hearing, 1978(50)

HIGH-ENERGY PHYSICS CONTRIBUTES TO SPIRITUAL VALUES

BILL MOYERS: What do you say to my cousin, that lives in the panhandle of Texas who's about to retire after 35 years in one job and is worrying about whether or not his $900 a month pension will be there, and whether or not taxes won't take too much of it to say, "Why do we want to spend billions of dollars to find out what happened in the first hundredth of a second of the universe, or whether the universe is enlarging, expanding, why?"
DR. STEVEN WEINBERG: You know, there are a lot of things in our society that are not needed to get through the next day or not needed to feed and clothe and house us, but somehow make life worth living. I think finding out about the universe is one of them. I can't point to practical applications of the superconducting supercollider. There are spinoffs from the building of the thing. You know, we learned how to have super-conducting magnets. We learned how to do on-line computing. It's going to be the largest liquid helium facility in the world. So we'll get a lot of technological expertise out of building the thing. But I'd hate to rely on that. I think that doesn't do justice to the value, I would say spiritual value.

MOYERS: Spiritual value?

WEINBERG: Well, what else is it? We're finding out what kind of a world this is. We're finding out what the rules are. I think it's a great value to our society. It's one of the things we're proud of in this country. We're proud of doing this sort of thing.

MOYERS: You said it helps spiritually, and yet, there's so many people who feel that science - I'm not one of them - but they feel that science does not give them a feeling for how to live their lives. That, in fact, it removes the ground of their faith.

WEINBERG: Whatever faith you have you ought to be willing to confront it with the discoveries of science....

Bill Moyers' World of Ideas, broadcast
September 23, 1988(51)

HIGH-ENERGY PHYSICS IS THE HIGH CULTURE OF OUR TIME

From time to time in the course of history men have been swept up by intense currents of creative activity. In the pyramids of Egypt, in Greek sculpture and in Florentine painting we find monuments to such bursts of expression. My favorite example is the Gothic cathedrals that so magically sprang up in 12th- and 13th-century France, for I like to relate that magnificent
preoccupation with construction to an obsession of our own time - the building of nuclear accelerators.

Robert R. Wilson in Scientific American, 1958(52)

HIGH-ENERGY PHYSICS IS THE DUTY OF MANKIND

... The universe astonishes us by its very comprehensibility. In this we find our call: Being born upon an obscure planet located at the rim of a middling galaxy among a hundred billion galaxies of an aging universe, it is our sacred duty to know its deepest secrets, as well as we are able. Dolphins and chimpanzees can be made to speak, after a fashion. Yet, only humans will look at the stars with wonder and find it necessary to understand just what they are and how they work and why we are here to see them. No better mousetrap or wrist tv here - just the triumph of the human imagination. It is simply the need to know that compels us to build a bigger and better accelerator and to approach an understanding of the mother of us all - the Big Bang - and its curious byproduct, the matter of which we are made.

Sheldon L. Glashow and Leon M. Lederman in Physics Today, 1985(53)

HIGH-ENERGY PHYSICS IS ONLY THE LATEST CHAPTER IN A LONG AND HONORABLE TRADITION

The intellectual promise of this field lies in the fundamental quest toward an understanding of the ultimate structure of matter, which can be traced back to the early Greeks....

HEPAP subpanel report, 1974(54)

There are basically two ways by which an elite devoted to essentially private intellectual pursuits can establish some means of contact with broadly shared values. One way
is for the elite to express those pursuits in terms of broadly shared values. This expression is what I have been documenting so far in this chapter.

The other, much more difficult, is to attempt to add the private pursuit to the canon of broadly shared values. Such "evangelism," as Daniel Greenberg called it, began to appear with some regularity as a promotional strategy during the early 1960's, when the momentum deriving from the experience of the war finally began to dissipate and national security arguments no longer carried the same weight.

An early milestone in particle physics evangelism was the release of Nature of Matter: Purposes of High Energy Physics in 1965, at the height of the debates over the 200 BeV accelerator. The stated purpose of the volume of essays by well-known theoretical physicists was to counter "misunderstanding of the objectives of high energy physics, not only among the general public, but also among the scientific community as a whole."(55) After a period of relative dormancy, evangelism experienced a revival during the battle for the SSC.

IV. SECONDARY/TERTIARY (HYBRID) RATIONALES

WE CAN'T TELL WHAT WILL COME OF HIGH-ENERGY PHYSICS, BUT THERE WILL SURELY BE SOMETHING, BUT YOU SHOULD NEVER JUDGE
IT ON THAT BASIS ANYWAY

I would like to digress for a moment, and tell you what Professor Lawrence said on the occasion of the initial operation of the bevatron, because it illustrates the nature of our work. A reporter asked him if he could foresee any practical usefulness of the research work which would be carried out on the big machine in the years to come. Dr. Lawrence replied that although he felt quite sure that nothing of direct practical value would come from it, he was afraid to say so, because he had felt the same way about the previous two machines, and had been wrong each time. He had expected both of them to give a great deal of valuable information about nuclei, which would help us in our basic understanding of nature. The scientific community considers such information to be ample justification for the building of those big machines. We feel that a more complete understanding of the fundamentals of physics will eventually prove to be of great value in our modern world, which is so thoroughly grounded on science. But in addition to the basic physics which came out of the machines, and which, I cannot emphasize too strongly, we feel is the real product of our laboratory, the 60-inch cyclotron produced the first plutonium, the first tritium, and the first carbon 14. All of these have been of practical importance in ways that are quite familiar to all of you.

Professor Lawrence was surprised to find he had been wrong about the 60-inch cyclotron, but he felt he was quite safe in predicting that the 184-inch cyclotron would have no practical importance. It was intended to explore the realm of meson physics, and certainly nothing practical could come of that! But the 184-inch cyclotron has proved to be of value in the practical field of cancer therapy, as Dr. Tobias told you 2 years ago at the hearings on medicine and biology. Although most of us feel that the 184-inch cyclotron will be longest remembered for the important meson work done with it, we are, of course, exceedingly pleased that it has turned out to be useful in the medical field as well.

With this background, one can appreciate Professor Lawrence's reluctance to predict that nothing of practical importance would come from
the bevatron. But again, I would hope that no one ever judges the worth of the bevatron by such a criterion. I am confident that future generations will feel that the people of our country were well repaid in basic scientific knowledge for the money this generation invested in research at the bevatron and other high energy accelerators.

Prepared statement of Dr. Luis Alvarez for the JCAE, 1958(56)

In my own opinion the high energy physics program must be justified almost entirely by its contribution to the understanding of the laws of nature and the interaction of the fundamental particles. There are, of course, some side benefits that have come from the high energy physics program and we expect more. I am sure we would be wrong to mislead this committee by saying that you should support it because of these other things....

Dr. Paul McDaniel before the JCAE, 1964(57)

The SSC... is a no-lose proposition. It is a little bit like Columbus sailing west from Spain. Columbus promised that he would get to the Indies if he could sail far enough West. Well, that was wrong, but what he should have said if he had been more careful, what he could have said, which would have been correct, is that if he sailed far enough West, he would get to the Indies unless something equally interesting got in the way.

The SSC is in that position. It will discover the Higgs boson unless something equally interesting gets in the way....

... There is a whole list of hypothetical new particles and forces that we would like to look for with the SSC, and if history is any indication, some of the most exciting results from the SSC will be things that no one has dreamed of yet...

History shows that research at this kind of fundamental level always has tremendous impact on other branches of science, and it also leads eventually to applications that affect our daily lives....
However, let me close by saying that I suspect that for our fellow citizens, not just for physicists but for people in general, these spinoffs and applications are not the most important aspect of elementary particle physics....

There is reason to believe that in elementary particle physics we are learning something about the logical structure of the universe at a very, very deep level....

I think that this kind of discovery is something that is going on in our present civilization at which future men and women, and not just physicists, will look back with respect.

Dr. Leon Lederman before the House Science, Space and Technology Committee, 1987(58)

This argument, combining faith and evangelism, captures in its purest form the schizophrenic "standard formula" of George Daniels: "Utility is not to be a test of scientific work, but all knowledge will ultimately prove useful."(59)

AS A VIRTUOUS PURSUIT, HIGH-ENERGY PHYSICS HELPS TO SET THE TONE OF SOCIETY

The value of fundamental research lies not only in the ideas and results it directly produces. The spirit that prevails in the basic sciences affects the whole scientific and technological process because it determines the way of thinking and the standards by which its creations are judged. An atmosphere of creativity is established that penetrates to every frontier.... This is one of the important social functions of pure science; it establishes the climate in which all scientific and technological activities flourish; it pumps the lifeblood of ideas and inventiveness into laboratories and factories.
There is another point which must be considered here: It is the spirit of creativity and determination directed towards the exploration of nature that pervades the centers of pure research. The people working in such centers are less prone to the feelings of aimlessness that is observed in some segments of our society. The existence in our society of strong centers of activity with well defined goals may be of great import to our present national situation. The scientific research centers exert their influence in this direction together with many other activities of the same character in public affairs, industry and art. The orientation of these groups also produces an atmosphere which is conducive to easier solutions of problems concerning social and racial differences among their members.

HEPAP report, 1969(60)

High-energy physics is a field known for its audacity; it is a necessary survival skill for an endeavor which proposes to spend huge amounts of money in an attempt to reveal the foundations of the universe. Perhaps, then, it should not be a surprise to find the field behind this unusual argument, extraordinary for its explicit linkage of spiritual values with the solution of real social problems.

HIGH-ENERGY PHYSICS SHOULD BE SUPPORTED BECAUSE THE BEST SCIENTISTS ARE ATTRACTION TO IT

... The importance of this field is perhaps best shown by the number of able scientists who are now working in the field both in this country and abroad. This represents a judgment that the field is both important and fruitful for basic science....

NSF advisory panel report, 1954(61)
One test of the value and importance of a field of science is the quality of the scientists who are attracted to it; in this respect high energy physics draws many of our most talented people.


The sooner the SSC is built, the sooner we will be able to actually see and study the most basic components of life, have a facility to train our scientists and attract super-brains and develop technology which will launch American industry's competitive edge in the world marketplace.

Rep. Ron Packard before the House Science, Space and Technology Committee, 1987(63)

For the high-energy physics community, this argument represents both an evangelistic claim that high-energy physicists are the best scientists and that the best scientists have the best judgment about what is important in science, and an assertion that therefore the government should fund whatever it is that the best scientists want to pursue if it wishes to maximize practical benefit. For Congress and the Administration it represents a statement about the incentives necessary to attract the best scientific talent to the United States and keep it, with an implicit assumption that in some way the efforts of the scientists thereby attracted will be harnessed to some broader national purpose.
HIGH-ENERGY PHYSICS IS THE MOST FUNDAMENTAL BRANCH OF SCIENCE

... It is a whole new field of science that is building up for the first time around these large machines. Our progress is very encouraging. It is very fundamental. It is the most fundamental thing you can think of in the structure of matter....

Dr. T.H. Johnson before the House Appropriations Committee, 1957(64)

Instead of feuding with one another for public favor, it would be fitting for scientists to think of themselves as members of an expedition sent to explore an unfamiliar but civilized commonwealth whose laws and customs are dimly understood. However exciting and profitable it may be to establish themselves in the rich coastal cities of biochemistry and solid state physics, it would be tragic to cut off support to the parties already working their way up river, past the portages of particle physics and cosmology, toward the mysterious inland capital where the laws are made.

Dr. Steven Weinberg in Nature of Matter, 1965(65)

The fundamental objective of high-energy physics - which in this context I shall identify with elementary-particle physics - is no less than trying to understand the fundamental forces and general physical laws which constitute the rules of behavior of all inanimate matter. For this reason, all other physical sciences (and probably also all life sciences) must ultimately rest on the findings of elementary-particle physics....

Dr. W.K.H. Panofsky, 1965(66)

For we do not think that we know a thing until we are acquainted with its primary conditions or first principles and have carried our analysis as far as its simplest elements.

Aristotle, quoted by NRC Physics
The advances of the past decade have brought us tantalizingly close to a profound new understanding of the fundamental constituents of nature and their interactions. The standard model based on quarks and leptons organizes current knowledge and defines the horizon of particle physics at constituent energies of about 1 TeV and the horizon of cosmology at times of about $10^{-15}$ second. Important answers are to be found on the 1-TeV scale. There we await new discoveries about the unification of the forces of nature, the patterns of the fundamental constituents of matter, and the origin of the universe. The SSC is the instrument to lead this quest. The boldness of the project and the significance of the questions it will address give it the potential to be one of the great examples of the United States' commitment to excellence in science.

Drs. Chris Quigg and Roy F. Schwitters in Science, 1986(68)

This final rationale expresses most directly the high-energy physics community's understanding of its role in science and in society. The claim that high-energy physics is the most fundamental of the sciences is a claim that it is the foundation upon which the entire edifice of scientific knowledge, and by extension the entire edifice of technology, rests. This claim reflects a widely held reductionist view of the nature and purpose of science. As an assertion in epistemology, it is an arguable claim, as Britain's Kendrew panel pointed out:

The search for the ultimate laws of matter is surely a "fundamental" area of science, but we would not support the contention of some witnesses that particle physics should be regarded as the most fundamental study imaginable. Rather we
agree with the view taken by Feynman in *The Character of Physical Law* that the fabric of science is a "net" in which every part is interconnected with every other part, but where no scientific discovery is accorded a position of unique significance. From this point of view, the discovery of the W particle is no more (and no less) "fundamental" than the discovery of the structure of DNA.(69)

As an assertion in the history of science, as implied by the common statements about high-energy physics serving as the continuation of a tradition dating back to the ancient Greeks, it is arguable as well, as Stephen Brush has noted:

... But physics was not always the "master science," and the study of elementary particles was not always the most fundamental kind of research; it only became so in the first half of the present century. Twentieth-century atomic physics, with the help of accelerators, earned the status of "most fundamental science." It is only by going back in history to a time when the search for elementary particles was not considered the most fundamental goal of science that we can appreciate the magnitude of that achievement, and at the same time recognize that some other science may in the future become the most fundamental.(70)

Finally, as an assertion about the day-to-day practice of science, it is arguable too, whether on grounds of the limitations of the human mind or the nature of reality. Consider the words of Eugene Wigner:

About 25 years ago... one of the greatest living physicists - Professor Dirac stated that in principle, all the questions of chemistry and everyday physics have been solved.

There are able chemists who told me so far, quantum mechanics has contributed essentially nothing to their field. It is because the basic principles are so far removed from the eventual application that it is not in the purview of human intellect to derive many of the applications from the basic principles.(71)
- or the comments of Philip Anderson:

The first slide in many general talks given by my high-energy colleagues is a length scale spreading from the "Planck length" (way below elementary particle size) to the size of the cosmos. They gesture deprecatingly towards the center of this scale (where we and our atoms and all of everyday life sit) and say, "of course, we know everything there, and the only fundamental science is at the extreme scales." Well, we don't "know everything there": we haven't the foggiest idea what drives the new high-temperature superconductors, or what makes a snowflake, or how the mind or the economy works. What is more, nothing high energy physics can do will ever be of the slightest direct help in solving these overwhelmingly hard problems.

The reason is the idea of "emergent properties": that complicated systems have properties which are not implied by the elementary pieces of which they are made. To a brain or a computer, it just doesn't matter what the physics of the hardware is, for example. We have long since learned everything particle physics can tell us about the behavior of ordinary matter, even of nuclei, and probably of the stars themselves. If the particle physicists tell you they will understand even the Big Bang better as a consequence of the SSC, they are being wildly optimistic; and if they claim any other relevance, they are wrong. Their fundamental physics has become so "fundamental" as to be almost totally irrelevant, even to the rest of science. (72)

It is not my purpose here to take sides in this debate. I have described it in some detail, however, in order to emphasize the important role played by quasi-cultural assumptions about science in debates over public policy. The view of particle physics as somehow the most fundamental of the sciences has been repeated so often that it is taken for granted by many, and as an unexamined assumption it has almost certainly had a profound influence on patterns of
government support. Whether existing policy mechanisms are capable of recognizing and dealing wisely with such biases is an important issue for the policy analyst.

*     *     *

I conclude this survey with two general comments about the nature of rationales conjured in the heat of battle for public favor.

First, when a program has become totally dependent on government support for its survival, the incentive to tell those holding the purse strings whatever it is that they are believed to want to hear is both obvious and overwhelming. The evidence for this phenomenon goes beyond the telling progression of rationales in step with national preoccupations to the give and take of testimony during Congressional hearings. The exchange which opened this chapter is a case in point. On one level, Robert Wilson plays the true and honorable scientist, taking pains to drive home the point that no guarantees can be made of practical results from science, and that the real reason for doing the work is the pursuit of knowledge for its own sake. On a deeper level, however, Wilson cannot leave Rep. Rudd with nothing to show for his troubles. "We always have hope... I've never seen a time when this kind of knowledge didn't result in something..." And when Rudd offers Wilson
the opportunity to put his money where his mouth is, as it were, and cast his vote for a transfer to NSF, the proudly independent scientist disappears, replaced by the hungry supplicant: "There are very good arguments for the research to be with ERDA, because the things that we are studying are fundamental to the production of energy, and because of the expensive nature of the projects." It is important to emphasize that this does not necessarily represent any conscious attempt to deceive on the part of scientists or other proponents of funding. Given the way the system works, the pressure is great for those who need federal money to express their position in terms which, strictly speaking, do not promise anything that cannot be delivered, but which are ambiguous enough to be allow the listener plenty of room for an alternative interpretation.

Second, virtually without exception, the rationales cited in behalf of high-energy physics testify to links between high-energy physics and the achievement of desirable goals without addressing the question of whether support of high-energy physics is the only way, or even the most efficient way, of achieving those goals. There can never be enough resources for the Administration and the Congress to do everything they might imagine a government ought to do, so the task they face is always to match available resources most efficiently to programs designed to achieve as many desirable goals as possible. Of course, in doing so they
work within constraints set not only by fiscal limitations but by realities of political and social acceptability as well. Nevertheless, the key question for policy is not whether high-energy physics can somehow be linked conceptually to national competitiveness (or to keeping ahead of the Russians, educating our youth, finding new energy sources, pursuing our cultural destiny, etc.) but rather, whether spending X billions of dollars on high-energy physics rather than specific alternatives - or not spending it at all - is the most efficient way of achieving the best balance of desired goals which cannot all be achieved in full.
Notes:


9. **Superconducting Super Collider**, Hearings before the House Committee on Science, Space and Technology, p. 4.

10. **Superconducting Super Collider**, Hearings before the House Committee on Science, Space and Technology, p. 551.

11. **Superconducting Super Collider**, Hearings before the House Committee on Science, Space and Technology, p. 557.


23. 1978 ERDA Authorization (Conservation, High Energy Physics and Basic Energy Sciences), Hearings before the House Science and Technology Committee, pp. 119-120.


31. AEC Authorizing Legislation Fiscal Year 1965, Hearings before the Joint Committee on Atomic Energy, p. 1323.


33. AEC Authorizing Legislation Fiscal Year 1965, Hearings before the Joint Committee on Atomic Energy, pp. 1322-1323.


36. Superconducting Super Collider, Hearings before the House Science, Space and Technology Committee, p. 551.


41. In 1984, Leon Lederman reported that of the approximately 130 people who receive doctorates in high-energy physics in the United States each year, about 50 remain within the field, with the rest leaving for a wide variety of other endeavors. See Leon M. Lederman, "The Value of Fundamental Science," *Scientific American* 251(5):40-47, November 1984; see also Congressional Budget Office, *Risks and Benefits...*, p. 9.

42. *Stanford Linear Electron Accelerator*, Hearings before the Joint Committee on Atomic Energy, p. 218.


44. Excerpt of DOE SSC briefing book, reprinted in *Superconducting Super Collider*, Hearings before the House Science, Space and Technology Committee, p. 15.


49. ERDA Authorizing Legislation Fiscal Year 1976, Hearings before the Joint Committee on Atomic Energy, pp. 1722-1723.


57. AEC Authorizing Legislation Fiscal Year 1965, Hearings before the Joint Committee on Atomic Energy, p. 1322.

58. Superconducting Super Collider, Hearings before the House Committee on Science, Space and Technology, p. 245.

59. See Daniels, "The Pure-Science Ideal and Democratic Culture."


63. Superconducting Super Collider, Hearings before the House Committee on Science, Space and Technology, p. 551.


72. Written statement by Philip Anderson, reprinted in *Superconducting Super Collider*, Hearings before the House Committee on Science, Space, and Technology Committee, pp. 904-905.
If somebody would get up on the Senate floor and say, "We think we ought to have a billion and a half dollars for cancer," it would probably pass. I think that is our attitude. Whatever the cost is, we will vote for it without knowing what we are voting for. That is not a requirement around here, but sometimes it would be very helpful....

Sen. Robert Dole(1)

That people turn to government-sponsored research for salvation in the face of a perceived medical crisis, and that Congress and the Administration respond with generous allocations of tax dollars, seems so natural to most as to be unworthy of comment. But in historical terms, the assignment of a central role to the federal government in medical research, as in scientific research more broadly, is a relatively recent development, with origins in the years immediately preceding the Second World War, and full flowering only in the postwar years.

The nature of the relationship between Congress, the Administration, the medical research establishment, and the nation in the continuing war against dread disease is best epitomized by the longest-running battle in that struggle: the so-called War on Cancer.(2)

Strictly speaking, federal involvement in cancer research predated the formal "declaration of war" by more than a decade. By the early 1920's, the Public Health
Service, still concerned primarily with traditional "public health" issues associated with epidemic infectious disease, was sponsoring limited cancer investigations both within its own Hygienic Laboratory in Washington, DC, and at Harvard Medical School in Boston. Expenditures for the intra- and extramural work, taken together, totaled only $11,000 for 1922.(3)

In 1927, Senator Matthew Neely of West Virginia made the first attempt to place the problem of cancer on the formal agenda of the federal government, introducing a bill that would pay a $5 million reward "to the first person who discovered a practical and successful cure for cancer."(4) Neely's proposal died in the Senate, but not without attracting considerable publicity; within a year the senator received 2,500 letters, constituting in the aggregate a veritable encyclopedia of American folk medicine and quack nostrums.

