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BY THE COMPTROLLER GENERAL

Report To The Congress

OF THE UNITED STATES

AD A089166

Increasing Costs, Competition May Hinder U.S. Position Of Leadership In High Energy Physics.

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The United States is seeking to maintain world leadership in making significant discoveries in high energy physics. However, with increasing costs and competition, the U.S. physics community is concerned that its leadership is being lost to Western Europe.

For the past 30 years, the United States has led the world in high energy physics research; other countries are now challenging that lead. This research--the study of the structure and properties of matter and energy in their most basic form--is basic research at the frontiers of science. Maintaining a leadership position in the field is considered important because it provides a nation's or group of nations' physicists with greater opportunities for making major discoveries and thus advancing the state of knowledge--the principal goal of high energy physicists.

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COMPTROLLER GENERAL OF THE UNITED STATES
WASHINGTON, D.C. 20548

B-197675

To the President of the Senate and the
Speaker of the House of Representatives

This report addresses the status and problems of the U.S. program's efforts to maintain a leadership position in high energy physics. It discusses the program's objectives, plans, and funding; presents selected alternative policy options; and suggests that some actions be taken to determine the appropriate level of Federal funding and improve planning.

We are sending copies of this report to the Directors, Office of Management and Budget, Office of Science and Technology Policy, and National Science Foundation; the Secretary of Energy; interested Members and Committees of Congress; and other interested parties.

Comptroller General
of the United States

COMPTROLLER GENERAL'S REPORT TO THE CONGRESS

INCREASING COSTS, COMPETITION MAY HINDER U.S. POSITION OF LEADERSHIP IN HIGH ENERGY PHYSICS

D I G E S T

For the past 30 years, the United States has led the world in high energy physics research; other countries are now challenging that lead. This research--the study of the structure and properties of matter and energy in their most basic form--is basic research at the frontiers of science. Maintaining a leadership position in the field is considered important because it provides a nation's or group of nations' physicists with greater opportunities for making major discoveries and thus advancing the state of knowledge--the principal goal of high energy physicists. (See ch. 2.)

The Federal Government provides nearly all of the funding of the U.S. high energy physics efforts. This is in line with the overall Federal role of supporting basic research and graduate education. The Office of Science and Technology Policy has oversight responsibility over all federally funded basic science and works with the Office of Management and Budget (OMB) in developing basic science budgets. The National Science Foundation is the principal Federal agency for the support of basic research across all fields of science and science education. In fiscal year 1980, the Foundation is providing about $23 million, or about 8 percent of the nearly $350 million of Federal support for high energy physics. The Department of Energy (DOE) has primary responsibility for implementing a sound national high energy physics program and is supporting the program with about $325 million in fiscal year 1980. (See p. 29.)

Whether the funding of high energy physics research is appropriately balanced with support of research in other basic science fields is not clear. GAO found little documented evidence that the needs of other basic sciences were adequately considered in establishing DOE's level of support, which represents over 90 percent of the Federal funding. DOE's funding is based on an agreement with OMB to annually fund DOE's high energy physics at a constant level of $300 million (in 1979 dollars) from fiscal years 1979 through 1984. This was the minimum amount the physics community believed needed to maintain a viable U.S. program with adequate diversity. The amount of funds provided by DOE has not been based on a comprehensive plan for maintaining a leadership position. (See p. 44.)

GAO believes the program has been faced with trying to do more than available funds would allow. Since the funding agreement was reached, DOE has not formally prepared a comprehensive plan which is consistent with the agreed upon funding level. At current funding, the program has been emphasizing the development and construction of accelerators--machines which can be used in more sophisticated or higher energy research directed toward significant physics discoveries. Other key program elements, such as long-term research and development of new accelerator technologies, the use of existing accelerators for physics research, and the support of university researchers, have been inadequately funded. The present efforts may help maintain leadership by providing needed accelerator capabilities, but inadequate funding of these other program elements may have detrimental effects on long-term accelerator technology and the participation of the brightest and most talented U.S. scientists, which may jeopardize the maintenance of leadership. (See ch. 4.)

GAO believes that the objective of maintaining a world leading position in high energy physics; a plan for achieving that objective; and the level of funding needed should be examined in light of the program's needs and importance relative to other basic sciences. A number of policy alternatives and approaches are available for the U.S. high energy physics program, each of which offers advantages and disadvantages. GAO's exploration of these alternatives and approaches disclosed no easy answers, but showed that a given policy and strategy should not be pursued unless the amount of funds needed are made available. (See ch. 5.)

During the course of GAO's review, DOE initiated a study, conducted by its advisory panel, which was to develop near-, mid-, and long-range plans for present and increased funding levels. GAO believes the development of such plans is commendable and should be formally instituted on a periodic basis and submitted to the Congress for its information and use in carrying out its budgetary and oversight responsibilities. (See p. 58.)

RECOMMENDATIONS

In his capacity of having overall oversight responsibility over federally funded basic research, GAO recommends the Director, Office of Science and Technology Policy, assemble a work group to conduct a study to determine the appropriate level for funding the U.S. high energy physics program taking into consideration the program's needs and importance relative to other basic sciences. Based on the results of such a study, GAO recommends the Director prepare a policy paper setting forth the objectives of the U.S. program, a strategy for achieving such objectives, and the appropriate annual funding levels for carrying out the strategy. Such funding levels should consider projected amounts for major functions such as construction, accelerator operations, accelerator research and development, equipment, equipment research and development, and physics research. GAO further recommends the Director consult with the appropriate oversight committees of the Congress for their views and input in helping to formulate the policy. (See p. 95.)

GAO recommends that the Directors, OMB and the National Science Foundation, and the Secretary of Energy fully cooperate in the study. (See p. 95.)

In carrying out his responsibility for implementing a sound national high energy physics program, GAO recommends that the Secretary of Energy formally institute the development of near-, mid-, and long-range plans on a periodic basis. This would ensure that agreed upon program objectives and strategies are appropriately pursued. To provide visibility over the direction of the program, GAO recommends that these plans be submitted to the Congress for its information and use in carrying out its budgetary and oversight responsibilities. (See pp. 95 and 96.)

MATTERS FOR CONSIDERATION BY THE CONGRESS

GAO believes the Congress, through its position of having oversight and budgetary responsibilities over all Federal basic science programs, could provide valuable input into the final determination of what the overall objectives of the U.S. high energy physics program should be, as well as the appropriate strategies and necessary funding levels. (See p. 96.)

AGENCY AND LABORATORY COMMENTS

GAO obtained comments from four agencies and six laboratories on a draft of this report. Generally, they were concerned with some of GAO's interpretations of the facts presented in the draft report. They pointed out a need to better recognize the scientific merit of the efforts pursued and the merits of program flexibility. They also generally disagreed with the proposed conclusions and recommendations. (See app. VI to XV.) Several expert consultants made similar comments in providing GAO advice.

GAO carefully considered the comments, and where appropriate, made changes. Throughout this report, greater recognition has been given to the scientific merit of the efforts pursued and the need for flexibility. Proposed recommendations intended to help ensure integrity between funding and the efforts pursued through fiscal accountability were deleted because they may have resulted in unduly restricting the flexibility needed for physicists to pursue scientifically meritorious efforts in the future. Instead, to help ensure integrity between efforts pursued and funding GAO now recommends that a three-tier planning system, similar to that proposed by the former Energy Research and Development Administration in 1975, be formally instituted. In addition, while the agencies and laboratories questioned whether the proposed Office of Science and Technology-led study would be helpful or needed, GAO continues to believe such a study would be helpful and should be pursued. Hence, although substantial changes have been made to portions of the draft report, this report's basic thrust remains the same. The agencies' and laboratories' views, and GAO's evaluation, begin on page 97.

Contents

ABBREVIATIONS

AEC	Atomic Energy Commission
CERN	European Organization for Nuclear Research
DOE	Department of Energy
ERDA	Energy Research and Development Administration
Fermilab	Fermi National Accelerator Laboratory
FY	fiscal year
GAO	General Accounting Office
GeV	billion electron volts
M	million
NSF	National Science Foundation
OMB	Office of Management and Budget
OSTP	Office of Science and Technology Policy
R&D	research and development
TeV	trillion electron volts

CHAPTER 1

INTRODUCTION

High energy physics, sometimes called elementary particle physics, is the study of the structure and properties of matter and energy in their most basic form. Research is carried out by using accelerators which generate high velocity beams of particles, such as electrons or protons, with energies of 1 billion electron volts (GeV) 1/ or greater. The principal result of this research is knowledge of the fundamental properties of matter, energy, and their transformations. The Department of Energy (DOE) and the National Science Foundation fund accelerator laboratories and university researchers, the Office of Management and Budget (OMB) is principally involved in approving the funding levels, and the Office of Science and Technology Policy (OSTP) is a source of scientific and technological advice for the President and OMB. The United States is seeking to maintain its world leadership in high energy physics. In fiscal year 1980, the U.S. program is supported with about $350 million in Federal funds, or about 8 percent of the $4.5 billion total Federal funding of basic research. A more detailed discussion of the Federal role and funding is presented in chapter 3.

NATURE OF HIGH ENERGY PHYSICS

High energy physics is a basic science which studies and investigates the nature of subnuclear, or "elementary," particles, their interaction with one another, and the energy which binds them together. The related research includes experimental studies and theoretical analyses which seek to provide new insights into the ultimate constituents and structure of matter, the nature of the known fundamental forces of nature, and the relationship among the forces. Physicists expect to uncover phenomena which could revolutionize man's way of thinking about nature.

To probe deeply into the nature of matter and energy, physicists use intense beams of very energetic particles which are available only through high energy particle accelerators or exploding stars. Beams of particles produced by an accelerator, in effect, provide a "light" for the physicist

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1/One electron volt is the amount of energy gained by a particle of unit charge accelerated through an electric field produced by one volt. This report does not discuss medium-energy facilities and research which involve accelerators that operate at energy levels below 1 GeV.

to "see" the inner nature of protons and electrons, their antiparticles (antiprotons and positrons), and other subnuclear particles. In this sense the accelerator is analogous to a super microscope that enables man to study the substructure of nuclear particles that have dimensions billions of times smaller than the smallest object visible through an optical microscope. As physicists have been delving deeper into the "heart" of matter, higher energy accelerators have been required to study the particles. These large accelerators, as well as the detection and analysis apparatus for specific experiments, are expensive and are often developed using state-of-the-art technology.

Historically, the accelerator energies for the processes studied by the high energy physicists have increased by a factor of 10 roughly every 7 years. Such increases have been made possible by the development of new particle accelerator and storage ring technologies. 1/ However, present discussions about future machines tend to focus on extensions of existing technology. For the frontier machines capable of providing the most sought after physics discoveries (such as (1) electron-positron colliding beam storage rings, (2) proton-proton colliding-beam storage rings, and (3) proton circular accelerators producing beams on fixed targets), future costs may increase rapidly with energies. Thus, unless new technology becomes available to limit future costs, future growth in energies may not be affordable.

The growth of particle accelerator technologies has paralleled a corresponding development in the techniques of detecting and identifying particles and in measuring their trajectories. A variety of particle detectors have been developed, such as spark chambers and bubble chambers, for observing and distinguishing elementary particles and their interactions. In the bubble chamber, electrically charged particles pass through low-temperature liquid hydrogen and leave trails of bubbles. The trails are photographed and subsequently analyzed. In spark chambers, charged particles pass through a parallel array of electrically charged metal plates. The spaces between the plates are filled with an inert gas. Events occurring within the chamber are revealed by sparks which jump between the plates at the points traversed by the charged particles. The events are recorded optically or electronically.

1/Although some colliding beam storage ring machines technically are not accelerators because they only maintain particles at the energies at which they enter the rings, for simplicity we refer to them as accelerators throughout the report.

Other detectors have been designed as a result of the rapid development of electronic devices in recent years. Among the modern detectors are proportional chambers and drift chambers, which use the ionization of gasses along the path of a charged particle. Scintillation counters use the light emitted by a charged particle passing through that detector. Each of these detectors has properties which make it especially suitable for studies of specific properties of particles and the forces which hold them together.

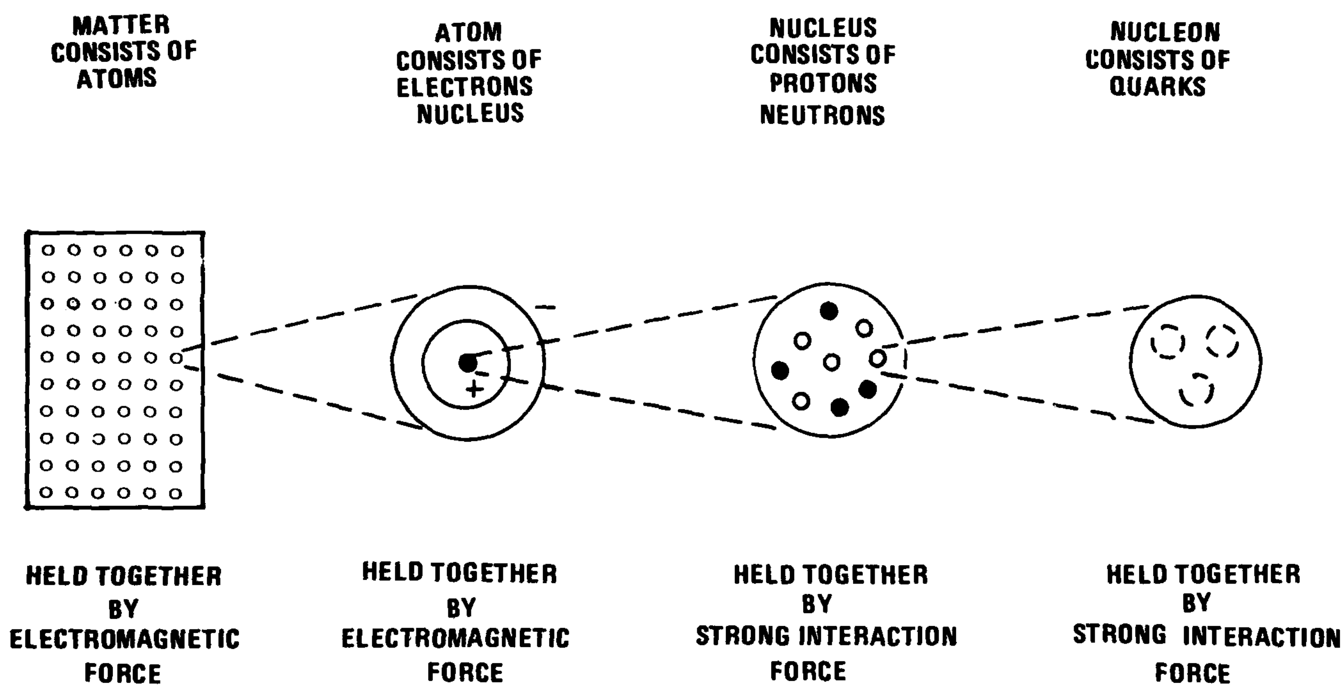
After an experiment is completed, the resulting data are analyzed and evaluated. Related findings are generally published in scientific journals.

The U.S. high energy physics program is currently synthesizing the results of recent discoveries, as well as continuing with exploration that could lead to new discoveries. Based on recent experimental data, physicists believe that the weak interaction and the electromagnetic interaction, two of the four fundamental forces of nature, are different manifestations of the same unified force. The weak interactions describe the forces governing the radioactive decay of many of the observed particles and nuclei. Electromagnetic interactions describe the forces which hold electrons to the nucleus of an atom and hold atoms into molecules. The other two forces are the strong interaction and the gravitational force. In addition, recent discoveries of "quarks" (the name given the basic set of constituents that protons, neutrons and other elementary particles are theorized to consist of) and "gluons" (believed to be the energy-carrying particle of the strong force) indicate progress toward a basic understanding of strong interaction forces, which hold nuclei together. Physicists' interpretations of the data indicate that the strong force may in the future be unified with the weak and electromagnetic forces, which would bring them a step closer to the goal of describing all of the four basic forces in a unified framework pioneered by Albert Einstein. An illustration of physicists' emerging understanding of matter is on the following page. Physicists believe that such knowledge will enhance man's understanding of nature and help him use his resources to control the environment.

BENEFITS OF HIGH ENERGY PHYSICS

The principal result of high energy physics research is fundamental knowledge. The value of this knowledge is not easily quantifiable in dollars, but is more of a subjective value. In addition, the benefits of high energy physics research can be categorized as (1) the potential practical applications of research discoveries, (2) its cultural value to society, and (3) spinoff benefits from the means and methods

EMERGING UNDERSTANDING OF BASIC STRUCTURE OF MATTER



Source: Division of High Energy Physics, U.S. Department of Energy used in carrying out the research. In addition, economists and scholars generally agree that a high positive correlation exists between research and development (R&D) and economic growth.

A somewhat technical listing of the major U.S. high energy physics accomplishments, as provided by DOE, is presented in appendix I.

Potential practical applications of research discoveries

Although high energy physics research cannot guarantee, or even predict, practical applications of a research discovery, knowledge gained about elementary particles and their interactions may have future applications. When first found in other physics fields, many discoveries such as electricity, magnetism, and radioactivity, were considered to be unimportant side effects of nature. Today, these phenomena have been exploited for numerous technological applications. Thus, although high energy physics research seemingly deals with very esoteric activities that are well understood only by physicists, the knowledge gained from such research may someday have useful applications.

Research discoveries in high energy physics have helped the scientists in fields of astrophysics and cosmology study the nature of the universe. Some research is also being carried out in other fields, such as cancer radiation therapy and inertial confinement fusion, to apply the knowledge gained of elementary particles toward a practical use. At this time, however, no one knows for sure whether or where past or future high energy physics research discoveries will find practical applications. In this regard, a 1979 National Academy of Sciences report 1/ pointed out that the study of matter is much like an "act of faith"--past successes justify having faith in achieving future successes.

Basic science successes of the past are often cited as being analogous to the potential payoff for high energy physics research. For example, the work of Copernicus, Galileo, and Kepler preceded Newton's formulation of the basic laws of dynamics and the expression of the gravitational law in a mathematically symmetrical form, which was expanded by Einstein in his general theory of relativity. Today this theory can be used to explain the motion of everything affected by

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1/Science and Technology, A Five-Year Outlook, The National Academy of Sciences, W.H. Freeman and Co., 1979.

gravity, from baseballs to satellites to planets. The laws of electromagnetism required over a century for their refinement, and James Maxwell's search for a mathematically symmetrical treatment of electric and magnetic forces was an essential element in their perfection. These laws now permit an understanding of electromagnetic radiation, including light, and enabled the development of devices such as the radio, television, radar and computers. Einstein's development of the special theory of relativity was an essential key to the development of the atom bomb and the production of nuclear energy.

DOE high energy physics program officials pointed out:

> "Most discoveries in fundamental science have led to strong practical use. The inverse is also true: rarely has there been an important practical technical innovation that was not based upon some insight of basic science."

Perhaps some day high energy physics will uncover a new law of nature which may provide the insights to matter and energy needed to solve the problems of tomorrow.

Cultural value

The high energy physics community often refers to the cultural value of the field to society. This value is--as with any basic science--impossible to fully quantify, or for that matter, render a judgment as to the significance of its worth. DOE officials, however, provided us with the following view.

> "In the past two hundred years, mankind has found some truly fundamental causes for what is going on in the natural world and we have learned to penetrate below the surface of the phenomena that are ordinarily observed around us. Basic science has searched for and has found a regular world beneath the seemingly irregular flow of natural events and has studied its laws and interrelations. This search goes on and reaches ever deeper layers of nature finding at the same time new and unexpected forms of natural events. High energy physics is a spearhead in this endeavor, it tries to reach the deepest level of the material world.
>
> "Basic science is one of the cornerstones of our Western civilization. No other civilization has created anything like it. It is probably the major contribution of our time to the great creations"

"of the human spirit. It is one of the positive, constructive elements in the time when so many values are undermined and overthrown.

"A vigorous basic science creates a spiritual climate which affects the whole intellectual life of the Nation by its influence on the way of thinking and by setting standards for many other intellectual activities. Applied sciences and technology adjust themselves to the highest intellectual standards which are strived for in the basic sciences. It is the style, the scale, and the level of scientific and technical work in pure research that attracts some of the most inventive spirits and brings the most active scientists to those countries where science is at its highest level. This is why many outstanding scientists have moved to the United States from other countries in the recent decades.

"The case for generous support for pure and fundamental science is as simple as this. Fundamental research sets the standards of modern scientific thought; it creates part of the intellectual climate in which our modern civilization flourishes. It pumps the lifeblood of ideas and inventiveness not only into the technological laboratories and factories, but also affects other cultural activities. It is a most vital and active part of our intellectual life, a part which we all should regard with pride as one of the highest achievements of our century."

Spinoff applications

The means and methods of carrying out high energy physics research can provide spinoff benefits. Through the development of technologies for use in the accelerators or detectors used as tools by physicists, other science fields have been enhanced or advanced. In addition to the technological spinoffs, the methods and experiences gained by high energy physics researchers tend to crossfertilize other sciences and technologies when researchers transfer to those other fields.

In developing new accelerators and detectors, the technologies used have been advanced. One recent example is a good illustration. The development of superconducting magnets for detectors and accelerators has helped advance the state-of-the-art of superconducting technology. In addition to advancing the state-of-the-art, the large accelerators currently being developed require large quantities of superconducting magnets and help establish a market for new technological products. By establishing such a market, the industrial capability to produce such materials has been enhanced.

In recent years, the accelerators and apparatus used for high energy physics research have been used for research in other science fields. For example, the radiation emitted by electrons circulating in a synchrotron or storage ring (referred to as synchrotron radiation) is being used as an important scientific tool for the study of matter in other research fields. Synchrotron radiation was once considered an undesirable by-product of particle accelerators. However, scientists are now harnessing this by-product into beams of ultraviolet and x-ray photons, 1/ which are being used in such areas of research as biology, condensed matter (study of matter in solid and liquid states) and surface matter physics, chemistry, and materials science.

In the late 1960s, a medium energy ring at the University of Wisconsin, called Tantalus (240 million electron-volts), became the first storage ring to be used as a source of synchrotron radiation. The success of synchrotron radiation as an experimental tool has resulted in other facilities being built expressly for this purpose in two modes--(1) as dedicated facilities and (2) as a synchrotron radiation laboratory attached in a "parasitic" mode to high energy physics colliding beam facilities. The Stanford Positron Electron Asymmetric Ring at Stanford Linear Accelerator Center has associated with it a major synchrotron radiation laboratory operating in a parasitic mode. In 1980, about 50 percent of the Stanford ring's operating time will be dedicated to the synchrotron radiation laboratory. A synchrotron radiation laboratory is being built in a parasitic mode to the new 8-GeV electron-positron storage ring at Cornell University and the National Synchrotron Light Source is being built at Brookhaven National Laboratory. Physicists point out that high energy physics accelerators and apparatus with higher intensity beams have helped expand the capabilities of carrying out synchrotron radiation experiments at a minimal cost.

Physicists also point out that in recent years, the gap between fusion R&D and high energy physics has narrowed. One of the inertial confinement fusion systems currently being researched uses heavy-ion fusion beams. Although large high energy particle accelerators have not yet been used for heavy-ion fusion research, some scientists believe that accelerators

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1/A photon is a quantum (a quantity) of electromagnetic radiation.

similar to the large accelerators in operation today will someday be used to produce energy from heavy-ion fusion reactions. Also, accelerator experts, including some from high-energy physics, have been designing a light-ion beam fusion facility for which construction is expected to start at Sandia, New Mexico, in fiscal year 1981.

One prominent physicist told us that he predicts that the next important spinoff application of accelerator technology will be pulsed neutron sources. Currently, low-energy, steady-state neutron beams from reactors are used as tools for investigating the characteristics of condensed matter. The use of steady-state neutrons, however, has disadvantages which impose long periods of data collection time on experiments. Scientists believe that pulsed, high-energy charged particles (electrons or protons) have the potential for producing neutrons, through various nuclear reactions, that are better suited for some experiments. Thus, scientists envision that pulsed neutron sources will open up many new scientific areas for experimentation. In fact, work with such sources has already begun.

High energy physics research also provides physicists with experience in tackling unexplained phenomena and scientific methods for exploring them. The physicists are exposed to challenges, such as creating conditions prevailing in exploding stars, under exacting conditions within a laboratory. According to DOE, more than half of the high energy physics graduate students ultimately use and apply their knowledge and experiences gained in jobs in other science and technology fields. The physics community believes that this transfer of persons trained in high energy physics to other scientific disciplines helps crossfertilize those other disciplines and ultimately helps scientific and industrial growth.

SCOPE AND METHODOLOGY OF STUDY

In February 1978, the Director of Fermi National Accelerator Laboratory (Fermilab), the largest U.S. accelerator laboratory, resigned because of dissatisfaction with the U.S. high energy physics program. He cited budgetary issues, his inability to secure funds for a major accelerator advancement project, and lost U.S. prominence in the field. The resignation generated considerable publicity, kindled congressional interest, and called the U.S. program's status into question.

As part of our continuing interest in the vitality of U.S. basic science, we surveyed the U.S. high energy physics program. During our survey, we identified four major issues surrounding the vitality of the program.

--Is the United States losing its world leadership position in high energy physics?

--Can the infrastructure for managing the high energy physics program be improved?

--How much independence should accelerator laboratory managers have?

--What should be the U.S. policy toward high energy physics?

We then proceeded with the review in two phases.

Phase 1

In the first phase, we collected general information related to the four issues from literature searches and reviews of previous studies, Government reports, and technical journals; legislative histories; Federal agencies; and U.S. high energy physics accelerator laboratories. Based on this information, we developed an approach for reviewing the issues. This first phase culminated with an October 7, 1978, meeting with experts in which we obtained their views on the four major issues identified and our approach for reviewing them. The principal purpose of the meeting was to validate the issues and to obtain insights to some of the pitfalls to avoid in reviewing them.

The experts assembled represented those with an extensive knowledge of high energy physics and/or broad knowledge and experience in dealing with science policy and management matters. The experts present at the October 1978 meeting are listed in appendix II. In general, the experts validated the issues and, with some modifications, the review approach.

Phase 2

We then proceeded to the second phase of the review. Since most of the moneys for high energy physics relate to accelerator facilities and equipment for conducting the physics experiments, we focused our review principally on factors relating to the experimental research rather than theoretical research. We did not review other basic science fields, attempt to determine the appropriate level of Federal funding of high energy physics, make relative value judgments concerning high energy physics in comparison to other basic science fields, nor assess the merits of the results of the research itself. Instead, we analyzed the strategy, the compatibility of resource allocations with the strategy, the mechanisms for ensuring that resource allocations are adhered to, and explored some strategies and costs of selected alternative policy options. The laboratories' and agencies' input to the options is presented in appendix IV. The options, strategies, and costs are presented in this report, not as definitive plans to be pursued, but to merely illustrate the significant impact alternative options and strategies may have on the program and the Federal funds needed to carry out those options and strategies.

Our specific review objectives for this second phase were to

--outline the long-term changes in resources and results and the recent concerns of the scientific community which pertain to the vitality of the high energy physics research program;

--compare the trends in resources and results of the U.S. high energy physics research program with similar trends in the European high energy physics program;

--examine the compatibility of the Federal policy, planning, and funding for high energy physics;

--determine the mechanisms for allocating funds to and within high energy physics laboratories and examine whether the related decisions are made in the context of overall national program objectives; and

--identify the costs, advantages, and disadvantages for selected alternative policy options for the U.S. high energy physics program in the 1980s.