Recognizing the futility of his initial effort, Neely took a more conservative approach when he tried again in 1928, proposing a grant of $100,000 to the National Academy of Sciences to investigate the cancer problem and report to the Congress on possibilities for federal involvement. This time Neely's bill passed the Senate in revised form, only to die in House committee.

Though they proved to be premature, the West Virginian's efforts foreshadowed not only the fact of
federal involvement in cancer research, but some of the style and substance of later debates as well. Neely’s address to the Senate, "Cancer – Humanity’s Greatest Scourge," was filled with overblown metaphors and inflammatory rhetoric. He cited statistics demonstrating increasing mortality attributable to cancer, and emphasized the economic cost to the nation of the morbidity and mortality due to the disease; and he appealed to his fellow lawmakers’ sense of priorities: how could the government spend $10 million a year to eradicate the corn borer and $5 million to investigate tuberculosis in animals, while the scourge of cancer went unaddressed? In the course of debate, one colleague questioned the validity of Neely’s statistics, wondering whether the apparent increase in cancer mortality was not merely an artifact of changing standards of diagnosis. But the voice of medical authority, in the person of Royal Copeland, MD, Senator from New York, stepped in to assure the assembled legislators that cancer was indeed increasing. (5)

The push for formal government involvement in the struggle against cancer was not to succeed until 1937, when the introduction of new bills in both the House and the Senate touched off a surprisingly fast-moving process which culminated in extraordinary joint hearings and the swift passage of a compromise bill. With the President’s signature, the National Cancer Institute was created on
August 5, 1937.

The rhetoric of 1937 was virtually identical in substance to that of 1928. Why, then, did cancer legislation pass in 1937? Two broad developments helped to create a receptive environment for the new legislation.

First was the still-growing reputation of the medical profession. Developments in medical science and education, changes in the internal structure of the medical profession, and changes in the attitude of influential segments of the American population toward professional expertise combined to raise doctors to a position of cultural authority. Major contributions by private philanthropists to the crusade against cancer testified to a widely-shared faith in the potential of scientific medicine.(6)

A second factor was the growing willingness of Americans to turn to government for solutions to societal problems. While this tendency had manifested itself before, notably during the Progressive era, it was greatly strengthened by the Depression and by Franklin D. Roosevelt's New Deal.

But why cancer, and why in 1937? Then as now, cardiovascular disease killed many more people than cancer. People had long had a special dread of cancer, to be sure, but there were few substantive developments in 1937 to account for any significant change in such perceptions. Public awareness did increase in the wake of newly-
invigorated promotional efforts by the American Society for the Control of Cancer (later to become the American Cancer Society) and subsequent attention in mass-circulation periodicals such as Time, Life, and Fortune. Some accounts of the events of 1937 emphasize the role of public pressure in explaining the actions of Congress.(7)

At the same time, the creation of the NCI can be traced through a series of purposeful actions by a fairly small group of research scientists, public health officials, progressive politicians, and, in the end, spokesmen for the cancer society as well, who took advantage of a generally favorable political context and "joined hands to support legislation that no one but the hard-hearted could resist."(8)

In any event, the precedent was set. Prophetic critics warned of the inevitable establishment of a whole series of disease-specific institutes, following the cancer example.(9) Dr. James Ewing, the prominent physician-scientist associated with the renowned Memorial Hospital of New York, foreshadowed yet another facet of later debates when he argued that breakthroughs in cancer research would come only when science was ready, and could not be hastened by the infusion of large sums of money to the effort.(10)

Yet hardly anyone noticed. The initial appropriations for the National Cancer Institute were small, and were to remain so for years to come - and none of the principals
behind the developments of 1937 expected otherwise.(11)

It is not clear whether the Congress had any great expectations concerning the impact of their action against cancer. All things considered, the bill creating the NCI was a relatively trifling matter against the broad range of other issues confronting legislators at the time. But the intent of the Congress was clear. The purpose of the NCI was

Conducting researches, investigations, experiments and studies relating to the causes, diagnosis and treatment of cancer; assisting and fostering similar research activities by other agencies, public and private; and promoting the coordination of all such researches and activities and the useful application of their results with a view to the development and prompt widespread use of the most effective methods of prevention, diagnosis and treatment of cancer.(12)

"Researches, investigations, experiments and studies," both intra- and extramural, were to be the means, but "the development and prompt widespread use of the most effective methods of prevention, diagnosis and treatment of cancer" were the ends. The control of cancer, and the alleviation of the suffering of countless Americans, were the point of the new venture. Medical science was to be supported not for its own sake, but for its contribution to such greatly desired ends.

The National Cancer Institute had hardly begun to function when war came, and the attention of the Congress, medical researchers and the nation as a whole was diverted to more pressing matters. During the war years, however,
the groundwork was laid for radical changes in the relationship between the federal government and medical research. As did their colleagues in other branches of science, medical researchers learned to appreciate the advantages of substantial, continuing support from a well-endowed patron. The results of their wartime work, including such well-publicized breakthroughs as the rapid development and mass production of penicillin, confirmed the great promise of medical science, and taught Americans the power of generous government funding to make that promise a reality. The new-found appreciation of science inspired by the war was to spur increased support not only for basic research in the physical sciences but for medical research as well.

But one behind-the-scenes development in particular was to have a profound impact on the shape and size of the federal involvement in medical research. When Mary Lasker decided to take action on her long-standing interest in matters of health, she had the benefit not only of her own energy and intelligence, but also of the considerable financial resources of her husband Albert, president of a prominent and very successful advertising firm. In 1943 she took an interest in the affairs of the American Society for the Control of Cancer. With a number of friends and associates, she brought an infusion of vigor, new ideas and new money for which the sluggish, physician-dominated
society was unprepared. In the resulting struggle the Laskerites soon gained the upper hand, and the new American Cancer Society was born. (13) From her early experiences with the American Cancer Society, Mary Lasker learned that the American people would support the cause of medical research generously if its benefits were suitably dramatized. At the same time, with a clearer conception of the huge resources that a truly effective research effort would demand, she began to think of the role that government could play. As the war wound to a close and the question of the government's postwar research effort drew increasing attention, Mary Lasker turned to politics.

In 1946, Matthew Neely, now serving in the House after a term as governor of West Virginia, picked up where he had left off nearly two decades before. With the support of Claude Pepper in the Senate, Neely proposed that the federal government spend $100 million toward the conquest of cancer. The Public Health Service and the National Institute of Health, as yet unaccustomed to new realities, balked at the huge sum, provoking a move, supported by the Lasker forces, to give the money to a new agency outside of the jurisdiction of the Surgeon General. Ultimately the bill died in the Senate, not for any lack of support but because negotiations over its details dragged on for just a bit too long to be completed prior to adjournment.
With the risk of being bypassed in the postwar reorganization of research suddenly clear, Surgeon General Thomas Parran and his colleagues took preemptive action. When funding for medical research came up for consideration the next year, the NIH requested $23 million and received $26.5 million for fiscal 1948, more than three times its budget for 1947, while the NCI requested an increase from $1.8 million to over $14 million.\(^{(14)}\)

Thus began the dramatic expansion of the government’s medical research effort. Within a few short years, an institute for research in mental health was created, and then one for heart research. Others followed, and all were consolidated under the jurisdiction of the newly plural National Institutes of Health.

Several interrelated factors can be identified which shaped and propelled nearly two decades of rapid growth in the size and scope of the federal government’s program of biomedical research.\(^{(15)}\)

A program which became known as a model of bipartisanship got an early boost from an exercise in old-fashioned partisanship. The Republicans captured control of the Eightieth Congress in the elections of 1946, marking the end of the bipartisan wartime coalition. By adding to the Truman Administration’s fiscal 1948 budget requests for medical research, the responsible appropriations subcommittees in the House and the Senate asserted their
control over an important area of policy. But along with the Republican/Democrat struggle, there was also an element of the equally traditional desire of the Congress, whatever its membership, to assert its power relative to the Administration. Once the Congress had gained the upper hand in the arena of medical research policy, it guarded its prerogative tenaciously, not only during those years in the 1950's when a Democratic Congress confronted a Republican Administration, but also later, when both Congress and the Administration were controlled by Democrats.

The unusual success of the Congress in retaining control over an area of policy with substantial public appeal was in part the result of a chance conjunction of political factors. In 1944, the Public Health Service had received a continuing authorization to spend money on research, with no fixed budget ceiling. This rare privilege cut away half of the time-consuming process by which agencies' budgets are determined each year, and simultaneously bestowed potentially extraordinary power on the chairmen of the relevant appropriations subcommittees. In the Senate, the responsibility fell quite naturally on Lister Hill. The son of a distinguished surgeon who had named him for his great mentor, Joseph Lister, Sen. Hill had already made a mark in health legislation through his sponsorship of the Hill-Burton Hospital Survey and Construction Act of 1946.(16) The long tenures of Hill, and
of his counterpart in the House, Rep. John Fogarty, in their chairmanships, starting in the mid-1950's, gave both men the time to develop and consolidate the parliamentary skills which converted political potential into reality.

The long, bitter struggle over direct federal involvement in the nation's health care system through financing of health insurance and medical education remained in a stalemate until the enactment of Medicare and Medicaid in 1965, as the American Medical Association exerted the full force of its authority and its lobbying power in favor of the status quo. However, preoccupied by this apparent threat to the foundations of private medical practice, and no doubt well aware of public interest in the progress of medical science, the AMA chose not to oppose federal funding for medical research. Thus, yet another set of political forces operated to channel power to Fogarty and Hill: the only way for members of Congress to respond to the concern of the American people over matters of health was to join in supporting medical research. It may or may not have been true that "medical research," as Rep. Melvin Laird observed, "is the best kind of health insurance." But for many years it was the only kind of "health insurance" that was within the power of Congress to deliver.(17)

Fogarty and Hill on the one hand, and Mary Lasker and her colleagues on the other, found in each other valuable allies in the advancement of a common cause. The Lasker
lobby skillfully used campaign contributions to cultivate a group of sympathetic legislators. They took advantage of the mass media to inform and arouse the public and focus the attention of the Congress on pressing concerns. They developed unique data on the impact of disease and the costs and projected benefits of research programs, data which time and again was used by the appropriations subcommittees to motivate and justify their initiatives. And with an extensive network of contacts in the medical and scientific communities, the Laskerites could always be counted upon when expert "citizen witnesses" were needed for hearings or to help fill advisory panels and review boards.

Yet another key element was the political skill and vision of Dr. James Shannon, whose tenure as director of NIH coincided almost exactly with those of Fogarty and Hill as subcommittee chairmen. Shannon proved unusually adept at walking the fine line of defending the Administration stand on research funding while building a strong working relationship with Fogarty and Hill. Shannon understood that the mission of the NIH, as defined by Congress, was "the abolishment of major disease."(18) In his view, the best way of achieving that goal was for NIH to develop a comprehensive, high-quality program in basic biomedical science. Learning much from Shannon, but from many other sources as well, Fogarty and Hill came to share Shannon’s belief in the value of basic science, and together they
helped guide the growth of the world’s largest and most important program in basic biomedical science. That there remained some divergence in expectations with respect to the nature and timing of the payoffs from the venture, however, was clear practically from the start.

Fogarty and Hill did not pump money blindly into the NIH. They took an active interest in the content and scope of the programs which they supported. They took advantage of sources in the NIH and the broader medical and scientific communities, as well as in the research lobby, to keep up with important developments in medical science and with emerging health concerns of the public. Regularly, they directed NIH and its component institutes to pursue these developments.(19)

What was true of NIH in general was especially true of the National Cancer Institute, responsible for attacking the most dreaded disease of all. Under the influence of the Lasker lobby and respected clinician/scientists such as Dr. Sidney Farber, Congress appropriated substantial and increasing sums each year, starting in the mid-1950’s, for the development of potential chemotherapeutic agents. The need for large-scale, quasi-industrial processes in the isolation and screening of potential cancer drugs led to substantial NCI use of contracts, rather than the more traditional grants, to fund directed research, and in time the NCI devoted substantial effort as well to the
development of management techniques for such programs. Similar approaches were used in the 1960's to build large programs in both viral oncology and carcinogenesis research.

These programs aroused controversy over issues of competence in research management and quality of research, and the emphasis on basic vs. applied research, directed vs. undirected research, and peer-reviewed grants vs. non-peer-reviewed contracts. For the purposes of this analysis, it is worth noting that many partisans of undirected, grant-supported basic research saw in these programs the heavy hand of interference from ignorant and unsophisticated Congressmen and lobbyists, impatient for results immediately applicable to patient care. Whether Fogarty, Hill and their allies were guilty of ignorance and lack of sophistication is debatable. But the critics were correct in perceiving, beneath the surface of a commitment to basic medical research, a more fundamental interest in accelerating tangible progress against disease.(20)

Yet the diligence and seriousness of purpose which the responsible members of Congress brought to the task of building a great national program in biomedical research during the years through the mid-1960's was not matched by a commensurate dedication to objective analysis of the fruits of their labors. Congress as a whole, with the occasional exception of a handful of fiscal conservatives who were primarily concerned with the cost of the programs rather
than their substance, deferred to the judgment of Fogarty, Hill and their respective subcommittees. This was the usual practice, and it was especially congenial when an area of such great public interest was obviously being served so well. It also did not harm the cause of biomedical research that medical schools and teaching hospitals, to which much of the money went, were well-distributed geographically, so that members of Congress from all corners of the nation could count on bringing some of the bounty home to their constituents. This was not always the case in other branches of science, where the dominance of elite universities on the east and west coasts was often a political sore point.

Acting more like true believers than seers of truth, the congressional leaders routinely packed their hearings with one-sided testimony from friendly witnesses. An unsystematic mix of reports of genuine advances, assurances from scientists and clinicians, selective statistics and anecdotal reports served to reassure those involved that their efforts were not in vain. And should the faith waver, the periodic appearance before the subcommittees of citizens who testified to the devastating impact of a dread disease on their lives sufficed to remind everyone of the urgency of their task.

The only serious questioning of the rapid growth of biomedical research expenditures during this period was
initiated in 1959 by Lawrence Fountain, chairman of the Intergovernmental Relations Subcommittee of the House Government Operations Committee. But, perhaps recognizing that a frontal assault would be fruitless, Fountain chose to focus his inquiry on the NIH's management of research grants, where some tidying-up was clearly in order. The irritating probing by the Fountain committee continued for several years and ultimately provoked, among other responses, a call by the President for a full review of the national medical research effort as embodied in the programs of the NIH. In the end, the committee appointed to carry out the review, headed by Dr. Dean Woolridge, reached conventionally reassuring conclusions in its 1965 report: "... the activities of the National Institutes of Health are essentially sound and... its budget of approximately one billion dollars a year is, on the whole, being spent wisely and well in the public interest." Yet the report made it clear that the Woolridge Committee viewed the NIH as a success especially insofar as it had been able to overcome a "scientifically inappropriate," disease-oriented organizational structure to build a broad program of undirected basic research.(21) Thus, the fundamental issue of whether the expenditures appropriated by the Congress were buying tangible progress toward better health for Americans was never seriously addressed.

The golden age of growth for the NIH and its
constituent institutes lasted through the mid-1960’s, and then came to a screeching halt as the exquisite balance of forces behind it fell apart. In short order, Rep. Fogarty died, Sen. Hill retired, and Dr. Shannon left his position as director of NIH. Larger forces were at work as well. The years of plenty for basic research were ending, as the expenses of the war in Vietnam collided with Johnson’s Great Society programs to generate a budget crisis. The sudden leveling off of research funding, and the renewal of demands for social relevance, hit biomedical research no less than research in the physical sciences. President Johnson himself put the biomedical research community on notice by calling the NIH leadership to the White House in 1966 in order to urge a stronger focus on the transformation of research results into tangible medical benefits. (22)

The Lasker lobby, while sharing a belief in the value of basic scientific knowledge, had become increasingly impatient with the seeming reluctance of Shannon and the NIH to place equal emphasis on the task of applying available knowledge as quickly and as efficiently as possible to the prevention and treatment of disease. An attempt had been made to refocus attention on the transfer of research results to practice through the President’s Commission on Heart Disease, Cancer and Stroke, appointed by Lyndon Johnson in 1964 and dominated by Lasker associates and friends. But the commission’s ambitious recommendations for
a network of centers for clinical research, teaching and patient care ran into a political crossfire, and the bill ultimately passed by Congress was considerably watered down. The original concept decayed further in the implementation phase, so that in the end the impact of the program in the areas of concern to the panel was minimal, especially as far as cancer was concerned.

By the end of the decade, the Laskerites were becoming impatient yet again for some way to both push the nation’s biomedical research effort in the direction of practical value and renew growth in funding. The latter goal, at least, would meet no objections from the biomedical research community, which, along with colleagues in other branches of science, was eager for a break from the pinch of level-funding at a time of increasing inflation.

It was against the backdrop of these developments that a book entitled *Cure for Cancer: A National Goal*, by physician Solomon Garb, appeared in mid-1968. In it, Garb drew an analogy with the space program, arguing that the key to real progress against cancer was to establish the cure for cancer as a national goal, and assign responsibility for the achievement of that goal to a generously-funded, independent agency which would report directly to the President. Mary Lasker was much impressed by Garb’s book, and it helped her settle on a direction for her new initiative.
The great lobbying machine swung into action. A full-page ad in the December 9, 1969 edition of the New York Times declared: "MR. NIXON: YOU CAN CURE CANCER." Capitol Hill maneuvering, focused especially on Sen. Ralph Yarborough of Texas, chairman of both the Committee on Labor and Public Welfare and its health subcommittee, led to the creation of a Panel of Consultants on the Conquest of Cancer, to advise on the adequacy of the present level of support for cancer research and recommend action needed to achieve cures for the major forms of cancer by 1976. Rep. John J. Rooney, a senior member of the House Appropriations Committee and an old ally of Rep. Fogarty, introduced the following extraordinary resolution before the House:

That it is the sense of the Congress that the conquest of cancer is a national crusade to be accomplished by 1976 as an appropriate commemoration of the two hundredth anniversary of the independence of our country; and

That the Congress appropriate the funds necessary for a massive program of cancer research and for the buildings and equipment with which to conduct the research and for whatever other purposes are necessary to the crusade so that the citizens of this land and of all other lands may be delivered from the greatest scourge in history.(23)

In due course, with only the word "massive" deleted, the resolution was adopted unanimously by both the House and the Senate.

The Panel of Consultants was assembled with great care in order to maximize the impact of its recommendations. Its membership was meticulously balanced between medical-
scientific and lay members, and between veterans of past Lasker campaigns and newcomers. Prominent Republican businessmen were added to the panel, in hopes of counterbalancing the Democratic leanings of many of the key participants and neutralizing opposition from the Administration. Most important among the Republicans was the panel's chairman, lawyer and financier Benno Schmidt. Schmidt's political ties to his home state of Texas were concentrated among Sen. Yarborough's political opponents, and Yarborough's staff lobbied strongly against the appointment. But Yarborough himself still recalled favorably the talent and personality of his former student and colleague at the University of Texas Law School. In the interest of the great cause which he had pledged to support, the senator agreed to offer Schmidt the job.(24)

The Panel of Consultants delivered a report at the end of 1970 containing three principal recommendations. First, it proposed the establishment of a National Cancer Authority, independent of the NIH, to absorb the functions of the National Cancer Institute and be responsible by statute for the conquest of cancer at the earliest possible time. Second, it called for the development of a comprehensive plan for the conquest of cancer, but described it in language sufficiently ambiguous to avoid confronting the contentious issue of "targeted" vs. "creative" research. Finally, the panel recommended a rapid expansion of cancer
research from the approximately $200 million of the NCI’s current budget to a level of $800 million to $1 billion by 1976.

Yarborough, having lost his bid for reelection in the interim, received the official report as a lame duck in December, 1970. But without losing a step the baton was passed to Sen. Edward Kennedy, who took responsibility for transforming the recommendations of the Panel of Consultants into successful legislation when the new Congress reconvened in January, 1971. Although the presentation of the panel’s report attracted little publicity, it marked the beginning of a year in which cancer research was to be prominent on the national political agenda. Caught in the crosswinds of larger political forces, the push for legislation on cancer research soon escaped the control of the Lasker lobby. But the final result was President Nixon’s signature of the National Cancer Act of 1971 on December 23, bringing a renewed mandate, a newly elevated organizational status (but still within the framework of the NIH) and substantially increased funding to the National Cancer Institute.

Beyond the traditional struggle between Congress and the Administration over control of health research legislation, several new factors complicated cancer politics in 1971. (25) Despite the political damage resulting from the Chappaquiddick incident of 1969, Sen. Kennedy was still perceived within the Nixon administration as a serious
presidential contender for 1972, and Nixon was determined to stay out in front of Kennedy on an issue of such great popular interest as cancer research. Within the Congress, Rep. Paul Rogers, new chairman of the health subcommittee of the House Interstate and Foreign Commerce Committee, had his own ideas. Needing to establish himself in his new position, he had no interest in being railroaded into accepting the bill initially passed by the Senate, and associated so closely with Sen. Kennedy. Finally, the biomedical research community, at last awakening to the virtual certainty of new legislation, stirred itself to oppose those aspects of the Panel's recommendations which they perceived as threatening the viability of the NIH and its existing funding policy, dominated by investigator-initiated, peer-reviewed grants. As these players, along with the Laskerites, ably captained for this occasion by Benno Schmidt, maneuvered for advantage, the substance of the proposed legislation evolved until a final compromise emerged late in the year.