We made our review principally at DOE and the National Science Foundation headquarters offices, Washington, D.C., and at the three major U.S. accelerator centers--Brookhaven National Laboratory, Upton, New York; Fermi National Accelerator Laboratory, Batavia, Illinois; and Stanford Linear Accelerator Center, Stanford, California. We also conducted work at Argonne National Laboratory, Darien, Illinois; Wilson Synchrotron Laboratory, Ithaca, New York; Lawrence Berkeley Laboratory, Berkeley, California; European Organization for Nuclear Research (CERN), Geneva, Switzerland; Deutches Electronen Synchrotron, Hamburg, Germany; and the Office of Management and Budget, Washington, D.C. At each organization, we obtained information on past, current, and planned high energy physics activities by examining records and interviewing management officials. We also contacted an official of the Office of Science and Technology Policy, Washington, D.C., obtained his views, and reviewed documents he provided.

In addition, we obtained views on the vitality of the program from experimentalists in user groups conducting experiments at the accelerator laboratories. We also attended meetings of DOE's High Energy Physics Advisory Panel 1/ and obtained and reviewed its reports on various matters of concern to the high energy physics community.

Comments from Federal agencies, laboratories, and experts

We provided a draft of this report to all cognizant Federal agencies and U.S. accelerator laboratories for their review and comment. In addition, we concurrently provided 13 experts with copies of the draft to obtain their views and comments.

We received official comments from four Federal agencies and six U.S. accelerator laboratories. We considered their comments as discussed in chapter 7 before preparing the final report. The full texts of the Federal agencies' and laboratories' comments are included as appendixes VI through XV.

On February 26, 1980, we met with six experts to obtain their comments and views on our draft report. Seven other experts invited to the meeting were unable to attend. Experts commenting on the draft report are listed in appendix II.

This report reflects changes made to the draft after we considered the experts' comments. We greatly appreciate the time and effort that many of the agencies, laboratories, and experts spent providing us input and commenting on the report. Their comments contributed significantly toward assuring the quality and accuracy of the report, and in lending balance to the positions we have taken. We wish to emphasize, however, that the analyses, conclusions, and recommendations in this report are ours and do not necessarily agree, in whole or in part, with the views of any of the individual experts, agencies, or laboratories.

1/The High Energy Physics Advisory Panel reports directly to DOE's Director of Energy Research. Its charter is to review the program and provide advice on overall program balance, scientific priorities, and special problems.

CHAPTER 2

A PERSPECTIVE ON WORLD LEADERSHIP IN HIGH ENERGY PHYSICS

During the past 30 years, U.S. high energy physics research efforts have led the world in terms of significant physics results, but Western Europe is now spending more money than the United States and challenging that lead. Although the principal goal of high energy physics is to further man's understanding of matter and energy in their most basic form and their transformations, competition to be the first to discover such phenomena helps set the pace of the research efforts. Physics theoreticians develop physics theories based on past experimental results and predict future discoveries. The physics experimenter then seeks to make the major discovery which would prove or disprove the theory or theories. To have an opportunity to make the signficant discovery is important to the drive and morale of the high energy physicist.

HISTORICAL PERSPECTIVE

At the beginning of this century, physicists found that the atom, previously thought to be indivisible, is made up of smaller particles. Following the discovery that atoms are composite structures, the scientific forefront became nuclear physics. In 1930, two British physicists, John D. Cockcroft and Ernest T. S. Walton, achieved the first artificially produced nuclear disintegration by accelerating protons within a nucleus using what became known as the Cockcroft-Walton accelerator. In the same year, Ernest O. Lawrence, working at the University of California at Berkeley, developed an accelerator, called the cyclotron, which eventually yielded particles with energies of 8 million electron volts.

Prior to World War II, university funding of basic research was common practice and Lawrence's cyclotron was funded by the University of California. At the end of the war, the Federal Government established a peacetime atomic research program and provided $170,000 for the construction of a 200-million electron volt accelerator at Berkeley. Berkeley later built a 6-GeV accelerator called the Bevatron. To provide scientists and universities on the East Coast with a research facility, Brookhaven National Laboratory was established and a 3-GeV proton accelerator was constructed. The accelerator went into operation in 1952 and, including subsequent improvements, cost a total of $9.3 million to construct. A photograph of the present high energy physics facilities at Brookhaven is on the next page.

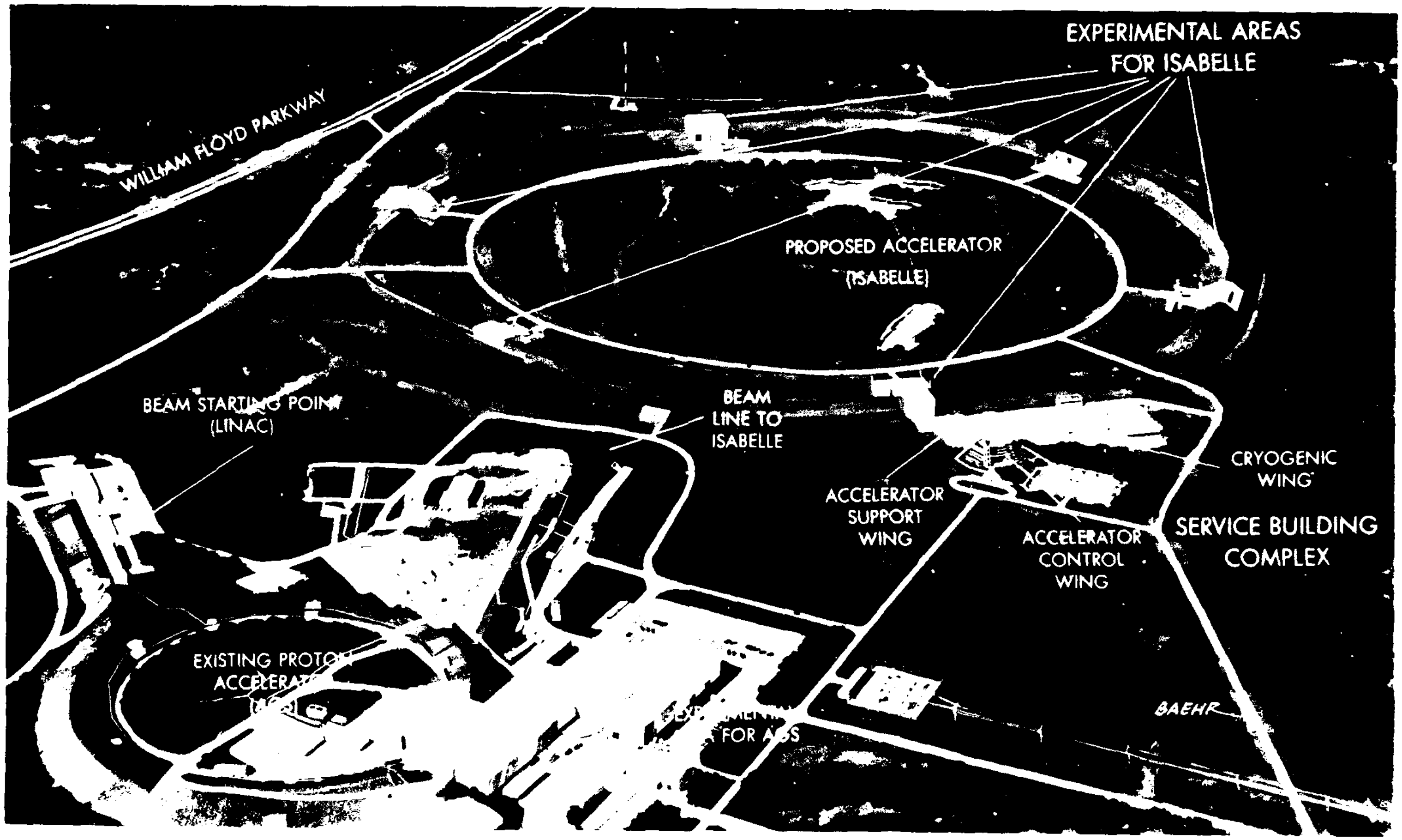


PHOTO COURTESY OF BROOKHAVEN NATIONAL LABORATORY

HIGH ENERGY PHYSICS FACILITIES AT BROOKHAVEN NATIONAL LABORATORY

During the 1950s the Berkeley and Brookhaven accelerator facilities were upgraded with higher energy accelerators and American supremacy in high energy physics was unchallenged. Under the former Atomic Energy Commission, high energy physics and nuclear physics were combined in one basic research program. In March 1964, as high energy physics grew with costlier and larger facilities, the Commission formally established a separate program for providing high energy physics support.

Although the United States clearly led the world in high energy physics research, concern existed that other nations might build larger accelerators and assume world leadership. In the midst of the "cold war" of the 1950s, the Soviet Union's construction of a 10-GeV accelerator and plans to build a larger accelerator were cited in justifications for building larger accelerators in the United States. For example, Soviet competition was a major factor in seeking authorization for building an accelerator at Argonne. Although the Soviets eventually built the planned accelerator, and at times have had the world's highest energy accelerator--the 10-GeV accelerator in 1957 and the 76-GeV proton synchrotron at Serpukhov in 1967--technical or other problems have limited their experimental effectiveness. To date, few significant physics discoveries have been attributed to research conducted on Soviet accelerators.

In 1959, the European Organization for Nuclear Research, a consortium of 12 Western European nations known as CERN, 1/ began operating a 28-GeV proton synchrotron accelerator. This accelerator enabled Western Europe to begin challenging for world leadership in the field. Both Western Europe and the United States have continued to build larger and/or more sophisticated accelerators.

The energy levels, construction costs, and year of initial operation of U.S. high energy physics accelerators completed after 1960 or currently under construction are listed on the following page.

1/CERN, located in Geneva, Switzerland, has 12 member nations --Austria, Belgium, Denmark, France, the Federal Republic of Germany, Greece, Italy, the Netherlands, Norway, Sweden, Switzerland, and the United Kingdom.

Accelerator	Energy level	Construction costs	Year of initial operation
	(GeV)	(millions)	
Alternating Gradient Synchrotron	30	$ 30.6	1960
Cambridge Electron Accelerator	6	10.2	1962
Princeton-Pennsylvania Accelerator	3	11.6	1963
Zero Gradient Synchrotron	12.5	51.4	1963
Cornell Electron Synchrotron	12	11.3	1965
Stanford Linear Accelerator	a/23	113.6	1966
Fermi National Accelerator	400	243.5	1972
Stanford Positron Electron Asymmetric Ring	b/2.5x2.5	c/6.0	1972
Positron Electron Project	18x18	78.0	1980
Cornell Electron Storage Ring	8x8	d/20.7	1979
Isabelle (note e)	400x400	275.0	1986
Energy Saver (note e)	500	46.6	1982

a/Modified in 1979 to achieve energies of 30 GeV.

b/Modified in 1974 to achieve energies of about 4-GeV x 4-GeV.

c/Not authorized as a construction project; authorized as an experimental facility primarily using equipment funds.

d/Estimated cost includes about $5.9 million for a magnetic detector and an upgrade of a computer facility.

e/Estimated; project is authorized and under construction.

The newer accelerators build upon or use previous accelerators. The diagram on the following page illustrates how the Alternating Gradient Synchrotron will be used to inject accelerated particles to Isabelle, which is being constructed at Brookhaven. In addition, the Stanford Linear Accelerator is being used as an injector to the Positron Electron Project at Stanford, the Cornell Electron Synchrotron was converted to the Cornell Electron Storage Ring, and the Fermilab accelerator is being converted to the Energy Saver.

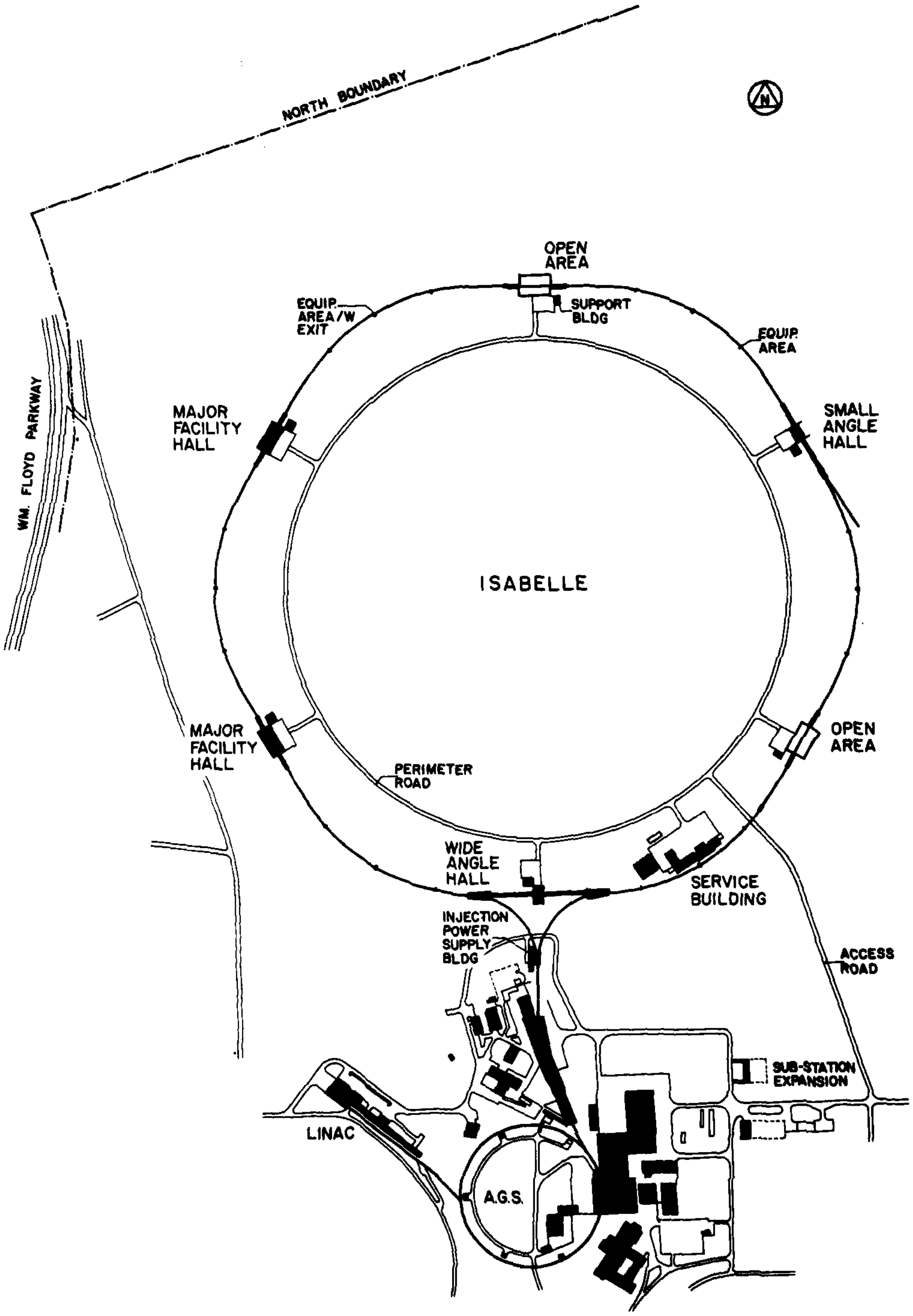
Some accelerators have been shut down. The Cambridge Electron, Princeton-Pennsylvania, and Zero Gradient Synchrotron accelerators are examples. Although these accelerators could have continued to be used to contribute to the physics research, such shutdowns have been made to conserve funds for the more forefront research. Western Europe similarly has been shutting down accelerators which are no longer capable of being used for forefront research.

Funding trends

The span of 8 years between the construction of the Fermilab accelerator and the Positron Electron Project is reflected by the Federal funding of high energy physics. Although Federal funding levels rose slightly from fiscal years 1966 to 1976 in current year funds, funding declined in constant dollar value after the construction of Fermilab which began operations in 1972. With the initiation of the construction of the Positron Electron Project, funding levels have again increased, but the levels are still lower than the peak funding achieved in 1970. A graph showing Federal funding of high energy physics--in both current year and fiscal year 1979 dollars--from fiscal year 1966 through 1980 is on page 19.

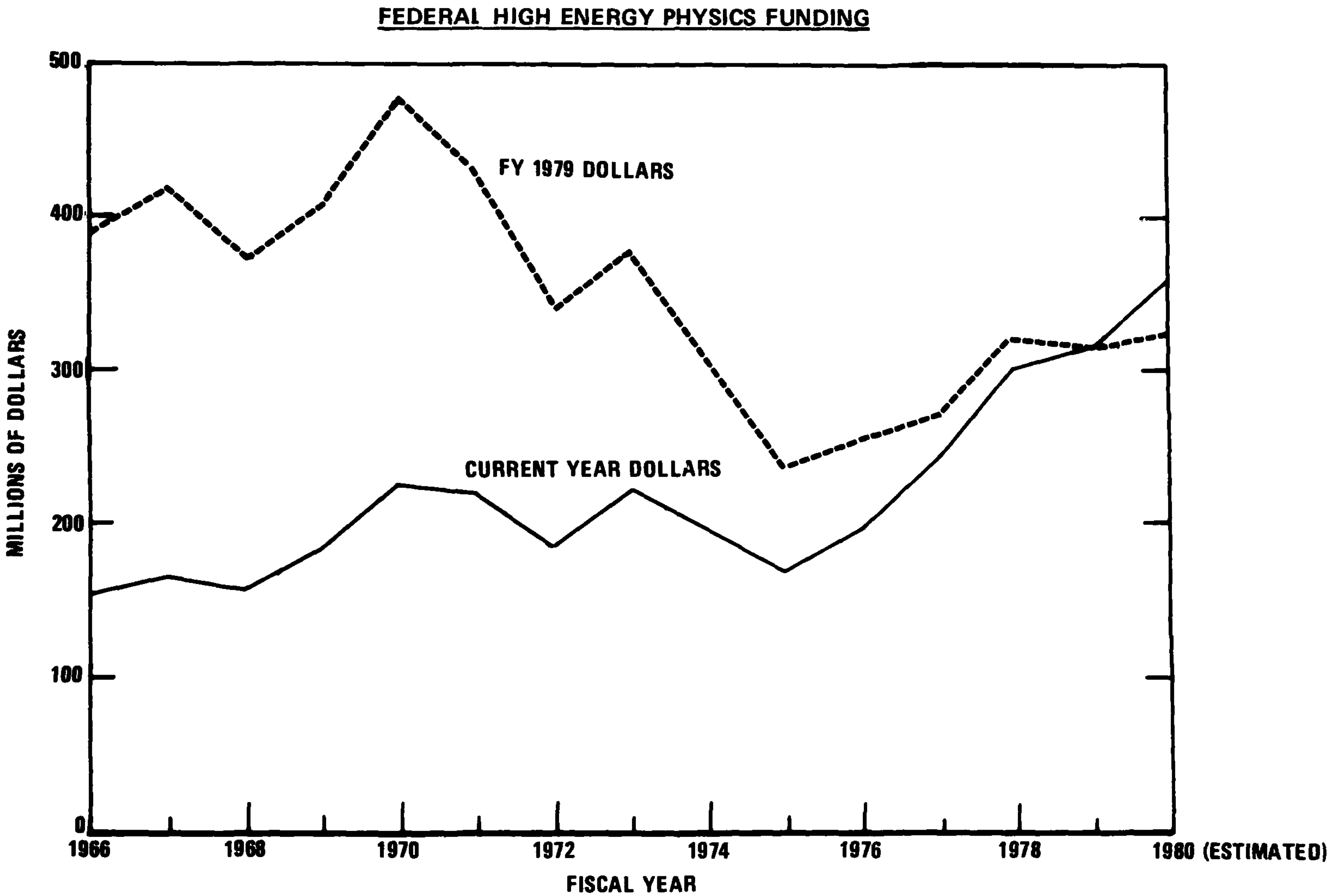
While U.S. high energy physics funding peaked in 1970, Western Europe's funding continued to climb, peaking in 1974. In recent years a number of studies have been made comparing the funding of high energy physics in Western Europe and the United States. Although such studies vary because of differences in cost indexes and monetary conversion factors used, they each show that Western Europe's high energy physics funding equalled that of the United States about 1970 and since has exceeded U.S. funding. Western European funding has been over twice U.S. funding in recent years on a strict currency conversion basis.

The physics communities in both continents agree, however, that a more appropriate comparison is the relative buying power for high energy physics purposes. In 1978, Stanford Linear Accelerator Center staff studied the



SOURCE: BROOKHAVEN NATIONAL LABORATORY

DIAGRAM OF THE EXISTING ALTERNATING GRADIENT SYNCHROTRON AND THE PLANNED ISABELLE.


FEDERAL HIGH ENERGY PHYSICS FUNDING
FY 1979 DOLLARS
CURRENT YEAR DOLLARS
MILLIONS OF DOLLARS
0
100
200
300
400
500
1966
1968
1970
1972
1974
1976
1978
1980 (ESTIMATED)
FISCAL YEAR

comparative funding trends in terms of fiscal year 1978 funds, using a ratio of 3.14 Swiss Francs to the dollar. They estimated that Western Europe's funding exceeded the United States' by about $50 million in 1978. However, due to the increasing strength of the Swiss Franc, European officials told us their studies have indicated that the monetary exchange rate which would reflect buying power in 1978 was closer to 2.5 Swiss Francs to the dollar. Hence, they concluded Western European funding of high energy physics exceeded that of the United States by about $100 million to $140 million in 1978. Regardless of the precise amounts, Western European high energy physics funding has clearly exceeded that of the United States in recent years.

IMPORTANCE OF U.S. LEADERSHIP

In broad terms, the principal objective of the U.S. high energy physics program is to further the quest of knowledge. To be at the forefront, which is considered essential to furthering this quest, the program seeks to maintain its world lead in the field. This goal of maintaining the U.S. position of leadership has been cited in various congressional hearings since at least 1963.

Although the principal goal of high energy physicists is to make significant physics discoveries and thus advance the state of knowledge, a leadership position is important because it provides a nation's or group of nations' physicists greater opportunities for making those significant discoveries. To help illustrate this, we will discuss current high energy physics efforts toward discovering a particle called the intermediate vector boson. This particle is theorized to be the carrier of the weak interaction force. Discovery of this particle would provide experimental evidence needed to support a physics theory which would unify the weak interaction and electromagnetic forces of nature. To have the opportunity to discover new particles such as the intermediate vector boson is important in maintaining the drive and morale of the high energy physics experimenter. We would like to point out that the accelerators referred to in the following discussion would be capable of carrying out other physics research, and their sole purpose is not the discovery of this particle.

Present physics theories imply that the intermediate vector boson can be found through proton-proton, proton-antiproton, or electron-positron collisions. The 28-GeV x

28-GeV 1/ Intersecting Storage Ring at CERN was once believed to be capable of exploring a part of the energy range where the particle was theorized to be. This accelerator has been in operation since 1971, but no indication of the intermediate vector boson has been found and more recent theory predicts the particle can be found at a higher energy. Thus, the best hope of discovering the particle with proton-proton colliding beams is with very high-energy, high-luminosity 2/ beams such as those that would be available from Isabelle, a 400-GeV x 400-GeV proton-proton colliding beam accelerator currently under construction at Brookhaven. Isabelle, however, is not scheduled to be completed until 1986.

Although no proton-antiproton colliding beam accelerators are yet in operation, CERN is constructing a 270-GeV x 270-GeV ring which is expected to begin operations in 1981. Proton-antiproton collisions from the ring are expected to be able to help discover the intermediate vector boson. However, CERN has encountered some technical difficulties in building the ring, and it may not be able to attain the desired luminosity. If sufficient luminosity is not attained, the particle might not be detected.

Fermilab has plans to build a 1,000-GeV x 1,000-GeV proton-antiproton colliding beam ring. Its design has some advantages to that of CERN's ring and laboratory officials are optimistic that it would have sufficient energies and luminosity to be capable of discovering the particle, if it is not already discovered by CERN's ring. However, construction funds, requested for fiscal year 1981, have not yet been authorized for the project. If funds are provided, Fermilab believes it could be completed in fiscal year 1983.

High energy physicists are anxious to achieve electron-positron collisions at energies around 100 GeV (50-GeV x 50-GeV) as soon as possible. Current physics theories predict that the neutral intermediate vector boson has a mass near 90 GeV. Present physics knowledge indicates that electron-positron collisions will provide the best type of interactions for studying in detail the merging of the weak and electromagnetic interactions. Thus, it may be possible to discover the intermediate vector boson through electron-positron

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1/The expression "28-GeV x 28-GeV" signifies the collision of of two 28-GeV particle beams, resulting in an energy level of 56 GeV at the point where the two beams collide.

2/Luminosity is proportional to the rate at which particles collide in a colliding beam.

collisions if it is not previously found through proton-proton or proton-antiproton collisions. Or, if it is previously found, such collisions would be useful for studying its characteristics. The problem, however, is that an electron-positron colliding beam accelerator of sufficient energies and luminosity does not exist.

CERN has plans to build a large electron-positron accelerator which is to be built in phases at a cost of about $1 billion and provide energies of up to 130 GeV per beam. Funds have not yet been authorized, but CERN expects the first phase of the ring to be built and operating in the mid-1980s (possibly 1986). If CERN does not proceed with the project, West Germany expects to build it or a similar accelerator.

Stanford has proposed to build an accelerator, called the Single Pass Collider, estimated to cost about $60 million, which would provide electron-positron collisions of up to 50-GeV x 50-GeV from its linear accelerator. If they are able to obtain funding and start construction in fiscal year 1982, Stanford officials believe they can start operations in October 1984. CERN's planned machine would have 10 times the luminosity and be better suited to study the characteristics of the intermediate vector boson. However, Stanford's proposed collider could begin to operate 1 year or more ahead of CERN's machine and present another opportunity to discover the intermediate vector boson with a U.S. machine.

Some U.S. and European physicists and science policy experts told us that to carry out the most significant physics such as the discovery of the intermediate vector boson, physicists require access to the higher energy, sophisticated accelerators. In this regard, some U.S. science policy experts told us that "leadership" should be sought using the latest frontier techniques, accelerators, and equipment because to be content with less would undermine the morale and drive of U.S. scientists and risk falling further behind. For example, a former laboratory director told us that the U.S. style has been to "push the accelerators to the limit" and that this is one of the reasons for the program's success.

Without U.S. frontier accelerators, a temporary transfer of top physicists to other countries may allow U.S. physicists to be actively involved in significant physics discoveries. However, carrying out experiments overseas tends to preclude U.S. students and post doctoral personnel from participating in those discoveries. The U.S. physics community is concerned that researchers and young people will be missing out on the excitement of being involved in making significant discoveries and may seek careers in other fields. Furthermore, they are concerned that the intellectual stimulation and excitement, which accompanies teaching by individuals who have been leaders in making new discoveries, will be diminished.

U.S. POSITION OF LEADERSHIP
IS BEING CHALLENGED

As our discussion of the importance of leadership and the funding indicates, Western Europe is challenging the U.S. lead in the field. Western Europe is developing new accelerators, and many members of the physics community are concerned that the U.S. funding level is insufficient to withstand the Western European challenge.