More important for this study than the details of the political endgame, however, are the implications of the entire process for the way research policy is made.

Rettig has pointed out the unusual role of a relatively small group of private citizens in setting the agenda for legislative action on cancer research.(26) In the present context, however, even more important than the identity of
the activists was the impact of their definition of the relevant alternatives on the political debate which followed. The Panel of Consultants asserted that new legislation was needed for cancer, and that cancer research needed a higher priority, and hence more money, than it had previously received. Yet while the panel's report provided no solid evidence to justify the proposed legislation or to support its claim that there were unusually promising scientific developments which would be brought to fruition by substantially increased funding, both propositions went unquestioned.

The early, feeble attempts of the Nixon administration to oppose new legislation failed to stop a bandwagon propelled by apparently authoritative expert opinion. The Administration operated primarily in a reactive mode, with no coherent policy of its own and no orderly procedure for developing one. As Rep. Rogers pointed out, "you can't fight something with nothing."(27) At the same time, the biomedical research community, poorly organized and unsophisticated in the ways of politics, was ill-positioned to address the substantive issues. To argue that money would not buy a solution to cancer would undercut their own claims for increased support, and for researchers in non-cancer specialties to object to the special emphasis on cancer smacked of selfishness in the face of a national disaster. Possibly as a result of political decisions taken
within the administration, scientists in positions of responsibility, such as NIH Director Robert Marston and presidential science advisor Edward David, chose not to dispute the scientific-opportunities claim, thus reducing the possibility of a careful critique. (28) In any case, such scientific opinion as was expressed tended to be anecdotal and impressionistic, and wholly inadequate as a counter to the boosterism of proponents of the initiative.

The political debate of 1971 focused largely on the question of whether an independent agency should be created to prosecute the war on cancer. Even on this question, the debate was characterized by unexamined assumptions about the past and future effects of alternative organizational arrangements, and was colored by the many contentious issues about funding strategy which lay underneath the surface. But the goal of obtaining a substantial increase in funding for cancer research was probably secured before the organizational autonomy debate was even joined.

Between fiscal years 1971 and 1977, appropriations for the National Cancer Institute increased from $233 million to $815 million. (29) The controversies over research priorities and funding and management strategies continued to simmer, though the magnitude of the increase in the NCI budget meant that virtually all of the parties to such debates were receiving large increases for their favored approaches.
Congress routinely renewed the National Cancer Act in 1974 with what one observer has called a "striking" lack of debate. (30) The Watergate hearings absorbed congressional attention, and there was a tacit, shared belief that it was too soon to expect results. Nevertheless, as part of the reauthorization, a temporary President's Biomedical Research Panel was created to conduct a broad review of the substance and the administration of research supported by the NIH and the National Institute of Mental Health. (31) The impact of the 1971 legislation was to be an important focus of the inquiry.

The panel's April, 1976, report was reminiscent of the Woolridge report in its conclusions. Once again, NIH was judged to be largely successful in pursuing its primary mission, which was understood to be scientific. On the NCI in particular, the panel concluded that "on balance the National Cancer Institute continues to serve the nation's interest well." (32) As for the many difficult questions about the impact of the National Cancer Act, observed Stephen Strickland,

... there was no deep or consistent exploration of such issues.... [The panel] concluded without systematic examination that the cancer program had not, as the original critics expected, caused problems in biomedical research policy, or in the organization of the federal effort against the whole spectrum of disease and health problems, or in the coherence and balance of the national medical science enterprise. (33)

In fact, while the budget of the National Cancer
Institute had soared, the budgets of the other component institutes of the NIH, with the exception of the National Heart and Lung Institute, had not kept pace with inflation. (34) The continuing strength of congressional support for cancer research was demonstrated in 1975 when an attempt by Senators Alan Cranston and Gaylord Nelson to cut $100 million from the NCI appropriation and $50 million from the NHLI appropriation and distribute $50 million of the savings to the other institutes of the NIH was easily squashed by subcommittee chairman Warren Magnuson. (35)

While congressional support remained strong, congressional oversight and guidance of the nation's biomedical research effort had become a pale shadow of its former vigor during the days of Fogarty and Hill. Sen. Magnuson maintained a reputation as an authority on medical research, and cancer in particular, deriving from his involvement as a young member of the House in the legislation which created the NCI. By the mid-1970's, however, the aging Magnuson had become lax in the fulfillment of his duties, oversight of NIH appropriations among them. In 1976, when the time arrived for the annual NIH budget review, a committee staffer, covering for the Senator, staged a phantom hearing. A hearing transcript was compiled, complete with written statements from the usual lineup of witnesses and simulated banter between Magnuson and the witnesses. In due course, the truth was leaked and
the staffer resigned. But the leadership void remained. (36)

There was certainly plenty to occupy any congressman who took an interest. Despite the optimistic report of the President’s Biomedical Research Panel, there were many questions which would have warranted active oversight as 1976 approached. Charges of mismanagement and poor scientific judgment dogged the NCI, and controversy continued to surround the industrial-scale, contract-based programs, especially the virus cancer program, which continued to swallow huge sums with little apparent result.

Science journalist Daniel S. Greenberg drew public attention in 1975 to the most important question of all when he argued that "after approximately two decades and several billion dollars expended on research for cures, official figures on trends in five-year survival rates do not, by any reasonable standard, provide grounds for optimism." (37) Greenberg argued that the staunch defense by the NCI and the American Cancer Society of the old but unchanging official line on cancer - said to be one of the most curable of the major diseases - and cancer research - said to be making slow but steady progress - masked an unwillingness to allow critical review of cancer programs. Noting that the lack of scrutiny helped preserve the status quo and shielded NCI from pressure to redirect resources to neglected but promising lines of research, Greenberg charged that

The vast and ill conceived undertaking that was created by the National Cancer Act of 1971 has
inevitably spawned a monolithic bureaucracy with a heavily supported public-relations apparatus that is simply misleading the American public on a dreadfully serious subject. That the intentions are sound and humane is no excuse for the snow job that is being performed by these tax-subsidized institutions.(38)

As the Bicentennial celebrations passed and the problem of cancer remained seemingly as far as ever from solution, the embarrassing congressional resolution of 1970 was conveniently forgotten. Indeed, many of the key actors of 1971 had adopted the cautious, qualified tones of the laboratory scientist. Benno Schmidt, writing in 1977 as chairman of the President's Cancer Panel, observed that there has been during this period an enormous extension of our science base and our knowledge as a result of the vast amount of highly excellent fundamental basic research that has been supported. But this extension of our knowledge only underlines how vast are the areas of ignorance which remain. Just as the past five years have brought a greatly enlarged science base, they have also brought important improvements in the clinic in dealing with cancer, but here again our progress only serves to emphasize how far we have to go....

The scientific and medical community and all of us connected with the program must continue to explain at every opportunity to the American people and to the Congress that the cancer program is a vast undertaking that will require long-term support and great patience. We are still far away from being able to put either a date or a price tag on the ultimate conquest of cancer....(39)

Writing a decade later, historian James Patterson observed that "by the late 1970's the alliance against cancer had lost some of the glow that had illuminated it from the 1950's through the debates over the war on cancer
in 1971. (40) Growth in the cancer effort had paused briefly during the late 1960's, before the war was renewed. Now, amidst the malaise of the Carter years, a more serious backlash took shape.

The attack, unprecedented in its strength and its openness, came from many sources, but was most powerful where professional critics tapped into broader trends in American culture. (41) Critics, Mary Lasker among them, had long charged that the cancer effort overemphasized basic science at the expense of cancer control. But while Mrs. Lasker and her allies had responded to this imbalance by attempting to fix the presumed mechanism whereby basic science was linked to clinical testing and ultimately to broadly disseminated curative techniques, a new generation of public critics rejected such tinkering, arguing that a new approach was needed. While massive efforts at finding a cure had yielded only modest gains, they charged, the enormous opportunities offered by preventive approaches were being systematically ignored.

Drawing on the popularity of the environmental movement, radical critics turned their rhetorical guns on many parts of the governmental and business establishments; the NIH and NCI did not escape their fire. Pointing to observed correlations between rates of cancer and certain "environmental" factors, these critics asserted that environmental contaminants - meaning man-made chemicals -
were responsible for a substantial fraction of cancer cases. Their solution was to redirect funding toward large-scale testing of chemicals in order to identify carcinogens, and to act on such findings via the regulation of American industry. Others focused on nutritional factors, and called for revitalization of the science of nutrition, trials of nutritional interventions, and even large-scale efforts to change the American diet.

The alliance had lost its glow - and "the political steam ha[d] gone out of the annual budget drive."(42) Carter Administration Secretary of Health, Education and Welfare Joseph Califano, preoccupied with runaway costs in the health insurance entitlement programs, believed that a war against cigarette smoking would have a far greater impact on cancer than additional billions of dollars for cancer research.(43) NIH Director Donald Fredrickson stated publicly that cancer research funding had reached an "optimal" level, and that the emphasis on cancer had come at the expense of balance in the nation's biomedical research program.(44) And the Congress was gingerly feeling out the implications of a new budgetary reconciliation process which, at least in principle, placed the full range of federal government programs in more direct competition with each other under an overall budget ceiling.(45)

From $815 million in fiscal year 1977, the budget for the National Cancer Institute increased by 7% for 1978, 6.9%
for 1979, and 6.7% for 1980, topping off at just under $1 billion, where it was to remain for the next four years. (46)

Congressional interest in the cancer program during these years of budgetary stagnation in part reflected trends in criticism by professionals and the interested public, as congressmen pursued their own interests and tried to anticipate and express the concerns of their constituents. In 1978, liberal Democrat Senator George McGovern, chairman of the Subcommittee on Nutrition of the Senate Committee on Agriculture, held hearings on nutrition and cancer research. McGovern had long urged the government to take a more active role in guiding the American people toward healthier diets. Now, together with an unlikely ally, conservative Republican Sen. Robert Dole, he blasted NIH and NCI for failing to adequately research the relationship between diet and cancer. (47) Interestingly, following opening statements by himself and by Sen. Dole, McGovern began the first hearing with testimony by Richard Rettig, who outlined some of the lessons from his study of the National Cancer Act of 1971. (48) The questions raised by Rettig would have made an excellent rhetorical starting point for a wide-ranging review of the fundamentals of the National Cancer Program. Instead, his appearance was used merely to introduce McGovern’s and Dole’s more narrow interest.

McGovern’s hearings were primarily an exercise in consciousness-raising, because the Agriculture Committee had
no direct influence over the NCI budget. More important for NCI was the interest of congressmen such as Rep. Andrew Maguire, who as an influential member of the House Health and Environment Subcommittee had a hand in the periodic renewal of the National Cancer Act, required by statute. Representing a heavily-industrialized area in northern New Jersey, Maguire expressed his concern over the relationship between environment and cancer by successfully sponsoring amendments to the National Cancer Act mandating an increased emphasis on programs to prevent cancer from occupational and environmental causes, including the issuance of an annual report on carcinogens. Maguire told the National Cancer Advisory Board that the amendments "reflected a feeling in Congress that [NCI] has tended to neglect the original concern of Congress with achieving, as rapidly as possible, some beneficial impact on public health."(49)

In 1979, Sen. Edward Kennedy called hearings before his Labor and Human Resources Subcommittee on Health and Scientific Research, with the stated purpose of general oversight of the efforts of the National Cancer Program, or, as the subtitle of the hearing transcript put it, "examination of the cancer program on its accomplishments to date, where it has succeeded, where it has fallen short of expectations, and why."(50) The first of two days of hearings was devoted primarily to a conscientious but inconclusive attempt by the Senator to absorb and digest a
presentation by officials of the NCI and the National Center for Health Statistics on the treacherous subject of cancer statistics. In keeping with hallowed tradition, the session included testimony by a panel of cancer patients and close relatives, some of whom took advantage of the occasion to present articulate appeals for the support of specific programs or areas of research. The key event of the second day was a quasi-debate between prominent representatives of the cancer establishment and some vocal critics, including environmentalist Dr. Samuel Epstein and Ralph Nader ally Dr. Sidney Wolfe. Those who attended the hearings could be forgiven for feeling as confused at the end as they had been at the start. But it is a measure of the importance of the National Cancer Program on the broader congressional agenda at the time that the Senate itself was represented only by Senators Kennedy and Richard Schweiker, with a briefer appearance by Senator Jacob Javits, who had co-sponsored the original Senate legislation of 1971 with Senator Kennedy.

The election of 1980 brought a Republican majority to the Senate and led to the elimination of the subcommittee from which Sen. Kennedy had dominated health care politics. Newly-arrived Senator Paula Hawkins, chairing a new subcommittee on investigations and oversight, announced her intention of investigating the NIH and the broader "cancer establishment," including the American Cancer Society: "We've spent a lot of money on cancer in this country. Why
isn't the cure around the corner a la polio?" (51) But neither Hawkins nor the two aides she asked to assist her in investigating the cancer program had any prior knowledge of cancer research, and the hearing that she called for May, 1981 turned into a lesson in the workings of the NCI, delivered by NCI director Vincent DeVita and members of the National Cancer Advisory Board. (52)

A hearing on the NCI called before the full Labor and Human Resources Committee by new chairman Sen. Orrin Hatch threatened to be a more serious challenge. But once again, questions about the nature and direction of the cancer program became peripheral issues as Hatch stayed in more familiar territory for a member of Congress - investigating allegations of fraud and abuse in NCI contracting practices. The bulk of the hearing was devoted to NCI's handling of the case of Marc Straus, a researcher at Boston University who had been accused of falsifying clinical data. (53)

The NCI budget resumed a path of slow growth after fiscal 1983, allowing it, roughly, enough to keep pace with inflation, but little more. In an attempt to rekindle enthusiasm and a sense of mission, the NCI announced in 1984 the specific goal of cutting cancer deaths in half by the year 2000. (54) But the announcement coincided with a waxing of the old debate about the meaning and validity of cancer statistics, and response to the NCI's announcement and to a more detailed strategy statement in 1986 was skeptical. In
a controversial paper, epidemiologist/biostatistician John Bailar renewed his charge that by reasonable statistical measures, the nation was losing the war against cancer.(55) Cancer scientist John Cairns asserted that the emphasis on curative chemotherapy was misplaced, and that "none of the important causes of death has been primarily controlled by treatment."(56) Along with others, they called for a redirection of research toward preventive measures; Cairns issued a stinging indictment of the U.S. government for failing to act more effectively against cigarette smoking.(57)

By the last half of the 1980's, Congressional involvement with the nation's cancer program had settled into a stable pattern. The appropriations committees continued to play their traditional role of increasing funding for health research beyond the equally traditional tight budget proposed by the Administration each year. Routine oversight during the appropriations process remained a perfunctory mix of superficial inquiries about general progress and specific questions about pet interests of subcommittee members.(58) The active leadership of Fogarty and Hill was a distant memory. Some of the initiative had passed back to the substantive subcommittees, for the anomalous freedom of the health research programs from continual reauthorization had been repealed, allowing activists such as Rep. Henry Waxman to take the lead in
micromanagement of the research effort. As during the early years of growth, the NIH would be directed to set up committees and direct attention to specific diseases; now this was supplemented more frequently than in the past by pointed inquiries to the various institutes and to the General Accounting Office about specific concerns.

But the pattern of Congressional involvement did not add up to a coherent policy with a strong sense of direction. While policy during the Fogarty-Hill era was equally fragmented in its details, it constituted in sum a strong push for steady, broadly-based growth in biomedical research. Today the National Cancer Institute behaves like a prototypically mature government program: too entrenched to face the risk of radical cuts, but too low on the Administration and Congressional agendas to stand much chance of resuming rapid growth during the Gramm-Rudman era.

At this writing, the chances for a Lasker-style coup to regain for the cancer effort the aura of crisis and opportunity which fueled a spurt of growth appear slim. While the NCI remains the largest component of the NIH budget, its share has declined to almost the level at which it stood prior to the passage of the National Cancer Act of 1971. The focus of political attention is on AIDS, which in little more than half a decade has grown from nothing to a $744 million dollar item in the federal budget. The NCI shares in the AIDS bounty, though the bulk of the funds go
elsewhere within NIH. But this fact simply underscores the diminished urgency of the war on cancer itself.

It may be true, as historian Patterson asserts, that "neither AIDS nor any other threat to health [has] as yet displaced the tenacious grip that cancer ha[s] on the American popular imagination."(62) But for the moment, at least, cancer has been thoroughly domesticated as a political beast.
Notes


2. The question of when and why American politics expresses the search for solutions to difficult social problems in terms of military metaphors is interesting in its own right. Such metaphors have been implicit in public anticancer educational efforts since at least the early part of this century, and became explicit in the publicity of the American Society for the Control of Cancer as early as the late 1920's. See James T. Patterson, The Dread Disease (Cambridge, MA: Harvard University Press, 1987), p. 91. Even the expression "war on cancer" itself considerably predates the National Cancer Act of 1971 with which it is often associated; see, for example, Patterson, The Dread Disease, p. 148.


4. Quoted in Strickland, Politics, Science and Dread Disease, p. 2.

5. Patterson, The Dread Disease, pp. 88-89.


7. See, for example, Strickland, Politics, Science and Dread Disease, p. 13.

8. Quote from Patterson, The Dread Disease, p. 136; see his discussion of the developments of 1937, pp. 114-136.

9. See, for example, Patterson, The Dread Disease, p. 130.


11. Annual appropriations for the NCI remained at around half a million dollars through the war years. It was not until after the end of the war, when Congress assumed an
active interest in the expansion of medical research, that the NCI budget was boosted, first to about $1.75 million, and then, for fiscal year 1948, to over $14 million, as described later in this chapter. For data on the early appropriations history of the NCI, see J.R. Heller, "The National Cancer Institute: A Twenty-Year Retrospect," Journal of the National Cancer Institute 19(2):147-190, August 1957.


14. Strickland, Politics, Science and Dread Disease, pp. 41, 47-49, 76.

15. See Strickland, Politics, Science and Dread Disease and, for a brief summary and overview, "Integration of Medical Research and Health Priorities," Science 173:1093-1103, September 17, 1971.

16. Strickland, Politics, Science and Dread Disease, pp. 91-94.

17. Strickland, Politics, Science and Dread Disease, pp. 154-156, 212-213; and "Integration of Medical Research and Health Policies."


20. On these developments, see Rettig, Cancer Crusade, pp. 56-72, and Patterson, The Dread Disease, pp. 195-198.


22. Strickland, Politics, Science and Dread Disease, p. 207.

23. Quoted in Rettig, Cancer Crusade, p. 82.

25. See Rettig, Cancer Crusade, for a detailed review and analysis of the events of 1971.


27. Quoted in Rettig, Cancer Crusade, p. 288.


29. Rettig, Cancer Crusade, p. 299.


32. Quoted in Strickland, Research and the Health of Americans, p. 20.


34. Rettig, Cancer Crusade, pp. 310-311.


36. The episode of the bogus hearing is described in Daniel S. Greenberg, "What Ever Happened to the War on Cancer?" Discovery 7(3):47-64, March 1986.


39. Benno C. Schmidt, "Five Years Into the National Cancer
Program: Retrospective Perspectives – the National Cancer Act of 1971," *Journal of the National Cancer Institute* 59(2 suppl):687-692, August 1977. Schmidt had been appointed by Nixon to head the three-member President’s Cancer Panel, which had been created by the National Cancer Act of 1971 and charged with responsibility to "monitor the development and execution of the National Cancer Program... and... report directly to the President...." See "The National Cancer Act of 1971 with Changes Made by the National Cancer Act Amendments of 1974," *Journal of the National Cancer Institute* 59(2 suppl):701-707, August 1977. Within a few years, Schmidt had managed to establish himself as "the most powerful figure in the leadership of the cancer program," and, in effect, "chairman of the board" of the National Cancer Program. See Rettig, *Cancer Crusade*, p. 297. The 1971 act also created a new National Cancer Advisory Board with a mixed professional and lay membership, to assume expanded authority for review of NCI grant programs and other activities from the discontinued National Advisory Cancer Council.

40. *The Dread Disease*, p. 256.

41. See Patterson, *The Dread Disease*, especially pp. 251-294.


43. Greenberg, "What Ever Happened to the War on Cancer?"

44. Greenberg, "'New Broom' at the Cancer Institute?"


57. In addition to the Bailar and Smith and Cairns papers, see Marie M. Cohen and Jared M. Diamond, "Are We Losing the War on Cancer?" Nature 323:488-489, October 9, 1986.

58. Recent transcripts examined for this review include Departments of Labor, Health and Human Services, Education, and Related Agencies Appropriations for 1984, Hearings before the House Committee on Appropriations, pp. 361-547; Departments of Labor, Health and Human Services, Education, and Related Agencies Appropriations for 1986, Hearings before the House Committee on Appropriations, pp. 231-431; and Departments of Labor, Health and Human Services, Education, and Related Agencies Appropriations for Fiscal Year 1989, Hearings before a Subcommittee of the Committee


11. THE RISE AND FALL OF OCEAN SCIENCE

Mr. HEYWARD [committee counsel]. ... Why does the President's budget describe NOAA under the title of "Science and Technology?" Are you limited to science and technology?

Mr. TOWNSEND [assoc. administrator, NOAA]. No, sir, we are not limited to science and technology, but the Department of Commerce, when it submits its budget, in brief, to the Congress, has usually included us in that category.

When we were the Environmental Science Services Administration, or rather when some of us were, that activity was underneath the Assistant Secretary for Science and Technology, in Commerce.

That is no longer true at the present time. I think it is just a convenience categorization.

We are more scientific and technical than we are anything else.

Mr. HEYWARD. I do not doubt that, but I think that maybe a change of image would be helpful to indicate that you are not restricted to science and technology.... The image might be better if the budget title did not give the impression that you are predominantly a science and technology agency....(1)

By comparison with high-energy physics, with its high-tech atom smashers and rip-roaring site-selection derbies, and the war on cancer, with its strong appeal to both our deepest fears and our sense of compassion, oceanography has for the most part been relatively invisible, both to the public at large and in political terms. Yet the federal government's involvement with the science of the oceans dates back almost to the earliest days of the republic, long
before the pursuits of the most fundamental secrets of matter and energy and of the living cell acquired their glamour and became fit objects of federal patronage.

The earliest oceanographic ventures of the government were consistent in the most literal way with the principle of supporting science for its practical returns, for they came at a time when it was generally agreed that the financial support of science was not within the limited range of powers which were allowed to Congress by the Constitution. However, the support of commerce, a less controversial rationale for government action, had already proved sufficient to call forth resources for the speculative expedition of Lewis and Clark. How much more, argued commercial interests of the seaboard states, were "the lives of our seamen, the interests of our merchants, and the benefits to the revenue... ample compensation for making a complete survey... at the public expense"!(2) In 1807 the Congress authorized the remarkable sum of $50,000 - twenty times the amount approved for Lewis and Clark - to launch the Coast Survey.

For a variety of reasons political, diplomatic and logistical, the Coast Survey did not become operational until 1832, and it was to be embroiled in confusion and controversy for yet another decade before it became genuinely productive. In the meantime, a second public agency destined to become involved in ocean science was
born. In 1830, the Secretary of the Navy authorized the establishment of a Depot of Charts and Instruments, to provide for the hitherto neglected care, storage and testing of the Navy's navigational instruments and charts. But it, too, had little directly to do with the oceans during its early years, which were devoted primarily to astronomical observations relevant to the calibration of instruments. (3)

The most spectacular of the government's early oceanographic ventures, and the one most explicitly identified with the pursuit of science, was the United States Exploring Expedition. In this case as in earlier ones, it was the pursuit of commerce, with a dose of national prestige added for good measure, that persuaded Congress in 1836 to appropriate $300,000 to cover the projected costs. In the event, the actual expedition of 1838-1842 came and went with little impact. Logistical confusion and mismanagement were sources of continual troubles; large portions of the collections of scientific specimens were ruined, lost, or given away; and the publishing program dragged on endlessly, generating a trickle of limited-edition reports, generally incomprehensible to the layperson. The interests of commerce may have spurred the expedition, but when Congress finally cut off funding for its still-incomplete series of reports in 1874, it was science that had earned a reputation as little more than an expensive headache. (4)
More productive were the scientific activities of the revitalized Coast Survey and Navy Depot of Charts and Instruments, both of which gained new leadership in the early 1840's. Both Matthew Fontaine Maury, at the Depot, and Alexander Dallas Bache, at the Survey, managed, on the one hand, to provide enough evidence of practical results to Congress to keep alive the flow of funding, and on the other, to carry on a remarkable range of innovative scientific investigations arising from their personal interest in and enthusiasm for the sea. Unlike Maury, a Navy man well-removed from the mainstream of science, Bache, who was both politically well-connected and one of the leading lights of the newly emerging scientific establishment in America, was particularly well-positioned to make the most of the opportunity afforded him by the directorship of the Coast Survey. During his tenure the Survey moved beyond traditional functions of map- and chart-making to carry out wide-ranging hydrographic and geomagnetic investigations, and even a smattering of biology and natural history in collaboration with the famed naturalist Louis Agassiz.

At their peak, appropriations for Bache's Coast Survey approached half a million dollars per year, a huge sum for those days. As historian A. Hunter Dupree observed,

While its tendencies in the direction of basic research never got completely away from the practical business of the Survey, they were sufficiently extensive to give some validity to
the claim that this potentially perfunctory operation was in reality the general scientific operation of the government.(5)

In fact, the Survey was "the largest employer of mathematicians, astronomers, and physicists in antebellum America."(6) But because the scientific activities of both the Depot and the Survey were largely surreptitious, they were dependent for their survival on the unique skills and personalities of the men who led them.

With the outbreak of the Civil War, Maury left for the Confederacy. And while Bache managed fairly successfully to adapt the activities of the Survey to wartime conditions, he became seriously ill toward the end of the war, and died in 1867. In 1866 the Depot was officially separated from the Naval Observatory and renamed the Hydrographic Office. Maury's systematic investigations of large portions of the sea rapidly decayed to an irregular series of minor surveys, and then disappeared entirely, leaving behind a small, routinized surveying and chart-making activity.(7) The Coast Survey had a residual scientific vitality under Bache's successor, Harvard mathematician Benjamin Peirce, but its oceanographic interests became diluted as the Survey increasingly took on land-based tasks until, in 1878, it became the Coast and Geodetic Survey.(8)

In 1885, Grover Cleveland became President. Returning to power for the first time since 1861, the Democrats were eager for their share of patronage jobs, and Survey director
J.E. Hilgard succumbed to the ensuing general hunt for
"corruption" and "wrongdoing." His replacement, F.M. Thorn,
was a crony of Cleveland and head of the committee which
investigated the Survey. The Survey's programs of marine
research were cut back and discouraged, with only Lieutenant
John Elliot Pillsbury's studies of the Gulf Stream surviving
as a last gasp through the end of the 1880's.(9)

But the federal government was not to withdraw entirely
from ocean science just yet. By 1870, there was much
concern about dramatic reductions in the annual yields of
fisheries, along with bitter arguments about cause and
effect between traditional fishermen using lines and their
more aggressive competitors using nets and weirs. Faced
with the possibility of being pressured into adopting
regulations which were bound to infuriate one or the other
half of this vocal constituency - if not its entirety - the
Congress found it convenient to adopt a proposal by Spencer
F. Baird for a new organization to investigate the nation's
fish stocks

with the view of ascertaining whether any...
diminution in the number of the foodfishes of the
coast and the lakes of the United States has taken
place; and, if so, to what causes the same is
due; and also whether any and what protective,
prohibitory, or precautionary measures should be
adopted....(10)

Not surprisingly Baird, a prominent naturalist who was also
director of the National Museum and assistant secretary of
the Smithsonian Institution, had other purposes in mind as
well. Frustrated by the limited and haphazard nature of marine biological investigations up to that point, Baird wanted to create a permanent bureau which could support extended, systematic study of biological aspects of the sea. In the hope of minimizing political interference, Baird included in his proposal a clause providing that the directorship of the new entity be unsalaried. President Grant offered him the job, and the United States Fish Commission was off and running.

Baird immediately launched a series of investigations, conducted largely during the summer months, which over a period of several years covered coastal regions from Maine to Connecticut. By 1881, plans were made for a permanent facility in Woods Hole, Massachusetts. Bearing in mind the government's desire to stay clear of basic science, Baird obtained substantial private gifts of land and money, impressing Congress sufficiently to elicit additional appropriations totaling $117,000 for a laboratory. In 1882, Congress also approved construction of the nation's first specially built research vessel for the Fish Commission.

The laboratory and the research vessel Albatross became magnets for the American marine biological community. Each summer, scientists and their graduate students came from universities throughout the east to take advantage of the unique facilities - and of the opportunity to have the resulting reports published at government rather than
personal expense. But a familiar pattern was to recur. Baird became seriously ill in 1886, and died in 1887. The lively research center he had built was to give rise to the private Marine Biological Laboratory, which carried on the tradition of basic scientific research at Woods Hole and, decades later, assisted in the birth of the Woods Hole Oceanographic Institution. But after Baird’s death, the Fish Commission itself was absorbed back into the mainstream of the government bureaucracy; the directorship became a regular, salaried position—a patronage plum like so many others. Already, by the year of Baird’s death, the Commission’s budget for hatcheries and fish culture was eight times that for research. Soon, statistics became another major responsibility. By the turn of the century, the press of routine responsibilities and the continuing insistence of the government on immediate practical results combined to remove the Fish Commission, as they had the Depot and the Survey as well, from any significant involvement in basic science. A half-century of active, if largely unintentional, government support of marine science was over. Until World War II, American oceanography was to be supported primarily by private institutions.

The Navy did have occasion to dabble in what could be called marine science during World War I. One of the most difficult problems facing the Allied powers was the great success of the German U-boats, for which the respective
natives were poorly prepared. Arranging shipping in the form of destroyer-escorted convoys cut some of the losses, but the overall impotence of naval tactics left science as the only hope for a definitive solution. In the United States, two efforts toward the development of methods for detecting submarines were pursued. One, based in Nahant, Massachusetts, under the jurisdiction of the Naval Consulting Board headed by Thomas Edison, built on pioneering work by the Submarine Signal Company of Boston. A competing effort launched by the National Research Council and based in New London, Connecticut, was eventually entirely taken over by the Navy.

A number of promising prototype devices were developed, with some hurried into production. But the war ended before their full impact could be demonstrated. The wartime efforts did lead to continuing work on an increasingly successful line of deep-sea echo sounding devices, in which the Navy maintained an interest. And in 1923, the Navy finally brought to fruition a plan that had been authorized during the war, but stalled by a siting dispute—the creation of the Naval Research Laboratory.(12) In some sense, the NRL was an expression of the Navy’s interest in science; Dupree has attributed to it "a certain esprit among its civilian scientists and a taste for fundamental work."(13) But the focus of the laboratory was not particularly oceanographic. Indeed, of all of the lines of
work pursued there during the years leading to World War II, the most often-cited is that which led to the development of radar, reflecting the Navy's primary concern - the development of technology directly and immediately applicable to the Navy's operational requirements. In any case, it was not a large operation, and was not in any sense a source of general-purpose funding for extramural marine science.

Certainly there were those, both in science and in government, who felt that the Navy had a prominent role to play in supporting the nation's oceanographic effort. President Coolidge himself, on the occasion of Navy Day, proclaimed that

(t)he depths of the oceans have been soured, the floors of the seas have been mapped, by the scientific specialists of the Navy. It is through such activities as these that the sea as a reservoir of food and other necessities of mankind, will ultimately come to be fully realized and exploited. The Navy has always taken a leading part and interest in explorations and in the studies of newly discovered regions, particularly in the Polar and Pacific areas....

(W)e may be sure that such services will continue to be multiplied in the future. We cannot doubt that they will continue to justify the maintenance of the full naval capacity of which we have agreed under the terms of the Washington Conference [Treaty for the Limitation of Naval Armament, 1922]. We may be sure that in the future as in the past the Navy's service to industry and the arts of peace and science will continue completely to justify its maintenance in the highest efficiency.(14)

The Coolidge administration was a time of consolidation, of fiscal conservatism, of withdrawal from international
entanglements, of disarmament - and the Navy was not immune to the wave of budget slashing. Yet here was the president himself seemingly offering science as a route to salvation for the beleaguered Navy.

The Navy Hydrographic Office moved to position itself for such a possibility, emphasizing the broader value of its services:

It is not alone of matters strictly nautical that the scope of research in the Hydrographic Office must take account. The demands of the cognate sciences require a conspiring interest on the part of marine hydrography to supply elements of knowledge derived from the field of oceanic observations as when the meteorologist looks to the oceanographer for the distribution of temperature in the sea; or the biologist for the physical properties by which the forms of life in its waters are influenced in their development; or the geologist and seismologist for the configuration of the oceanic basins and the conformation of the topography of their submerged tracts.(15)

On June 2, 1924, Acting Secretary of the Navy Theodore Roosevelt, Jr., who as Assistant Secretary had delivered the dedication address at the Naval Research Laboratory the year before, invited the heads of various federal agencies, as well as the Carnegie Institution, the American Geophysical Union, the Library of Congress and the National Research Council / National Academy of Sciences, to join in a Conference on Oceanography in order to plan a naval expedition for oceanographic research.

The conferees agreed that the primary purpose of the venture was to generate scientific results of practical
economic value, and proceeded to elaborate both a detailed scientific program and a list of benefits to be expected from it in the areas of fisheries management, weather forecasting, air and sea navigation, construction and maintenance of navigational channels and harbor works, ship construction, depth sounding, oil and mineral resources, and radio and cable communications. Nor were they entirely naive in the ways of politics. One of the participants emphasized the importance of selling the Midwest on the virtues of the project: "As the population of our coasts already appreciate, to some extent at least, the value of oceanographic work, emphasis must be placed on the importance of oceanographic research to the middle west, (which is) at present quite apathetic..." The expedition ought to begin in the Gulf of Mexico and the Caribbean because "anything done there can be explained in terms of personal economics to our middle western people, as far as the Canadian border..."(16)

But when it came to the more basic problem of interesting the Navy in taking up a new line of work, the conferees demonstrated that they had ventured into waters too deep. The principles on which they based their deliberations anticipated with uncanny precision the issues which would frame debates about the oceans forty and fifty years later when the role of ocean science in the Great Society and the interests of the nation in the international
Law of the Sea conference captured attention. The immensity and universality of the oceans; the enormous resources both biological and mineral that they contained, and mankind’s meager knowledge of how to exploit them; the need to turn to the sea to meet the needs of a population growing faster than the ability of the land to sustain it; and perhaps above all the long history of international conflict over the seas and the vision of a future made brighter by international cooperation in exploring and exploiting this common heritage of all nations: these were the lofty concepts which animated the 1924 conference.

Unfortunately for the assembled visionaries, the Navy had a narrower and less utopian view of its reason for being. Besides, temporary fiscal discomfort had been endured before, and hardly justified tossing out a century and a half of honorable tradition in order to send this proud service off on quixotic new pursuits. Roosevelt resigned following the arrival of a new Navy Secretary whose special interest was in the expansion of naval air power. The ship which was set aside to support the proposed expedition was redirected early in 1925 to land Marine reinforcements in Nicaragua, and then scrapped shortly thereafter. The grandiose plan evaporated, leaving hardly a trace.

The federal government did provide some informal, relatively feeble forms of support to marine science during
this era. A particularly distinguished example of fine oceanographic work done on a shoestring was the long, systematic series of studies of the Gulf of Maine carried out by Harvard zoologist Henry Bryant Bigelow. Federal support of Bigelow came by way of the Bureau of Fisheries, successor to the Fish Commission, which over approximately two decades repeatedly lent him one or another of its vessels for his cruises and published three monographs with the results of his work.(17)

After the proposed Navy expedition fizzled, an independent initiative did arise with a goal of bolstering funding for American oceanography and keeping it competitive with the better-organized European effort. Arrangements were made in 1927 for the National Academy of Sciences to appoint an advisory committee on oceanography in order to examine the potential of the field. In due course, a set of recommendations for expanding and strengthening the field was produced. However, the primary target of the recommendations was not the federal government, but the great private foundations, especially the General Education Board of the Rockefeller Foundation. The GEB responded favorably to many of the proposals, and provided substantial sums to support existing programs including those at the University of Washington and the Scripps Institution of Oceanography in California, as well as the extraordinary sum of three million dollars over several years starting in 1930
to establish the new Woods Hole Oceanographic Institution.

With the coming of war, oceanographic institutions received substantial infusions of funds to finance dramatic expansion of work relevant to the war effort. At WHOI, for example, a summertime staff of 60 expanded to a year-round staff of 335 to meet the needs of war research. Like other branches of science, oceanography was profoundly affected by the experience of the war. To a greater extent than many other fields, it changed not only in size but in substance, as the wartime emphasis on studies of immediate practical value to the Navy shifted the field away from marine biology and toward a greater emphasis on physical oceanography. As in other branches of science, expanded horizons made the prospect of returning to the old style of research after the war unattractive, even as inflation and the increased complexity of ships and instrumentation made such a retreat impossible on fiscal grounds in any case.

Comprehensive studies of the Bikini atoll in connection with the early postwar tests of atomic weapons kept many oceanographers busy for a time after the war, and there were other projects supported by the Navy as well. But the first relatively stable commitment to federal funding for marine science came from the Office of Naval Research.

Oceanography would seem to have been a natural focus for an Office of Naval Research. Yet, while ONR support proved to be the catalyst which launched other branches of
science on paths of steady and sometimes spectacular growth, oceanography remained a quiet backwater of research.

Several factors account for the postwar slide of oceanography back into relative obscurity. (19) First, only a tiny handful of the nation's scientists and engineers were concerned with marine science and technology, and this group constituted an almost static body; the first advanced degree in oceanography was not awarded until 1930, and even by 1958 only 13 out of 2,780 science PhD's had to do with the sea. (20) The entire structure of oceanographic research served to further, at minimal cost, the research programs of a limited number of senior scientists, with graduate education taking the form of unusually long, low-paid apprenticeships. As Edward Wenk notes, "unsympathetic critics of this system observed that the field was frequently steered by individuals infatuated with yachting who cloaked their love of the sea with more socially relevant purpose." (21)

Oceanography was not itself a well-defined, separate science, but rather the application of classical basic sciences such as physics, chemistry, biology, geology and mathematics to the study of a distinctive environment. Paradoxically, despite the inherently interdisciplinary nature of oceanography, the field was internally fragmented between professionals with classical science backgrounds, those with specialized marine science backgrounds, and those
who had begun their careers as generalists. A further split in professional identity arose between those specializing in fisheries and in more general oceanographic work.

A second type of schism arose over exploration philosophy: whether marine science was best organized in the form of comprehensive regional surveys or theory-driven research focused on particular scientific questions.

The oceanographic community was further splintered by institutional rivalries among the three dominant research centers: Scripps, Woods Hole, and Columbia University's Lamont-Doherty Geological Observatory. The resulting mutual disdain worked against any united efforts to promote the general cause of oceanography. Indeed, the possibility of new entrants to the field was often seen as a threat rather than an opportunity.

Mirroring the fragmentation of the oceanographic community was the organization of federal government involvement with the sea, which was scattered among a large number of bureaus buried deep within their respective cabinet departments, including most prominently the Navy Hydrographic Office and the Office of Naval Research in the Department of Defense, the Bureau of Commercial Fisheries in the Department of the Interior, the Coast and Geodetic Survey in the Department of Commerce, and certain research programs within the Atomic Energy Commission.

Finally, oceanographic research was virtually ignored
by those interests in the private sector which might have been considered natural consumers of the knowledge produced - the fishing industry and the merchant marine, both of which had already begun a steady decline in vitality and international competitiveness. Even the new and relatively dynamic offshore oil industry remained at arm's length, or beyond, from academic oceanographers suspicious of commercial motives.

Over the decade from 1948 to 1958, support for oceanography increased by only 50%, against a background of overall R & D growth of 500%; total funding in 1958, including support for quasi-scientific survey work, was less than $30 million. (22)

A report issued in 1949 by the second National Academy of Sciences Oceanography Committee had had little impact. By 1956, officials from several government agencies, seeking some way to boost their funding interests to a higher priority, joined to support a letter from the Chief of Naval Research to the NAS, asking that the Academy try again. NAS President Detlev Bronk, who had chaired the 1949 committee, was pleased to oblige. This time, the more-experienced Bronk assembled a committee with more careful consideration of political realities. While all three of the major oceanographic institutions were represented on the panel, the chairmanship went to a young and energetic outsider, Harrison Brown of Caltech. Sumner Pike, the Maine
businessman who had previously served on the Atomic Energy Commission, was initially resented as an overtly political appointee, but ultimately brought to the committee much-needed experience and sophistication in the affairs of government.

The launch of Sputnik shocked the nation a month before the official appointment of the committee, catalyzing a drastic change in the backdrop against which the committee was to work. But the marine sciences gained little in the initial burst of support for science. If particle physicists were the aristocracy of science, oceanographers were at the very bottom of the pecking order, disdained by most of the influential leaders of the scientific community for their antiquated methods and the primitive-to-nonexistent theoretical foundations of their work. If the committee's report was not to sink into oblivion like its predecessors, a new strategy would be necessary.

Instead of waiting for his nominal clients in the executive branch to react to the committee's findings, chairman Brown opened a behind-the-scenes sales campaign in the Congress even before the final draft was completed, with special attention to the Senate Commerce Committee and the House Merchant Marine and Fisheries Committee, which had jurisdiction over a number of federal agencies involved in marine sciences. The response from the Congress was surprisingly swift and positive. In part, the reaction was
a natural extension of "keep up with the Russians" fever to a new field, of seemingly obvious strategic importance, which had previously been neglected. Brown was also fortunate to have as his target audience committee chairmen who were inclined to respond favorably - Sen. Warren Magnuson of Washington and Rep. Herbert Bonner of North Carolina. In addition to serving constituencies with strong marine interests, Magnuson had long been an advocate of funding for various initiatives in science, while Bonner was searching for ways to strengthen the scope of his committee's jurisdiction, which was the most limited of all the committees which had emerged from the legislative reorganization after the war.

Since marine science affairs constituted a completely new area for the Congress, however, it would take some time for it to get the formula right. Early moves in the House were both fueled and complicated by intercommittee jockeying for jurisdiction. Hearings, speeches and bills proliferated, but little substantive legislation was passed. Nevertheless, it was clear that the Congress had taken the initiative in raising oceanography to a much more prominent place on the political agenda.

Within the executive branch, there was little response. The Navy had itself released a planning document entitled Ten Years in Oceanography on January 1, 1959, shortly before the NAS report was released. But high-level interest within
the Navy evaporated when the report proved ineffective in gaining support from the House Armed Services Committee. On the civilian front, President Eisenhower, who had already been pushed by Sputnik and a Democratic Congress into a far faster expansion of R&D than his usually cautious style would have allowed under ordinary circumstances, was not looking for any more initiatives to fund. With little independent knowledge of science, Eisenhower depended on his newly invigorated advisory apparatus to guide him, and as far as oceanography was concerned, the message was negative. The President’s Science Advisory Committee was dominated by physical science, and the traditional disdain for oceanography held sway. To make matters worse, the entrepreneurial maneuvering which helped the NAS report make such a big splash was not appreciated, especially insofar as support for more established fields seemed to be threatened.