With respect to existing accelerator capabilities, the United States appears to be second to Western Europe. The accelerators can be classified into groups based on their types and the particles accelerated. Comparing the larger worldwide accelerators within each type indicates the current competition for leadership in high energy physics is principally between the United States and Western Europe as indicated below.

--Proton synchrotron. Fermilab and Western Europe's CERN each have 400-GeV proton synchrotrons. These accelerators started operations in 1972 and 1976, respectively, and have current energy capabilities about six times higher than other accelerators of this type. A photograph of Fermilab is on the following page. CERN's accelerator is comparable to Fermilab's, but its operations are better funded. The Soviet Union has been operating a 76-GeV proton synchrotron since 1967. Fermilab is modifying the present accelerator by means of the Energy Saver superconducting magnet ring to achieve energies of up to 500 GeV. Fermilab plans to ultimately achieve energies of up to 1 trillion electron volts (TeV), but the additional modifications have not yet been authorized. The Soviet Union recently approved plans for a 3-TeV accelerator which is to be completed in about 1988.

--Electron synchrotron. While electron synchrotrons still operate, current technology limits the maximum energy range that can be achieved. During the period March 1968 to October 1977, the United States had the highest energy accelerator of this type--the Cornell Electron Synchrotron. This accelerator was operating at 12 GeV when it was shut down for conversion to the Cornell Electron Storage Ring. West Germany and the Soviet Union still operate

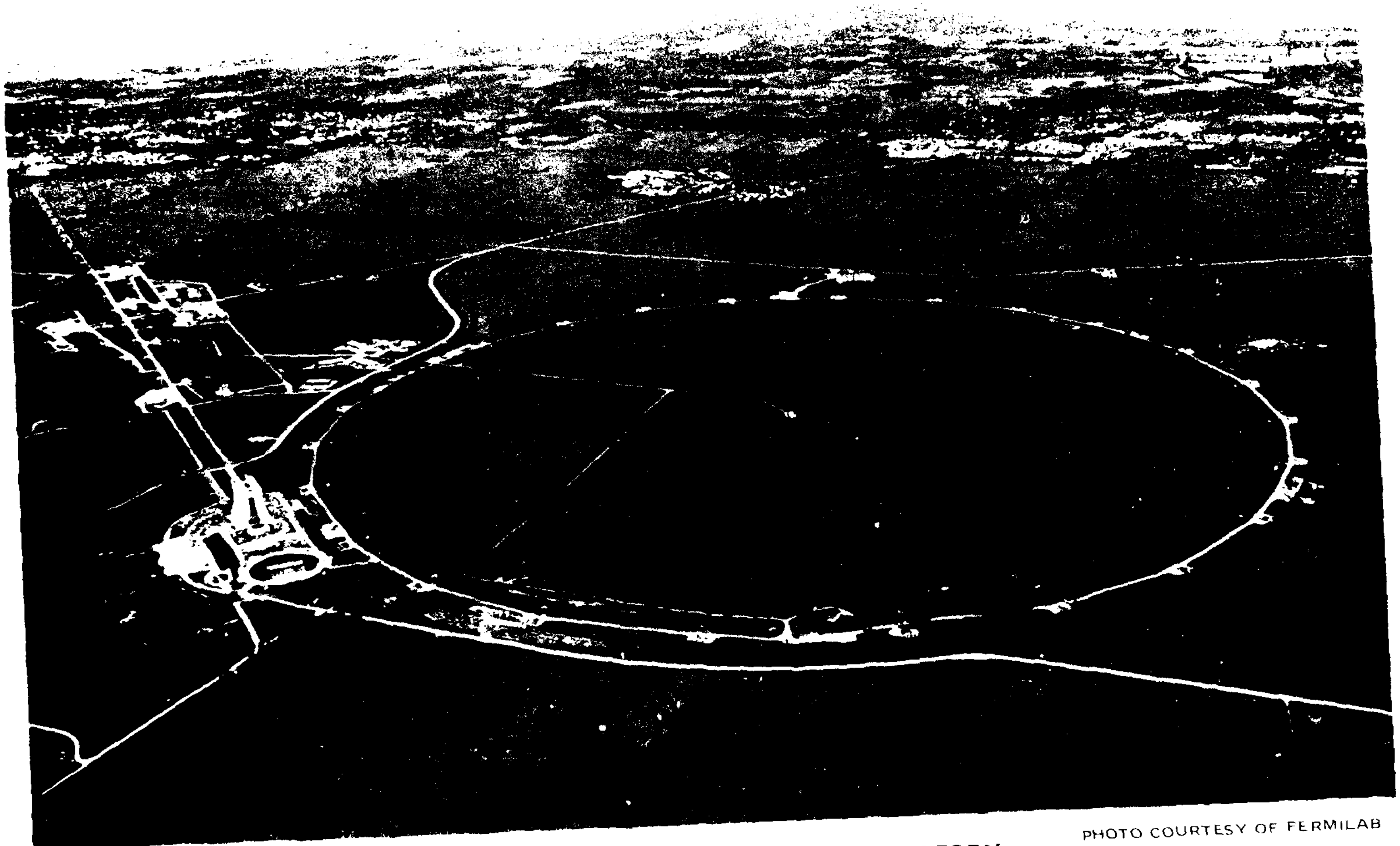

PHOTO COURTESY OF FERMILAB

FERMI NATIONAL ACCELERATOR LABORATORY

electron synchrotrons with energies of 7.5 and 7 GeV, respectively.

--Electron linear accelerators. The United States has the highest energy accelerator of this type. In 1966, Stanford started operating its linear accelerator, which now achieves energies of 30 GeV. This is about 15 times higher than the energies of the linear accelerators in France and the Soviet Union.

--Proton-proton rings. Europe, with CERN's 28-GeV x 28-GeV Intersecting Storage Ring, built in 1971, has the only proton-proton colliding beam accelerator in the world. The United States is building Isabelle, scheduled to be operational in 1986, which will have 400-GeV rings.

--Electron-positron rings. The United States and West Germany have been competing with this type of accelerator for some time. Stanford's Positron Electron Asymmetric Ring, built in 1972, can achieve energies of up to 4.2-GeV x 4.2-GeV. In 1974, West Germany began operating a similar accelerator at 5-GeV x 5-GEV. In 1978, West Germany clearly took the lead with an accelerator called Petra, which provides energies of up to 19-GeV x 19-GeV. The United States with Stanford's Positron Electron Project, which provides energies of up to 18-GeV x 18-GeV and began operating in 1980, is seeking to catch up. (See photograph on the following page.) The United States also has built an 8-GeV x 8-GeV accelerator of this type at Cornell, and the Soviet Union a 7-GeV x 7-GEV accelerator at Novosibirsk. Other existing electron-positron rings are relatively small. Western Europe would clearly dominate the area of electron-positron accelerators if it builds a proposed large electron positron accelerator at CERN, which may provide energies of up to 130 GeV per beam. West Germany also plans to complete 30-GeV x 30-GeV rings by late 1986.

--Proton-antiproton rings. No such rings are currently operating. CERN is constructing 270-GeV x 270-GeV proton-antiproton colliding beam rings, scheduled to begin operating in 1981. While Fermilab plans to build similar higher energy rings, construction funds for the project have not yet been authorized, and CERN is likely to have the world lead on this type of accelerator.

--Linear electron-positron colliding beams. No such accelerator is currently operating or under construction.

PHOTO COURTESY OF STANFORD LINEAR ACCELERATOR CENTER

STANFORD LINEAR ACCELERATOR CENTER WITH THE POSITRON ELECTRON PROJECT RING LOCATION SHOWN.

Stanford has proposed to build the Single Pass Collider, which will have two beams (one electron and one positron) from its linear accelerator collide at energies of up to 50-GeV x 50-GeV. This accelerator would also have the capability to be upgraded to about 70-GeV x 70-GeV, if the physics dictates.

--Electron-proton rings. No such accelerator is operating or under construction. West Germany has proposed that an accelerator, colliding 30-GeV electrons with 820-GeV protons, be built at Hamburg, possibly to begin operations in late 1988. A U.S. group at Columbia University is developing a proposal to build a similar accelerator, possibly at an existing laboratory such as Fermilab or Brookhaven, but the location has not yet been selected. A Canadian group is developing a proposal to build an electron ring to collide with Fermilab's proton ring.

U.S. and Western European physics officials told us that future frontier high energy physics research is expected to be carried out on proton synchrotrons, proton-proton rings, electron-positron rings, and proton-antiproton rings--not necessarily in this order of priority. As indicated above, Western Europe appears to have the world lead in each of these categories. However, when Isabelle is completed, the United States should have the world leader proton-proton rings.

The United States may also lead the world in proton synchrotrons and proton-antiproton rings if Fermilab's proposed projects are built--until the Soviet Union builds its 3-TeV accelerator. Maintaining a U.S. leadership position with such accelerators would depend on whether the significant physics discoveries are ultimately made on these or other types of accelerators.

Many members of the U.S. physics community believe that the existing and planned accelerators will provide the tools needed to withstand the present European challenge, but they are concerned that fund limitations will preclude them from successfully exploiting the new accelerators and from carrying out the R&D needed for future higher energy accelerators. They point out that the Positron Electron Project, Isabelle, the antiproton-proton ring, and the 1-TeV proton synchrotron may provide the accelerators needed to carry out the physics. However, rising operating and construction costs have forced difficult choices of priority, and adequate support has not been devoted to some aspects of the program. These physicists noted that fund limitations have resulted in experiments using less than optimal detectors, experiments being stretched out over time, a low utilization of existing accelerators, and insufficient R&D for the technology needed for future accelerators. They cautioned that Western Europe, with greater funding, can better exploit its accelerators and may make the significant physics discoveries and assume leadership of the field.

In this regard, in December 1979, a prominent Nobel laureate in physics told the Subcommittees on Energy Research and Production and on Science Research and Technology, House Committee on Science and Technology that in 1979 much of the new and most exciting results of particle physics came from Europe. He attributed this to the problem of the funding not matching the U.S. program's goals to be competitive.

During visits to U.S. and Western European accelerator facilities, we noted that both the U.S. and Western European physics communities were seeking to make the significant physics discoveries. Although Western European national programs are viewed as being complementary to the collaborative CERN program with respect to accelerators, we noted that competition also appeared to exist among the Western European countries with respect to making the physics discoveries.

While U.S. and European officials told us that high energy physics may eventually evolve into an international mode, they believed this would only happen when one nation or regional group of nations could no longer afford an accelerator in each of the technological frontiers. They explained that to date, the United States and Europe have often duplicated each other's machines, but the high costs of future accelerators may make it prohibitive to continue to do so. In this regard, current discussions on jointly funded interregional accelerators center on those costing in the range of $10 billion.

CHAPTER 3

FEDERAL ROLE IN HIGH ENERGY PHYSICS IN THE CONTEXT OF ALL BASIC SCIENCES

The Federal Government provides nearly all of the funding for the U.S. high energy physics efforts. This is in line with the overall Federal role of supporting basic research and graduate education. OSTP has oversight responsibility over all federally-funded basic science and works with OMB in developing basic science budgets. The Foundation is the principal Federal agency for the support of basic research across all fields of science and science education. In fiscal year 1980, the Foundation is providing about $23 million, or about 8 percent of the nearly $350 million of Federal support for high energy physics. DOE has primary responsibility for implementing a sound national high energy physics program and is supporting the program with about $325 million in fiscal year 1980.

The appropriate level of Federal support, however, is not clear. The United States is seeking to maintain its lead in high energy physics, but the U.S. program's relative position has been declining. The physics community is concerned that the U.S. position of leadership in the field will be lost to Western Europe and believes additional funds are needed. However, in determining DOE's existing funding level, little consideration appears to have been given to the needs and relative priorities of other basic sciences. In our opinion, the current objective of maintaining a leadership position with accelerator capabilities in each of the frontier physics' technologies needs to be reevaluated in light of the costs of achieving that objective and the relative priority of the program with other basic sciences.

FUNDING IN THE CONTEXT OF FEDERAL SUPPORT OF BASIC RESEARCH

The Federal Government supports about two-thirds of the Nation's basic research effort, that is, the search for new knowledge and understanding of fundamental natural phenomena and processes. Universities, other nonprofit organizations, and a number of industrial firms also support basic research. But from a national point of view, as a whole they tend to underinvest in such research either because their resources are limited (as in the case of universities or nonprofit organizations) or because the results do not lead in the near-term to the development of patentable and marketable new processes and products (as in the case of private industry).

In the 1950s and 1960s, Federal funding of basic research was large, relative to the available scientific resources. However, in the early 1970s, Federal funding leveled off while the scientific establishment continued to grow. In addition, national priorities shifted toward seeking solutions to national problems such as social and energy problems. As a result, Federal funding of basic research in fields, such as high energy physics, which principally seek knowledge, has been marginal for carrying out the program goals in those fields.

In the fiscal year 1981 budget message and the 1980 State of the Union message, the President made clear that the administration was again giving basic research special consideration, and this was reflected in the budget request. As in recent years, emphasis has been given to the potential of basic research for breakthroughs to the solution of critical problems. However, in fiscal year 1981, special emphasis is also being given to strengthening basic research in the physical sciences and engineering, which have not fully shared in the recent increases in Federal support of basic research. As a result, the basic research funding in the President's fiscal year 1981 budget request is about $5.1 billion, or a 12-percent increase over 1980 in current dollars and, using OMB's inflation factor of 9 percent, an estimated 3-percent increase in constant dollars. 1/

The Federal funding of basic research by major science fields, including breakouts for high energy and other physics fields for fiscal years 1967 to 1981, is graphically presented on the following page. The graph indicates that in the 1970s, Federal support of other basic sciences, such as those for environmental and life sciences, grew significantly compared to that for high energy physics. Thus, high energy physics now has a smaller proportion of the total Federal basic research funding. In this regard, high energy physics' portion of the Federal basic research funding has decreased from a peak of about 13 percent in fiscal year 1970 to about 8 percent in fiscal year 1980.

On pages 32 and 33, we graphically isolate the trends for Federal funding of basic research and high energy physics from fiscal years 1967 to 1981 in current year and constant 1972 dollars. A comparison of the two graphs shows that both

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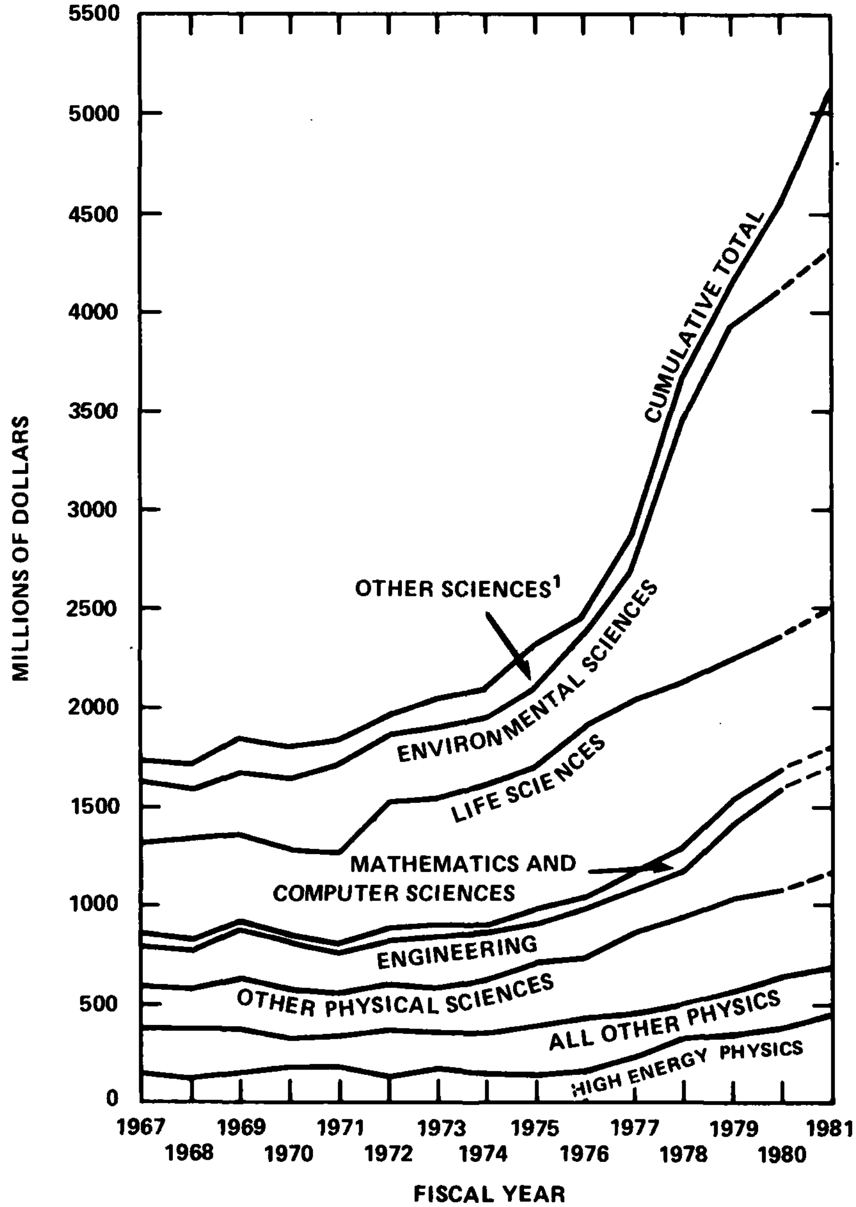
1/The term "current dollars" refers to the value of the dollars in the year the expenditures occur. The term "constant dollars" refers to the value of the dollars in relation to the value in a specific year.

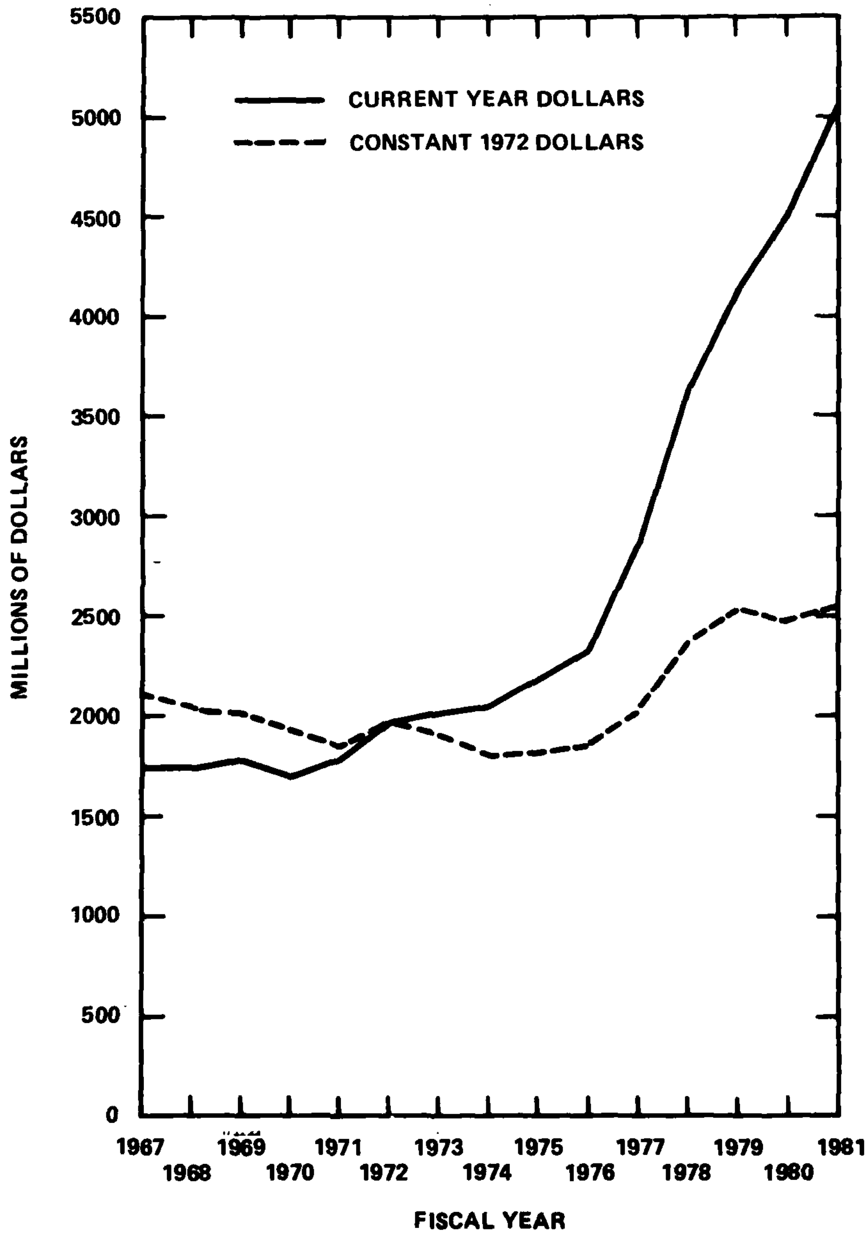
FEDERAL FUNDING FOR HIGH ENERGY PHYSICS, ALL PHYSICS, AND TOTAL BASIC RESEARCH – FISCAL YEARS 1967 TO 1981 (EST.) (CURRENT YEAR DOLLARS)



[1] INCLUDES AMOUNTS FOR PSYCHOLOGY, SOCIAL SCIENCES, AND MULTIDISCIPLINARY AND INTERDISCIPLINARY PROJECTS THAT CANNOT BE CLASSIFIED INTO ONE OF THE BROAD SCIENCE FIELDS.

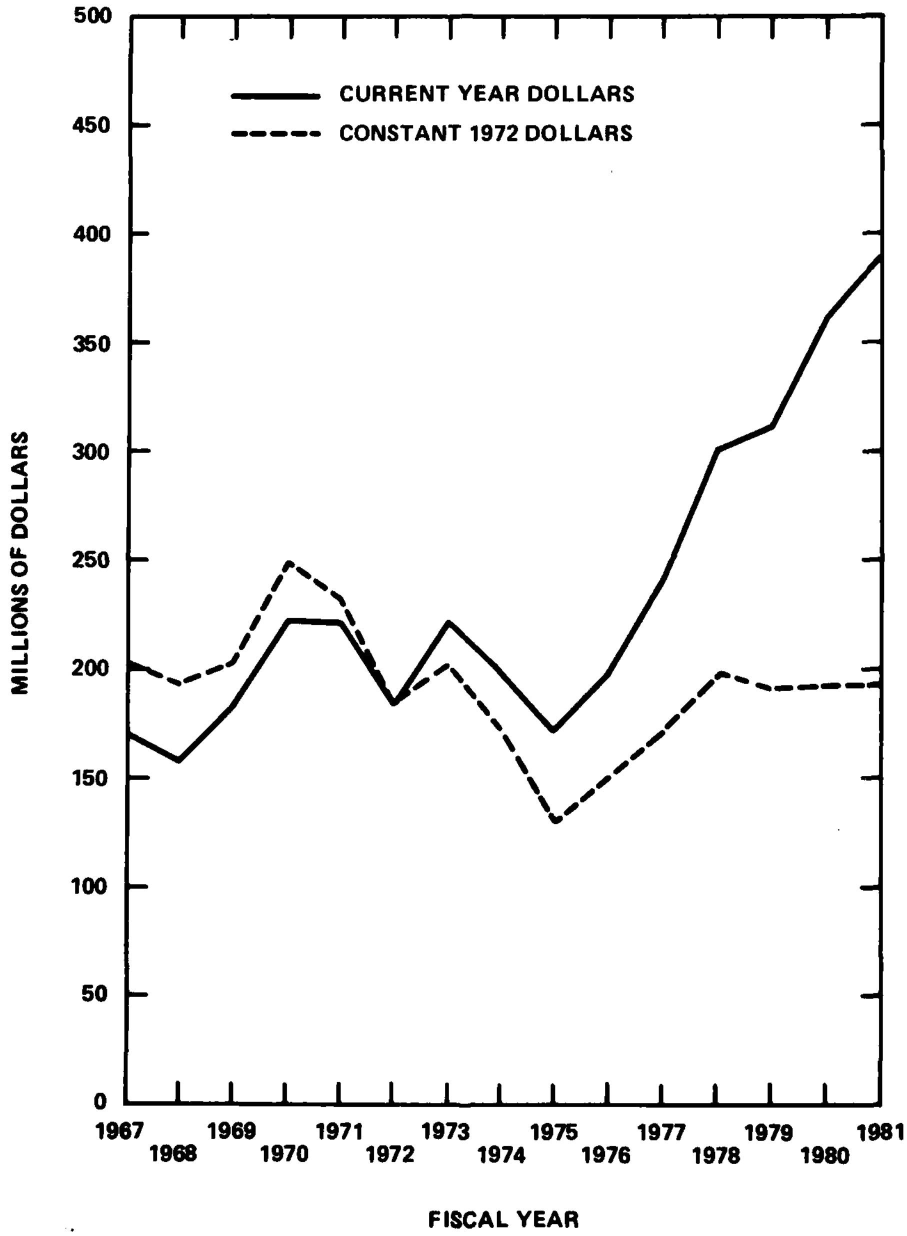
SOURCE: "SCIENCE INDICATORS – 1978," NATIONAL SCIENCE BOARD, 1979, AS UPDATED BY GAO USING "FEDERAL FUNDS FOR RESEARCH AND DEVELOPMENT, FISCAL YEARS 1978, 1979, AND 1980," (NSF 79-318), NATIONAL SCIENCE FOUNDATION.

FEDERAL FUNDING FOR BASIC RESEARCH IN CURRENT YEAR AND CONSTANT 1972 DOLLARS FISCAL YEARS 1967 TO 1981 (EST.)



SOURCE: "SCIENCE INDICATORS – 1978," NATIONAL SCIENCE BOARD, 1979, AS UPDATED BY GAO USING "FEDERAL FUNDS FOR RESEARCH AND DEVELOPMENT, FISCAL YEARS 1978, 1979, AND 1980," (NSF 79-313), NATIONAL SCIENCE FOUNDATION, AND GNP IMPLICIT PRICE DEFLATORS.

FEDERAL FUNDING FOR HIGH ENERGY PHYSICS IN
CURRENT YEAR AND CONSTANT 1972 DOLLARS
FISCAL YEARS 1967 TO 1981 (EST.)



SOURCE: DOE AND GNP IMPLICIT PRICE DEFLATORS.

high energy physics and total basic research funding has increased in recent years in current year dollars, but has leveled off since fiscal year 1978 in constant dollars.

The major difference in the trends occurred in the mid-1970s. High energy physics funding decreased in both current and constant year dollars, while basic research funding increased in current year dollars and decreased slightly in constant dollars. Hence, the recent constant dollar leveling of Federal basic research funding would seem to indicate a greater impact on high energy physics than basic research in general. In constant dollars, high energy physics funding has leveled off below its funding in the 1960s and early 1970s, while Federal basic research funding has leveled off at or near its peak funding.

FEDERAL APPROACH TO BASIC RESEARCH

The overall Federal approach to basic research has been characterized by the Director, OSTP, as a highly decentralized and pluralistic activity. The Government has not centralized science and technology into a single department; instead, it is found throughout the mission agencies. Science and technology efforts are not consolidated into a single or even a few budget accounts, but are integral components of the many mission activities of the Government. As a result, the executive branch and the Congress are limited in their capability to deal comprehensively with science and technology issues during their budget review and oversight efforts.

To complicate matters, established criteria for establishing funding priorities for basic science does not exist. The following excerpt from a 1970 report by the National Science Board to the Congress provides some insight into how such priorities are determined in the absence of a formalized analytical mechanism.

> "The fact that much of science does not use a highly visible, centralized, priority-setting mechanism does not mean that other mechanisms do not exist. Actually, science uses a multiplicity of such mechanisms. One priority-setting mechanism operates when a scientist determines the problem on which he works and how he attacks it within the resources available. This determination is made taking into account other similar and related work throughout the world. Another mechanism operates as proposals of competing groups of scientists are evaluated and funded on the basis of systematic refereeing and advice of peer groups. Still"

"another mechanism operates as aggregate budgets for various fields of science are influenced by the number and quality of research proposals received in that field. Like any market mechanism this system is not perfect and requires regulation and inputs from outside the system itself. Such inputs come from the mission-oriented agencies which balance their needs for new knowledge against their operating needs and from a whole host of outside judgments implicit in the budgetary and appropriation process. Trouble occurs either when these external judgments are completely substituted for the priority setting of the scientific community or when the priority setting of the scientific community becomes too autonomous."

Despite the difficulty of establishing criteria for determining priorities among the fields of science, some officials of the Executive Office of the President and Members of Congress believe that improvements can be made to the approach for planning and budgeting for R&D. Increased attention is being paid by the Congress, executive agencies, and interested non-Government organizations in attempts to improve Federal decisionmaking processes affecting science and technology resources. Improving those processes will presumably result in enhanced development of science and technology resources and more effective application of those resources to national needs.

Attempts to improve the Federal role in developing and applying science and technology resources have been in a variety of forms. Better criteria for allocating resources are receiving attention. Improved output measures and science indicators are under study and development. The Federal organizational structure for science and technology (such as OSTP, the Foundation, and Federal laboratories) is constantly being reassessed. The education, training, and utilization of scientists and engineers receive continuing attention.

We previously reported on the potential for improving the Federal approach for planning and budgeting for R&D. 1/ The traditional Federal budgeting approach focuses on how funds are to be spent. In our previous report, we discussed mission budgeting, which initially focuses on what the money is for and why it is needed, and then how the money is to be spent. While progress had been made toward focusing on end

1/"Mission Budgeting: Discussion and Illustration of the Concept in Research and Development Programs" (PSAD-77-124, July 27, 1977).

purposes, we noted that in many situations attention was still focused on the means. We concluded that mission budgeting appeared to be one way of overcoming the problem. To facilitate cross-agency comparisons, in another report, we recommended a unified presentation of all Federal R&D funding which would indicate the amount of Federal funds each agency commits to specific national objectives. 1/ OMB's development of the financial classification of the budget has eased the problems somewhat, but difficulty still remains.

The Congress and the executive agencies have since continued their efforts to improve the planning and budgeting approach for R&D. In April 1979, the House Committee on Science and Technology held oversight hearings on R&D in the Federal budget and related policy issues. The principal objective of those hearings was to obtain a better understanding of the R&D budget. This was the Committee's first attempt "to learn how it [the R&D budget] is fashioned, managed, monitored, and evaluated," rather than investigating the merits or demerits of specific R&D programs or projects. Discussions during those hearings indicated that planning and priority-setting decisions are primarily the responsibility of the various agencies. The various priorities set by those agencies are reviewed and assessed by OMB and OSTP during the budget process.

OMB and OSTP have been working to improve the planning and budgeting for R&D. For example, OMB and OSTP are now moving toward longer range planning. According to an OMB official, this has been largely limited to 2 years beyond the budget year and is chiefly an extrapolation of the budget impact of current policies and program objectives. He noted that the technique for developing long-term goals and objectives is not yet highly developed or widespread and precise budget numbers cannot be assigned to such goals and objectives across the Government.

We have an ongoing study that is attempting to develop a better understanding of the R&D planning and decision-making process in the Federal Government. The specific objective of the ongoing study is to provide the Congress with more comprehensive "before-the-fact" information and analyses on the R&D budget process.

1/"Need for a Government-wide Budget Classification Structure for Federal Research and Development Information" (PAD-77-14, March 3, 1977).

While a comprehensive examination of such issues was beyond the scope of this review, it appears that the Congress and the Executive Offices need greater involvement in the planning and decisionmaking processes for R&D. In light of this apparent need, we examined the Federal role in the high energy physics program.

FEDERAL ROLE IN HIGH ENERGY PHYSICS

The Federal Government provides nearly all of the funding support for the U.S. high energy physics program. Such a role seems appropriate because the benefits of the research are primarily of a long-term, unpredictable nature, and private sponsors are unlikely to be able to realize the full benefits. In establishing funding levels for DOE's support of the program, however, inadequate consideration appears to have been given to the amounts needed to carry out program goals and its relative priority with other basic sciences.

The levels of Federal funding of the program have been largely established on a piecemeal basis. Congressional and the Executive Offices' review and oversight have traditionally focused on specific efforts highlighted in annual budget requests, such as high-cost construction projects. As a result, important but costly construction projects were often deleted during the budget review processes.

Principally to overcome the tendency to delete high cost items such as construction projects and to provide continued leadership and a stable level of funding on which the program can develop plans, OMB and DOE agreed to fund DOE's portion of the program (about 92 percent of the U.S. program) at about $300 million in 1979 dollars. While this has provided funding stability, visible long-range plans for accomplishing the various specific program goals within this level of funding have not yet been formally developed. Furthermore, though the tendency to delete high-cost items has decreased, congressional and Executive Office review and oversight has continued to primarily focus on those items.

A certain amount of attention to such items seems appropriate because of their high initial costs, the subsequent costs of operating and exploiting them, and the need to monitor the progress being made. However, in our opinion greater attention needs to be placed on the merits of the program's goals and on providing sufficient funding to carry out the goals deemed appropriate.

Congressional role

As with any federally funded program, the Congress has review, oversight, and legislative responsibilities over the moneys authorized and spent in support of high energy physics. In the past, congressional support of basic research, such as high energy physics, has been largely based on the past successes of the Nation's scientific endeavors and a faith that future successes and benefits will be derived from continued support. Review and oversight have traditionally focused on specific budgetary items, such as the need and costs of specific projects proposed in the annual budgets. Hence, its activities have principally focused on "after-the-fact" matters in the sense that planning and priority setting decisions have essentially been made by the time funding is requested for the projects.

The Congress has long sought to become familiar with the research objectives and the guidelines upon which major decisions in high energy physics are based. In 1965, at the request of the former Joint Committee on Atomic Energy, U.S. Congress, the President forwarded a report prepared by the former Atomic Energy Commission (AEC) entitled "High Energy Physics Program: Report on National Policy and Background Information." Included in the report were guidelines to assist the Congress and the Nation by providing a yardstick against which funds requested for high energy physics could be measured. The 10 planning guides are listed in appendix III.

With respect to determining the level of support, the AEC reported:

> "The level and character of support for high energy physics should be determined and periodically reassessed in the context of the overall national science program (rather than in relation to the applied research and development programs of the AEC), advances and promise of advances in the field itself, and the then existing fiscal situation * * *."

Our literature searches and discussions with various officials associated with the program indicated that such assessments have not been formally made in the context of an overall national science program. Hence, while such assessments may have been made subjectively on an informal basis, the bases of decisions made are not clearly visible to the Congress.

Since 1975, the House Committee on Science and Technology has been seeking to link various R&D efforts and goals by obtaining a better understanding of the planning and priority-setting processes. With respect to high energy physics, this appears to be a difficult task not only because of the technical complexities of the field, but also because a long-term plan for achieving the program's goals or objectives has not been formally prepared and presented to the Congress.

Executive Offices' roles

OMB and OSTP are the two principal Presidential level Executive Offices involved in high energy physics. Their principal involvement is during the budget process, but OMB has become involved in long-term planning decisions.

As part of the budget process, OMB helps establish the President's annual budget request, including amounts for high energy physics. OMB has had a significant impact on high energy physics because in an effort to stabilize funding support, OMB and DOE agreed to a $300-million (plus inflation) funding level for fiscal years 1979 to 1984. According to the U.S. physics community, this represents a minimum level of funding for maintaining a viable program, and additional funds are needed to provide flexibility to respond to new initiatives. OMB officials told us, however, it is highly unlikely that additional funds beyond those for inflation would be provided. This agreement is discussed in more detail later in this chapter.

OSTP provides a source of scientific and technological analyses and judgments for the President and OMB with respect to major policies, plans, and programs of the Federal Government. OSTP works with OMB in analyzing annual budget submissions of the various departments and agencies, and by undertaking analyses or studies of specific science and technology issues. OSTP has not analyzed or studied the U.S. high energy physics program. However, OSTP was instrumental in helping set the current U.S. policy to support all basic research at a real growth rate--a level of about $4.5 billion in fiscal year 1980.

OMB has taken steps toward obtaining a better understanding of the long-range goals and needs of the program. In 1975, it requested the former Energy Research and Development Administration (ERDA) to prepare a long-term strategy for the construction and operation of high energy physics facilities.

In response to the request, in October 1975, ERDA set forth a plan entitled "Long-Term Strategy for Construction and Operation of High Energy Physics Facilities." The report outlined construction and operating priorities and plans for 10 years. Although three other funding levels were also considered, the report set forth a 10-year plan assuming that the national program, also including the Foundation, would be funded at an average annual level through 1985 of about $295 million in 1976 dollars--equivalent to about $375 million a year in 1979 dollars. The principal considerations and assumptions included (1) the increasing interdependence of the research done with different types of particles thereby mandates a three-pronged approach to higher energies via positron-electron colliding beams, proton-proton colliding beams, and protons colliding with fixed targets; (2) scientific redundancy, which is important for verification purposes, would be possible only on an international basis; and (3) firm funding constraints would remain on the program.

ERDA's report placed high priority on improving accelerator utilization to the 70- to 75-percent level. With respect to construction, improved capabilities at three DOE laboratories (Stanford, Brookhaven, and Fermilab) were recognized as being interdependent, but time wise, first priority was given to the positron-electron project at Stanford, followed by the proton-proton colliding beam at Brookhaven. The plan gave low priority to the proposed construction of the Cornell Electron Storage Ring.

The Foundation disagreed with ERDA's proposed strategy. In commenting on the report to OMB, the Foundation noted that alternative construction approaches were not adequately considered. It suggested that consideration be given to (1) alternative sites for proposed accelerators, (2) the economics of building capabilities at additional sites (such as Cornell), and (3) whether an international funding approach could better accommodate the costs of constructing the proposed facilities.

While OMB did not follow through on the Foundation's criticism, it also neither approved nor disapproved the strategy. OMB gave no assurance to the agencies nor the physics community that the proposed strategy would be funded. This study did, however, provide OMB officials some insight into the long-range plans and goals of the high energy physics community and was one of the major inputs in establishing the DOE/OMB funding agreement.

<u>DOE role</u>

In fiscal year 1980, DOE is providing about $325 million of support to high energy physics. According to DOE officials, DOE has responsibility to support high energy physics as "the executive agent for a national trust." 1/ Within DOE, primary responsibility for managing the program has been assigned to DOE's Office of High Energy and Nuclear Physics, which reports to the Director, Office of Energy Research.

The DOE program presently includes three large accelerator centers, each with different types of accelerators and management approaches; groups of experimentalists at 7 DOE laboratories and 52 universities; and groups of theorists located at 7 DOE laboratories and 50 universities. The three large centers are located at: Brookhaven National Laboratory, Fermi National Accelerator Laboratory, and the Stanford Linear Accelerator Center. Most of the advanced technology R&D efforts are conducted by the three accelerator centers, Lawrence Berkeley Laboratory, and Argonne National Laboratory.

The Office of High Energy and Nuclear Physics' management responsibilities include

--planning and formulating the program, proposing and defending budgets, allocating funds, and reviewing and evaluating performance;

--determining the level of accelerator operations;

--determining the extent and direction of accelerator and facility R&D efforts; and

--establishing priorities for new facility construction.

Nearly all high energy physics research is carried out under contracts with universities and university consortia, including those which operate the laboratories. DOE primarily relies on its field offices to provide fiscal management and control over these and other contracts made for the program.

While DOE staff seek to maintain ultimate control over the program, in practice, decisionmaking regarding detailed program content is extensively decentralized and very active use is made of scientific advisory and review groups. This management approach is designed to bring expertise from throughout the Nation to bear on the difficult judgments relating detailed program content to overall program goals, while simultaneously nurturing creativity.

1/By executive agent, DOE views itself as having primary responsibility for implementing a sound national high energy physics program.

Most notable of the advisory groups is DOE's High Energy Physics Advisory Panel. This Panel:

--advises on the status of the field, program priorities and changes of emphasis, levels of operation, and facility needs;

--identifies planning options; and

--assists DOE in coordinating its efforts with the Foundation.

At each of the three accelerator centers, program advisory committees provide advice on high energy physics efforts at the respective laboratories. The committees are primarily composed of physicists who are generally appointed for 3-year terms by the laboratory directors, except at Brookhaven where the Associate Director for High Energy Physics makes the appointments. The committees' principal activities include

--reviewing proposals for specific experiments in detail and recommending those experiments to be carried out,

--providing advice on accelerator operations schedules,

--recommending needed accelerator and facility modifications or new technologies, and

--serving as consultants on major aspects of construction designs and advising on the use of facilities.

These program committees are also able to help coordinate efforts at the laboratory level by having representatives from other laboratories as members of the committees. In this regard, we noted that a representative from a European laboratory was a member of the committee at Stanford.

In addition, each accelerator laboratory has a user group organization which provides an organized channel for interchanging information between laboratory users and the laboratory administration. The principal activities of user groups include:

--exchanging ideas on research plans, opportunities, and needs;

--identifying problems in facility use and suggesting solutions to those problems;

--advising on operational procedures and identifying special operational requirements;

--identifying needed new technologies; and

--helping plan new facilities and advising on the impacts of trade-offs in detailed facility designs.

Foundation role

The Foundation is the principal Federal agency for the support of basic research across all fields of science and science education. With respect to high energy physics, the Foundation provides about 8 percent of the total Federal support. During fiscal year 1980, the Foundation is providing about $23 million of support. It is supporting the construction and operation of the Cornell Electron Storage Ring at the Wilson Synchrotron Laboratory and providing about one-third of the Federal support for U.S. university groups' performing experiments at this and other accelerator facilities.

The Foundation is headed by a Director and the National Science Board. The Board is the Foundation's policymaking body and consists of 25 members, including the Director. The Foundation's Division of Physics within the Directorate for Mathematical and Physical Sciences directs its high energy physics activities under a program called "elementary particle physics."

As with DOE, the Foundation relies heavily on advisory groups to help with detailed programmatic decisions. The Foundation's Advisory Panel for Physics, which is concerned with all physics supported by the Foundation's Division of Physics, makes periodic reviews of the elementary particle physics program's activities. In addition, specific proposals initiated by researchers and submitted to the Foundation for funding are reviewed and evaluated by peer reviews. The evaluations, along with consideration of other relevant information such as recommendations of program advisory committees at accelerator laboratories, form the basis of the Foundation's funding decisions with respect to specific proposals.

A program advisory committee also exists for experiments performed at the Foundation-supported laboratory at Cornell. This committee performs essentially the same activities as the counterpart committees at DOE-supported laboratories. We also noted that Cornell's committee has representatives from other laboratories, including one from Europe, as members.

The Foundation does not have a funding agreement similar to DOE's. Instead, the Foundation attempts to establish an appropriate balance of its support to high energy physics relative to its support of other science fields. During its planning and budgeting processes, the needs of high energy physics are first weighed against other physics needs. The needs of the entire field of physics are then considered in relation to those of chemistry, mathematics, materials science, and computer science supported by the Foundation. Finally, the physical and mathematical sciences are balanced against other broad science fields and engineering. In addition, the National Science Board reviews the overall balance in its planning and budgeting activities for the Foundation.

In regard to long-range planning for high energy physics, the Foundation is largely dependent on DOE. This is because of its relatively small program and because Foundation-supported users perform most of their experiments at DOE-supported laboratories. Thus, the Foundation's involvement in long-range planning for this program consists primarily of program staff interactions with DOE staff and participation in DOE's High Energy Physics Advisory Panel meetings.

FUNDING LEVELS NEED TO BE ESTABLISHED IN THE CONTEXT OF ALL BASIC SCIENCES

While the U.S. high energy physics community believes additional funding is needed, whether such funding is warranted in light of the needs of other basic sciences is not clear. Recent U.S. basic science policy has emphasized efforts which may help resolve national problems. Since high energy physics is one of the most fundamental of the basic sciences, its likelihood of making substantial contributions toward solving such problems is a relative unknown. DOE's $300-million funding level was established largely on the basis of factors internal to high energy physics. On the other hand, as previously noted, the Foundation considers other basic sciences when funding high energy physics.

The Foundation's support recently has been decreasing, and might continue decreasing in the future, because some of the Foundation's upper level management officials believe high energy physics has been receiving a larger proportion of the Foundation's budget than warranted and are emphasizing the funding of other basic sciences (but not with respect to other physics).

Relative importance of high energy physics with other basic sciences

The relative merits of basic science fields cannot be easily quantified, and formal criteria for establishing scientific priorities does not exist. In the absence of such criteria, decisions on scientific priorities are based on scientific, social, political, and economic judgments. The recent U.S. policy toward basic science emphasizes the funding of those sciences that may help solve national problems. In a July 1978 memorandum, the Directors, OMB and OSTP, jointly stated, in part, that:

> "It is the policy of this Administration to assure effective support of basic or long-term research, particularly to provide a better basis for decisionmaking or for dealing with long-term national problems * * *."

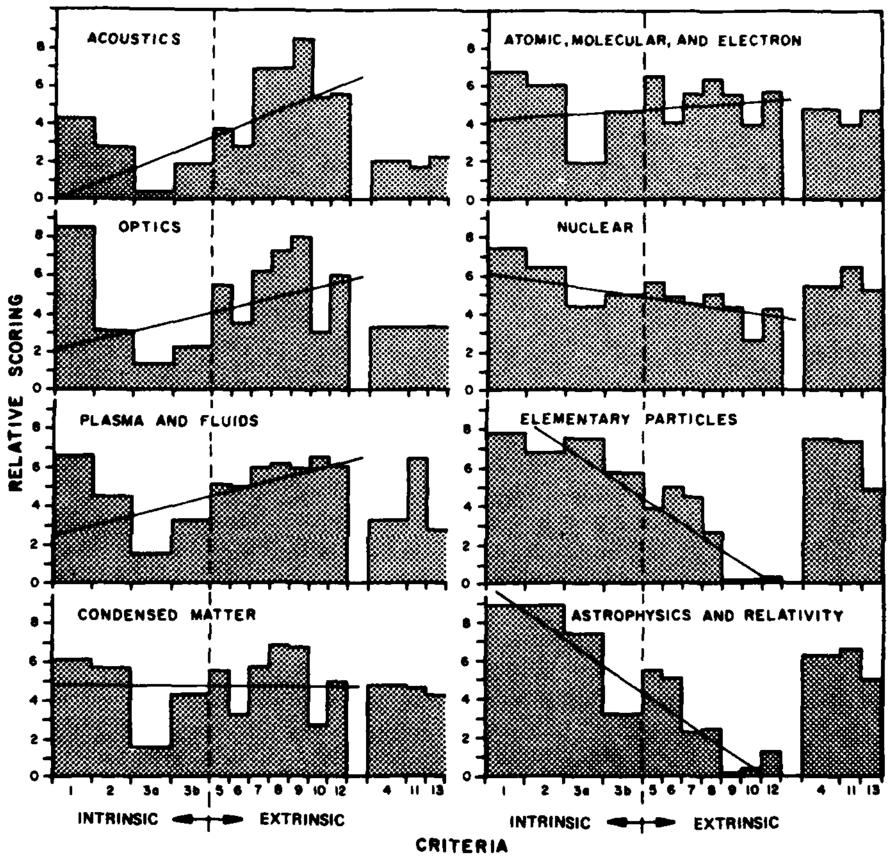
In this regard, some science policy experts from industry, universities, and Government believe more practical benefits can be derived from emphasizing research areas where less esoteric subject matters are explored and where possible solutions to national problems might be derived. The physics community on the other hand, believes that high energy physics helps set the pace for all research and provides fundamental insights which can be useful to all other research activities.

Ironically, critics and proponents of high energy physics can both find support for their contentions in a 1972 National Academy of Sciences report, Physics in Perspective. This report presents the results of the Academy's Physics Survey Committee's examination of eight subfields of physics, including the relative merits of the subfields. The Committee scored the subfields using two general categories of criteria: intrinsic merit and extrinsic merit. Intrinsic merit is benefits internal to a science such as the potential to open new areas of that science or discovering new fundamental laws of nature. Extrinsic merit is concerned with the impact on other scientific fields, technology, and national goals. High energy physics scored high on intrinsic merit and low on extrinsic merit, as graphically represented on the histograms on the following page. (High energy physics is labeled "elementary particles.")

The relatively low score given to the extrinsic factors could be interpreted to indicate that high energy physics is too esoteric to be of significant practical value. This interpretation has been made by critics that believe those areas of R&D that might help solve the Nation's problems should be emphasized. A former laboratory director, who participated in the study, has made this argument.

On the other hand, another former laboratory director, who also participated in the Academy's study, told us that the extrinsic score merely reflects that many scientists participating in the study failed to recognize the value of high

HISTOGRAM OF THE EIGHT CORE SUBFIELDS OF PHYSICS
(note a)



1 - RIPENESS FOR EXPLORATION
2 - SIGNIFICANCE OF QUESTIONS ADDRESSED
3a - POTENTIAL FOR DISCOVERY OF FUNDAMENTAL LAWS
3b - POTENTIAL FOR DISCOVERY OF GENERALIZATIONS OF BROAD SCIENTIFIC APPLICABILITY
4 - ATTRACTIVENESS TO MOST ABLE PHYSICISTS
5 - POTENTIAL CONTRIBUTIONS TO OTHER SCIENCES
6 - POTENTIAL STIMULATION OF OTHER AREAS OF SCIENCE
7 - POTENTIAL CONTRIBUTION TO ENGINEERING, MEDICINE, APPLIED SCIENCE
8 - POTENTIAL CONTRIBUTION TO TECHNOLOGY
9 - POTENTIAL FOR IMMEDIATE APPLICATIONS
10 - POTENTIAL CONTRIBUTIONS TO SOCIETAL GOALS
11 - CONTRIBUTION TO NATIONAL PRESTIGE AND INTERNATIONAL COOPERATION
12 - CONTRIBUTIONS TO NATIONAL DEFENSE
13 - CONTRIBUTION TO PUBLIC EDUCATION

SOURCE: "PHYSICS IN PERSPECTIVE, 1972," NATIONAL ACADEMY OF SCIENCES

[a]THE NATIONAL ACADEMY OF SCIENCE'S SURVEY COMMITTEE'S RATING OF THE INTERNAL PHYSICS SUBFIELDS IN TERMS OF INTRINSIC AND EXTRINSIC CRITERIA. THE STRAIGHT LINES ARE DRAWN TO REFLECT THE CHARACTERISTICS OF EACH SUBFIELD.

energy physics to other science fields. He pointed out that a more recent study by the Academy recognizes the unity of the physical sciences. 1/ Because of a commonality of the underlying principles, there is a unity of method in both theory and experiment.

A number of physicists and science policy experts cited the Academy's 1972 report as evidence that high energy physics is one of the most fundamental of the basic sciences. The high intrinsic score accorded high energy physics seems to support them. However, these factors are internal to the field and do not indicate whether or not high energy physics makes a substantial contribution to other fields or toward solving the Nation's problems. Thus, a simplistic view of the issue appears to be the often raised question, "how much emphasis should be placed on basic research for fundamental knowledge versus those research areas which are more likely to provide near-term practical benefits?"

DOE/OMB funding agreement

In formulating the fiscal year 1979 budget, DOE and OMB reviewed the funding of Isabelle's construction. To ensure that funds are made available for future construction and equipment needs while limiting the funding impact on other basic sciences, DOE and OMB agreed to establish an annual funding level at $300 million, in 1979 dollars, through fiscal year 1984. The agreement was reached after OMB noted that the large commitment of funds required for the construction and subsequent operation

> "* * * of Isabelle, unless carefully considered in a broad scientific context, could be viewed * * * as a potential mortgage on future growth of Federal funding for all basic research."

DOE's review focused on the project's high energy physics merits and the needs of the program through the mid-1980s. DOE's only documented consideration to other basic sciences was an acknowledgement that an accelerated construction program would result in a major "bow wave" in the program. DOE noted that large funding levels for the project in any 1 year would place a heavy burden on the Government-wide basic research budget. DOE also noted that while the science is important, the U.S. high energy physics program would not be threatened by either slipped construction of Isabelle or the

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1/Science and Technology, A Five-Year Outlook, National Academy of Sciences, W. H. Freeman and Co., 1979.

appearance of any foreign accelerator during the period. Accordingly, DOE recommended a stretched construction schedule as part of the stabilized funding level.

The $300-million level recommended coincides with a low base budget considered by the 1977 Subpanel on New Facilities of the High Energy Physics Advisory Panel. Even under this level of funding, the Subpanel believed the construction of new facilities essential. The Subpanel, however, cautioned that such a level would adversely affect accelerator operations and reduce support to university user groups. The Subpanel reported, in part:

> "The consequences to the program would be a drastic reduction or termination of the lower energy fixed target and colliding beam programs, and a reduction in the level of support and number of university groups. In addition, levels of use of the new electron colliding beam facility will only be at about the 50 percent level. Further, the provision of equipment necessary to exploit these facilities will be substantially slowed. This funding level on a long-term basis is close to that which would require a drastic revision in the long-range program and probably is not adequate to keep the United States in a world competitive position in this field."

Nonetheless, largely based on the results of this and the 1975 ERDA studies, DOE concluded that a $300-million funding level would provide continued leadership in the field. This level was negotiated between DOE and OMB, agreed to by the High Energy Physics Advisory Panel, and concurred with by OSTP.

In December 1977, DOE and OMB documented their funding agreement with a memorandum of understanding. In addition to the $300-million funding level, other points agreed to include the following:

--Isabelle should be funded as part of a balanced, multi-year high energy physics program.

--This program is essential to preserve U.S. world leadership in high energy physics.

--It permits a high energy physics program with three national accelerator laboratories (Stanford, Brookhaven, and Fermilab).

--It is in the best interest of DOE and OMB to avoid large changes in total funding levels from year to year. A level of $300 million over the next several years would provide for programmatic stability, continued leadership in the field, and completion of Isabelle on a reasonable schedule.

--The escalation rate for inflation will be jointly determined and agreed upon by DOE and OMB each year.

--The distribution of total funding between operating expenses, capital equipment, and construction will be subject to OMB review each year.

--If, in future years, Government policy is to provide for real growth in basic research, it is reasonable to expect that high energy physics would share in that growth, to some extent.

In the memorandum, DOE's Director of Energy Research also noted that even though the level is below the $350-million annual level recommended by the High Energy Physics Advisory Panel, the high energy physics community will support the agreement because of the promise of funding stability over the next several years.