Action on the part of the executive branch during the remaining years of the Eisenhower administration was limited to committee shuffling. A subcommittee of the new Federal Council for Science and Technology was created to deal with oceanography, and before long it was spun off as an independent Interagency Committee on Oceanography, with responsibility for coordinating activities among the many agencies involved in marine affairs, but no budgetary or policy authority. (23)

The Kennedy administration opened with a sudden boost
in funding for marine sciences which arose from the political imperative of offering the Congress specific proposals to back up a stirring inauguration address. But funding requests leveled off thereafter, revealing the absence of an underlying plan and raising the ire of Congress once again. Presidential science adviser Jerome Wiesner, who took a more activist approach to his responsibilities and maintained a closer liaison with Congress than had his predecessors, responded by moving to strengthen the Interagency Committee on Oceanography as a vehicle for articulating and coordinating marine science interests and gaining presidential support. Focusing on an agenda of building the infrastructure for an expanded oceanographic research effort, the ICO became an increasingly effective voice for marine science interests. By the end of the Kennedy administration it had elaborated a long-range plan which had an impact on the budget requests for FY 1964 and 1965.

But at that point, in the face of larger forces, the ICO ran out of steam as a mechanism for articulating a coherent long-term policy for ocean science. The post-Sputnik momentum in R&D funding had run its course, to be replaced by growing questioning of the societal relevance of investments in science and technology. As part of this questioning, the Congress was taking an increasing interest in the nature and effectiveness of executive branch
mechanisms for organizing and coordinating science efforts. Oceanography drew special attention in this respect, for both positive and negative reasons. On the positive side, the invigorated ICO was held up as an example of effective coordination of related work scattered among many different agencies. But when Congress began to demand more coherent justifications in policy planning, the fundamental weaknesses of the ICO mechanism, which lacked any formal authority in the realm of policy, became clearer. At the same time, the fragmentation of federal ocean efforts among many low-priority bureaus had been a key theme of ocean science advocates, starting with the NAS report. It was a theme which Congress picked up readily, in keeping with its traditional concern over "waste" and "duplication" in the executive branch and its more recent interest in the coordination of science. But as one congressional reorganization proposal after another for ocean science failed through parliamentary accident or more serious structural deficiencies, the level of congressional frustration rose steadily.

By the beginning of the Johnson administration, the future of the nation's oceanographic program appeared to be in stalemate. With the departure of Wiesner, the science advisory apparatus again closed ranks against proposed major initiatives in ocean science, temporizing in response to continuing pressure from Congress. At the same time,
Congress had yet to find a legislative formula for upgrading the priority of marine science affairs which the executive branch did not find disruptive for one reason or another. (24)

When the first session of the 89th Congress got underway at the beginning of 1965, however, it soon became clear that the balance of power had shifted decisively. Sen. Magnuson introduced a new, more carefully crafted bill to create a cabinet-level council for ocean affairs analogous to the existing space council; the statement of national purpose in the bill even borrowed language from the statement of policy of the space act. Members of the House, fearing that the council proposal invited a presidential veto because of the threat it posed to the independence of the existing agencies, especially the Navy, rallied around an alternative bill introduced by Rep. Paul Rogers. The Rogers bill would create a presidentially-appointed commission to undertake a comprehensive review of national requirements in the marine area and lay the groundwork for future steps, including the possible creation of a new oceanographic agency.

Science advisor Donald Hornig tried to head off the gathering momentum in Congress by forming a panel of the President’s Science Advisory Committee to undertake yet another review. By this point, however, the Congress was ready to bypass the science advisory apparatus entirely.
The Magnuson and Rogers bills, suitably refined in committee, were passed, and a joint conference committee succeeded in hammering out a compromise bill which provided for both a council and a commission, and addressed concerns of the Bureau of the Budget and the executive agencies by making both the commission and the council temporary (and from the agency point of view, presumably ineffectual). The simultaneous creation of two temporary and potentially conflicting mechanisms for policy development in marine affairs represented an odd compromise for a Congress which had focused so strongly on both the priority of ocean sciences within the executive branch and the problem of duplication and coordination in federal marine programs. But for those members of Congress who had fought long and hard to raise the profile of oceanography the prospect of victory at last was too much to resist, and the ranks of their supporters were swollen by new recruits attracted by the proposed Sea Grant College program, which promised to spread new federal funds widely as had the land grant program begun a century before. The bill passed (and was followed soon after by another creating Sea Grant), and despite the fears of marine advocates, Johnson not only signed it but moved promptly to implement it, showing due personal and professional regard for his friend and former colleague, Sen. Magnuson. (25)

The Marine Resources and Engineering Development Act of
1966 was a landmark in many respects. It is particularly noteworthy that the language of the bill represented an evolution from the earlier and more traditional focus of the NAS, the Congress and the ICO on improvement of the nation's science effort, to a broader mandate emphasizing applications of ocean science to the development of ocean resources and the management of the marine environment. As Edward Wenk was to describe it two decades later,

That the bill was directed to more than support for science was emphasized in title and text; technology was to connect marine resources to human need, with engineering playing a role as mediator. (26)

The vice-president, as chairman of the new Council, became the president's chief aide for oceanographic policy, replacing the science adviser and by extension the scientific establishment. This assignment by law of a specific task to the vice-president was itself quite unusual, and it was to have an unusual effect. If the forces within the executive branch which hoped to preserve the status quo in ocean affairs believed that with the shaping of the compromise bill they had succeeded, they were mistaken. Hubert Humphrey, an intelligent and energetic former senator who now was forced to deal with the constrained role of the vice-presidency, embarked upon his new assignment with enthusiasm. One of his first moves was to hire Edward Wenk as executive secretary of the Council. Wenk, an engineer-administrator who had been involved as a
key staffer in the shaping of federal marine affairs from both the congressional and executive sides, proved to be an ideal co-conspirator.

Under Humphrey and Wenk, the Council moved beyond the traditionally weak role of coordinating existing programs to identify unmet needs and propose new initiatives to address them. While the Council had no budgetary or policy authority over the operating agencies, its staff took skillful advantage of the Council's special cabinet-level status and of its implicit ability to appeal directly to the president through Chairman Humphrey on sufficiently important matters.

Knowing that the impact of the Council's work depended on its ability to gain Johnson's attention and approval, Wenk and his staff worked to develop proposals which would appeal to the President in light of his recognized interests and priorities. From the beginning, Humphrey made sure that the president was aware that the Marine Council was "evaluating problem areas where redirection or increased emphasis in marine science affairs would immediately contribute to broad national interests and to the Great Society concept."(27) Wenk's later account recalls some of the flavor of the effort:

... Johnson had said himself, "Next to the pursuit of peace, the greatest challenge to the human family is the race between food supply and population increase. That race is now being lost." The pointer spun to our Food from the Sea initiative to attack protein malnutrition. In the
area of urban decay, we argued in the first annual report that "urban development does not end at the water’s edge...." ... We searched for marine solutions in coastal management and pollution abatement. Potential programs in tune with Johnson’s interests were also examined in relation to underemployment and economic distress. Aspects to be considered were the problems of American fishermen; deterioration of the coastal economics [sic]; lost lives and property damage from coastal storms; long-range weather forecasting that could be improved to reduce the misery from unexpected and prolonged drought – a matter well understood in the heart of Texas. International cooperation in space had been one of Johnson’s proudest achievements in his influence on drafting of the Space Act, and it had a marine analogue. And, of course, the Sea Grant program was in harmony with Lyndon Johnson’s deep-seated interest in education.(28)

One further observation by Wenk is worth noting, for it touches directly on the question of what the Congress and the administration understood themselves to be doing through passage and implementation of the 1966 legislation:

One avenue we deliberately suppressed was bald support of science for science’s sake. That sector had found a warm reception in the intellectual ferment of the Kennedy administration, both because the need to strengthen our oceanographic research capabilities was so conspicuous, and because science was selling. But the broad rationale that science was good for you because its fruits of knowledge would eventually benefit mankind was tattered and torn.(29)

In its immediate context in Wenk’s memoir, this was a statement about Johnson, his attitudes toward science and his increasingly strained relationship with the scientific establishment. But as the evolution of oceanographic legislation in the years leading up to 1966 made clear, it was equally applicable to the Congress as well.

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During its brief heyday in the final half of the Johnson presidency, the Marine Council succeeded in raising ocean affairs to an unprecedented level on the national agenda. It was certainly not a top priority, but neither was it any longer virtually ignored by the executive branch. Several new ocean-related programs were launched, and existing programs were evaluated in terms of their roles in a larger overall picture of federal ocean activity.

Despite the aura of success, however, it is possible with hindsight to point to signs that the foundations being laid by the Council were neither deep nor solid. For the fiscal 1967 budget, finalized as the new Council was getting underway, the federal ocean program as defined by the Council in its annual reports gained a substantial boost in funding. But in succeeding years, the ocean program ran into the overall budget squeeze from which no discretionary programs were immune; the budget increases tailed off rapidly. In retrospect, the period of steady growth in federal funding for ocean programs was ending, not beginning, as the Council appeared on the scene.

Wenk has described the difficulties that the Council faced in trying to recruit a strong and vocal constituency from marine interests outside the federal government.(30) But the unpleasant truth was that the Council was not representative of any larger activist movement in the field. There were some new players, notably some aerospace and
high-technology contractors, who, eager to diversify in the face of cuts in defense and space spending, spied "a pot of gold at the end of the marine rainbow." But most of the ventures launched by these newcomers ultimately failed, the victims of unrealistic expectations.

Wenk and his Council staff exerted considerable effort in an attempt to elaborate a coherent "ocean strategy" which would provide a conceptual grounding for the nation's ocean activities analogous to that provided for generations of naval officers by the classic seapower doctrine of Alfred Mahan. But no magic formula was found.

As the Council was pursuing its task of coordinating and strengthening current marine programs, the presidentially-appointed Commission on Marine Science, Engineering and Resources proceeded with its own mandate of comprehensive review and long-term planning. At the completion of its work in January, 1969, it issued a multivolume report including well over one hundred recommendations covering virtually all aspects of marine affairs. Probably its most eagerly anticipated recommendations were those bearing on the organization and coordination of federal marine activities. The Commission addressed the matter forthrightly:

The Commission examined many alternative forms of Federal organization. We are convinced that a system relying upon coordination of organizationally dispersed activities, no matter how well administered, is not a substitute for a single operating agency having authority and
capability commensurate with the scope and urgency of the national ocean program....

The Commission recommends the creation of a major new civilian agency, which might be called the National Oceanic and Atmospheric Agency, to be the principal instrumentality within the Federal Government for administration of the Nation’s civil marine and atmospheric programs....

The Commission recommends that the... Agency be established as an independent agency reporting directly to the President.(31)

Here, at last, was the "wet NASA" for which many marine science advocates had long hoped, although the Commission had studiously avoided that particular analogy out of concern for the budgetary worries it would evoke.

The Commission never did have an opportunity to present its recommendations directly to President Johnson. From the perspective of NOAA proponents, it was just as well; it was soon recognized that Johnson would have killed the recommendation for a new agency because its planned inclusion of the Coast Guard would have threatened one of his own favorite initiatives, the fledgling Department of Transportation. Thus, all depended on the response of the incoming Nixon administration.

It became clear within the first six months of the new administration that Vice-President Agnew was both unable and unwilling to fill the vital role of Marine Council chairman as it had been defined and filled so effectively by his predecessor. The turnover of Council membership with the arrival of a new set of Cabinet officers and senior agency
staff dealt another blow. The effectiveness and vitality of the Marine Council began a precipitous decline which marked the end of its active life, although it would linger for almost another two years before it was officially terminated, in accordance with the original plan.

As for NOAA, it ran into an initial blast of opposition from the Budget Bureau, ever-fearful of a new, NASA-like entity, and the science advisory office, reasserting its traditional opposition to giving ocean science undue priority. The proposal was referred to the new President's Advisory Council on Executive Organization, headed by Litton president Roy Ash, which was created to consider the implications of a more far-reaching reorganization of the executive branch contemplated by Nixon. The NOAA concept was almost killed by the Ash Council. Only last-minute maneuvering by advocates in the executive branch and the Congress succeeded in conveying to Nixon the political cost of snubbing this particular Congressional aspiration. NOAA finally became a reality on October 3, 1970, as the National Oceanic and Atmospheric Administration within the Department of Commerce.

And with the creation of NOAA, marine affairs dropped once again from presidential notice. The cabinet-level Marine Council was gone, to be replaced in due course by a new Interagency Committee on Marine Science and Engineering, a rerun of the old-style, low-level coordinating council.
with neither authority nor visibility. NOAA itself as finally constituted did not consolidate all of the components of the federal ocean program. Besides Defense Department programs, which were expected to remain separate, important marine functions remained in Interior, State, Commerce apart from NOAA, the National Science Foundation, NASA, AEC and the Coast Guard (which had remained in the Department of Transportation). NOAA's own organizational chart looked more like a haphazard jumble of unrelated functions than a single, focused organization. To add insult to injury, the new agency's first administrator was a meteorologist, representing the atmospheric component of the assembled programs.

Above all, NOAA was by no means a science agency. Neither was NASA, for that matter, but behind the popular confusion between space flight and space science NASA did sustain a sizable program in space science. NOAA's science programs, on the other hand, were practically vestigial and were to remain so. The bulk of the nation's scientific study of the oceans was now supported by the National Science Foundation, taking over from the Navy, whose activities in ocean science proper had begun to contract by the late 1960's.

Over the decade from 1960 to 1970, ocean policy gained an unprecedented visibility on the national political scene and then receded into the background once again. Within
ocean policy, however, the decade marked a steady evolution, in which the Marine Council played a significant role as both follower and leader, from an almost exclusive emphasis on problems of marine science and technology to a primary focus on economic, legal, diplomatic and political issues.

By the middle of Nixon’s term in office, a new status quo had been established in ocean affairs, with some striking similarities to the pre-1966 situation. Ocean affairs in both the federal government and the private academic and industrial sectors remained fragmented and poorly coordinated. Congress had resumed sniping at the executive for failing to give ocean affairs adequate priority. NOAA notwithstanding, old proposals for the reorganization of federal marine activities continued to be dusted off and recycled in every conceivable permutation.(32)

It is revealing of the status of ocean science proper that when the Congress found occasion to actively pursue questions of ocean affairs beyond the continuing preoccupation with reorganization, it focused on such perennially contentious matters as fisheries management, offshore oil drilling and management of the outer continental shelf, coastal zone management, environmental protection and the United Nations-sponsored Law of the Sea negotiations. Lip service was routinely paid to the importance of scientific research in resolving problems in
these areas, but the real attention was always directed to more pressing economic, legal and political controversies related to the oceans.(33)

There has been no sustained attempt to kill scientific research on the oceans in the nearly twenty years since the demise of the Marine Council, although repeated attempts were made during the Reagan administration to terminate specific components of the federal ocean program such as the Sea Grant College Program and even NOAA itself. Instead, oceanographic science has been relegated to that backwater of stagnant funding reserved for inoffensive programs which have lost their places in the political spotlight.

Because of confusion and instability in the relevant definitions, it is extremely difficult to determine with any degree of precision the course of federal funding for oceanographic science, whether basic or applied, but distinguished from engineering development and routine service programs and operations. Nevertheless, a sampling of available statistics will convey some sense of the magnitude of the overall program. By one accounting, the "oceanography" component of the federal ocean program reached a peak in real terms in the early 1970's, declined somewhat and then plateaued for the remainder of the decade, then dropped to a lower plateau through the first half of the 1980's at a level similar to that of the mid-1960's.(34) For fiscal year 1985, the Ocean Science program of the
National Science Foundation had a budget of approximately $120 million, which, according to the program's director, represented "about half of all ocean research conducted at U.S. universities and about 70% of the nation's basic academic ocean research."

(35) In a response to Congressional questioning, the Office of Naval Research stated that FY 85 funding for the Navy's "basic research ocean science program" amounted to about $85 million. (36) Other programs within NSF and programs in other agencies contributed lesser amounts. For FY 88, the NSF's Ocean Sciences program was budgeted at $135 million. (37) NOAA, primarily a service rather than a research agency, had total FY 88 obligations of about $1.28 billion, including a budget line of about $140 million for "Oceanic and Atmospheric Research." (38)

By the end of the 1980's, ocean science advocates looked with renewed optimism to the prospect of associating their work with a compelling national interest: the resurgent environmental movement. Oceanographers and meteorologists had long appreciated that global weather patterns were the manifestation of complex processes influenced by tight linkages between oceanic and atmospheric systems. With the rise of concern over possible global warming, oceanography's chances of returning to the public spotlight appeared greater than at any time in the preceding two decades. But the truest measure of federal support
remains the budget. Whether public attention can be sustained and translated into federal dollars for ocean science in the age of Gramm-Rudman remains to be seen.
Notes


15. Quoted in Nelson, "Oceanographic Prescience..."

16. Quoted in Nelson, "Oceanographic Prescience..."


23. The work of the NAS Committee on Oceanography and the responses it elicited within Congress and the administration are discussed in detail in Wenk, *The Politics of the Ocean*, pp. 39-66.


27. From memorandum from Humphrey to Johnson, quoted in Lambright, *Presidential Management of Science and Technology*, p. 34.


When I became a federal science administrator, I was surprised.... There seemed to be no canons or even craft rules giving guidance on what constituted reasonable and legitimate analysis and advice to decisionmakers. In contrast to the doing of science, the doing of science and technology policy was casual. There were no standards for debate or argument. The most bizarre kinds of reasoning and the weakest kinds of evidence were offered in support of action recommendations. Scientists, engineers, and university administrators offered views and made assertions that could not pass minimal standards of rigor... Yet action recommendations were seriously offered, passionately defended, and sometimes followed.

Harvey A. Averch(1)

The substance of science is unique to science. But the politics - except to the extent that it is flavored by the peculiar traditions of science, is not. Science, like agriculture, the military, labor, business, or the civil rights movement, has its vested interests, elites, downtrodden, alliances, bosses, loves, and hates. The politics of science is in essence no different from other politics. It is a bit more obscure and, it seems to me, usually better mannered, though perhaps it would be appropriate to borrow a line from Wordsworth and describe it as "Nor harsh, nor grating, though of ample power to chasten and subdue."

Daniel S. Greenberg(2)

It is generally agreed that funding for science in the United States is driven primarily by the expectation of practical benefit. Yet, as I pointed out in my introduction, some areas of science which promise little practical payoff have fared remarkably well in their quests
for support, other fields which have never delivered on their promise continue to attract generous funding, and certain programs which seem to offer great practical benefits remain comparatively poor.

In this study I have reviewed the history of the American programs in high-energy physics, cancer research and oceanography, focusing on the politics of the financial relationship between these programs and the broader society, represented by its agent, the federal government. The key finding of this study, contrary to what a superficial interpretation of "pragmatism" might suggest, is that the relative success of different branches of science in the competition for funding depends in large measure on institutional and political factors only indirectly or not at all related to the substance of science and its practical applications. If American science policy is driven by pragmatism, it is a "pragmatism" that deals in faith and symbolism, rather than in the matter and energy of the physical world.

Yet the common wisdom about the force driving science policy is not entirely wrong. Rather, it is only slightly misplaced. The historical record shows that the present system of allocating money for science in America is a success in political terms, offering substantial benefits at relatively low cost to those politicians associated with it.

On the surface, every large scientific program appears
to be funded for pragmatic reasons. This appearance is rarely, if ever, the product of conscious deception. Congress and the administration genuinely and continually strive to understand science programs and to justify support for them in terms of compelling national goals.

Perceptions and assumptions about the contributions of high-energy physics to the availability of technical manpower, to relevant basic knowledge, and to possible new superweapons played a key role in its establishment as a cornerstone of the nation’s science effort over the period from the end of the Second World War through the Kennedy administration. With subsequent changes in public opinion and in geopolitical realities, this rationale has lost its public luster, but it refuses to die.

The Reagan administration linked high-energy physics to the nation’s international economic competitiveness, and much of the debate in connection with the SSC project has been over the extent to which Congress and the interested public believe in the claimed relationship. The long-term effectiveness of this rationale for particle physics remains to be determined.

Underlying the other substantive rationales and serving effectively in its own right during the times when there was no crisis to which high-energy physics could be readily related, has been the field’s perceived role as an essential part of the nation’s scientific infrastructure, and thus as
an indirect contributor to every aspect of the nation’s strength that is believed to depend on a healthy scientific effort: defense, economic growth, education, national prestige - even culture and moral fiber.

The importance of a cure for cancer is obvious to all. The old saw about how you can’t vote against a cure for cancer may account for a certain legislative sloppiness tolerated in the name of medical progress, but it also reflects strongly-held beliefs about the most basic needs and interests of constituents and the power of government to do something about them. Once the question of whether government was to be involved at all in matters of science and health was definitively settled in the affirmative, federal involvement with cancer research was inevitable, though the shape and magnitude of the involvement was an open question.

The study of the oceans has been closely associated with pragmatic goals from its very beginnings, and American scientists with interests in the oceans learned to couch their fundraising efforts in such terms long ago. During the oceanographic revival of the 1960’s, the power of appeals to concrete national goals to sway Congress and the President when the inherent virtues of science alone could not was well demonstrated. Preserving naval strength, improving navigation, nurturing fisheries and feeding the hungry third world, exploiting mineral and petroleum
resources, preserving the environment — these were among the (sometimes competing) goals which gained new life for ocean science.

But if every large scientific program is associated with pragmatic rationales, then other forces must be at work as well. The three programs studied here have had substantially different fates. Evidently, not all pragmatic rationales are equally effective at all times. I have referred to the fates of the three programs as "anomalous." Evidently, the political process selects and evaluates facts about different areas of science according to a distinctive logic, generating judgments and conclusions which will not always be consistent with a naive conception of "rationality."