The Advisory Panel, in an effort to obtain stabilized funding for the program, agreed to this level. The Chairman of the Advisory Panel told us that the Panel agreed to the level as the floor level and not the ceiling. He explained that this level stretches out construction, underuses facilities, and precluded the exploitation of new initiatives. If this floor becomes the ceiling, he and others in the physics community are concerned about future U.S. competitiveness in the field.

OMB officials told us that the level of support the physics community believes needed is being provided. They told us that rather than annually subjecting high energy physics funding requests to trade-offs made by bureaucrats during the budget process, the funding agreement provides the physics community with a working level at which they can make definitive plans and programmatic trade-offs. They added that the $300 million, plus inflation, is only a working level which can be adjusted up and down if warranted, but it is highly unlikely that the level would be raised significantly.

OMB and DOE officials told us that a cost-benefit study of funding high energy physics versus other sciences was not made. They noted it would be difficult to make because the value of the results of high energy physics research is highly subjective. They pointed out, however, that in 1972 the

National Academy of Sciences reported that high energy physics is one of the most fundamental of the basic sciences and that OSTP concurred with the funding agreement.

According to one OSTP official, though they had not made a study, OSTP concurred with the level because OMB, DOE, and the High Energy Physics Advisory Panel had agreed that this level was needed and would be helpful to the U.S. high energy physics program. This official told us that OSTP has not attempted to balance high energy physics needs versus those of other basic sciences. He said this may be an impossible task. He added, however, that the United States should continue to seek world leadership in high energy physics because, much more than in other basic sciences, such leadership is essential to maintain the morale and drive of the practitioners.

DOE officials provided us with a graph presenting DOE's high energy physics funding, in fiscal year 1979 dollars, which shows the intended impact of the funding agreement. The graph, which breaks out funding into construction, equipment, and operating categories, is shown on the following page.

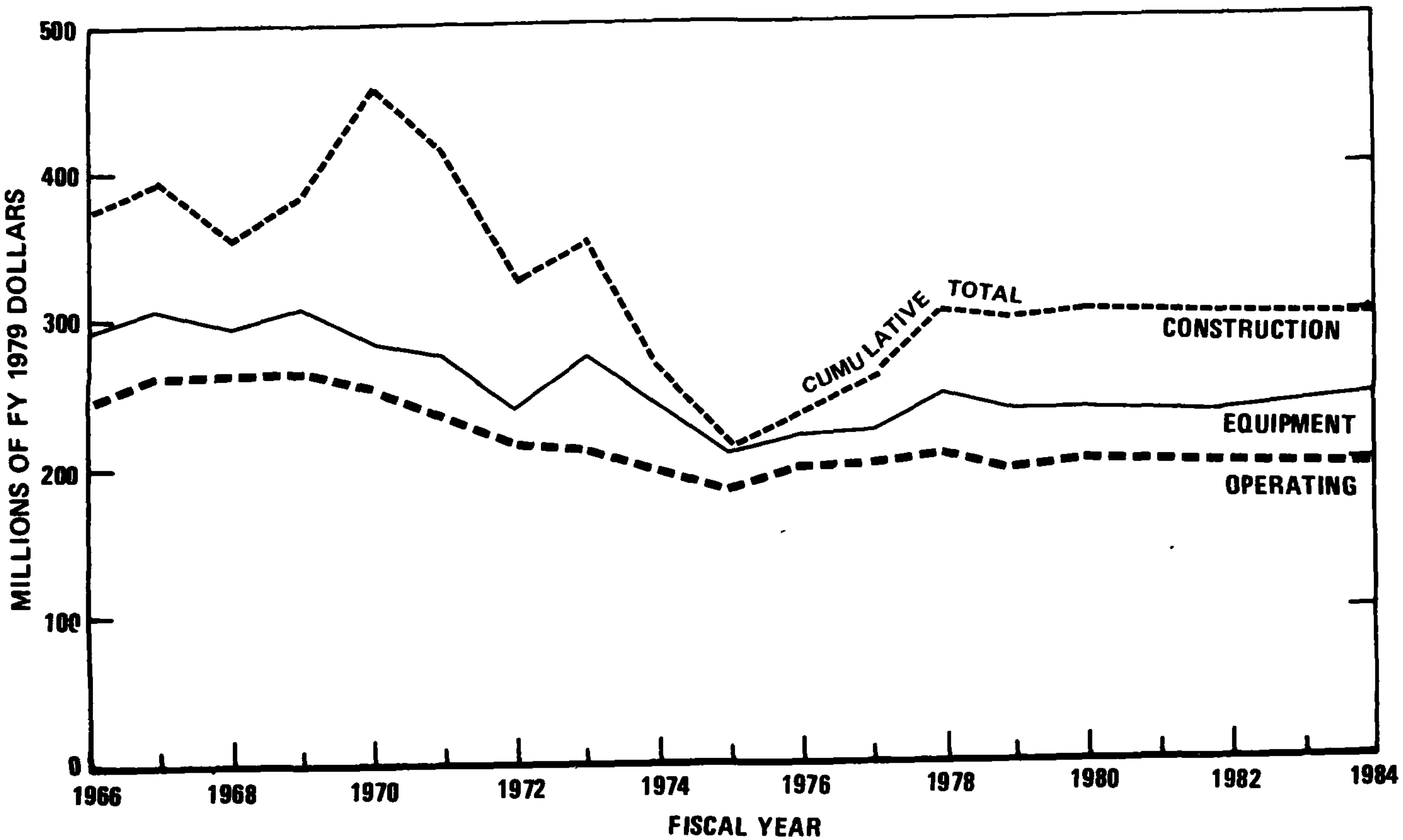
Some Foundation officials perceive high energy physics may be overfunded

In funding high energy physics, the Foundation not only weighs the internal merit of the research, but as part of its budget formulation process, it weighs the needs and benefits of the research against those of other basic sciences. While these decisions are made largely on subjective factors, each program official is provided the opportunity to present the reasons for requesting funds and the various impacts of not receiving them.

According to Foundation program officials, some of the Foundation's upper management perceive high energy physics to be receiving more than its share of the Foundation's basic research funds. Accordingly, the Foundation's support of high energy physics has leveled off in recent years and may decrease in the future.

In commenting on this matter, one upper level management official told us that this perception is based largely on subjective factors influenced by one's background. He said that the Foundation's upper management believes high energy physics is a worthwhile pursuit, but its share of funding is a matter of priorities. He explained that he knows of no well-defined objective way of evaluating the merits of high energy physics against other basic sciences. High energy physics is long-term esoteric research while other smaller

DOE HIGH ENERGY PHYSICS PROGRAM
DOE/OMB LONG RANGE FUNDING AGREEMENT



Source: Division of High Energy Physics, U.S. Department of Energy basic science fields, such as chemistry, have potentially nearer term practical applications. He could not quantify the value of the fundamental knowledge gained from high energy physics, but if other factors were considered he believed the field would not fare well. As an example, he cited the training of students for the Nation's scientific needs. In his opinion, there is little need for additional high energy physicists because of declining university enrollments and their skills are not easily applied in industry.

This official cited the large amounts of funds needed to carry out high energy physics as another factor influencing Foundation management's views. He noted that the Foundation has a larger impact on the smaller sciences. For example, the Foundation provides about one-half of all the Federal support for chemistry, but less than 10 percent of the Federal support for high energy physics. He also pointed out that high energy physics requires facilities which require large, long-term commitments of funds for construction and operations. He expressed concern that if the Foundation supported such facilities, it may lose the flexibility to respond to new scientific ideas.

In formally commenting on this matter, the Foundation, in a letter dated March 17, 1980, stated:

> "The Foundation regards high energy physics as an essential component of its physics programs and a subject which is intellectually alive and vital * * *."

The Foundation went on to note the complexity of this issue and the need for continued reexamination of any proposed balance. It also pointed out:

> "The NSF [Foundation] support of elementary particle physics is roughly the same fraction of the total support of physics at NSF in FY 1980 as it was in FY 1975. Thus, the NSF is not deemphasizing the support of particle physics relative to other areas of physics. The support of particle physics at NSF has not expanded as fast as the needs of the field, however. This is also true for all areas of physics as well as for essentially all fields of science supported by NSF."

We noted, however, that while its support of particle physics has remained roughly proportional to its support of other physics areas, it has not received the same emphasis as has other fields of science supported by the Foundation. For example, in fiscal year 1980, the Foundation's support for physics increase (in current year dollars) was 3.2 percent while its overall increase for basic research was 11.8 percent. After inflation is taken into account (estimated by OMB to be 9 percent), the Foundation's support for physics decreases.

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The United States is seeking to maintain a leadership position in high energy physics. However, according to the physics community, sufficient support to maintain such leadership is not being provided. In establishing the present funding level for high energy physics, little apparent consideration has been given to the relative merits of other basic sciences. No criteria is known for accomplishing this. Instead, it requires subjective judgments on the part of knowledgeable decisionmakers. A study is needed to determine the appropriate level of funding for the program while considering the relative merits of other sciences. Such a study would appear to be beyond the type of study that could be provided by DOE's High Energy Physics Advisory Panel, which primarily provides advice on factors internal to the field. This Panel, however, could provide input on the program's approach to various policy options or alternatives.

A study of the relative merits of other sciences was beyond the scope of our review which focused on high energy physics. In light of the less than an optimal amount of funds available, the following chapter discusses how DOE and the Foundation, with the input of advisory panels, have planned and used available funds toward achieving the program's goals.

CHAPTER 4

NEED FOR INTEGRITY BETWEEN OBJECTIVES, PLANS, AND BUDGETS

The long time scales inherent to high energy physics, the relatively high costs of large accelerators and their associated facilities, and the continual infusion of new technological capabilities all demand that compatible objectives, plans, and budgets be established well in advance. Since less than optimal funding has been available, the U.S. program has had to make sacrifices in various program elements needed to help maintain a long-term leadership position in the field. Although the amount agreed to under the OMB/DOE funding agreement is about $50 million less than recommended by the DOE High Energy Physics Advisory Panel, DOE still seeks to maintain a leadership position with this level of funding. DOE considers the agreement to be the "plan" being followed, but the agreement does not document a new detailed strategy or long-term plan as to how leadership is to be maintained.

DOE recently initiated an Advisory Panel study to develop near-, mid-, and long-range program plans. We believe such plans could be extremely useful to the Congress by providing visibility over (1) the program's planned efforts under present and alternative funding levels and (2) the benefits and/or sacrifices of those efforts.

The U.S. program has emphasized the construction of accelerators within a constrained budget, thereby adversely affecting other key program elements. New construction is needed to provide the tools for maintaining a leadership position, but current research support and long-term accelerator R&D have been inadequately funded. As a result, the U.S. program may have difficulty in maintaining its leadership position.

In our view, the program has been faced with trying to do more than available funds would allow. Thus, the alternatives appear to be either changing the objectives or increasing funding.

NEED FOR BETTER PLANNING

Under current funding, DOE has continued to assert that the U.S. program is to maintain its leadership position in the field, but has not developed detailed plans showing how such an objective is to be met. DOE considers the OMB/DOE funding agreement to be the "plan" being followed. However, as discussed on page 48, the agreement basically sets forth overall funding levels for operations, equipment, and construction, including that for Isabelle, and points out that the funding permits a three national accelerator-laboratory high energy physics program (Stanford, Brookhaven, and Fermilab).

DOE recently initiated a study for developing general strategies and long-range plans under alternative funding levels, including the presently agreed upon level. The study was also to assess the program balance for the mid-term (fiscal years 1982 to 1987) and make specific recommendations for the program in fiscal year 1982. We believe such studies and plans could be extremely beneficial and should be formally instituted on a periodic basis.

Background on past and current plans

DOE and the Foundation have obtained considerable program advice and guidance in the decisionmaking process from the longstanding and highly regarded High Energy Physics Advisory Panel. Over the past 10 years, this Panel and its various subpanels have provided DOE and its predecessors with numerous reviews, reports, studies, and recommendations on many topics, such as advanced accelerator technology R&D, major construction proposals, equipment funding, accelerator utilization, and manpower levels. The advice is also available to the Foundation, and its program staff regularly participate in the Panel's meetings. The Foundation has its own Physics Advisory Committee, whose concerns, however, extend beyond high energy physics to other physics supported by the Foundation. Thus, the Foundation has relied heavily on DOE's Panel for advice on long-range planning and budgeting for the program.

DOE points to Panel reports as supporting documents for its funding level. These reports recommended program plans and construction priorities, prior to the funding agreement. Since the agreement, DOE has not developed a plan which formally sets out how each of the various program elements needed to maintain a leadership position is to be achieved. For example, for maintaining leadership under constrained funding, DOE has not documented the number of research groups that should be supported, the extent those groups should be supported, and the extent of accelerator utilization.

ERDA staff, relying heavily on Advisory Panel studies, responded in October 1975 to an OMB request for a 10-year strategy for construction and operating high energy physics facilities. (See pp. 39 and 40.) ERDA issued a report which dealt primarily with a facility strategy--construction priorities and utilization levels--but also addressed OMB questions on the lack of agreed-on long-range planning and budgeting and how the annual budget process might be improved.

In responding to these questions, the ERDA staff pointed out that substantial problems had arisen in the past from unexpected fluctuations in funding. It identified layoffs, unused improvements, and unanticipated reductions in accelerator operations as examples of damaging events which could have been avoided or reduced by longer range budgeting. It cited long-range budgeting procedures at CERN, which reportedly has served European members well by providing a reliable guide to future funding and realistic long-range plans.

The CERN budget is divided into six research programs, four of which are related to the facility's four accelerators. Annually, CERN projects its budget for the next 4 years. Detailed budgets are submitted for the first year and firm commitments are made before the year begins. The second year budget is considered a reliable estimate, while the third and fourth years are provisional estimates with no firm funding commitments. CERN officials pointed out to us that CERN's budget has declined 3 percent since 1975 and its projected out-year budgets do not necessarily represent the amounts that will ultimately be committed.

According to a CERN official, the only long-term funding commitments received from member nations have been for supporting major construction projects, such as CERN's Super Proton Synchrotron accelerator. However, each member nation pays a fixed contribution to the capital and operating costs of the CERN programs which it agrees to support, according to a fixed scale reestablished every 3 years on the basis of net national income.

In any event, the 1975 ERDA report outlined a possible long-range planning and budgeting procedure which has some precedent and considerable merit when one considers program events since the 1975 study. The procedure involved a three-tier planning process. The three tiers were to differ in the period covered, the degree of firmness, and the level of commitment expected from OMB and the supporting Federal agencies.

The three tiers were:

--a 10-year long-range plan, with general operating and construction guidelines, which would be updated every 2 years;

--a 2- to 4-year short-range plan which would evaluate specific requests and justifications, following the general guidelines of the 10-year plan; and

--a firm annual budget based on the short-range plan, adjusted for (1) cost index changes, (2) unforeseen technical and economic developments, and (3) prior year changes by the Congress.

These plans were expanded upon in an appendix to the ERDA report. The report itself was generally along the lines of the suggested long-range plan. It included

--plans and priorities for constructing and shutting down accelerators through 1985,

--guidelines for efficient operation and utilization of present and planned accelerators, and

--the impact of several funding levels suggested by OMB.

The ERDA report drew heavily on the deliberations of 1974 and 1975 subpanels on new facilities of the High Energy Physics Advisory Panel and an extensive history of Panel considerations. The report was supplemented by a June 1977 subpanel report, which considered the long-range needs of the U.S. program. This latter report made specific recommendations for fiscal year 1979 construction and again addressed the probable effects of the funding levels considered in the 1975 report. Alternate funding levels were discussed because a funding plan had not yet been agreed on for the future.

Having full knowledge of these studies, OMB and DOE, during formulation of the high energy physics fiscal year 1979 budget, agreed to a $300 million a year DOE budget for the period through fiscal year 1984. This agreement, although it covered 5 years (not 2 to 4), could have formed the basis for the second-tier plan explained in the 1975 report. It represented a funding commitment by OMB that was explained to the Congress. Unfortunately, it was not integrated with a detailed long-term plan, and the commitment was $50 million a year below that which had been recommended for the program in the most recent Advisory Panel study.

As discussed in chapter 3, the OMB/DOE agreement coincides with the low-base budget studied by a subpanel of the High Energy Physics Advisory Panel. According to the subpanel, this level was

"* * * close to that which would require a drastic revision in the long-range program and probably is not adequate to keep the United States in a world competitive position * * *."

Nonetheless, DOE officials told us the program is basically following the construction initiatives in the subpanel's 1975 report.

The agreement has provided stability in program funding, but it should be integrated into a systematic planning process. The three-tier planning process could work well for the high energy physics program. Obviously, plans would have to be updated and changed from time to time. However, more clearly defined overall objectives, a congressionally approved 10-year strategy, and a 5-year OMB commitment for stabilized funding could resolve many of the present inconsistencies between the program's goals, plans, and funding.

<u>DOE initiative to develop program plans</u>

In July 1980, a subpanel of DOE's High Energy Physics Advisory Panel reported on its review and planning for the U.S. program. DOE initiated the subpanel study and provided the following charge:

> "Within the context of changing worldwide high energy physics activities and opportunities, review the status and prospects of the U.S. program, taking into consideration all aspects of the program, including:
>
> o Physics progress and achievement of scientific understanding.
>
> o Physics research programs at Universities and Laboratories.
>
> o Laboratory facility operations (including program scope, operating effectiveness, utilization levels and user research opportunities).
>
> o Technology R&D for accelerators and experimental facilities.
>
> o Program on facilities recently completed or under construction.
>
> o Future construction proposals and possibilities.
>
> Develop a general strategy and long-range plan for the U.S. program over the next decade and, in particular, assess the program balance required for physics research, facility operations, technology development, equipment and construction"

"over the period from FY 1982 to FY 1987, and make specific recommendations for FY 1982; all under the following funding constraints:

1. The same guidance as that under which the program now operates; i.e., an average annual funding level for the U.S. program (DOE plus NSF) of $325 million in FY 1979 dollars.

2. A funding constraint 10-15% higher than that above.

3. Are there important high energy physics activities which cannot be conducted on a timely basis within the above constraints? What are they and what would be the implications of addressing or excluding them? * * *"

DOE's charge added that within each of the above funding constraints, the subpanel is to assess the prospects and sufficiency of the strategy and long-range plan to maintain a forefront U.S. high energy physics program.

Thus the subpanel was to develop near-, mid-, and long-range plans for the program, while addressing the status and prospects of each of the key program elements. While the subpanel recommended areas to emphasize for a balanced program in the near-term, it did not develop general strategies and plans for the mid- and long-range. Instead, it cited difficulties with current construction efforts and uncertainties with respect to the specific electron accelerator facilities that should be built to pursue the existing physics opportunities, and recommended that another study be convened in 1 or 2 years.

We believe DOE's goal to develop near-, mid-, and long-range plans is commendable. Hence, we believe another study should be initiated to develop such plans as soon as practical. Such plans should provide improved visibility over the program and could be extremely helpful to the conduct of oversight and legislative activities. Accordingly, we believe such plans should be developed and instituted formally on a periodic basis, and submitted to the Congress for its review and consideration.

EMPHASIS ON CONSTRUCTION OF NEW ACCELERATORS

To maintain a leadership position in high energy physics, the U.S. physics community believes the tools for frontier physics to be essential. In line with this belief, the U.S. program has been constructing accelerators and improving existing accelerator capabilities. In addition to the relatively high costs of the construction itself, related costs greatly increase the funding required to pursue construction efforts. The following discusses some of the estimated costs of accelerator construction and development efforts at the laboratories.

Construction at Fermilab

Fermilab has been working toward the Tevatron project. DOE estimates that total construction and related equipment and R&D costs will exceed $200 million. This project is proceeding in phases, with the first phase, the Energy Saver, authorized and under construction. The Tevatron is to (1) increase the energy of the existing proton accelerator from 400 GeV to 1,000 GeV, (2) make possible colliding-beam experiments with center-of-mass energies 1/ of up to 2,000 GeV, and (3) possibly lead to the discovery of the intermediate vector boson. Fermilab has redefined the Tevatron project several times, but its basic elements include

--superconducting magnets that will reduce the power consumption of the present accelerator and permit increases in beam energy to 500 GeV,

--proton-antiproton colliding beams with two new experimental areas, and

--modifications to (1) permit the superconducting ring to deliver a 1,000 GeV-proton beam to fixed targets, and (2) upgrade targets for receiving the higher energy beam.

A schematic of the Tevatron is shown on the following page.

Fermilab began its R&D efforts on the project in 1975. The objective at that time was a 1,000 GeV accelerator, estimated to cost $51.6 million. Because difficult technical problems had to be solved, the project was begun with accelerator R&D funds. In fiscal year 1979, after OMB became concerned that the accelerator would be completed without OMB and congressional approval, the Energy Saver project was submitted for congressional approval as a construction project. Through fiscal year 1978, according to an independent audit

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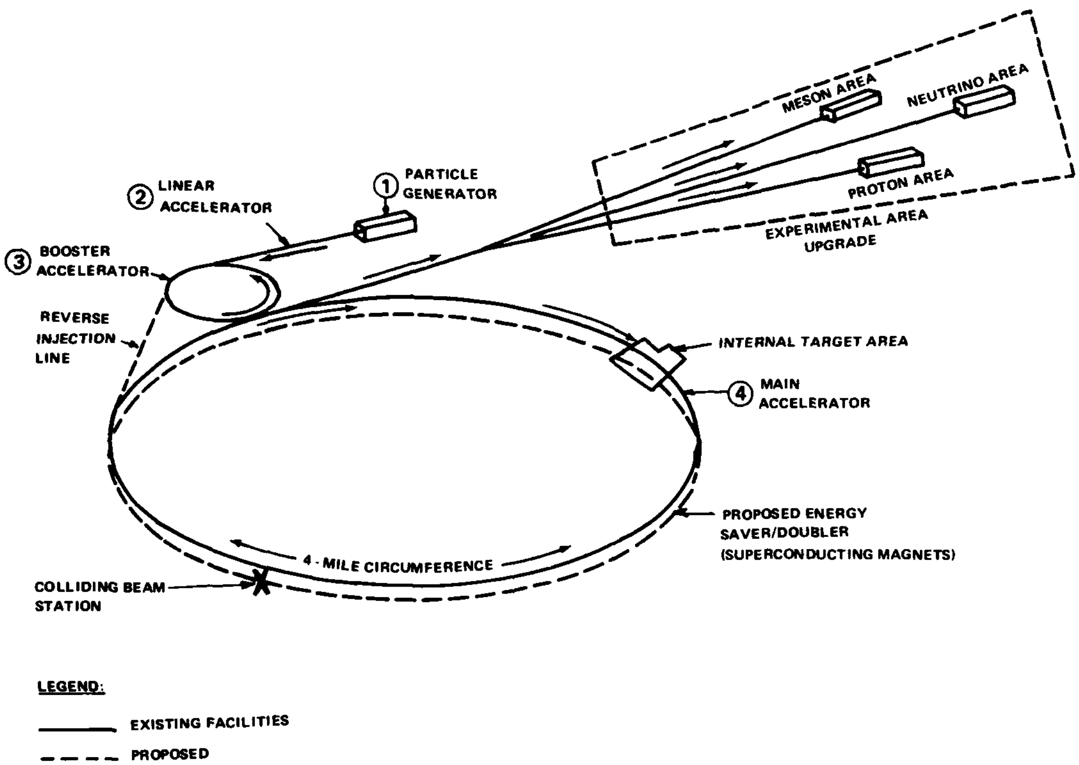
1/In a colliding beam accelerator, the center-of-mass energy is the sum of the two beams and represents the amount of energy available to make new particles.



SOURCE: FERMI NATIONAL ACCELERATOR LABORATORY

THE FERMILAB ACCELERATOR - WITH THE INCLUSION OF THE TEVATRON PROJECT

by a public accounting firm, Fermilab had spent about $31 million on accelerator R&D for the project.

Although the Energy Saver will reduce power consumption requirements for the present accelerator, according to Fermilab officials, its chief purpose is being the first step toward the Tevatron. DOE recognizes that the Energy Saver is a necessary prelude to a higher energy accelerator and intends to complete the Tevatron. In fiscal year 1978, DOE provided Fermilab $800,000 in planning and design funds for the new machine. The President's 1981 budget submission showed the Energy Saver's construction cost to be an estimated $46.7 million and related R&D, equipment, and pre-operating costs of about $52 million, including $24 million of the $31 million reported by the public accounting firm.

The President's fiscal year 1981 budget request also included funding of Tevatron, Phase I. This phase will include the proton-antiproton collider, additional refrigeration, two experimental areas, and R&D applicable to the collider. The construction cost was estimated to be $39.5 million and related costs of $18.8 million, making the total Phase I costs $58.3 million.

Yet to be requested are funds for Tevatron, Phase II. This phase will provide fixed target capabilities, including the extraction of the 1,000-GeV beam from the main accelerator and upgrades of the switchyard and experimental areas to accept the beam. DOE estimates construction costs for this phase at $46.5 million. Estimated total related costs as of March 1980 were $7.9 million bringing the total Phase II cost up to $54.4 million. Thus, the total estimated cost for these three phases is about $212 million. Adding $7 million of accelerator R&D prior to project authorization which is not included in DOE estimates, brings the total estimated cost up to $219 million.

In addition, Fermilab's plans call for $4.8 million for facility improvements. Improvements include a particle detector development area, a superconducting engineering complex, and road construction.

<u>Construction at Brookhaven</u>

Brookhaven is constructing a 400-GeV x 400-GeV proton-proton colliding beam facility, Isabelle. This project, which is to use superconducting magnets, has been authorized by the Congress for completion in 1986. Isabelle is to collide protons at center-of-mass energies more than 10 times higher than any accelerator now available. Experiments at Isabelle are expected to make definitive tests of the theory to unify the electromagnetic and weak forces of nature and to provide data for further development of models of the strong interaction force. As with any new, higher energy facility, the most important discoveries may be completely unexpected at the present time.

The President's fiscal year 1981 budget request showed Isabelle's estimated construction cost to be $275 million, and related operating and equipment costs (includes costs for accelerator R&D, pre-startup operations, and the initial complement of equipment) to be $168.9 million. An additional $11 million in accelerator R&D funds and about $1.7 million of planning, engineering, and design funds were spent prior to project authorization. Also, Brookhaven officials plan to spend some accelerator improvement funds for upgrading the Alternating Gradient Synchrotron's experimental capabilities. This upgrade will also improve its injection capabilities for Isabelle.

Construction at Stanford

Stanford recently completed the Positron Electron Project. The initial operating test for the accelerator was made in April 1980. This accelerator is designed to provide electron-positron collisions at center-of-mass energies up to 36 GeV. Physicists believe electron-positron machines are important because their collisions are relatively easy to "see" and interpret, when compared to others such as proton-proton collisions.

Electron-positron collisions produce "clean" experiments in which the production of particles can be observed without large background radiation. The annihilation of electrons and positrons in the collisions first creates an intermediate state of pure electromagnetic energy, which rematerializes into a variety of newly created elementary particles. Since the electromagnetic force is probably the most completely understood force of nature, physicists believe electron-positron annihilation is an ideal starting point for studying the properties and behavior of elementary particles. Experiments to be conducted on the Positron Electron Project are expected to complement experiments on Petra, a similar West German accelerator.