What does history tell us about this "distinctive logic"? How does this process work? In seeking to explain the differences among the three programs, I have found that the historical details point to several important factors. These factors are interrelated and tend to feed back upon each other in complex ways, so that any attempt to separate them is necessarily somewhat artificial. Nevertheless, the exercise can contribute substantially to our understanding.

Decision-makers in Congress rarely interact with science directly. Rather, what they know of the achievements and potential of science is what they learn, either through direct personal contact or through staffers,
colleagues or other trusted advisors, from the scientific community. Accordingly, the first step in understanding how science policy is made is to understand the interface between Congress and the scientific community.

On one side of this interface is the Congress. Relatively few members of the House and the Senate deal on a regular basis with affairs of science. Both houses are divided into committees and subcommittees in order to fulfill their responsibilities in program budgeting and oversight. Such specialization is a necessity given the enormous volume of business which the Congress must transact each year. A corollary of specialization is the standard practice in each chamber of deferring to the recommendations of each committee in the areas of its jurisdiction. This deference reflects both mutual respect and the recognition that constant challenges to committee decisions would result in legislative paralysis.

In some cases, the interests of individual legislators or the pressure of legislative work lead to the development of an additional level of specialization within the committee, so that individual members tend to concentrate on specific areas within their substantive jurisdictions. In practice, specialized members, especially those who have built political clout via seniority or otherwise, will carry a substantial degree of authority in determining the fate of legislation relevant to their interests. Other legislators,
who have little knowledge of the issues in question, will allow their votes to be guided by the specialists, knowing that when the situation is reversed, their own expertise will be accorded similar deference.

Nevertheless, it is rare for members of the House and Senate to have any significant scientific expertise. Legislators who are not economists or sociologists still feel competent to argue and pass judgment on welfare policy, for example, but rare indeed is the legislator who is prepared to dispute expert opinion on an issue that has been cast as a "scientific" problem, or to dispute a "specialist" legislator in a position of authority who vouches for the reliability of such opinion.

Infrequently, key committee members will retain their congressional seats and committee assignments for long enough to allow them to acquire a respectable grasp of the technical issues with which they must deal. Perhaps the outstanding example of this was the core membership of the Joint Committee on Atomic Energy, which through more than a decade of close attention and hard work acquired a reasonable understanding of what particle physics was about. By the mid-1960's the JCAE was no longer easily intimidated by the aura of authority of a distinguished scientist. To a certain extent, Sen. Hill and Rep. Fogarty acquired specialized expertise in evaluating medical research over the course of their long tenures; they made especially
skillful use of the appearance of expertise on the part of committee members and carefully-selected witnesses in order to promote a specific agenda of advocacy and decision-making. Even in the past, however, these were unusual cases. In recent years, because of more rapid turnover in committee jurisdictions related to science and technology and in the memberships of such committees, the likelihood of developing substantial technical expertise on the job has decreased still further.

It is important to understand, however, that advanced scientific training, as such, is no guarantee of superior ability to deal with issues of science policy within the context of the legislative process. For one thing, science itself is so specialized that a biologist, for example, would be hardly any better equipped than an intelligent layperson to evaluate the state of high-energy physics. More importantly, in operational terms, specialized expertise in Congress means not superior knowledge in a strictly technical sense, but a functional blend of comparatively greater knowledge with political skills and position within the Congressional hierarchy which is effective in influencing voting patterns. Thus while Republican Don Ritter, an SSC opponent, might understand enough about superconductivity to discuss it intelligently with scientific witnesses during a hearing, it was non-scientist Robert Roe, Democratic chairman of the House
Science Committee, who had the effective authority in 1989 to cut deals and sway votes on behalf of the SSC.

But just as legislators defer to their resident specialists, those specialists must themselves defer to expert professional opinion whenever there is no firm basis upon which a layperson could arrive at a judgment. What remains is a decision about which experts can be relied upon for trustworthy advice. In the realm of science policy, inasmuch as the details of science are incomprehensible to the layperson, the only people who are thought to qualify as experts are the scientists themselves.

When legislators have sought expert opinion about science, what have they been told? To understand the answer to this question we must look first at the other half of the interface between the Congress and science: the scientific community, and in particular, those parts of the scientific community that have served as counsel to the Congress and the higher ranks of the executive branch. This distinction is important, because the scientific community is highly differentiated internally and does not always speak with one voice on matters of policy. What politicians learn about science is in part a function of which sectors of the scientific community manage to position themselves within hearing range. And this positioning in turn has been the result of complex interactions between chance historical developments both inside and outside of science and the
internal dynamics of the scientific profession.

As I pointed out in chapter 9, high-energy physics has for the better part of the twentieth century occupied an exalted rank in the implicit sociological hierarchy of science. This status was both strengthened and made effective in the context of policy by the events of World War II. As is widely recognized, the war served in a general way as the key event in the breakdown of both the government's reluctance to spend large sums on science and scientists' reluctance to be dependent on such funding. But the special role of physicists in general, and nuclear physicists in particular, in the scientific elite which was responsible for the atomic bomb, placed the physicists in position to shape the consequences of that momentous change.

The assumption of a key advisory role by leaders of the physics community was not so straightforward as might be imagined. The simple version of the familiar story has a grateful nation and government recognizing the value of science in light of such extraordinary achievements as the atomic bomb, and hastening to provide the necessary support to a balanced range of basic and applied science. In fact, while the topic of how the government should support science did indeed become prominent on the national political agenda, the main debates on the subject raged inconclusively for years. The presumed vehicle for a coherent policy of support, the National Science Foundation, was not created
until 1950 and remained largely irrelevant until a full
decade after the end of the war. Even after it had begun to
function effectively, its role was peripheral, filling in the
gaps left by the large programs in the mission agencies.
While confusion reigned, however, the outlines of a national
science policy were being created behind the scenes, on an
ad hoc basis.

Early postwar support for particle physics itself was
certainly closely related to the role played by physicists
in the atomic bomb project. But the players who mediated
the transition from federal support for the bomb project to
federal support for basic particle physics operated on a
relatively low level, and followed their own agendas -
General Groves taking unilateral action to preserve a
nucleus of hard-earned scientific capability in the midst of
postwar chaos, and the Office of Naval Research operating
first in the pursuit of Admiral Bowen's agenda and then as a
relatively invisible, free-lance research agency.

Later, when continued funding for high-energy physics
finally became a matter for Congress and the executive
branch to consider in the course of normal business,
widespread lay perceptions of the general role of physicists
in the war effort and of the specific link between particle
physics and possible new weapons influenced the decisions
made. Such perceptions, along with the close personal ties
between the weapons research community and many academic
physicists, led naturally to domination of the nascent
science advisory apparatus by physicists. When the advisory
system was elevated to a direct interface with the
presidency in the wake of Sputnik, the ascendancy of the
physicists as policy spokesmen for the scientific community
was complete.

The influence of the particle physicists' status within
the scientific community and the political system on
government support for their field has been profound and
long-lasting. Of course, during the heyday of the
presidential science advisory apparatus, physicists lobbied
effectively for their own cause. But over the years, most
scientists from across the range of other specialties as
well, themselves committed to a reductionist approach to the
understanding of nature, have readily affirmed the
fundamental status of elementary-particle physics, implying
and sometimes even saying outright that all other sciences
depend on its findings. Until recently, even the occasional
dissenter usually conditioned his opposition on funding
limitations, avoiding any challenge to the inherent merit of
the pursuit. More vocal opposition began to surface during
the long gestation of the SSC project, though scientific
witnesses testifying more or less in favor of the program
still vastly outweigh confirmed opponents at Congressional
hearings.

The unusual status and role of physicists has not only
served to promote the interests of high-energy physics and other branches of physical science, however, but to suppress less-favored fields as well. If the high-energy physicists were the aristocrats of science, then the oceanographers were at the bottom of the pecking order. As in the case of the particle physicists, the status of the ocean scientists reflected historical accidents of both intellectual fashion in science and the sociology of the oceanographic profession.

Oceanography had from the start been a fuzzily-defined, interdisciplinary blend of basic and applied science, with only a partial and quite limited foundation of mathematical theory. While the war resulted in some changes in emphasis within the field, there were no dramatic breakthroughs to draw public attention to the achievements of ocean science and to earn for ocean scientists the opportunity to shape policy at a high level.

Yet at the least, changed attitudes about the role of the government in funding science offered unprecedented opportunities for growth in oceanography as in other sciences. But rivalries within the small oceanographic community prevented the unity needed to take advantage of the situation. When Admiral Bowen disappeared from the scene, leaving behind an Office of Naval Research with a relatively free hand in the funding of research, an aggressive and well-coordinated oceanographic community
might have worked through the ONR to cultivate a special relationship with a powerful naval patron. Whether such a bid would have succeeded is an open question, for the Navy typically concerns itself with only those branches of ocean science which it perceives as immediately relevant to well-defined operational needs, which are dominated by two themes: safe, accurate and efficient navigation for all ships, and the special requirements of detecting the enemy without revealing one’s own presence in the course of submarine operations. But the attempt was never made. While ONR did serve as the most important source of funding for ocean science in the years immediately following the war, control of the agency and its agenda was left to the physicists and their allies. By the time the oceanographers managed to launch a serious bid for increased federal attention in the late 1950’s, the dominance of the physicists as science advisors was so complete that the ocean advocates felt compelled to approach Congress directly, behind the scenes, in order to circumvent it.

The relationship between Congress and science in the arena of medical research policy has worked differently. Here, the most important interface was that between Congress and certain segments of the medical profession, as mediated by an active lay lobby which helped shape the lines of communication between legislators and medical scientists. While support for pure science has always been treated by
the political process as advancing a continuum of progress through applied science and into technological applications, in medical research the "applied" part of the continuum, medical practice, has from the start been foremost in public awareness. Within medical research, the hierarchy of Congressional interest among specialties has closely reflected public concerns, notably in raising cancer to the highest rank. By contrast, within the medical research profession, specialists in laboratory biology have often tended to look down upon their colleagues in clinical research and practice in much the same way that physicists condescended to oceanographers. Among medical research specialties, cancer research has often been viewed as one of the less-reputable fields. And finally, even within cancer research, prominent clinical scientists often resisted the idea of government forcing the pace of research through massive funding.

In some respects, then, the relationship between Congress and cancer science was an inversion of the relationships observed in other areas of science. Congressional activists, in collaboration with a vigorous and effective lay lobby and cooperative elements within the medical community, fueled the rapid expansion of medical research in general and cancer research in particular. In many ways, the role of the basic biomedical scientists was reactive. Once the trend in government support was
apparent, the basic scientists, ably led by James Shannon, moved to shape the system in their own interests. By and large they have been successful in doing so. As a result, however, the tension between the goals of basic scientists and the very practical ends sought by the lay lobbyists has resulted in the kind of periodic crisis which is never seen in a field like high-energy physics, which is driven from within by the scientists themselves and has no significant external constituency demanding and expecting timely practical payoffs, or oceanography, whose natural constituencies in fields such as the merchant marine or commercial fishing have for reasons of their own proven weak and uninterested in science.

Thus what Congress "knows" about science is what it learns primarily through selective and highly filtered channels of communication with the scientific profession. Interpreting this knowledge in light of their own varied interests and (overwhelmingly nontechnical) backgrounds, members of Congress develop their own judgments as to which scientific opportunities merit substantial government support.

But science is far from the only claimant for a share of federal resources. Every special interest has its own wish list to sell to Congress and the administration. The second step in understanding science policy is to consider the nature and implications of the competition among
programs - science and non-science - for funds.

Here, too, the situation is complicated by the irregular partitions imposed by institutional factors. Just as Congress does not "see" science in unedited form, the competition among different programs does not take place on an undivided, level playing field. Exactly where a program settles within the huge, complex ecosystem of the federal government bureaucracy can have a substantial impact on its fiscal fate. In considering this factor, however, it is important to recognize that what matters in an operational sense is not a program's position on the organizational chart per se, but with which other programs it is grouped - and thus, which it must compete with directly - for purposes of budgeting.

The bulk of the nation's high-energy physics program ended up in the Atomic Energy Commission, institutional successor to the Manhattan Engineer District. Particle physics benefited doubly from this placement. First, the General Advisory Committee of the AEC was dominated for some time by physicists. By the 1960's, when the influence of the GAC had faded somewhat, the chairmanship of the AEC was held by Glenn Seaborg, an alumnus of Lawrence's Radiation Lab.

More important over the long run from a budgeting perspective, however, was the status of high-energy physics as a small program within a large, powerful agency. For a
decade and a half, AEC's role on the front lines of the Cold War absorbed the attention of policymakers. Nuclear weapons and the many conceivable applications of nuclear reactors in war and peace were the stuff of headlines; if a small, esoteric scientific program, which in any case might someday provide the key to a new superweapon, tagged along for the ride, who was to quibble?

Of course, for all of its inertia, the federal bureaucracy is not immune to change. By the end of the 1960's, high-energy physics found itself a medium-sized program which threatened the stagnant budget of its parent agency. With the creation of the new, much larger Department of Energy, the particle physicists were off the hook. Nevertheless, there are no guarantees that overall budgetary stringency and the demands of other, less-controllable programs in the Energy Department will not at some point make high-energy physics an inviting target for budget-slashers.

Medical research has undergone an interesting transition in its bureaucratic status over time. During the two decades of vigorous growth following the war, medical research constituted a substantial portion of the federal government's involvement in matters of health. But as the only channel of support for health open to politicians during the years of stalemate over government involvement in the financing of health care, medical research pulled its
own political weight.

The creation of the Medicare and Medicaid programs in 1966 and their rapid growth thereafter changed the situation dramatically. Within a few short years, medical research had receded to a new status as a minor appendage on a huge health care financing system. While the pressure of medical cost inflation has limited the potential for rapid growth of the kind enjoyed through the mid-sixties, the tiny fraction of overall health care expenditures now accounted for by medical research makes it an unpromising target for congressional cost-cutters.

Among the specialties within medical research, cancer research has also for much of its history carried its own political weight. Only in the mid-seventies, after the sharp increase in funding brought by the National Cancer Act of 1971, did the share of the National Cancer Institute within the budget of the National Institutes of Health become so disproportionate as to induce a backlash, and the balance has been redressed not by significant cuts in the NCI, but slowly and through compensating growth in the other institutes of the NIH, with a special assist from the growth in AIDS research, which has been dominated by the National Institute of Allergy and Infectious Diseases. This development does serve warning that the continuing visibility of the cancer program within NIH may make it a prime target for funds to redistribute should budgetary
stringency coincide with relative quiet on the cancer research front and compelling demands for response to new developments on other medical fronts.

Oceanography has been affected by bureaucratic factors in an entirely different way. While particle physics was able to thrive under sympathetic leadership in a relatively affluent department, oceanography failed to assemble the critical mass of bureaucracy needed to attract attention. In the words of Edward Wenk,

those governmental agencies whose specialized responsibilities drew on marine science were small, low in the hierarchy, unnoticed in the public press, and while quietly doing their job were unable to muster internal support. (3)

This was in part historical accident, arising from the fact that narrowly-defined ocean-related tasks had been assigned on an ad hoc basis to new or existing federal bureaus starting long before large-scale federal support for research and development became a reality, whereas the National Cancer Institute and the Atomic Energy Commission had been created with mission-related R&D as a primary task. The historical development of bureaucratic fragmentation was in turn partly a reflection of the fragmentation of constituencies with ocean-related interests.

The highly-developed bureaucratic structure of the executive branch is reflected by a corresponding structure in Congress. Mirroring the departments and bureaus of the executive branch are the committees and subcommittees into
which the House and Senate are divided in order to fulfill their responsibilities in program budgeting and oversight. Just as a program's chances in the competition for support depend on where it is located within the executive branch, so they also depend on which congressional committees bear responsibility for its authorization and appropriations.

Thus, for approximately its first three decades after the war, high-energy physics was the responsibility of the Joint Committee on Atomic Energy, which for much of its existence was uniquely powerful. Just as particle physics was for many years too small a program to have much of an impact on the budget of the Atomic Energy Commission as a whole, it was similarly too small a program to be worth much attention from the JCAE, and once it passed the JCAE, it was more or less home free. Only when general fiscal circumstances conspired to render the high-energy physics program a potential threat to other AEC programs favored by the JCAE did particle physics become the object of close committee scrutiny.

The committee structure of Congress played a special role in the growth of cancer research. The anomalous wartime continuing authorization for research within the Public Health Service focused power and authority on the Senate and House appropriations subcommittees chaired by Sen. Hill and Rep. Fogarty, and they used that power effectively on behalf of medical research.
Again, oceanography has suffered by comparison. Where numerous small programs have been scattered through the executive branch, numerous small program jurisdictions have been scattered through the congressional committees and subcommittees. The most important committees in marine affairs have been the Senate Commerce Committee and the House Merchant Marine and Fisheries Committee. The former had and has plenty of responsibilities beyond ocean science to keep it occupied, while the latter has never been a bastion of power within the House. The House and Senate Armed Services Committees have, if anything, tended to be suspicious of the Navy frittering away scarce resources on extracurricular activities such as basic science.

An additional factor which affects the competition among all large programs for political support is the distributive consequences of big spending on any program, regardless of its substance. Science can help fill the pork barrel just as non-science does, so it, too, is affected by the never-ending struggle for geographic equity in federal spending.

Pork barrel dynamics have had a major impact on the high-energy physics program at least twice. During the mid-1960's, the opening of the proposed 200 BeV accelerator project to a competitive site-selection process helped build a broad base of congressional support. That support, along with Johnson's implicit promise to attend to geographical
equity in the wake of his negative decision on the proposed MURA accelerator, created a situation in which the political cost to Johnson of backing out of the 200 BeV project became too high to tolerate, regardless of what the president may have felt about the merits of committing more than $200 million for an accelerator at the time.

The site-selection process for the SSC helped keep the SSC R&D program alive through the 1980's, and when congressional support threatened to collapse after the award of the project to Texas, SSC project managers in collaboration with the Texas congressional delegation built a powerful lobbying effort around the projected economic impact of the numerous and broadly distributed subcontracts which SSC development and construction would generate.

From the start, extramural funding for medical research has gone to a well-distributed network of medical schools and teaching hospitals. While the political consequences of the elitist inclinations of particle physics have been exacerbated by the huge investments required to build state-of-the-art accelerators and the resulting concentration of financial resources, the relatively small size of individual projects in cancer research has meant that with the growth in the NCI budget there has been plenty of money to go around - and to keep the constituents back home happy.

Ocean science advocates have attempted to develop a similarly broad base of support through such strategies as
creation of the Sea Grant college program and extension of marine research to the Great Lakes. The geographical distribution of marine research programs can reasonably be stretched only up to a point, however. While the constituency represented by the coastal states is substantial, much of the nation's heartland inevitably remains more or less oblivious to marine affairs except at times of marine-related catastrophe or national crisis.

We now have a sense of what factors account for the relative success of different science programs in attracting support. But within the presumed context of pragmatism, we are still missing one element in our understanding of science policy. If the quest for practical benefit is overriding, why do programs which fail to deliver the goods never get turned off? Three factors account for this observed inertia.

The first is the incremental nature of budgeting. The most basic reality to which participants in the budget process must adapt is the impossibility of doing their jobs "properly." A budget of hundreds of billions or trillions of dollars is simply beyond the ability of humans to manage in a comprehensive way. Accordingly, if the machinery of government is to function at all, legislators and bureaucrats must work out economizing strategies which keep the amount of information with which they must deal and the number of decisions they must make within manageable
proportions while still preserving an adequate degree of control over the overall shape and size of the budget.\(^{(4)}\)

One such economizing strategy, as we have seen, is the dispersion of decision processes in Congress among specialized committees and subcommittees. Perhaps the most important economizing strategy, however, is to make budgeting an incremental, rather than a comprehensive process. This year's budget is constructed in terms of marginal increases or decreases on last year's budget, rather than on the basis of an active reconsideration of the rationale for the program's existence. Over a period of years, this leads to institutionalized expectations about a given program's "natural" or "fair" share of the budget. (Sometimes these expectations can be formalized, as was done in the DOE/OMB guideline for the particle physics budget of the late 1970's and early 1980's.) The net result is that if a program does more or less what it has promised and stays out of trouble, it can proceed unmolested for long periods.

In the context of budgeting, trouble can mean any of several different things. Major new authorizations are an unavoidable form of trouble for programs which periodically require expensive construction projects as a condition of continued progress. Proposed radical increases in a program's budget will attract attention as well. In the case of high-energy physics, these generally occur only in
conjunction with the construction of new accelerators. As we have seen, in one way or another the high-energy physics program has always survived these episodes of scrutiny, though not always entirely unscathed. Radical changes in the trajectory of the cancer program have been imposed from the outside, rather than arising from the logic of the research process itself. Ocean science periodically requires major capital investments for research tools such as ships and satellites, but the impact of these requirements has not been so great as in particle physics because of the relatively lower costs and longer working lives of the oceanographic tools, and the relatively less radical extrapolations of technology that their construction has demanded.

As my discussion of competition within the bureaucratic and congressional environments suggests, trouble can arise when the size or growth rate of a program threatens some other program with a strong political base. While there has been grumbling from time to time within some branches of the scientific community arising from perceptions that high-energy physics was receiving a disproportionate share of the government's science budget, such discontent has not translated directly into political terms. The greatest growth in the high-energy physics program occurred first when the program was too small to matter and then when a broad range of scientific programs were benefiting from
increased funding. Since the peak of funding reached in connection with construction of the 200 BeV project around fiscal year 1970, the high-energy physics budget has been stagnant in real terms, and has drawn a declining share of total expenditures for basic research.

The pressure of NCI growth on the budgets of other institutes within NIH did become a problem after the boost provided by the Cancer Act of 1971, but the effects of the resulting backlash have manifested themselves slowly and on the margins; the NCI remains easily the largest of the institutes after a decade of relative budgetary stagnation. Funding for the ocean science program, relatively small in any case, has been scattered among many agencies. If it has had any steady role in budgetary collisions it has been as the victim of more powerful interests, not as a threat in its own right.