The Positron Electron Project's estimated line-item construction cost was $78 million, but its total cost, when related equipment and operating costs are included, was about $143 million. In addition, since 1975 the laboratory spent $5.3 million through fiscal year 1979 to increase the energy of its linear accelerator, which is being used as an injector for the Positron Electron Project as well as for experiments.

The costs of this improvement were not reported as being related to the project because the upgrading of the linear accelerator was independently justified on the basis of improved fixed-target physics research capabilities for the linear accelerator. However, Stanford officials acknowledged that this improvement, while not necessary for its operations, will help the Positron Electron Project efficiently attain its design energy and luminosity.

Cornell Electron Storage Ring

The Foundation has supported the construction of the Cornell Electron Storage Ring, an 8-GeV x 8-GeV electron-positron colliding beam accelerator which began start-up operations in 1979. Foundation officials told us that this accelerator fills a gap in electron-positron physics. They pointed out the Cornell accelerator's energy range will be ideal for exploring indepth the nature and characteristics of the upsilon particle, 1/ discovered in 1977 at Fermilab. According to Foundation officials, the Stanford Positron Electron Asymmetric Ring at 4 GeV operates at too low of an energy range and the Positron Electron Project's energy range of 18 GeV is too high to effectively explore the upsilon particle. In 1976, DOE's High Energy Physics Advisory Panel agreed that the Cornell accelerator would permit a more rapid and comprehensive study of electron-positron physics in the 4- to 8-GeV energy range and strongly endorsed its construction. A photo of the Cornell Electron Storage Ring is on the following page.

The Cornell Electron Storage Ring was estimated to cost about $20.7 million. This includes about $13.5 million for construction and related equipment, computer, and start-up costs of about $7.2 million. With construction to be completed in September 1980, the Foundation estimates an underrun of $400,000; consequently, the project's total cost is expected to be about $20.3 million.

Development at Argonne

In addition to construction at the four accelerator centers, Argonne laboratory carried out several accelerator R&D projects intended to develop new accelerator technology and concepts which could be used to improve its accelerator's capabilities. These R&D projects were carried out during the

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1/The upsilon particle represents the fifth quark discovered, which is theorized to be one of the basic constituents of matter.

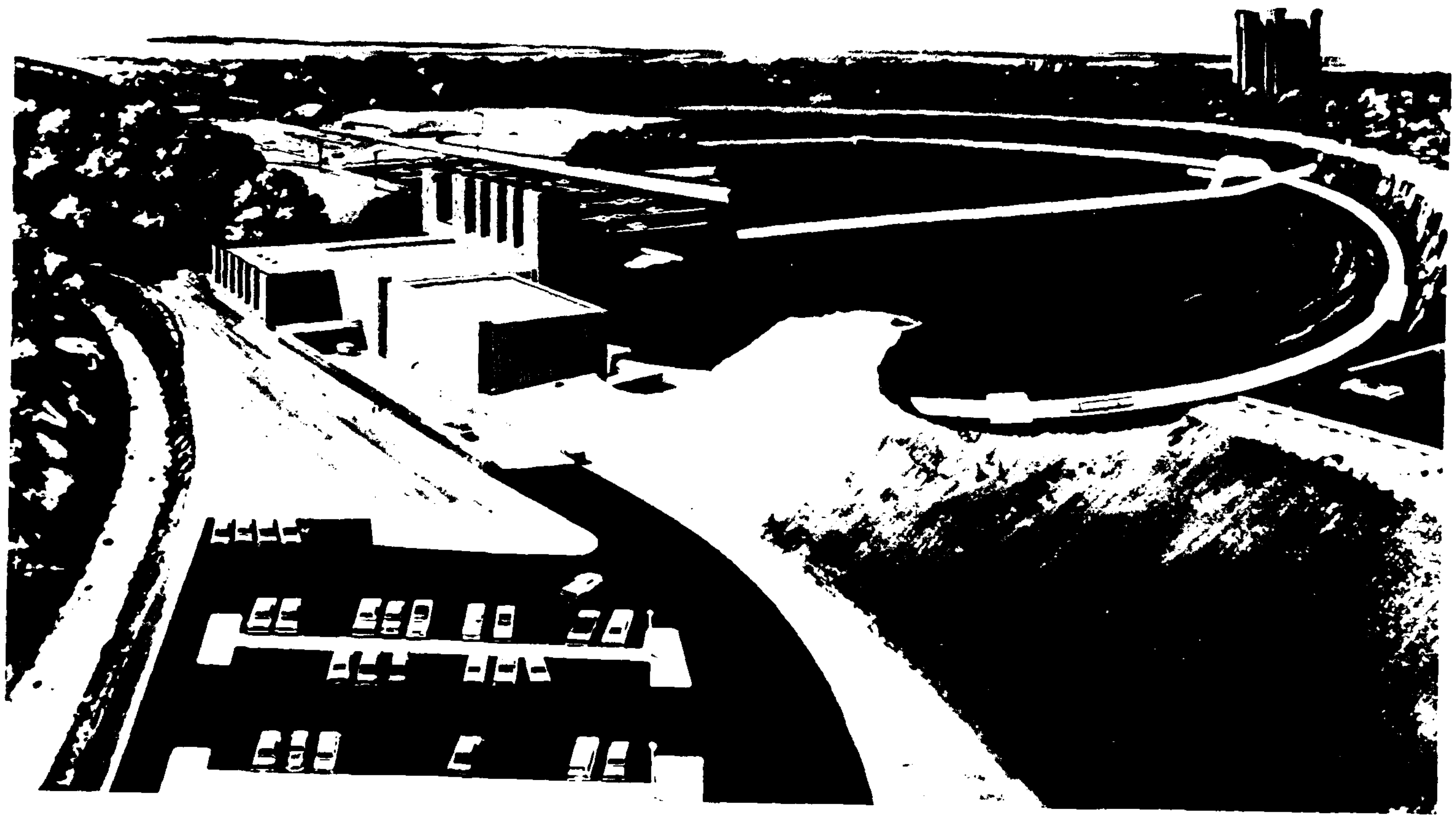

PHOTO COURTESY OF CORNELL UNIVERSITY

THE CORNELL ELECTRON STORAGE RING

1970s while the shutdown of the accelerator was under consideration. According to laboratory officials, allowing shutdown considerations to interfere with these projects would have made the shutdown a self-fulfilling prophecy. One area of Argonne's accelerator R&D work concerned superconducting magnets. A ring of such magnets, designed based on the results of this R&D work, was proposed three times as a line-item construction project, but DOE rejected it, in part, because the shutdown was being considered. Argonne, with DOE's approval, continued with R&D on superconducting magnets, spending over $400,000 to build and install 14 superconducting magnets in a beam line. Argonne officials told us this was the first practical experience with a high energy superconducting beam line, and it helped develop the power saving concept being used in Fermilab's Energy Saver. Another R&D project was a "booster" accelerator system, estimated to cost $4 million. According to laboratory officials, the booster system was completed too late to be used for the accelerator's unpolarized beam operations, but was fully justified by its contributions to accelerator technology and is being productively used as a pulsed neutron source for other research programs, such as solid state physics.

ADVERSE EFFECT ON OTHER PROGRAM ELEMENTS

Although the physics community has deemed the emphasis on accelerator development and construction efforts to be essential, such efforts have required large amounts of funds at the expense of less than optimal funding of other key program elements. Program elements such as long-range accelerator R&D, accelerator utilization, and experimental research support have suffered. Each of these elements are also essential to the long-term health of the program. Accordingly, even with the emphasis on construction, which is intended to help maintain a leadership position, the long-range health of the U.S. program may be jeopardized.

Long-range accelerator R&D

Accelerator R&D is concerned with developing the technology needed to build accelerators which produce the increasingly higher energy particle beams required for experiments. Historically, the U.S. program has spent about 10 percent of its operating funds on accelerator R&D activities. This includes both R&D applied to specific projects and long-range R&D not associated with a specific project. Today this fraction for all accelerator R&D is 14 percent, reflecting a heavy commitment to superconducting magnet development. Most of these funds are for specific projects, with only between 1 and 1-1/2 percent of operating resources devoted to long-range

R&D. This relatively low funding of long-range accelerator R&D appears to have adversely affected current construction projects which include superconducting magnets, and the physics community is concerned that the technology to build higher energy accelerators in the future is not being sufficiently advanced.

Most of the recent accelerator R&D efforts are for developing superconducting magnets. Research on superconducting magnet technology for Fermilab's Energy Saver and Brookhaven's Isabelle accounts for about three-fifths of the U.S. program's accelerator R&D funds. The U.S. program has sought to ensure leadership by constructing accelerators which are to use superconducting magnets that are still being developed. DOE program staff and laboratory officials recognize the high risks in this approach, but they believe the physics potential and reduced electrical power requirements of large superconducting accelerators justifies this "leap" in the state-of-the-art.

In 1971, the High Energy Physics Advisory Panel recommended that pilot projects be supported to develop new accelerator technologies, including a superconducting synchrotron of intermediate energy at the Lawrence Berkeley Laboratory, "to gain experience in the construction and operation of a synchrotron with high field superconducting magnets." The Panel recognized that before a large superconducting accelerator could be built, many problems had to be solved. A small superconducting synchrotron was seen as a possible pilot project to provide more experience before a single approach to the problems would be selected.

Following preliminary studies, the Berkeley project was funded as an R&D effort in fiscal year 1975, but at a lower rate than requested. Its purpose was to investigate the integration of superconductivity into a total accelerator system. Laboratory officials said the low rate of funding slowed the project's progress, increased its cost, and necessitated a change in its objectives. Originally expected to cost $3.7 million and take 2-1/2 years, DOE and Berkeley mutually agreed to terminate the project half-complete in mid-1978 after nearly $7 million had been spent. The Berkeley project was dropped because DOE and laboratory officials realized that the project was moving too slowly and results would be too late to be useful in the full-scale superconducting projects initiated at Brookhaven and Fermilab--Isabelle and the Energy Saver, respectively.

Technical problems on superconducting magnets at Fermilab and Brookhaven have since increased the cost of their R&D efforts. For example, Fermilab chose to mass-produce magnets and built 130 superconducting magnets 22 feet long.

These cost about $50,000 each or a total of about $6.5 million. These 130 magnets were produced as part of the engineering R&D effort to develop the mass production techniques needed to build the magnets needed for the accelerator, as well as to develop a good magnet design. None of these first 130 magnets were acceptable for use in the accelerator. DOE officials told us that these magnets are being used in experimental beam lines at Fermilab as an energy conservation measure. Brookhaven has been building magnets on a prototype basis and has also encountered problems in attaining the desired quality.

According to laboratory officials, some coordination on superconducting magnets between Lawrence Berkeley, Fermilab, and Brookhaven has taken place. They said they have had joint visits, exchanged information, and shared experiences. However, they said each laboratory encountered unique problems that needed special solutions, and they have not worked closely together on specific magnet designs. In March 1980, DOE took a step toward improving coordination by forming a Superconducting Magnet Technology Coordinating Committee.

According to DOE officials, unexpected problems encountered in the superconducting magnet efforts have tended to reduce accelerator R&D funds available for developing new technology for the next generation of accelerators. In this regard, the Director at Stanford pointed out that greater emphasis needs to be placed on the long-term development of new technologies to advance the state-of-the-art for new accelerators. He noted that a disproportionate R&D effort has been expended on already authorized or soon to be authorized construction projects. He considered new technology absolutely essential to increasing accelerator energies at a reasonable cost.

The concern that long-term accelerator R&D has been inadequate is shared by DOE's High Energy Physics Advisory Panel. In May 1980, a subpanel on accelerator R&D submitted a report to the full Panel, which the Panel endorsed. The subpanel reviewed the overall scope and quality of U.S. accelerator R&D, focusing on issues related to the long-term future of the field. The subpanel reported that greater efforts are needed to reduce the costs of future accelerators. It recommended that the amount of resources allocated to long-range accelerator R&D should be raised from between 1 and 1-1/2 percent of operating expenses up to about 4 percent.

Accelerator utilization

Accelerator utilization has been near 50 percent, compared to the 70 to 75 percent proposed in 1975 or the 85 percent the physics community considers optimal. Experimentalists complained that insufficient funds have limited laboratory support and needed experimental equipment. This in turn, has delayed and stretched out experiments, reduced their scope, and lowered researchers' morale.

Accelerator utilization rates are used by program managers to measure the ratio of research output to that possible with a given physical plant. Since high energy physics output is difficult to quantify objectively, utilization is measured by the delivery of the accelerator beam. While some disagreement exists within the physics community as to the best method of measuring accelerator utilization, measurements generally focus on the pulses, or number of particles delivered, and on the hours of accelerator operations. The utilization rates for fiscal years 1979-81, as computed by DOE for DOE-supported accelerators, are shown below.

	Percent of utilization (note a)		
Accelerator	1979	1980 (note b)	1981 (note b)
Fermilab	54	53	49
Alternating Gradient Synchrotron	51	54	50
Stanford linear accelerator	26	27	27
Stanford Positron Electron Asymmetric Ring	82	41	41
Positron Electron Project	(c)	82	82

a/DOE's formula for computing utilization rates is:

$$\text{Utilization} = \frac{\text{hours operated}}{\text{practical maximum hours}} \times \frac{\text{pulse rate used}}{\text{optimum pulse rate}}$$

b/Estimated.

c/Not operational.

The U.S. program's utilization of accelerators, as indicated by the rates shown above, is considerably lower than the 70 to 75 percent sought in the proposed 1975 program strategy or the 85 percent the physics community considers optimum. In comparison, CERN's utilization rates range from about 72 to

95 percent and West Germany's are from about 80 to 92 percent. Much of this gap can be explained by increasing costs of operating accelerators and a funding level that is about $50 million lower than that assumed in the proposed program strategy.

The accelerator laboratories, which are large consumers of electric power, face increasing power rates. Brookhaven's power rate recently jumped nearly 50 percent. Stanford's operation of the Positron Electron Project will require more power at higher rates; and its annual power bill is expected to increase from about $2 million to $5 million. Fermilab's annual electrical bill is expected to increase 20 to 25 percent when its contract for electricity expires in 1981. While power costs have been the most visible, the laboratories point out that other costs of operations have also increased beyond adjustments for inflation.

In 1977, a subpanel on new facilities of the High Energy Physics Advisory Panel reported that one of the consequences of a program funded at about $300 million in 1978 dollars would be utilization rates of about 50 percent. Since the current funding level is approximately that amount in constant dollars, the low utilization rates should not be surprising.

However logical the explanation, low utilization rates are not desirable. To produce scientific results, the Nation's physicists must exploit the accelerators that have been built. A former laboratory director compared the current situation to building a "Cadillac" and not providing the gas to operate it.

University experimentalists need additional support

In recent years, the number of U.S. institutions (about 80) and physicists (about 1,100) involved in high energy physics experiments have remained relatively constant. Meanwhile, accelerators and particle detectors have declined in number as their costs and sophistication have risen. Despite such reductions these institutions and physicists can productively contribute to experiments, which have become increasingly larger and complex and thus require more collaborations among research groups. From a purely short-range financial view, fewer groups might carry out the essential physics more efficiently. However, having a larger number of participating groups is generally recognized as helping to promote innovative ideas and allowing more students and university scientists to have firsthand research experience. The problem university experimentalists face is that as the sophistication of experiments has increased, so have their costs.

The accelerator laboratories have internal research groups which conduct a substantial part of the research at their respective laboratories. However, the majority of the physics research continues to be carried out by university faculty and students, despite the difficulties of working at a distance from the campus and of financing experiments.

Many of the participating universities' support groups have not kept up with the technological pace of high energy physics. For example, many universities lack the needed computer facilities. This, combined with larger and more complex experiments, has required increasing portions of the research to be carried out at the large laboratories with increasing support from laboratory personnel. While a laboratory's in-house physics research group conducts physics research, it also provides support services and direction to the overall experimental program and assistance to the university users. Each laboratory determines the strength and breadth of its in-house staff by the amount of support it provides for physics research and other technical support in its annual operating budget.

Experimentalist complaints at Fermilab

A number of prominent physicists and user groups have complained about the inadequacy of research support from Fermilab. In 1977, seven physicists, including a Nobel Prize winner, wrote to laboratory management that a stronger group of in-house physicists was needed to provide improved assistance. Similarly, user groups have complained about inadequate technical and engineering assistance at Fermilab.

The impact of research support by Fermilab is quite significant because about half of all high energy physics experiments conducted in the United States are carried out at the laboratory. This is largely because of its greater capability for handling simultaneous experiments, as shown below:

Laboratory	Typical number of experimental setups	Typical number of simultaneously operating setups
Fermilab	26	15
Brookhaven	9	5
Stanford (note a)	13	9

a/Includes the Positron Electron Project.

The Director of Fermilab said the laboratory has been aware of the need to increase research support for several years. He pointed out that the laboratory has frequently advised DOE of the need for greater staffing and is seeking to add about 50 research positions. However, he added that the R&D needs of the construction efforts at the laboratory currently have priority, and he lacks the resources to increase physics research support.

Besides the low level of research support, user groups have complained about inadequate technical and engineering assistance at Fermilab. The laboratory once tried to resolve this problem by distributing a list of local electronics firms and suggesting that the user groups hire their own technical support from firms on that list. More recently, the laboratory director stated, the laboratory reduced the number of experiments to increase the support for each.

<u>Support from funding agencies</u>

According to some university experimentalists, funds for technical and engineering support and for detectors and other equipment are almost always less than optimal for their experiments. They noted that their contracts or grants are usually negotiated at a level slightly below what is needed to complete the experiment. Several groups complained that funding limitations have forced them to reduce the scale of experiments or delay construction of detectors and other large apparatus. One experimentalist said his project took 8 years to complete when it should have taken 4. He said such slowdowns waste scientific talent and jeopardize the careers of young physicists; in frustration some young physicists decide to pursue other fields.

In light of the funding levels, however, a program official believed DOE has stretched out funds and supported university groups better than anyone in the physics community thought possible. The official said equipment has been "scrounged" and experimental collaborations have been encouraged. Foundation program officials similarly reported they only fund essential equipment, and experimentalists often have to make do with what they already have or can borrow. In addition, Foundation officials said they have stretched funds by reducing support to about six university research groups to "maintenance levels"--amounts for the physicists' salaries and travel expenses, but no funds for equipment or technical support.

Thus, university experimentalists are finding it progressively more difficult to keep up with the evolving technology needed to equip increasingly sophisticated experiments.

The Director of the Stanford Center recently suggested that either greater amounts of funds should be devoted to supporting university researchers or the program may have to reduce the number of researchers supported so that those remaining have adequate funding. However, another alternative might be to shift some of the emphasis from new construction and broad equipment capabilities toward support for specific experiments.

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Since establishing the program's funding level at $300 million per year, plus inflation, for fiscal years 1979 through 1984, DOE did not document in program plans how or to what extent each of the program's key elements were to be carried out. DOE initiated a study which was to develop near-, mid-, and long-range plans for present and increased funding levels. We believe the development of such plans is commendable and should be instituted formally on a periodic basis.

In the absence of such plans, the program has emphasized construction, while other key program elements such as long-range accelerator R&D, accelerator utilization, and experimental research support have been inadequately funded. Although the physics community believes this emphasis to be appropriate, the stretched-out funding over these latter elements may adversely affect the posture of the U.S. program. Inadequate funding of long-range accelerator R&D may preclude the U.S. development of new technologies needed for the next generation of accelerators. Low accelerator utilization and experimental research support limits or stretches out current research efforts, which adversely affects the morale of physicists, and might result in a transfer of top physicists to other nations or the most brilliant graduate students to other scientific fields. Thus, present efforts may help maintain a leadership position by providing the needed accelerator capabilities, but the U.S. program may lack the technology for future accelerators and/or the top physicists needed to make the discoveries which represent leadership.

While developing plans to fit the funding level should help, we believe the bigger issue of possible alternative objectives still remains. In light of our concern, we discuss some alternative policy options in the following chapter.

CHAPTER 5

SELECTED POLICY OPTIONS FOR THE U.S. HIGH ENERGY PHYSICS PROGRAM

In view of the problems the high energy physics program has encountered in apparently trying to do more than limited resources would allow, we explored the costs and ramifications of alternative policy options. The international standing of the U.S. program has been, and will continue to be, one of the key factors setting the pace of accelerator construction, which in turn is a major factor in establishing the level of funding needed. With increasing costs of high energy physics and competition from Western Europe, this may be the appropriate time to reconsider the desired international standing and/or approach for achieving it. To achieve the desired international standing, a cohesive plan should be established and funding levels consistent for carrying it out should be provided.

To explore the ramifications of some alternative policies, we asked laboratory and agency officials what actions they would undertake under the following policy options.

--To lead the world in all frontiers of high energy physics.

--To be competitive in all frontiers, lead in none.

--To forget about leadership and competitiveness and work in unison with European counterparts through greater cooperation and collaboration.

--To pull out of competition and reduce U.S. efforts to a derivative mode, in which the major experimental work in the field would take place abroad.

For each of these options, we obtained input from laboratory and program officials to derive a probable strategy, estimate minimal and optimal costs in fiscal year 1979 dollars of carrying out that strategy, and identify the advantages and disadvantages of pursuing the option, during the 1980s. Our requests to the laboratories and their responses are discussed further in appendix IV.

The laboratories' responses were directed at the technical frontiers of high energy research as defined in terms of accelerator capabilities. For the 1980s, three frontiers are generally recognized.

--Collisions between proton beams and/or proton-antiproton beams.

--Collisions between an electron beam and a positron beam.

--Collisions of proton beams against fixed targets.

These three frontiers are the main thrust of current plans and are likely to be pursued at specific laboratory sites. However, other capabilities also may be developed. For example, a site providing collisions between proton beams might also develop proton-electron collision capabilities.

Aside from the above stated four options various mixed approaches are possible. Laboratories could pursue their respective technical frontiers with different levels of emphasis. For example, the United States could decide to lead at one laboratory, compete at another, and be in a derivative mode at a third. For this mixed option, we extrapolated a few of the many possible mixes from the data provided.

From a practical standpoint, a multitude of possible strategies exist under each policy option. The appropriate strategy to be pursued under each policy option should be determined not only with scientific and technical input and judgments, but with input as to possible funding levels. We developed strategies based on laboratories' and agencies' input. We recognize that these strategies may not be the most practical to pursue under each option. We are presenting the following policy options, strategies, and possible costs to merely illustrate that alternative policies have significant impacts on the strategy to be pursued and the amount of funds needed to carry out that strategy.

WORLD LEADERSHIP IN ALL FRONTIERS

To lead the world in all frontiers of high energy physics research, the three major accelerator centers--Stanford, Brookhaven, and Fermilab--would each build a high energy accelerator and operate it at the optimum level. They and the other three laboratories--Argonne, Lawrence Berkeley, and Cornell--would exploit their facilities to the fullest. Program officials would expand university research efforts, computer capabilities, and staff.

This approach would maximize the benefits to be derived from high energy physics research. Once the frontline machines became fully operational, U.S. researchers would have the tools needed to make major new discoveries in the field.

The main drawback to this option is its cost--about double the funding level now authorized. Some science policy experts and program officials point out that the United States does not have to pursue everything that the physicists or laboratories believe is needed and that a preeminent U.S. program can be achieved at less cost.

Probable strategy and cost

Fermilab would build a 1-TeV, fixed-target proton accelerator by 1982 for leadership through the late 1980s and another accelerator by 1988 at energies of up to 5-TeV. Laboratory officials envisioned that the second accelerator would yield the highest energy proton-antiproton, proton-proton, and proton-electron colliding beams in the world through the early 2000s.

In its bid to lead in electron-positron collision research, the Stanford laboratory would improve and fully exploit its existing accelerators. In addition, beginning in 1983, Stanford would construct a new facility to compete with the large electron-positron accelerator proposed by Western Europe.

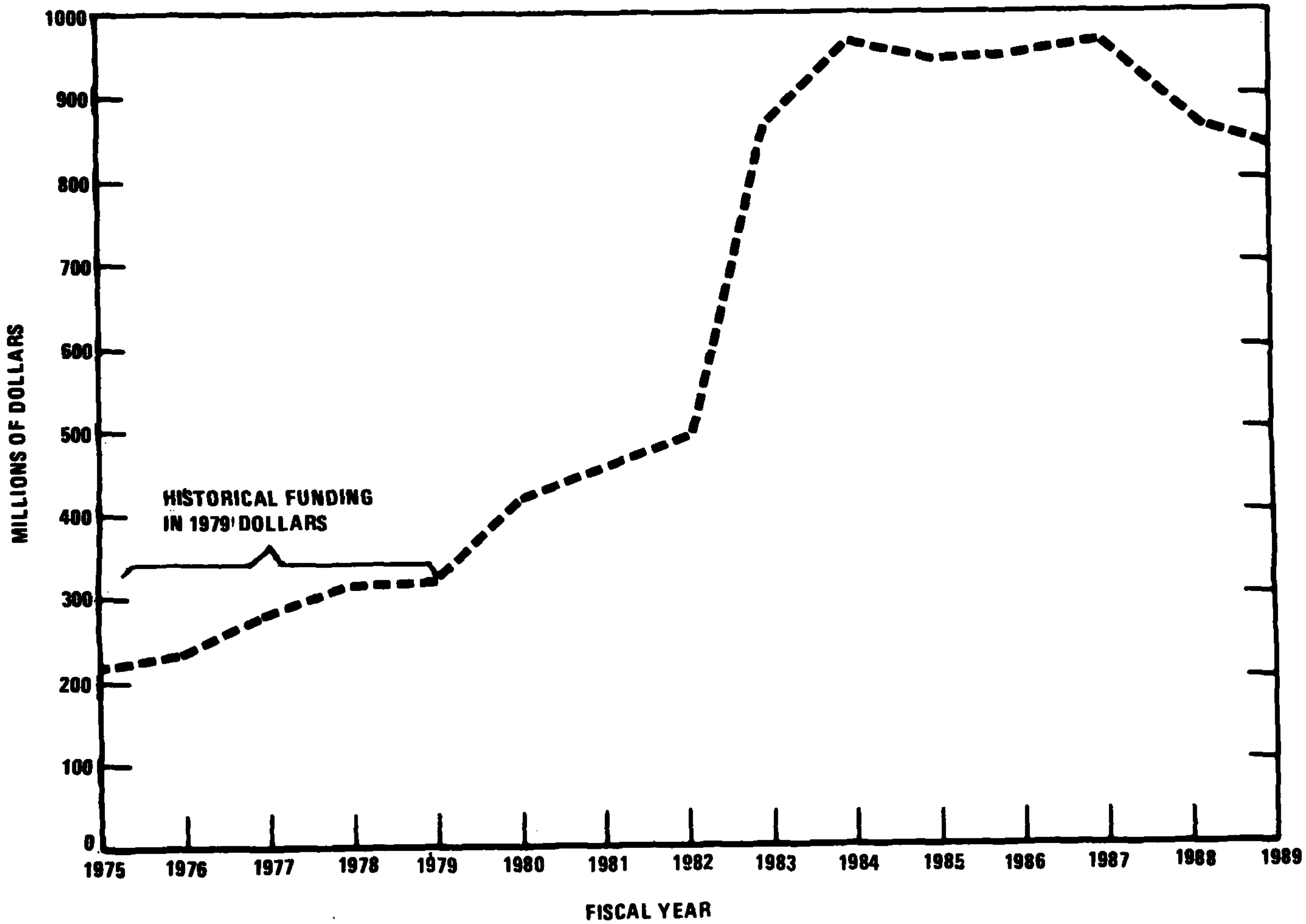
Under currently approved construction plans, Brookhaven's Isabelle would lead in proton-proton physics during the late 1980s. Brookhaven envisions constructing Isabelle with two additional experimental areas and further improving and exploiting the Alternating Gradient Synchrotron.