In organizational terms, there is in any case no unified pool of government funds for science for which high-energy physics, cancer research and ocean science compete with among themselves and with other sciences. There have been periodic calls for comprehensive coordination of research and development efforts. But the government's "R&D budget," as it appears, for example, in the "special analyses" of the Office of Management and Budget and its predecessor, the Bureau of the Budget, is an aggregate prepared after the fact.
In practice, budgets are conceived not in terms of R&D, but in terms of the programs or functions of which R&D is a component. High-energy physics has been relatively isolated within the AEC's physical research program and then ERDA's and DOE's general science program. The NIH and its component programs have been situated within the Department of Health, Education and Welfare and its successor, the Department of Health and Human Services. The largest piece of the ocean science program has been located within the National Science Foundation for over a decade, while the remaining pieces are scattered among many other agencies. Over the last few years, Gramm-Rudman has raised anew the issue of budgetary impacts across program lines within departments, and even across departments. As yet, however, no political mechanism has operated to translate such pressure into sustained, systematic changes in the budgets of the science programs examined for this study.

Finally, trouble occurs when something goes terribly wrong in the operation of a program, such as fraud, mismanagement or technical failure so serious as to draw the attention of the interested public and cause political embarrassment. Outright fraud or catastrophic mismanagement has simply not been an issue in high-energy physics. A certain amount of technical failure must be expected in the field, if only because new accelerators have tended to stretch the limits of current engineering practice in many
respects. In the case of the Isabelle project, the largest and most embarrassing program failure for the community, technical problems and petty mismanagement were compounded by stubbornness and arrogance, but ultimately no unanswerable questions were raised concerning the ability of the high-energy physics community to pick up the pieces and move on. By standards of military-procurement fraud, the Isabelle episode simply did not register, either in terms of the magnitude of the management problems or the size of the financial losses involved.

Serious management concerns persisted for years with respect to the large contract research programs of the NCI. But the primary direct impact on NCI was the redirection of funds within the cancer budget. The size of the cancer budget as a whole has been affected only indirectly, and weakly, insofar as controversy over the contract programs tended to tarnish the image of NCI.

Oceanography was probably associated for a time in the collective mind of Congress and the interested public with one of the grandest scientific debacles of all: the ill-fated Mohole deep-drilling project.(6) Yet the sheer diversity of work carried on in the name of ocean science and the ambiguity of the boundaries between ocean science and earth science with its specialties of geology and geophysics probably diluted the association of Mohole with ocean science proper. In Wenk's recollection, its effect
seems to have been primarily to restrain the freedom of the National Academy of Sciences to lobby Congress directly. (7)

Under ordinary circumstances, then, budgets change incrementally: we would not expect to see radical changes in the budgets of most programs in a given year. In addition, however, certain forces operate in all programs to frustrate change.

Bureaucrats, both political appointees and civil servants, are usually primarily concerned with promoting the well-being of their programs and of the external interests which serve or are served by those programs. This is sometimes portrayed as a fault. Yet if programs are not to be staffed with people who believe in their goals, defend them sincerely before Congress and work whole-heartedly on their behalf, what is the alternative?

When specialized professional expertise is believed to be essential to evaluation of policy alternatives, however, bureaucratic tendencies have unfortunate side effects, for qualified independent critics will be hard to find. Science advisors and administrators usually come from the scientific community and expect to return upon completion of their government duties. The constituencies they serve - their programs and their professional colleagues - will judge success in terms of increases in program budgets, not critical comparisons with alternative expenditures of resources. Their purpose is to serve as guardians of the
interests of their professional communities.

Their status shapes their frame of reference in ways which systematically warp the search for policy alternatives. As Harvey Averch observes,

because of the intense focus of the S & T [science and technology] bureaucracies on their own current and prospective budgets, any examination of alternatives will become a vehicle for raising budget and turf questions.... Thus, work on innovation policy becomes equivalent to work on revealing the latent technological opportunities that could be realized, if only more funds were made available. If energy policy is the problem, then more budget is clearly needed so scientists and engineers can explore the technical feasibility of unconventional alternatives like windpower or ocean thermal power. If there is excess demand for critical minerals, then more research budget is clearly needed to find technically satisfactory alternatives.(8)

Radical changes in policy require the acquiescence of the bureaucracies involved if they are to have any chance of succeeding, but the only acceptable changes involve increased budgets. Increased budgets for one field of science invoke cries in defense of "balance" from other fields. Redeployment of resources from certain fields to more promising alternatives within a relatively fixed overall budget, while honored in principle as the cornerstone of "rational" policymaking, becomes exceedingly difficult in practice. Under normal circumstances, the only way to "cut" a program's budget is to let it stagnate by comparison with continuing incremental growth of the budget as a whole.

For outsiders to take on the bureaucracy in the
interest of radical change requires a clear and powerful vision of attainable alternatives. But in science, access to the knowledge needed to elaborate such a vision is possible only through the bureaucracy and professional community whose interests are most at stake. The status quo is well defended.

Finally, and paradoxically, the very Congress which has an intense interest in launching programs for their practical benefits has little incentive or opportunity to question the outcome of its efforts.

I have stated that if a program does more or less what it has promised and stays out of trouble, it can proceed unmolested for long periods. But in operational terms, what does it mean to do "more or less what it has promised"? And does the Congress not have a responsibility not to leave programs "unmolested for long periods"? Whither congressional oversight?

Under the best of circumstances, congressional oversight, defined for the purposes of this study as the routine, rational evaluation of program performance and/or impact, occurs only sporadically and unsystematically. The range of demands made on members of Congress is greater than they can possibly meet in the time available, so priority goes to those activities that have particular value in promoting members' compelling personal interests, the foremost of which is usually political survival and
advancement. (9) Particularly in the realm of distributive policy, under which governmental subsidies are provided to activities deemed to be desirable to society as a whole, it is usually to the participants' advantage to maintain a low level of public visibility and a high degree of mutually rewarding cooperation, so that subsidies and their associated political benefits are perpetuated at minimal cost and risk. (10)

The effort needed to mount a successful challenge to the status quo rarely promises to yield commensurate political benefit, except on those usually infrequent occasions when one program threatens the budgets of more favored programs, and oversight can be used as a tool to keep the threatening program under control. In almost any specialized area of distributive policy, and especially in science programs, constituents other than those directly affected are unlikely to appreciate the significance of a challenge to existing programs, even if a member of Congress somehow musters the technical expertise to make a substantive challenge meaningful. Finally, program managers are likely to be able to muster substantially greater technical firepower than skeptical members of Congress, who therefore usually stand little chance of winning such a battle on the merits. (11)

Thus, oversight is likely to be limited to non-substantive matters such as questions of bureaucratic
reorganization or petty fraud and mismanagement within programs.

One other point is important in evaluating the potential and the reality of congressional oversight as I have defined it here. "Rational evaluation of program performance and/or impact" depends on the availability of standards by which one can judge observed achievement. For most science programs, the rationales used to justify launching and reauthorizing the programs leave Congress with either no standards at all, or standards which cannot be operationalized.

Whether high-energy physics has generated benefits to national security or economic competitiveness commensurate with the investment it has required is something that not only cannot be determined objectively but cannot yet even be argued rationally because we have no concrete measures of either goal from which we can begin to draw and analyze causal relationships. Not surprisingly, then, Congress has made no meaningful effort to do so. At least one prominent rationale for particle physics has been discarded. But the fear of Soviet advantage in the field collapsed of its own weight when the ineptitude of the Russian program became widely known, not because of systematic congressional review of the implicit and explicit claims of Russian competition made by American program advocates.

Even in the case of the JCAE review of the mid-1960's,
the relatively mild challenge which the committee mounted was expressed in the form of demands for a coherent prospective plan, not a rigorous review of the value of achievement to date, which was either ignored or taken for granted. Other "reviews" have been either perfunctory or focused on allegations of mismanagement or poor judgment, as in the Isabelle matter.

Cancer research might seem to provide the most obvious standard for evaluation: has cancer been cured? In fact, the question itself is not so straightforward, for to the best of our knowledge, "cancer" is not a single disease but a large group of diseases which share the common feature of uncontrolled cell growth but may have widely varying causes and natural histories. For as yet incompletely understood reasons having to do with the subtlety and complexity of cancer as a pathophysiologic process, medicine has to date proven incapable of "curing" any but a tiny handful of these many diseases in the definitive way that a suitable antibiotic administered in a timely fashion can completely eradicate many bacterial infections. In that sense, the effort to cure cancer has been largely a failure to date.

Yet our dread of cancer and our sense of compassion have meant that any apparent beneficial impact of medical interventions on cancer patients must be pursued. So the war on cancer continues, and the standard of evaluation has become not "cure" in the strict sense but whether
significant gains have been achieved in patient survival and mortality associated with the various cancers and over all cancers combined. This becomes a matter of statistical nuance, in an area where the statistics are treacherously incomplete and ambiguous. From time to time Congress has invited competing factions of cancer experts to pursue their battles before committee hearings, and there are routine queries to expert witnesses about "how we're doing in the war on cancer." Members of Congress are unequipped to evaluate critically the substance of what they hear in response to their questions, so such queries achieve little beyond establishing for the record that the responsible members of Congress have attended to their oversight duties.

The cancer program has faced its share of congressional investigations of petty fraud and mismanagement. More serious challenges, such as the series of reviews launched by the Fountain committee in the early 1960's and the ongoing controversies over the NCI's large contract research programs, have also been largely matters of form rather than of substance.

In ocean science, form has been dominant over substance from the start, as seen in the endless rounds of debate over reorganization and re-reorganization of federal ocean programs. When the Congress has demonstrated a serious interest in the substance of ocean programs, it has been primarily an interest in economic, legal, environmental and
political issues which are only incidentally associated with science, and not in science itself.

In practice, then, the success of scientific programs is viewed in terms of avoidance of major calamity and reasonable fulfillment of process measures such as grants awarded, students trained, papers published, Nobel prizes earned and the like, rather than outcome measures. If such indicators remain more or less in order, the status quo will usually remain unthreatened by anything scientists do or do not do. The national goals toward which scientific programs are intended to contribute remain abstractions.

These observations about the interface between science and politics help us understand why the American programs in high-energy physics, cancer research and oceanography evolved as they did. It is essential to realize, however, that while the paths that the three programs have followed are comprehensible, nothing that this study teaches us implies that they were inevitable. Chance developments in widely scattered fields ranging from science and domestic politics to culture and international relations interact in complex and unpredictable ways to shape the development of American science. This very complexity, however, while negating the possibility of a deterministic theory of the development of science policy, raises fascinating questions which may best be explored through an extension of the comparative approach employed here. In pursuing this study,
I have found two such extensions to be of special interest.

The first such extension would hold science as a constant and ask whether there is something special about American science policy as compared with that of other nations. High-energy physics, the purest and most esoteric of the sciences examined in this study, would make an interesting test case. The funding history of the most important rival to the American program, the cooperative European program embodied in CERN, appears on the surface to have many similarities to that of the American program. However, the extent and significance of differences in the relative weights of the substantive rationales and differences in the traditions and constraints of European politics compared with American awaits detailed exploration.

A number of interesting differences are apparent on a cursory review of the history.(12) First are the different circumstances under which CERN was born, and the significance of its role in helping to rebuild a battered continent’s scientific effort, rather than to bolster a newly-achieved status as a global superpower, as in the United States. Of equal importance is the symbolic role of CERN as part of the broader quest for the cultural, economic and political unification of Western Europe. Related to this quest are the delicate political and economic arrangements necessary to launch CERN and keep it alive, and the nature and timing of the periodic crises that have
threatened the stability of those arrangements.

Furthermore, each CERN member nation has its own history of debate over the scientific, political and economic consequences of joining and of ratifying the periodic subscriptions negotiated for the launching of new accelerator projects. For example, Great Britain, while in many respects one of the staunchest supporters of CERN, has seriously threatened withdrawal on several occasions.(13) Italy continues to accord high-energy physics an extraordinarily prominent status within a relatively small national portfolio of basic science activities.(14) Germany, while a major part of CERN, also maintains a significant independent program, represented most prominently by the DESY laboratory at Hamburg.

The Soviet Union’s high-energy physics program seemed for a time to be almost a mirror to the American program, tracking its progress and responding to similar national interests. With hindsight, however, it can be seen in some respects as a caricature of the genuinely successful American program. Uncovering accurate statistics concerning the extent of the Soviet investment in the program and understanding the program’s evolution and its role in that nation’s overall scientific effort would be both a major challenge and a substantial contribution to a comparative understanding of science policy.

The Japanese high-energy physics program represents
something of an enigma. Today Japan is portrayed as a technologically powerful nation on the verge of making a substantial commitment to the field. Japanese participation in major new projects like the SSC is both eagerly sought because of that nation's technical and financial strengths, and greatly feared because of the prospect of further fueling the Japanese advantage in key spinoff technologies. However, while the long and distinguished tradition of elementary-particle theory in Japan is widely appreciated, many are unaware of the extent of the structural parallels between the American and Japanese programs over a period running from the 1930's to the 1960's, culminating in simultaneous planning in the early 1960's for construction of large proton accelerator designs traceable to a common conceptual root. Lillian Hoddeson's pioneering analysis of the national differences which led to the subsequent radical divergence in the course of the two nations' programs lays the foundation for further investigation of the role of high-energy physics in the two nations' science portfolios and the perceived relationships in each case with specific national interests. (15)

A second extension of this study would hold the American political system as a constant and ask whether there is something special about science policy as compared with other areas of distributive policy in America. In discussing the impact of scientific professionalism on
bureaucratic politics, Averch, viewing the American political scene from a science policy perspective, observes that

From the perspective of the S & T community, federal S & T administrators are its representatives, in government on loan, to assure that no harm is done to its interests. There is nothing unusual in such a situation — certainly businessmen or farmers view administrators and advisers in the departments of Commerce or Agriculture as their representatives — but because research and education contain higher-order social values, the S & T community believes that it is different in kind from dairy farmers or automobile manufacturers and that it deserves kind treatment and attention. (16)

On the other hand, Ripley and Franklin, analyzing American politics from a broad political science perspective, lump cancer research with such federal programs as agricultural price supports, water projects, veterans' benefits and housing subsidies as examples of distributive policy. (17)

On some level, research does "contain higher-order social values," although it would be a mistake to assume that those values are universally shared by all sectors of American society. But it would also appear that most and perhaps all other subsidy programs that are successful politically have been so in part because of an ability to associate themselves with values that retain a similar symbolic power for American society at large. The virtues of the family farm, for example, or the honor and respect due servicemen who have sacrificed in defense of their country, are ideas that significantly predate the popularity
and respect presently accorded scientific research.

Furthermore, other subsidy programs are affected by the same general political factors that have affected science programs - for example, the relative invisibility of "small" programs against a large budget or the limited incentives for meaningful congressional oversight.

On the basis of this study I would propose as a hypothesis that science programs do indeed represent a subset of a more general category of distributive programs, albeit an extreme one. The distinctiveness of science arises from its perceived incompressibility, from its related status as a quasi-establishment, and from the scope of its claimed powers.(18) An understanding of the issues involved in weighing the claims of farmers or of veterans is felt to be within the reach of any American who is capable of gaining election to the Congress, and the consequences of erring marginally in one direction or another are rarely described in apocalyptic terms. Scientists, by comparison, are seen as masters of incomprehensible powers with potentially revolutionary effects. It would require both extraordinary self-confidence and unusual insight for a member of Congress even to question whether the policy-relevant aspects of scientific practice cannot be routinely extracted in a form comprehensible to laypeople, rather than assuming that policy assertions of scientists must be accepted or rejected on faith. The rare member of Congress
with scientific training, even when objecting to existing policy, usually simply asserts a competing voice of authority, reflecting the general lack of introspection within the practicing scientific community on such matters.

Two routes of inquiry appear potentially fruitful in pursuing this question: first, a detailed comparison of scientific programs with other programs from the perspective of their political roots in distributive policy; and second, a detailed comparison of the politics of scientific programs with the politics of military programs, which because of their sheer magnitude cannot escape scrutiny, but which would appear to present many comparable questions on the exercise of lay authority in the face of specialized professional expertise.

Some policy lessons are suggested by the results of this study. First, while the abdication of responsibility for the substance of one area of policy to a quasi-establishment will appear pathological to many who seek to improve policy, in fact the political costs of that abdication are small and the benefits large. The habit of associating science with important but difficult-to-measure goals such as national security, improved health or economic competitiveness makes support for science a useful and relatively inexpensive means by which politicians can demonstrate concern for those goals. Advocates of science in Congress or the upper reaches of the executive branch can
enjoy the benefit of a "teflon effect;" except in the most egregious cases, perceived failures can usually be blamed on the scientists, since everyone suspects that the laypeople never really understood what the scientists were doing anyway.

Yet the benefit of being seen as supporting science in the pursuit of compelling interests can carry only so far, for the American people long ago learned to appreciate the role of support for research as a way of equivocating on difficult policy questions and dodging pressure for real action. In this regard, the obvious response of a reform-minded policy analyst to the findings of this study, which is to seek ways to link scientific research more effectively and more directly with genuine pragmatic needs rather than with symbolic images thereof, may be seriously misguided if the implicit goal is to bolster science funding. It would appear that when the perceived connection between research and concrete practical benefits is too tight, political pressure grows to stop wasting so much time and money on the research and to go directly for the benefits.

Consider the fate of ocean science. Exactly what "ocean science" or "marine science" really means has long been a point of confusion for those who would try to attract funding and those who might consider offering it. Ocean science finally started to grow substantially only toward the tail end of the post-Sputnik science boom - and thus
faced a substantial risk of seeing that growth truncated before the field had reached a mass adequate to sustain visibility in harder times to come. Ocean science advocates, reading the mood of Congress and the Johnson administration, chose to cast their lot with the Great Society, and made a conscious effort to define "marine science affairs" as encompassing not only science proper but a wide range of applications, many having little direct connection with science, designed to bring tangible benefits of the seas to people as rapidly as possible.

It may be that had they done otherwise, the battle for the visibility of ocean science proper would have been lost then and there. Nevertheless, it is hard to avoid the impression that by tying their field too closely and confusingly to applications, the ocean science activists purchased short-term visibility for ocean affairs at the cost of long-term viability for ocean science itself. A decade later and beyond, confusion about the meaning of "marine science" persisted, (19) and the Congress, to the extent that it was interested, focused on controversial diplomatic, economic and regulatory matters such as the Law of the Sea conference and offshore oil drilling.

The cancer program is continually defending itself against critics who want to redirect resources toward more immediate gain. For some, that has meant relatively conservative attempts to make the existing arrangements
fulfill their promise by eliminating presumed bottlenecks between the laboratory and the bedside, while for others, it has meant supporting vastly expanded regulatory activities to save the nation from "environmental" catastrophe. Less tendentious analysts have emphasized the need to better integrate health research into a coherent overall national health policy in order to cope most effectively with the perceived crisis in health care. (20) Yet once health research in general, and cancer research in particular, are seen as part of the overhead on a "system" primarily devoted to financing and delivering services, rather than an independent search for "magic bullets," they will be extremely vulnerable to cost-cutting pressures.

Finally, for all of the concern of scientists about congressional "meddling" and "micromanagement," the present arrangements for allocating federal funds for science tend to work in the scientists' favor compared to politically realistic alternatives. Scientists are given the opportunity to build support for their programs through personal lobbying of an influential but relatively uncritical audience. Were the departments of the federal government funded en bloc from the top, the relative strength of science programs in the intradepartmental budget struggle would likely suffer considerably. Were all of science to be lumped into a "Department of Science," as has been proposed on numerous occasions over the course of the
nation's history, the different scientific specialties would likely be thrown into open warfare, with a public-relations fallout that would likely contribute to the relative ease of capping the budget of a single department, compared to the budgets of smaller programs scattered across the federal government bureaucracy.

* * *

The perceived linkage between science and national goals has provided an effective motivation for large-scale federal support of programs in a wide range of scientific disciplines. But the institutional context in which members of Congress and the administration work does not provide a similarly powerful incentive for establishing the mechanisms needed to close the feedback loop of rational policy-making. A mutually reinforcing framework of disincentives operates to discourage ongoing, critical review of priorities in resource allocation for science.

Probably the most important is the relative insignificance of science within the huge federal budget. At an annual budget that is now approaching $1 billion with the onset of significant construction funding for the SSC, the high-energy physics program constitutes less than one tenth of one percent of the federal budget. At roughly $1.6 billion for fiscal 1990, the National Cancer Institute does
not reach 0.2%. The total budget for all ocean science programs probably does not reach even a minuscule 0.04% of total federal expenditures.

Presidential attention to matters of science is necessarily fleeting, except for the periodic consideration demanded by project starts of the magnitude of the SSC. Despite specialization and subspecialization within the Congress, routine, comprehensive congressional oversight of more than $1 trillion worth of federal spending is an utter impossibility.

Vigorous, substantive congressional oversight is uncommon under the best of circumstances. Science programs carry the added shield of the technical expertise presumed necessary to argue the merits of program activities. When members of Congress can expect their questioning to be countered by the non-rebuttable assertions of expert authority, there is nothing to be gained from a serious attempt at oversight. Should congressional committees attempt to breach the barrier of superior expertise by setting competing scientific factions to debate, the likeliest outcome is a confusing stalemate which Congress will be in no position to resolve. Worse still is the possibility that such confusion, when publicized, will serve to tarnish the reputations of public programs - a catastrophe for distributive policy, whose political success depends on keeping directly affected interests happy while
drawing minimal attention from everyone else.