The other U.S. laboratories indicated that under a policy of leadership they would expand their existing efforts. For example, Lawrence Berkeley Laboratory, which supports Stanford's efforts, envisioned a 75-percent increase in staffing.

For the universities, a policy of world dominance would mean expanded efforts, including greater participation, more sophisticated experiments, and the development of better instruments. DOE and Foundation officials projected a 25- to 30-percent increase in funding for university research.

The probable increase in university research funding combined with the strategy proposed by the laboratories would require an average annual budget of between $678 million and $761 million in the 1980s. As the graph showing the optimal budget on the following page indicates, the funding needs under this option could increase to over $900 million a year (in 1979 dollars) by the mid-1980s. The graph reflects a slight decrease in the late 1980s; this decrease probably reflects a current absence of laboratory construction plans for that

WORLD LEADERSHIP IN ALL FRONTIERS -OPTIMAL BUDGET-IN MILLIONS OF DOLLARS-
BY FISCAL YEAR
FISCAL YEARS 1980-1989 PROGRAM BUDGETS
(IN 1979 DOLLARS)



period. Funding levels for the late 1980s, therefore, could exceed $1 billion annually, instead of declining.

DOE program staff pointed out the sum of the individual responses to a particular option can result in duplicative efforts and an overstated program. The three major laboratory centers currently have accelerators using different technologies, but the laboratories' proposed efforts under this option appeared to overlap. A multiplicity of efforts would help ensure that significant discoveries are made and we believe some duplication would exist under this option. Nonetheless, we eliminated some apparent duplications. After eliminating such duplications, we estimate the average annual budget needed would range from $598 million to $669 million a year during 1980-89, or about twice the current level.

Advantages and disadvantages

Some laboratory officials were enthusiastic about the benefits of a U.S. program for leadership in each frontier, while others believed such an option too costly and impractical. Stanford officials visualized maximum flexibility of experimentation and predicted substantial contributions to other fields of basic research. Fermilab and Brookhaven officials had similar views. For example, Fermilab officials stated:

> "The excitement generated by U.S. supremacy will surely sway young students, winning back those we have lost by the 'relevancy' issues of 1968 and by the budget cuts of the past 10 years. The subsequent strengthening of the basic research posture of the U.S. will win international respect."

Program officials were concerned with the duplicative efforts that such a program might entail. DOE program staff noted that essentially duplicative efforts might be pursued within the United States because it would reduce the risks of failure and enhance the probabilities of making significant discoveries. One Foundation official told us that program managers are responsible for effectively managing and allocating resources to achieve the desired balanced program. He believed it would be irresponsible to pursue everything that the laboratory directors thought was needed.

The new projects envisioned by the laboratories under this option would depend heavily on the results of substantial R&D. This can be viewed as both an advantage and a disadvantage. On one hand, the state-of-the-art for accelerators and equipment would be pushed, and more technological spinoffs may result. On the other hand, a larger investment in accelerator and equipment R&D would be necessary.

Perhaps the overriding disadvantage of a world leadership effort on all frontiers is its cost. This approach may double current funding levels and if the overall basic research funding is not also increased this could require reduced funding levels for other basic science fields.

Even if large amounts of funds were spent for high energy physics, world leadership in each frontier may not be attainable. Europe is considering building a large positron-electron accelerator at CERN. This machine is to be built using the latest state-of-the-art and cost about $1 billion. Similarly, the Soviet Union has approved plans for constructing a 3-TeV fixed-target proton accelerator. Thus, some officials pointed out that a program of world leadership in each frontier may not only be undesirable, but may be impractical.

COMPETITION ON ALL FRONTIERS

Under a policy of competition in each frontier, the United States would endeavor to make a fair share of the major discoveries with competitive accelerators. This option offers essentially the same advantages and disadvantages as the world leadership option, but to a lesser degree. On the other hand, if the U.S. program fails to make a fair share of the discoveries the enthusiasm of U.S. researchers might be dampened. A competitive approach would cost less than world leadership in each frontier, yet considerably more than current budget levels.

Probable strategy and cost

Brookhaven would complete Isabelle in 1986, as currently scheduled, and add some experimental facilities. Actually, Brookhaven would clearly have the world leader proton-proton colliding beam accelerator because another of equally high energy and luminosity probably will not be built.

For this option, Fermilab included only its currently authorized "Energy Saver" as a major improvement and proposed to carry out R&D for the 1-TeV accelerator. Officials pointed out that their present accelerator with the Energy Saver will compare favorably with its European counterpart, but not with the Soviets' proposed 3-TeV machine.

Stanford proposed to extensively exploit its accelerators, provide improvements sooner than currently envisioned, and in 1989 begin constructing the new laboratory proposed under the world leadership option. Laboratory officials believe the current DOE program significantly underuses existing facilities and provides improvements too late to compete with the Europeans.

Argonne proposed to make minor improvements to the Zero Gradient Synchrotron 1/ and operate it 6 months per year through fiscal year 1985. Argonne officials reasoned that continued operation would maintain the U.S. lead in polarized-proton physics.

Cornell's Wilson laboratory proposed to increase the energy of its new accelerator and construct an additional experimental facility. Wilson officials indicated such improvements will compete favorably with efforts in other countries and may provide opportunities for getting results not achievable elsewhere. Under a minimal budget, the laboratory did not include these improvements.

Agency program officials envisioned university funding increases of about 10 percent are needed to compete in discovering and better understanding phenomena. DOE and Foundation officials noted that universities' engineering and instrument development efforts need to be strengthened.

As the graph on the following page indicates, to maintain a competitive effort on three frontiers, the funding needs will increase through the 1980s, reaching nearly $600 million (in 1979 dollars) in 1989. Such an effort averaged over 10 years would cost the United States between $376 million and $471 million a year (in 1979 dollars) during the 1980s. This level is 16 to 45 percent more than what is currently being provided.

Advantages and disadvantages

The competitive option provides a multiplicity of efforts, but offers considerably reduced advantages compared to world leadership in each frontier. Technological spinoffs, for example, would be less likely because accelerator development would not be pushed to the state-of-the-art. The incentive for prospective young scientists to enter the field and for top scientists to remain may be similarly reduced.

This option would maintain the diversity of U.S. efforts, offering geographically dispersed high energy physics

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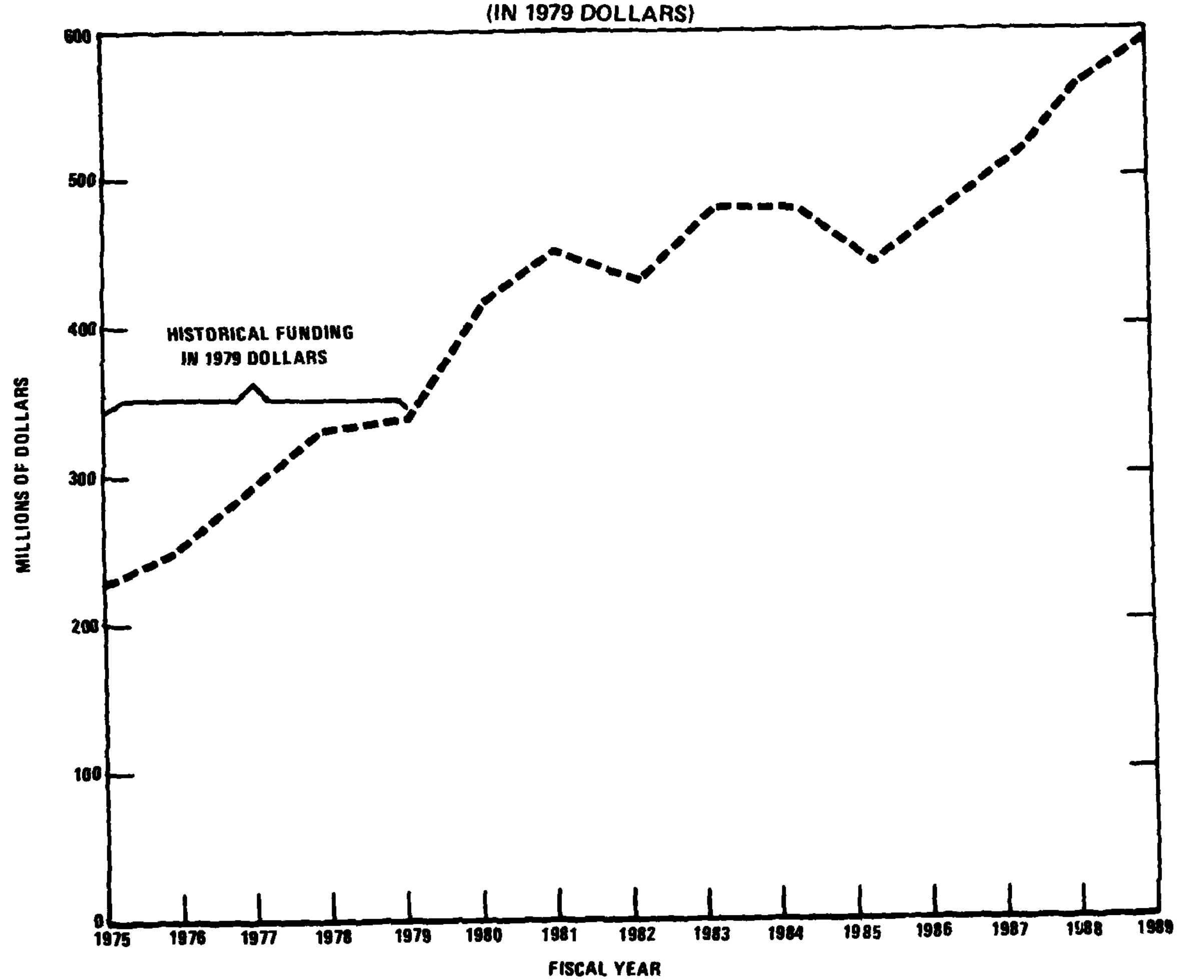
1/Argonne's accelerator ceased operations on October 1, 1979; Argonne's input to the options was provided prior to the shutdown.

COMPETITIVE AMONG NATIONS-OPTIMAL BUDGET-IN MILLIONS OF DOLLARS-BY FISCAL YEAR
FISCAL YEARS 1980-1989 PROGRAM BUDGETS
(IN 1979 DOLLARS)



laboratories with different capabilities. U.S. experimenters would continue to have opportunities to use different types of U.S. accelerators. This diversity would also assure that some U.S. physicists will be experimenting on the frontier where the major discoveries eventually will be made.

One problem with a competitive effort is that the laboratory officials' attitude appears to shift to "only" stay on a par with the competition, which increases the risk of "coming in second." While some new electron-positron and electron-proton capabilities were proposed, Europe is expected to make even greater leaps in these frontiers in the 1980s. In addition, Europe is currently building an antiproton-proton ring, which will have 3 years to "skim some of the cream" from Fermilab's higher energy antiproton-proton rings' research objectives before that machine begins operation in 1984.

In summary, a competitive effort as outlined by laboratory and program officials would cost considerably more than currently provided, yet may risk the United States falling behind in making the major physics discoveries. While the benefits of diversity are continued, the constraint to be "only" competitive seems to dampen the drive to be first with the impetus for developing new accelerators apparently left to others.

WORLD UNIFICATION

Although high energy physics has traditionally involved substantial international collaboration in experiments, a major factor in the program has been competition. As the higher energy accelerators have become more expensive to build, however, governments have begun to realize the impracticality of competing on all frontiers. This realization could lead to a focus away from international competition and toward joint exploration of the frontiers of high energy physics.

Probable strategy and cost

During the 1980s, U.S. action under a world unification option might be similar to that under a selected-frontier option discussed beginning on page 89. However, the United States would encourage increased international cooperation. Possible steps in this direction include deemphasizing competitive efforts, seeking agreements for accelerator use and cost-sharing, cooperating in the construction and operation of accelerators, and eventually sharing in the development of a world accelerator center.

The United States could withdraw from competition in those frontiers that other nations are concentrating on, and possibly focus resources to a frontier of its own. With the increasing costs of new higher energy accelerators, such an approach may naturally evolve. For example, the Chairman, International Committee on Future Accelerators, recently noted that if current plans are carried out, by 1989 each of the major competitors in high energy physics will have one frontier machine:

--The United States' accelerator, called Isabelle, to be operational in 1986, will have the highest energy proton-proton colliding beams.

--Western Europe will have the large electron-positron colliding beam accelerator. Although this machine has not yet received approval, considerable planning has been completed and it is expected to be in operation by 1988.

--The Soviet Union will have the leading fixed-target proton accelerator. This machine has been approved for construction and is expected to begin operations about 1988.

If the United States were to pursue this approach with Isabelle as the frontier accelerator and the other U.S. laboratories having derivative roles, the average annual U.S. program cost in 1979 dollars over this decade would range from about $251 million to $295 million.

Under a world unification policy, formal, multilateral agreements might be developed to assure international access to different types of accelerators. Formal agreements might also provide for cost-sharing arrangements. The United States already has formal agreements with China, Japan, and the Soviet Union for cooperative programs. Although U.S. experimenters frequently work at European laboratories and vice versa, these collaborative efforts have been arranged informally.

Thus far, the United States has independently funded the construction and operation of its accelerators. In contrast, 12 European nations have pooled their resources since 1955 at the CERN accelerator laboratory. The possibility of building "interregional" laboratories, with the cooperation of the nations on different continents, has been discussed since the early 1960s, but little action has been taken in this direction.

In responding to the unification option, Fermilab and Stanford suggested large interregional accelerator projects.

Fermilab proposed that the United States would pick up one-third of the expense of the 5-TeV project it proposed under the world leadership option. Stanford expected international R&D support during the 1980s but left undefined the amounts of construction and operating funds to be shared because most such costs would be incurred in the 1990s. Since Brookhaven is already constructing a frontier-type accelerator, laboratory officials did not believe an interregionally funded accelerator would be appropriate at that site. Program staff envisioned a 10- to 15-percent increase in university support under this approach.

While a number of alternatives are possible, if an international cooperative effort was initiated in this approach, the estimated U.S. average annual cost would be from $439 million to $516 million. Much of these funds, however, would be needed in the latter half of the decade after joint construction efforts are initiated. The graph on the following page indicates that if this approach were optimally funded, the program's budget may rise to nearly $600 million by 1989.

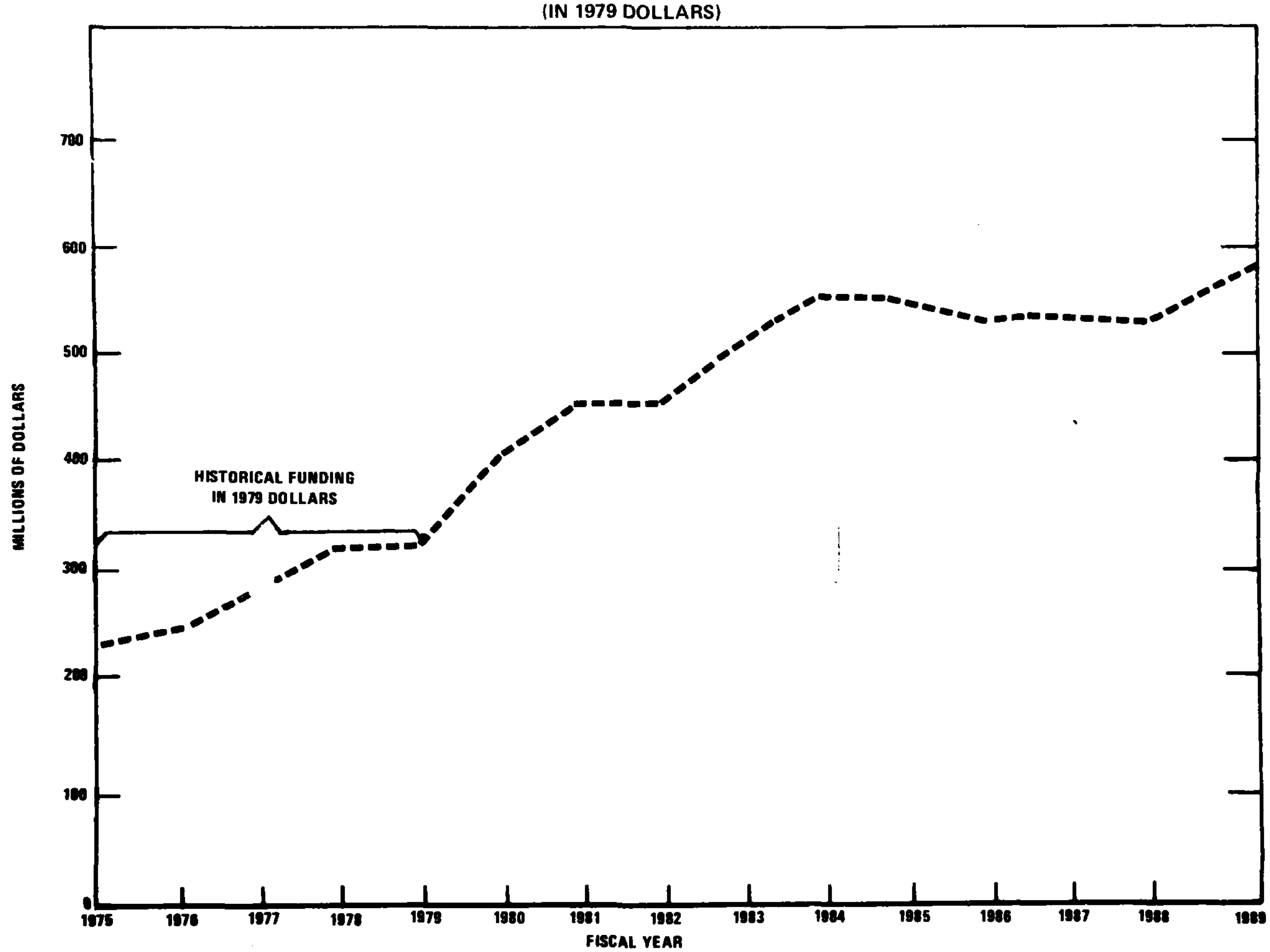
Advantages and disadvantages

Emphasis on unifying high energy physics research worldwide may permit the United States to limit investments in accelerator construction during the late 1980s, while assuring U.S. physicists access to frontier accelerators and fostering a spirit of international cooperation and understanding. Such a policy, however, will have to overcome a reluctance of nations to participate in international high energy physics ventures and the additional "red tape" which would be involved.

The advantages and disadvantages of focusing U.S. efforts on Isabelle's proton-proton capabilities would be similar to those under a selective approach. The United States can continue frontier physics at less cost, but risk having most of the significant physics discoveries found in other frontiers. Some science policy experts told us that the U.S. needs to have frontier physics capabilities to keep other nations interested in collaboration. Hence, if emphasis is focused solely on proton-proton physics, the United States may risk losing access to frontier physics accelerators, should other frontiers prove to be more significant.

The principal advantage of a strategy involving the joint funding of accelerators is that it would assure U.S. physicists access to the world's best accelerators, even if they are not built in the United States. For example, a Foundation program official told us that the "only way" to assure U.S. physicists have access to an accelerator is to at least partially fund it. He illustrated this by discussing CERN's

WORLD UNIFICATION-OPTIMAL BUDGET-IN MILLIONS OF DOLLARS-BY FISCAL YEARS
FISCAL YEARS 1980-1989 PROGRAM BUDGETS
(IN 1979 DOLLARS)



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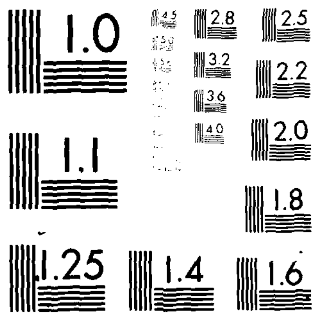

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NATIONAL BUREAU OF STANDARDS 1963 A

proposed large electron-positron accelerator. When built, many U.S. physicists will probably want to carry out experiments on it. While the top U.S. physicists with innovative experiments will probably be given access to the accelerator, he believed that CERN would have to assure its member nations that their physicists also have access. Hence, he thought CERN (1) would require that its member nations' physicists collaborate in experiments and (2) may limit the number of U.S. physicists allowed access to its accelerator. This official said that U.S. physicists would not be able to independently carry out experiments, and many good proposed physics experiments may be denied access to the accelerator. On the other hand, if the United States helped fund the accelerator, he believed that U.S. physicists would be granted access on the basis of scientific merit.

The international high energy physics community, however, is seeking to assure that physicists have access to the world's accelerators without joint funding. In this regard, physicists of many nations are represented by the International Committee for Future Accelerators. In October 1979, the Committee proposed worldwide adoption of a policy that all experiments be selected for an accelerator on the basis of scientific merit. The Committee is currently seeking endorsement of such a policy by each region and/or nation involved in high energy physics. However, whether nations would be willing to invest funds in building an accelerator and not have any assurance that their physicists have access to experiment on it is uncertain.

A major drawback to the joint funding of construction appears to be the reluctance of all parties to participate in a worldwide venture. This reluctance, according to DOE program staff, is because of concern over where the accelerator will be located and how it will be managed. Each principal region--the United States, Western Europe, and the Soviet Union--wants such an accelerator built within their respective regions. With many national and regional interests involved, DOE staff are concerned that the management of such an accelerator would be a "bureaucratic tangle" and U.S. access for experiments may be limited to a "fair share." However, DOE program staff told us the United States would follow such an approach if it becomes the "only way" to pursue the desired physics.

Another factor, national pride, has been a motivating factor in funding the research. Thus, the elimination or reduction of such emphasis may reduce national interest in contributing the funds needed to extend the research frontiers.

Although talk of worldwide collaborative efforts has persisted since the early 1960s, current discussions center on the "next generation after the next" accelerators which would cost in the range of $10 billion. The Director of the Stanford Center and the Chairman of the High Energy Physics Advisory Panel said that joint funding of accelerator construction would be premature during the 1980s. They and some DOE program staff noted that while joint funding of one-of-a-kind facilities saves any one geographical region construction costs, the need for travel, logistics, and the bureaucratic effort of managing such an interregional laboratory would partially offset this savings. Furthermore, only a part of the facility would be available to any one region, and would lead to less physics per dollar. These officials believed that jointly funded construction projects should be avoided as long as reasonable alternatives exist. In this regard, the Chairman, International Committee for Future Accelerators, suggested that increasing costs may result in all frontier accelerator capabilities existing at one world site by the mid-1990s. This presumably would involve participation and contributions by each nation involved in high energy physics.

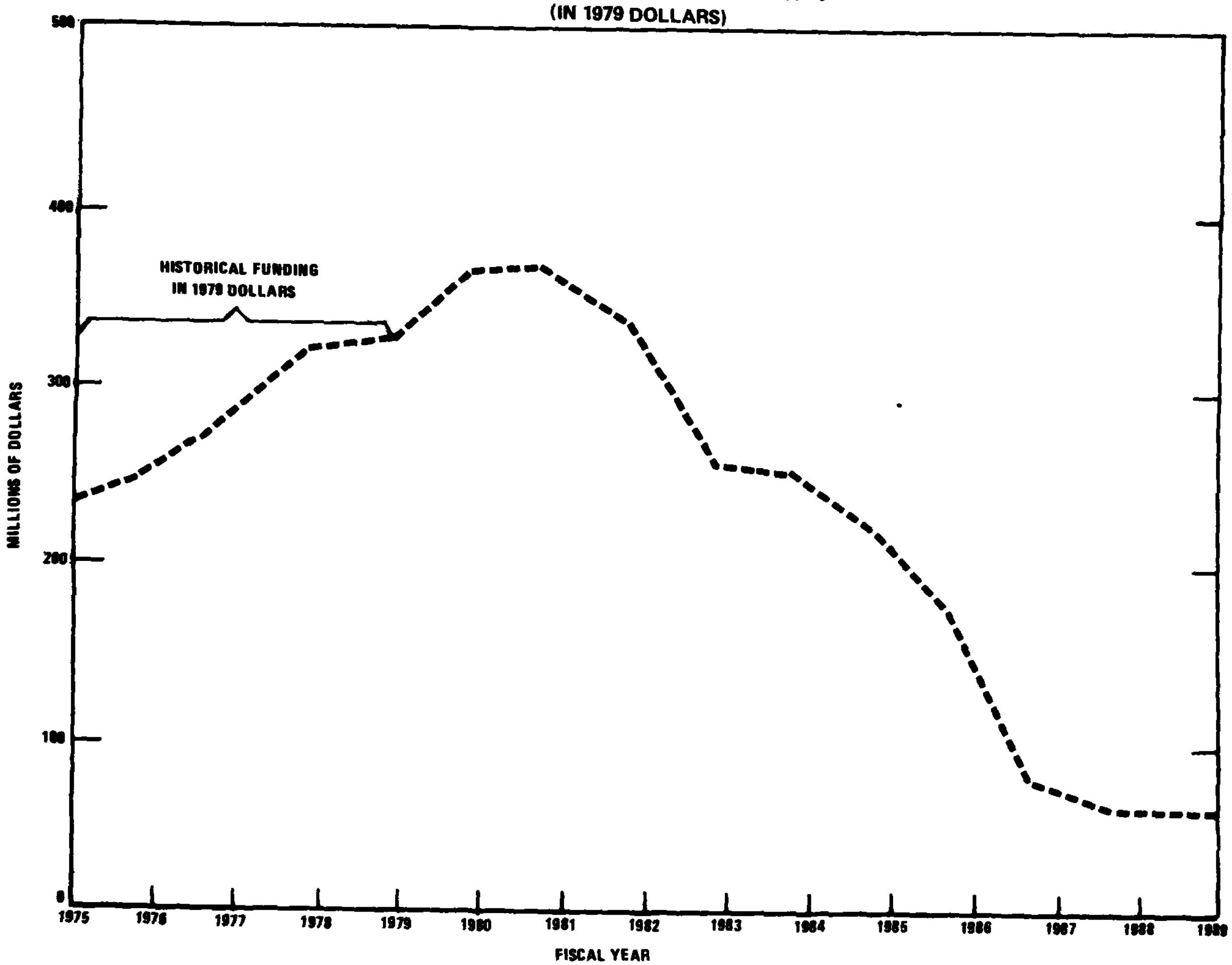
DERIVATIVE PARTICIPATION

Under derivative participation, the United States would support smaller accelerator facilities, along with theorists and experimentalists who would do frontier research abroad. Laboratory and agency officials viewed such an approach as a disaster that would sacrifice most of the program's benefits.