The common modes of justification for science programs provide no well-defined targets for a skeptical congressman. The language of advocacy is stylized and causally ambiguous. Support for science is linked with deeply-held societal values, and any wavering in that support is portrayed as inviting catastrophe. No standards for success are offered, so no program can ever achieve them and thereby become eligible for termination; by the same token, no program can ever be considered a failure. Should an outsider attempt to probe too closely, the banner of academic freedom can be raised in defense.

Any attempt to define policy alternatives for science is evidently difficult and costly for a non-scientist. So the task of definition, and by extension the question of the relevance of the alternatives to the broader national interest are left, by default, to the professional community whose sustenance is at issue. Acting in good faith, the scientists nevertheless tend to understand the national interest in terms of their predilections as scientists. Sensing the needs and inclinations of the politicians, but intent on preserving a maximum of autonomy and avoiding the establishment of goals they cannot be sure of meeting, the scientists provide vague pragmatic rationales in profusion. Finding such rationales useful for their own purposes and having in any case nowhere else to turn, politicians accept
them and thereby surrender the possibility of effectively questioning the substance of federally-supported science.

But budgets must be determined somehow. In the absence of means for objectively weighing cause and effect and of common-sense standards to fall back on, historical accident and idiosyncrasies of politics, the scientific professions and the culture at large become the dominant factors determining the balance of the nation’s scientific program. Whatever the outcome, Congress is in no position and has no incentive to quibble. Indicators of process remain robust—science itself continues to advance by leaps and bounds, and the Nobel prizes keep rolling in. So Congress continues to provide the money, secure in the knowledge that it has done the right thing in promoting the health of science—and the feedback loop of rational policy remains open with respect to the intent of the policy, or, more precisely, short-circuited by false signals of success or failure.

In 1967, Daniel Greenberg wrote of the old politics of science and the new politics of science. Under the old politics,

Government never wholly accorded the sovereignty demanded by the ideology of pure science. But though reins and restrictions existed, and the principle of accountability (loathsome to the scientists) was never absent, the essential point was that, in practice, scientists wrote most of the rules for the use of federal research money; scientists staffed the agencies that dispensed the money, and scientists from the university community advised these same staff scientists on the distribution of the money. As Don K. Price... observed... "the plain fact is that science has
become the major Establishment in the American political system: the only set of institutions for which tax funds are appropriated almost on faith, and under concordats which protect the autonomy, if not the cloistered calm, of the laboratory." (21)

Writing from the perspective of the Johnson years, when the rapid growth of government support for science had come to a halt amid constrained budgets and changing ideas about national priorities, Greenberg perceived the beginnings of a new politics of science built on the principle of greater accountability in the light of the needs of society. As one example of the changed circumstances, he pointed to the opening of the 200 BeV accelerator project to a site selection competition in place of its expected award to the Lawrence Radiation Laboratory at Berkeley, which had done preliminary design work and was considered next in line for the machine.

Lowi, Ginsberg et al. argued, contrary to Greenberg, that nothing had really changed - the high-energy physicists had, in essence, gotten everything that really mattered. (22) At the beginning of chapter 7, I argued that Lowi and colleagues overlooked a profound change in the environment in which the high-energy physics program operated. The way in which the 200 BeV project evolved pointed to the range of new constraints under which the field was now forced to operate.

Nevertheless, there was a grain of truth in that observation. The new constraints did have a real effect on
the way high-energy physicists do business. But in 1990, twenty-three years after Greenberg declared a new politics of science, the nation's science program continues to depend not on a well-reasoned understanding of the contributions of its components to concrete national goals, but on faith and symbolism. Those branches of science which, like high-energy physics and cancer research, are successful in securing for themselves a robust association with the pantheon of sacred national interests, live long and prosper. Those that do not, such as ocean science program, must continually struggle for crumbs from the federal government's scientific table.

* * *

From a strictly political perspective, the present system of allocating money for science in America must be judged a success. At relatively low expense, astute politicians are provided with a field in which to compete for favor while doing relatively little damage to the rest of us; a large group of intelligent and influential constituents is kept happy, more or less; and, miraculously enough, every once in a while the operation spins off something genuinely useful to society as a whole.

Yet only the cynic is likely to find this sufficient. For those who hold greater expectations of the political
process, difficult questions remain. Can our political mechanisms be made capable of weighing the substantive merits of alternative investments of resources in science, or must spending inevitably expand along a course set by historical accident, to a limit set not by the public interest but by the public tolerance for the burden of taxation in all of its forms? Can the political process be made to accommodate evaluative mechanisms which test the promises of science programs against reality on an ongoing basis? And can such mechanisms be harnessed for the enlightenment of those in whom the public trust is placed, or must our investment in science inevitably be guided by the whims of faith and fate?

The political rationality of the system as it stands fails many tests of coherence and efficiency. Faith and fate are surely lacking as guides to policy. But attempts to reform the system, if they are to have any hope of success, must take into account the deep roots of the present system, the forces that it reflects, the needs that it serves - and its successes, as well as its failures.
Notes


4. In thinking about the relationship between the budgeting process and the fate of government programs, I have found the lucid and insightful analysis by Aaron Wildavsky to be especially helpful. See The Politics of the Budgetary Process, 2nd ed. (Boston: Little, Brown and Company, 1974).


8. A Strategic Analysis of Science & Technology Policy, p. 189.

9. For a detailed analysis of the factors which affect the quantity and quality of congressional oversight, see Morris S. Ogul, Congress Oversees the Bureaucracy (Pittsburgh, PA: University of Pittsburgh Press, 1976).


12. A useful introduction to the political aspects of high-energy physics in Europe can be found in the following works: Armin Hermann et al., History of CERN, Volume I (Amsterdam: North-Holland, 1987); Robert Jungk, The Big Machine (New York: Charles Scribner's Sons, 1968); and


18. The use of the term "establishment" here follows the example of Don K. Price: "... the plain fact is that science has become the major Establishment in the American political system: the only set of institutions for which tax funds are appropriated almost on faith, and under concordats which protect the autonomy, if not the cloistered calm, of the laboratory." See "The Scientific Establishment," pp. 19-40 in Robert Gilpin and Christopher Wright, eds., *Scientists and National Policy-Making* (New York: Columbia University Press, 1964).


22. Theodore Lowi, Benjamin Ginsberg *et al.*, *Policide* (New York: Macmillan, 1976); see, for example, p. 102.
APPENDIX: BUDGETS AND BUDGET PROJECTIONS
FOR HIGH-ENERGY PHYSICS

Despite the seeming surfeit of statistics covering almost every conceivable aspect of the federal budget, the surprising fact is that it is actually extraordinarily difficult to reconstruct a long, historical series of budget data, even for a relatively narrowly-defined area such as high-energy physics. Category definitions used in budget documents change over time, and in ways which are often not specified. The particular aggregates published in connection with advocacy documents such as advisory panel reports reflect both limitations in resources available for data-gathering and the need to support recommendations made in the text of the report.

While difficulties in splicing together budget fragments from different sources make a precise and continuous accounting difficult, the pieces that are available do add up to a reasonably coherent qualitative picture both of actual funding and of a style of advocacy as represented in budget projections made by the advisory panels.
ABBREVIATIONS

AEC  Atomic Energy Commission
DOE  Department of Energy
ERDA Energy Research and Development Administration
JCAE Joint Committee on Atomic Energy
NSF National Science Foundation
ONR Office of Naval Research
OSR Air Force Office of Scientific Research
TABLE 1

"Annual High Energy Physics Support by Various Government Agencies Based onExisting or Authorized Accelerators"

from Piore panel report, 1959(1)

in thousands of dollars

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<th>FY</th>
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<th>AEC constr.</th>
<th>ONR</th>
<th>OSR</th>
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<td>1946</td>
<td>3,900</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>3,900</td>
</tr>
<tr>
<td>1947</td>
<td>500</td>
<td>4,000</td>
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<td></td>
<td></td>
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<tr>
<td>1948</td>
<td>3,400</td>
<td>600</td>
<td>2,400</td>
<td></td>
<td></td>
<td>6,400</td>
</tr>
<tr>
<td>1949</td>
<td>4,800</td>
<td>1,600</td>
<td>2,200</td>
<td></td>
<td></td>
<td>8,600</td>
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<tr>
<td>1950</td>
<td>3,400</td>
<td>7,500</td>
<td>1,600</td>
<td></td>
<td></td>
<td>12,500</td>
</tr>
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<td>1951</td>
<td>5,900</td>
<td>4,100</td>
<td>3,300</td>
<td></td>
<td></td>
<td>13,300</td>
</tr>
<tr>
<td>1952</td>
<td>6,300</td>
<td>1,700</td>
<td>1,600</td>
<td></td>
<td></td>
<td>9,600</td>
</tr>
<tr>
<td>1953</td>
<td>7,600</td>
<td>2,300</td>
<td>2,400</td>
<td></td>
<td></td>
<td>12,300</td>
</tr>
<tr>
<td>1954</td>
<td>7,400</td>
<td>1,900</td>
<td>1,800</td>
<td>270</td>
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<td>11,400</td>
</tr>
<tr>
<td>1955</td>
<td>8,300</td>
<td>1,600</td>
<td>1,500</td>
<td>320</td>
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<td>12,000</td>
</tr>
<tr>
<td>1956</td>
<td>10,200</td>
<td>3,200</td>
<td>1,600</td>
<td>610</td>
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<td>1957</td>
<td>16,000</td>
<td>7,000</td>
<td>2,000</td>
<td>930</td>
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<td>26,100</td>
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<td>1958</td>
<td>19,100</td>
<td>12,900</td>
<td>3,300</td>
<td>1,000</td>
<td></td>
<td>36,500</td>
</tr>
<tr>
<td>1959*</td>
<td>27,700</td>
<td>26,300</td>
<td>3,300</td>
<td>865</td>
<td></td>
<td>58,600</td>
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<tr>
<td>1960*</td>
<td>36,600</td>
<td>20,500</td>
<td>3,600</td>
<td>950</td>
<td></td>
<td>58,400</td>
</tr>
<tr>
<td>1961*</td>
<td>45,900</td>
<td>19,000</td>
<td>4,000</td>
<td>1,150</td>
<td></td>
<td>71,100</td>
</tr>
<tr>
<td>1962*</td>
<td>53,700</td>
<td>18,800</td>
<td>4,400</td>
<td>1,250</td>
<td></td>
<td>79,700</td>
</tr>
<tr>
<td>1963*</td>
<td>60,500</td>
<td>9,000</td>
<td>4,800</td>
<td>1,550</td>
<td></td>
<td>77,900</td>
</tr>
</tbody>
</table>

*projected

No inflation adjustment is reported for these numbers, which appear to represent actual expenditures in current-year dollars.
TABLE 2

Federal government support for high-energy physics, including construction but exclusive of cosmic ray research
from testimony of Leland Haworth, NSF Director, before the JCAE, 1965(2)
in millions of dollars

<table>
<thead>
<tr>
<th>FY</th>
<th>Total funds</th>
</tr>
</thead>
<tbody>
<tr>
<td>1955</td>
<td>12</td>
</tr>
<tr>
<td>1956</td>
<td>16</td>
</tr>
<tr>
<td>1957</td>
<td>24</td>
</tr>
<tr>
<td>1958</td>
<td>32</td>
</tr>
<tr>
<td>1959</td>
<td>51</td>
</tr>
<tr>
<td>1960</td>
<td>56</td>
</tr>
<tr>
<td>1961</td>
<td>87</td>
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<tr>
<td>1962</td>
<td>108</td>
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<tr>
<td>1963</td>
<td>129</td>
</tr>
<tr>
<td>1964</td>
<td>146</td>
</tr>
</tbody>
</table>

No inflation adjustment is reported for these numbers, which appear to represent actual expenditures in current-year dollars.
TABLE 3
"Program Costs in Millions of Dollars per Year"
from Ramsey panel report, 1963(3)
in millions of dollars

<table>
<thead>
<tr>
<th>FY</th>
<th>Base program</th>
<th>Users program</th>
<th>New program</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>1962</td>
<td>96</td>
<td>12</td>
<td></td>
<td>108</td>
</tr>
<tr>
<td>1963</td>
<td>128</td>
<td>15</td>
<td></td>
<td>143</td>
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<tr>
<td>1964</td>
<td>153</td>
<td>18</td>
<td>4</td>
<td>175</td>
</tr>
<tr>
<td>1965</td>
<td>176</td>
<td>28</td>
<td>14</td>
<td>218</td>
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<tr>
<td>1966</td>
<td>187</td>
<td>30</td>
<td>20</td>
<td>237</td>
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<tr>
<td>1967</td>
<td>190</td>
<td>32</td>
<td>46</td>
<td>268</td>
</tr>
<tr>
<td>1968</td>
<td>187</td>
<td>34</td>
<td>96</td>
<td>317</td>
</tr>
<tr>
<td>1969</td>
<td>190</td>
<td>36</td>
<td>125</td>
<td>351</td>
</tr>
<tr>
<td>1970</td>
<td>199</td>
<td>41</td>
<td>130</td>
<td>370</td>
</tr>
<tr>
<td>1971</td>
<td>206</td>
<td>46</td>
<td>156</td>
<td>408</td>
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<tr>
<td>1972</td>
<td>212</td>
<td>50</td>
<td>195</td>
<td>457</td>
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<tr>
<td>1973</td>
<td>219</td>
<td>54</td>
<td>250</td>
<td>523</td>
</tr>
<tr>
<td>1974</td>
<td>233</td>
<td>60</td>
<td>256</td>
<td>549</td>
</tr>
<tr>
<td>1975</td>
<td>230</td>
<td>66</td>
<td>300</td>
<td>596</td>
</tr>
<tr>
<td>1976</td>
<td>200</td>
<td>70</td>
<td>337</td>
<td>607</td>
</tr>
<tr>
<td>1977</td>
<td>170</td>
<td>80</td>
<td>355</td>
<td>605</td>
</tr>
<tr>
<td>1978</td>
<td>140</td>
<td>90</td>
<td>344</td>
<td>574</td>
</tr>
<tr>
<td>1979</td>
<td>120</td>
<td>100</td>
<td>352</td>
<td>572</td>
</tr>
<tr>
<td>1980</td>
<td>105</td>
<td>110</td>
<td>369</td>
<td>584</td>
</tr>
<tr>
<td>1981</td>
<td>103</td>
<td>115</td>
<td>382</td>
<td>600</td>
</tr>
</tbody>
</table>

In the Ramsey panel budget projections, "base program" refers to expenses associated with the existing (1963) major accelerators; "users program" refers to support for visiting researchers at the major accelerators, data analysis and theoretical work at their base universities, and certain expenses of smaller, university-based accelerators; "new program" refers to expenses associated with proposed new accelerators, including the MURA project, the 200 BeV accelerator, a 33 BeV storage ring project, a 10 BeV electron accelerator, a second phase at SLAC, and an 800 BeV accelerator.

Although the base year is not identified in the table, these projections appear to be expressed in terms of constant 1962 or 1963 dollars.
TABLE 4

Cost of operations for AEC high-energy physics program (construction excluded) and total AEC appropriations from AEC annual financial reports(4) in millions of current year dollars

<table>
<thead>
<tr>
<th>FY</th>
<th>High-energy physics</th>
<th>AEC total</th>
</tr>
</thead>
<tbody>
<tr>
<td>1964</td>
<td>86</td>
<td>2,743</td>
</tr>
<tr>
<td>1965</td>
<td>99</td>
<td>2,625</td>
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<tr>
<td>1966</td>
<td>113</td>
<td>2,366</td>
</tr>
<tr>
<td>1967</td>
<td>126</td>
<td>2,199</td>
</tr>
<tr>
<td>1968</td>
<td>137</td>
<td>2,509</td>
</tr>
<tr>
<td>1969</td>
<td>150</td>
<td>2,616</td>
</tr>
<tr>
<td>1970</td>
<td>152</td>
<td>2,222</td>
</tr>
<tr>
<td>1971</td>
<td>148</td>
<td>2,308</td>
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<tr>
<td>1972</td>
<td>147</td>
<td>2,293</td>
</tr>
<tr>
<td>1973</td>
<td>160</td>
<td>2,633</td>
</tr>
</tbody>
</table>
TABLE 5

"Program costs"
from AEC report, 1965(5)
in millions of dollars

<table>
<thead>
<tr>
<th>FY</th>
<th>Base program</th>
<th>New program</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>1965</td>
<td>161</td>
<td>5</td>
<td>166</td>
</tr>
<tr>
<td>1966</td>
<td>179</td>
<td>9</td>
<td>188</td>
</tr>
<tr>
<td>1967</td>
<td>205</td>
<td>33</td>
<td>238</td>
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<tr>
<td>1968</td>
<td>215</td>
<td>67</td>
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<td>1969</td>
<td>215</td>
<td>88</td>
<td>303</td>
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<td>1970</td>
<td>220</td>
<td>107</td>
<td>327</td>
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<td>1971</td>
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<td>118</td>
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<td>1972</td>
<td>230</td>
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<td>1973</td>
<td>230</td>
<td>157</td>
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<td>1974</td>
<td>230</td>
<td>189</td>
<td>419</td>
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<td>1975</td>
<td>225</td>
<td>222</td>
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<td>1976</td>
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<td>272</td>
<td>467</td>
</tr>
<tr>
<td>1980</td>
<td>190</td>
<td>264</td>
<td>454</td>
</tr>
<tr>
<td>1981</td>
<td>187</td>
<td>262</td>
<td>449</td>
</tr>
</tbody>
</table>

In the AEC budget projections, "base program" refers to expenses associated with the existing (1965) major accelerators, as well as costs associated with the university users of the existing machines; "new program" refers to similar expenses for proposed new projects, including upgrades at the Brookhaven AGS, an electron-positron storage ring, the 200 BeV accelerator, and an 800 BeV accelerator.

In the original document, the annual totals do not exactly match the sums of the "base program" and "new program" figures. Here I have reported the actual sums.

Although the base year is not identified in the table, these projections appear to be expressed in terms of constant 1965 dollars.
TABLE 6

"Total federal program in high energy physics, annual costs"
from HEPAP report, 1969(6)
in millions of constant 1969 dollars

<table>
<thead>
<tr>
<th>FY</th>
<th>Existing labs</th>
<th>NAL</th>
<th>UHEA</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>1966</td>
<td>175</td>
<td>2</td>
<td></td>
<td>177</td>
</tr>
<tr>
<td>1967</td>
<td>168</td>
<td>2</td>
<td></td>
<td>170</td>
</tr>
<tr>
<td>1968</td>
<td>180</td>
<td>6</td>
<td></td>
<td>186</td>
</tr>
<tr>
<td>1969</td>
<td>169</td>
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<td></td>
<td>178</td>
</tr>
<tr>
<td>1970</td>
<td>165</td>
<td>31</td>
<td></td>
<td>196</td>
</tr>
<tr>
<td>1971</td>
<td>189</td>
<td>111</td>
<td></td>
<td>300</td>
</tr>
<tr>
<td>1972</td>
<td>214</td>
<td>161</td>
<td></td>
<td>375</td>
</tr>
<tr>
<td>1973</td>
<td>240</td>
<td>98</td>
<td></td>
<td>338</td>
</tr>
<tr>
<td>1974</td>
<td>276</td>
<td>105</td>
<td></td>
<td>381</td>
</tr>
<tr>
<td>1975</td>
<td>282</td>
<td>130</td>
<td></td>
<td>412</td>
</tr>
<tr>
<td>1976</td>
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<td>416</td>
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<td>276</td>
<td>156</td>
<td>6</td>
<td>438</td>
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<td>1978</td>
<td>258</td>
<td>182</td>
<td>17</td>
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</tr>
<tr>
<td>1979</td>
<td>248</td>
<td>183</td>
<td>40</td>
<td>471</td>
</tr>
<tr>
<td>1980</td>
<td>237</td>
<td>155</td>
<td>88</td>
<td>480</td>
</tr>
</tbody>
</table>

In this table I have added together figures from several more detailed budget lines included in the HEPAP report. "Existing labs" refers to construction, operation and user programs at existing (1969) accelerators; "NAL" refers to expenses associated with the National Accelerator Lab (the 200 BeV); "UHEA" refers to a proposed ultra-high-energy accelerator.
TABLE 7
ERDA/DOE high-energy physics budget
including operation and construction,
total ERDA/DOE R & D budget,
and total federal obligations for basic research
from the federal budget(7)
in billions of current year dollars

<table>
<thead>
<tr>
<th>FY</th>
<th>High-energy physics</th>
<th>DOE R&amp;D</th>
<th>Federal basic research</th>
</tr>
</thead>
<tbody>
<tr>
<td>1975</td>
<td>0.156</td>
<td>2.071</td>
<td>2.6</td>
</tr>
<tr>
<td>1976</td>
<td>0.174</td>
<td>2.499</td>
<td>2.8</td>
</tr>
<tr>
<td>TQ</td>
<td>0.045</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1977</td>
<td>0.206</td>
<td>3.575</td>
<td>3.3</td>
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<td>1978</td>
<td>0.255</td>
<td>4.237</td>
<td>3.7</td>
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<tr>
<td>1979</td>
<td>0.294</td>
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<td>1980</td>
<td>0.320</td>
<td>4.737</td>
<td>4.7</td>
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<td>1981</td>
<td>0.360</td>
<td>4.948</td>
<td>5.0</td>
</tr>
<tr>
<td>1982</td>
<td>0.365</td>
<td>4.758</td>
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<tr>
<td>1983</td>
<td>0.431</td>
<td>4.491</td>
<td>6.4</td>
</tr>
<tr>
<td>1984</td>
<td>0.472</td>
<td>4.642</td>
<td>7.0</td>
</tr>
<tr>
<td>1985</td>
<td>0.542</td>
<td>4.901</td>
<td>7.8</td>
</tr>
<tr>
<td>1986</td>
<td>0.491</td>
<td>4.708</td>
<td>8.1</td>
</tr>
</tbody>
</table>

TQ represents the "transition quarter" when the government fiscal year was shifted on the calendar.
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