Probable strategy and cost

The large U.S. accelerators would be gradually phased out of operation during the 1980s. Fermilab officials would shut down their laboratory by 1986. Stanford would exploit its Positron Electron Project without major modification until 1987, when it would become an academic center. The construction of Isabelle would probably be abandoned. Cornell would phase its accelerator out by 1986, but would continue research and equipment R&D. The other laboratories are essentially heading toward derivative roles at present, but would further reduce their efforts. University support would be reduced because of dwindling opportunities for experimentation. A few universities with strong programs might collaborate on experiments at European centers. Annual costs would average around $200 million during the 1980s, with most of the costs in the early 1980s as a scaledown of efforts begin. The graph on the following page indicates that costs may level off at about $60 million a year (in 1979 dollars) by 1988.

DERIVATIVE PARTICIPATION-OPTIMAL BUDGET-IN MILLIONS OF DOLLARS-BY FISCAL YEAR
FISCAL YEARS 1980-1989 PROGRAM BUDGETS
(IN 1979 DOLLARS)



Advantages and disadvantages

The principal advantage of a derivative approach would be to reduce the funding levels for high energy physics. The country could use moneys "saved" for carrying out R&D directed toward achieving practical solutions to the Nation's present problems. Such an approach, though tempting in times of constrained budgets, may have long-term adverse effects.

Laboratory and program officials viewed the derivative option as a disaster to the program's purposes. For example, Brookhaven officials pointed out:

> "To be less than competitive in high energy physics is tantamount to losing the first rate minds to other areas. The less inventive, the less motivated, the plodders will take over and the brillance, the leadership will be gone from high energy physics * * *."

Laboratory officials envisioned top personnel departing to other fields or nations, opportunities for young physicists being curtailed, and the quality and quantity of publications being reduced. In addition to these effects, DOE program staff believed the effect on university instruction would be "catastrophic" and U.S. researchers would have little chance of making major physics discoveries.

WORLD LEADERSHIP OR COMPETITION IN SELECTED FRONTIERS

Another option is for the United States to concentrate on one or two frontiers. This approach would sacrifice some of the benefits of diversity in favor of maintaining a good chance of making major discoveries. Under this approach, program managers would have to make some difficult choices.

Possible strategies and cost

Under this option, the Nation would choose from the range of possibilities for each laboratory: a world leadership mode, a competitive mode, or a derivative mode. We asked DOE program officials to provid‹ us the most probable strategy under a selected approach. They did not believe all the initiatives proposed by the laboratories should be pursued, but said the appropriate strategy requires extensive scientific and technical judgment. Subsequent to our inquiry, they requested the High Energy Physics Advisory Panel to study the facility needs and the funding level needed to maintain a viable U.S. program. In August 1979, the Advisory Panel established a subcommittee to study the funding level needed in light of increasing operating costs, increasing competition from Western Europe, and the need to provide flexibility to fund new construction initiatives. The subcommittee reported on the results of its study in April 1980 (see app. V).

The subcommittee reported that a preeminent, competitive program would require a funding level about 20 percent higher, after inflation, over the level set forth in the DOE/OMB agreement. In 1982 dollars, the subcommittee recommended funding at $500 million in fiscal year 1982 and $545 million annually in each of the 4 subsequent years. The program set forth by the subcommittee included

--utilization of accelerators at about 50 percent, substantial increases in user group and new equipment support;

--completion of construction of Isabelle and the Energy Saver;

--increase of the Stanford linear accelerator's energies to 50 GeV; and

--construction of the 1-TeV x 1-TeV antiproton-proton collider, 1-TeV proton fixed-target accelerator, and a 50-GeV x 50-GeV electron-positron collider.

The subcommittee did not estimate needed budget levels for 1987 to 1990, but stated that higher energy machines such as the 5-TeV Pentevac, a 5-TeV x 5-TeV proton-proton collider, and a colliding electron-positron linear accelerator with energies of up to 1-TeV center of mass may be possible.

Although the subcommittee presented its report to the High Energy Physics Advisory Panel in May 1980, the Panel decided not to adopt the report. Instead, the report was provided to a subpanel studying what a balanced, healthy U.S. program should consist of. The subpanel's study was completed in June 1980 and its report issued in July 1980.

In the absence of a clearly stated strategy for this option, we arbitrarily selected four of the many strategies possible for illustrative purposes:

--Strategy A: Strive for world leadership at Brookhaven and Fermilab; keep Stanford competitive.

--Strategy B: Strive for world leadership at Stanford: keep Brookhaven and Fermilab competitive.

--Strategy C: Keep Stanford and Fermilab competitive: reduce Brookhaven to a derivative role.

--Strategy D: Keep Brookhaven competitive: reduce Fermilab and Stanford to derivative roles.

For each of these strategies, we assumed a derivative role for the others--Argonne, Lawrence Berkeley, and Cornell. As suggested by DOE program staff, we assumed direct funding levels for universities to be 20 percent of the total program budget. Total estimated average annual costs in 1979 dollars for each strategy are shown below.

Strategy	Average annual cost minimum	Average annual cost optimum
	(millions)	
A	$556.9	$648.8
B	504.6	608.9
C	311.7	426.7
D	250.6	280.2

Advantages and disadvantages

OMB and DOE officials told us that each U.S. laboratory does not necessarily have to be the world leader in each technological frontier for the United States to have a preeminent program. They pointed out that the diversity of approaches helps achieve a preeminent program. Thus, a principal advantage of such an approach would be that a program designed for U.S. preeminence in the field might be attained at less cost than if world leadership or competitiveness were sought at each laboratory. A factor in this option is that judgments as to which frontiers to pursue are not easy to make. DOE program officials pointed out that with a 7- to 10-year span between a construction proposal and the start of accelerator operations and the 3- to 4-year span between an experimental proposal and research results, choosing the frontier which offers the best opportunity for a physics breakthrough is extremely difficult and risky.

Compounding the difficulty of this decision are the harsh consequences for the laboratories not selected to proceed with frontier development. Such consequences could include the loss of top personnel, the abandonment of R&D efforts, a reduction in opportunities for young physicists, and eventual shutdown of high energy physics facilities. Yet decisions of this nature have been faced before, such as the recent shutdown of the accelerator at Argonne.

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A laboratory director of a medium energy physics facility proposed another way of dealing with the high costs of building world leading accelerators. He told us that rather than trying to continuously maintain a leadership position in selected frontiers, another possibility might be to pursue a "leap-frog" approach. Under such an approach, the United States would build a world leading accelerator and then exploit it for the maximum amount of physics, while consciously allowing other nations to take the lead with accelerators they may build. When the physics with the U.S. accelerator has been fully exploited, the United States would then build another world leading accelerator to regain the lead. We did not seek to develop such an approach, but it does raise interesting possibilities and shows that possible approaches other than those we have discussed merit consideration.

CHAPTER 6

CONCLUSIONS, RECOMMENDATIONS, AND MATTERS FOR CONSIDERATION BY THE CONGRESS

CONCLUSIONS

The United States is seeking to maintain a leadership position in high energy physics with a program which, in fiscal year 1980, is supported with about $350 million in Federal funds. The United States once dominated the field and still leads the world in terms of making significant physics discoveries. World leadership is considered important because it provides a nation's or group of nations' physicists greater opportunities for making significant physics discoveries. With increasing costs and competition, the U.S. physics community is concerned that the leadership is being lost to Western Europe. Whether leadership in high energy physics provides greater benefits than research in other basic science fields, however, is not clear.

The amount of Federal funding of high energy physics has been largely determined with little apparent consideration to the relative merits of other basic sciences. No formal criteria have been established for accomplishing this. Instead, it requires subjective judgments on the part of knowledgeable decisionmakers. A study is needed to determine the appropriate level of funding for the program while considering the relative merits of other sciences. Such a study would appear to be beyond the type of study that could be provided by DOE's High Energy Physics Advisory Panel, which primarily provides advice on factors internal to the field. The Panel, however, could provide input on the program's approach to various policy options or alternatives. The National Academy of Sciences has had prior experience in making similar studies and may be a good source for input on the relative merits of the basic sciences.

We believe OSTP should lead such a study. OSTP has oversight responsibility over all federally funded basic science, works with OMB in developing the budgets, and generally could provide a broader perspective than a single agency such as DOE or the Foundation. Recognizing OSTP's limited staff resources, the study could be conducted by a work group comprised of scientific or technical experts as well as individuals having a broad policy perspective and familiarity with the Government's involvement in science and technology. To supplement its staff with such expertise, OSTP may wish to hire consultants and/or have staff detailed from Federal offices or agencies, such as OMB, DOE and the Foundation. OSTP may also want to consider obtaining input from DOE's High Energy Physics Advisory Panel and/or the National Academy of Sciences. In any case, to ensure objectivity, we believe OSTP should assume full responsibility over the conduct and completion of the study.

DOE has primary responsibility for implementing a sound national program, but it has not established clearly visible detailed program plans which address how or to what extent each of the program's key elements are to be carried out under the presently agreed upon funding levels. DOE initiated a study, conducted by its Advisory Panel, which was to develop near-, mid-, and long-range plans for present and increased funding levels. We believe the development of such plans is commendable and should be instituted formally on a periodic basis and submitted to the Congress for its information and use in carrying out budgetary and oversight responsibilities.

In the absence of such plans, the physics community has emphasized construction while other key program elements such as long-range accelerator R&D, accelerator utilization, and experimental research support have suffered. New construction may provide the tools needed to maintain a leadership position, but the posture of the U.S. program still may be adversely affected. Inadequate funding of long-range accelerator R&D may preclude the U.S. development of new technologies needed for the next generation of accelerators. Low accelerator utilization and experimental research support limit or stretch out current research efforts which adversely affect the morale of physicists. This might result in a transfer of top physicists to other nations or the most brilliant graduate students to other scientific fields. Thus, present efforts may help maintain a leadership position by providing the needed accelerator capabilities, but the U.S. program may lack the technology for future accelerators and/or the top physicists needed to make the discoveries which represent leadership.

In our view, the program has been faced with trying to do more than available funds would allow. Thus, the alternatives appear to be to either change the objectives or better fund the program. Accordingly, we believe the program's objective with respect to its desired international standing and the funding and strategy for achieving that objective need to be reconsidered in light of its needs and importance relative to other basic sciences.

A number of policy alternatives and approaches are available for the U.S. high energy physics program, each of which offer advantages and disadvantages. An approach of seeking world leadership in each technological frontier, while maximizing most of the program's benefits, would be extremely costly. A competitive approach in each frontier accelerator technology would preserve the program's diversity, but would also be expensive and risk a second-best effort. A world unification approach would foster better international relations, but coordination and logistical problems would have to be solved. A derivative approach would minimize costs, but also the benefits. A selective approach offers the opportunity to support frontier physics at less cost, but would involve some difficult decisions and harsh impacts on some laboratories and universities. This latter approach appears to offer the best possibility of maintaining a preeminent U.S. program at or near current funding levels.

On the other hand, the OSTP-led study we are proposing may find that additional or fewer funds are warranted. Since we did not attempt to determine the appropriate level of funding, which would necessarily consider all basic sciences, we will not suggest a specific objective and/or strategy to be pursued. Our exploration of policy alternatives shows that a given policy and strategy should not be pursued without first considering the amount of funds needed and available.

RECOMMENDATIONS

In his capacity of having broad oversight responsibility over all federally funded basic science, we recommend the Director, OSTP, assemble a work group comprised of scientific and technical experts as well as individuals having a broad policy perspective and familiarity with the Government's involvement in science and technology--to determine the appropriate objectives and level for funding of the U.S. high energy physics program, considering factors impacting on its needs and importance relative to other basic sciences. Based on the results of such a study, we recommend the Director, OSTP, prepare a policy paper setting forth the objectives of the U.S. program, a strategy for achieving such objectives and the appropriate annual funding levels for carrying out the strategy. Such funding levels should consider projected amounts for major functions such as construction, accelerator operations, accelerator R&D, equipment, equipment R&D, and physics research. We further recommend that the Director, OSTP, consult with the appropriate oversight committees of the Congress for their views and input in helping to formulate the policy.

We recommend that the Directors, OMB and the National Science Foundation, and the Secretary of Energy fully cooperate in the study.

In carrying out his responsibility for implementing a sound national high energy physics program, we recommend that the Secretary of Energy, formally institute the development of near-, mid-, and long-range plans on a periodic basis. This would ensure that agreed upon program objectives and strategies are appropriately pursued. To provide visibility over the direction of the program, we recommend these plans be submitted to the Congress for its information and use in carrying out its budgetary and oversight responsibilities.

MATTERS FOR CONSIDERATION BY THE CONGRESS

The long-term nature of the benefits to be derived makes the determination of the appropriate Federal support for basic research programs an extremely difficult and subjective matter. To help ensure that the U.S. high energy physics program is appropriately funded, its needs and importance relative to research in other basic sciences should be considered.

Also, funding levels must be adequate for the strategy pursued to achieve the program's overall policy objectives. This has not been the case so far in the U.S. high energy physics program, where funding levels have been stretched too thinly and may be jeopardizing the vitality of the U.S. program. Accordingly, as noted above, we are recommending that OSTP study the situation and develop the most suitable policy objectives, strategies, and funding levels for the U.S. program. We are also recommending that OSTP consult with the appropriate congressional oversight committees for views and input. We believe the Congress, through its position of having oversight and budgetary responsibilities over all Federal basic science programs, could provide valuable input into the final determination of what the overall objectives of the U.S. high energy physics program should be, as well as the appropriate strategies and necessary funding levels. The committees can provide such input as part of normal oversight activities or during specific hearings on the U.S. program.

Once program objectives and strategies are established or reaffirmed, and plans are clearly visible, the Congress should be in a better position to ensure that integrity is maintained between program objectives, plans, and budgets.

CHAPTER 7

AGENCIES' AND LABORATORIES' COMMENTS AND OUR EVALUATION

Copies of a draft of this report were provided to the Department of Energy, the National Science Foundation, the Office of Science and Technology Policy, the Office of Management and Budget, and six laboratories involved in high energy physics for their review and comment. Generally, they were concerned with some of our interpretations of the facts presented in the draft report. They pointed out a need to better recognize the scientific merit of the efforts pursued and merits of program flexibility. The agencies and laboratories generally disagreed with the conclusions and recommendations of the draft report. The full text of the comments are included in appendixes VI-XV. Several of our expert consultants made similar comments in commenting on the draft report.

The following principal concerns or points were expressed:

--Priorities and objectives exist for the program and whether an OSTP-led study would be helpful or needed is questionable.

--Program plans and strategies exist which provide some diversity and a balanced program.

--The priority for the Cornell Electron Storage Ring increased because the project was slightly scoped down and the existing accelerator was shut down earlier than previously planned.

--Proposed actions to improve accountability would limit flexibility which is needed for future successes.

--Additional funds needed are not as great as the draft report implied.

--The policy options presented in the draft do not reflect present program goals and are not realistic.

--The output of the U.S. program is not measured.

Some additional concerns were expressed on specific statements or facts presented in the draft.

We carefully considered the comments and, where appropriate, made changes which are reflected throughout this report. Although substantial changes were made to portions of the draft report, including substitution of recommendations intended to help ensure integrity between funding and the efforts pursued, this report's basic thrust remains the same. The principal changes made are discussed below along with the concerns and points expressed by the agencies and laboratories.

PRIORITIES AND OBJECTIVES IN THE CONTEXT OF ALL BASIC SCIENCES

The agencies and laboratories generally agreed that an appropriate balance among basic sciences should be considered in establishing national priorities and goals for the program. However, some noted that this was already being done, and those commenting generally questioned whether an OSTP-led study of this issue would be helpful or needed. Although we recognize that such a study would necessarily involve largely subjective determinations, we believe the national priorities could be better established among the basic sciences.

Several noted that priorities and goals are already being established in the context of all sciences. The Foundation noted that few analytical tools exist, but stated that it has and will continue to examine the complex problem of a balance of support among scientific fields through its planning and budgeting activities. OSTP, Argonne, Lawrence Berkeley, and Stanford noted that DOE's High Energy Physics Advisory Panel already provides advice on the overall program. OSTP also stated that DOE and OMB already considered the appropriate balance with other fields in establishing the DOE/OMB funding agreement.

Although the Foundation considers its support for high energy physics in the context of its support for all basic sciences, its support represents only about 8 percent of the Federal support of high energy physics. The balance, or 92 percent of the Federal support, is provided by DOE.

During our review, we found little evidence that DOE and OMB considered the balance of Federal support among basic sciences in agreeing upon DOE's funding support of high energy physics at the $300 million level. The only indication we found was that a stabilized funding level would eliminate a "bow wave" in funding which may adversely affect the funding of other basic sciences. However, the level of support appeared to be arrived at based on factors internal to the field. In its formal comments, DOE referred to the funding agreement as the policy and plan currently being followed. It noted that this was based on:

"(1) a series of studies at Woods Hole in 1974, 1975 and 1977 by a subpanel of the High Energy"

"Physics Advisory Panel (HEPAP), (2) numerous considerations by the HEPAP and (3) a study report, 'Long Term Strategy for Construction and Operation of High Energy Facilities' issued by the Energy Research and Development Administration."

Each of those factors are largely input by the Advisory Panel. The first two factors are direct input by the Advisory Panel. The ERDA study, cited as the third factor, was largely based on input by the Advisory Panel. The ERDA report stated in part:

> "The scientific justification and technical feasibility of several specific construction projects have been studied within a general long range program context by two Subpanels on New Facilities of the High Energy Physics Advisory Panel (HEPAP), which met in the springs of 1974 and 1975 respectively. The long range plans and priorities of this report have drawn heavily from the deliberations of these two Subpanels and from an extensive history of study and discussions with HEPAP."

Hence, the agreement was largely based upon input by the High Energy Physics Advisory Panel and its subpanels.

DOE's High Energy Physics Advisory Panel focuses its review and advice on matters internal to the field. The Advisory Panel has two members from other science fields, but its meetings and studies focus on matters directly concerned with high energy physics. While the Panel has provided advice on various levels of funding in the past, such advice has been given without considering the balance of Federal support among the basic sciences. Hence, we continue to believe that the appropriate balance of Federal support among the basic sciences needs to be considered in establishing national priorities and goals for the program.

Although we do not believe DOE's High Energy Physics Advisory Panel is the appropriate entity to lead the study, it may be a good source of input to the study. The Panel's intrinsic bias toward high energy physics, in our opinion, would not be conducive to determining the appropriate level of Federal funding. The Panel, however, could provide input on the advantages and disadvantages of various alternative policy goals and cost estimates for carrying out those goals. The Panel could also advise on the priorities within the field of achieving various goals. Accordingly, we revised our discussion of this issue to reflect a possible role by DOE's Advisory Panel in the recommended study.

Some agencies and laboratories pointed out the difficulty of making a study of the appropriate balance among basic sciences. As previously noted, the Foundation pointed out the complexity of the problem. DOE stated that no basis from which to proceed exists for designating priorities among the federally funded basic research efforts. Similarly, Argonne pointed out that it is most unlikely that the proposed work group could do more than define various policy options. It further noted that comparisons between different areas of science are almost impossible to carry out in an objective way, and such studies are not likely to lead to results that are operationally useful. Lawrence Berkeley stated that such studies are no more than educated guesses. Stanford stated that such a study would accomplish little and would probably lead to other requests for similar studies for funding of other basic science fields competing for Federal funds.

While such a study will be extremely difficult and involve an extensive amount of subjective judgment, the difficulty or subjectivity of a study should not preclude it from being carried out. If limited amounts of Federal funds are to be available for basic research, as has been the case in recent years, the relative priorities of the various efforts to be pursued should be established. In this regard OMB pointed out in its formal comments that significant funding increases should not be recommended without addressing corollary impacts on support of other fields of science.

Information which would delineate such priorities or impacts is not presently available to the Congress. In the absence of formal criteria for establishing such priorities on a purely objective basis, we still believe such priorities should be established. If the proposed study proves to be useful to the Congress in establishing appropriate levels of funding for high energy physics, perhaps similar studies, as mentioned by Stanford, would also be useful in establishing funding levels for other basic science fields.

EXISTING PLANS AND STRATEGIES PROVIDE A BALANCED PROGRAM

In the draft we presented for comment, we concluded that the program appeared to overemphasize accelerator development and construction activities to the detriment of other key program elements. The agencies and laboratories generally stated that our proposed draft did not give sufficient consideration to existing program plans and strategies which provide some diversity and adequate program balance in light of constrained funding. After considering their comments and some additional information they provided, we deleted our discussions indicating that an overemphasis has been placed on accelerator development and construction activities. We continue to point out, however, that although deemed appropriate, emphasis has been placed on such activities and other key program elements have been inadequately funded.

The agencies indicated that input in the form of studies and advice by DOE's High Energy Physics Advisory Panel has been the basis of the program's plan and strategy. DOE referred to the DOE/OMB funding agreement as the plan being pursued. The Foundation stated that it is largely dependent on DOE for long-range planning, but pointed out that its staff participates in DOE's High Energy Physics Advisory Panel's planning activities. OSTP noted that the advisory panel mechanism and the DOE/OMB agreement are seen as models that other disciplines, such as nuclear physics, are seeking to emulate.

This report was revised to state that the DOE/OMB agreement is considered to be the existing plan. We had previously indicated that the 1977 subpanel of the High Energy Physics Advisory Panel's report on new facilities represented the plan being generally followed, though on a stretched construction schedule. We now point out that the current plan essentially establishes the funding level at $300 million a year in 1979 dollars. This level is about $50 million less than the level recommended by the High Energy Physics Advisory Panel. However, the agreement does not set forth specific projects, other than Isabelle, to be initiated, nor does it set forth goals for other key program elements such as accelerator utilization or physics research. Although the physics community agreed to the level, Stanford pointed out that the proper term may be "acquiesced to." We revised the wording from "concurred with" to "agreed to."

Five laboratories indicated that the program is balanced, considering the funding constraints. They pointed out that the new machines provide needed diversity and that the current emphasis on the development and construction of accelerators is appropriate. For example, in its formal comments, Stanford stated, in part, that:

> "The Draft Report correctly identifies numerous institutions where development and construction of new facilities has been carried out to the detriment of the ongoing operations of the research program using existing facilities. Yet had such new construction not been initiated, the program would have faced certain obsolescence and consequent loss of leadership and productivity. It can easily be shown, based on the analysis of the time cycles involved, that a long range viable program which is a precondition to world"

"leadership demands that about 25% of total funds be allocated to new construction. The failure of the program to initiate any new construction for nine years after authorization of Fermilab is partially responsible for the current world leadership crisis to which the Draft Report is addressed."

Stanford and Fermilab also indicated that an alternative methodology, not used in this study, would have to be used to measure output before one could conclude that an overemphasis has occurred.

The current emphasis on accelerator development and construction has been deemed appropriate by the consensus of the physics community, as represented by the High Energy Physics Advisory Panel. Since this is largely a technical judgment, we deleted our conclusion that an overemphasis has occurred. We continue to point out, however, that the emphasis has been on construction, and other key program elements have been inadequately funded.

PRIORITY OF THE CORNELL ELECTRON STORAGE RING

The Foundation and Cornell objected to our questioning in the draft report of the priority for constructing the Cornell Electron Storage Ring. While this project had been given relatively low priority by DOE's High Energy Physics Advisory Panel, they stated that the project had been rescoped and the existing accelerator shut down earlier to reduce costs sufficiently to justify its construction. They also indicated that the success of the project futher justifies the support provided. Argonne similarly commented that the success of the project justifies the support, and Fermilab called the project an "inspired investment."

We revised our discussion of the project's relative priority to reflect the change in the project's scope, place greater emphasis on its scientific merit, and acknowledge that flexibility has merit. We no longer present our view on whether the project had sufficient priority for funding because such a determination is largely based on subjective scientific and technical judgments.

ACCOUNTABILITY AND FLEXIBILITY

The draft report we provided for review and comment included a proposed chapter on the need to improve accountability. The agencies and laboratories strongly objected to our interpretation of the facts presented in the chapter.

They generally pointed out the successes of the various efforts undertaken, that program managers are aware of the various efforts, and that proposed actions to improve accountability would greatly restrict the flexibility needed to respond to innovative ideas and changes in the desired physics. We received similar comments from several of our expert consultants.

Our proposed chapter had focused primarily on fiscal accountability and had given little consideration to the scientific merit of the efforts pursued. We therefore reassessed the impact of our proposed actions to improve fiscal accountability on the program. The physics community generally asserts that the various construction projects cited in the proposed chapter (those cited in ch. 4 of this report) are examples of scientifically meritorious efforts. One former laboratory director told us that such projects justified "creative accounting." We obtained additional information, which generally supports the views that the projects cited are deemed to be scientifically meritorious. We concluded that our proposed "cure" for improved fiscal accountability may unduly restrict the physics community from pursuing similarly meritorious projects in the future. Hence, we deleted the proposed recommendations intended to "improve" fiscal accountability.

Nonetheless, we do not agree to the notion that "creative accounting" is justifiable because of the scientific merit of the efforts pursued. Hence, we have revised this report to better reflect the scientific merit of the efforts pursued and suggested the implementation of a three-tier planning and budgeting system. Such a system should help minimize the need for "creative accounting" by providing the Congress better visibility over planned activities, while providing the program flexibility to adjust plans to respond to innovative initiatives or changing physics objectives.

Since we made a substantial change in the thrust of our discussion, conclusions, and recommendations pertaining to fiscal accountability, most of the specific comments on this subject no longer apply to matters included in this report. Thus, we will not address in detail each comment on the subject. On the other hand, the extensive comments made with respect to the fiscal accountability over the Cornell project seem to warrant some discussion.

While we made only a limited examination of the fiscal controls exercised by the Foundation over the Cornell project, we had concluded that the contracting officer's fiscal controls appeared to be inadequate to ensure that problems encountered would be brought to management's attention in a timely manner. While we made this conclusion, we also pointed out that we did not note that any technical problems had been encountered on the Cornell construction project. Nevertheless, we expressed our concern that problems could arise in the future without appropriate management attention. To illustrate the nature of the potential problem, we cited previously reported problems of delays and cost increases with the construction of the Nevis Synchrocyclotron, 1/ a medium-energy accelerator.

Subsequently, Foundation officials provided us information showing that additional technical monitoring was instituted by program staff after problems were encountered with the Nevis project. Such controls included periodic technical progress reports, site visits, and annual reviews by Foundation-assembled teams of experts which evaluated the technical progress. During our review, we noted those technical controls were in place. The Foundation also pointed out that the Cornell project has now been essentially completed within the schedule and estimated cost. Hence, in the absence of any apparent adverse effect, we do not discuss the fiscal accountability over the Cornell project in this report.

ADEQUACY OF FUNDING

DOE, the Foundation, OSTP, and some laboratories commented that funding has not kept up with the needs of the field. DOE states, and OSTP implied, that the amounts needed were not as great as implied by the draft report. OMB similarly interpreted our draft report to call for increased funding and stated it would be extremely difficult to agree to a significant increase under the severe budgetary constraints presently being experienced. Fermilab commented that we summed up the responses of all laboratories and arrived at an unreasonable cost. Stanford attributed most of the program's problems to be a result of a shortfall in funding.

Those stating that the draft report implied an unreasonably large amount of funds would be needed, apparently assumed we were advocating that a policy of world leadership or competitiveness in each accelerator technology be pursued. This was not, nor is, the case. This impression was probably derived from the absence of a "preeminence" option in the draft report. We did not prepare such an option because considerable scientific and technical judgments are involved in developing the probable strategy and costs of such an option. Instead,

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1/"Modernization of Nevis Synchrocyclotron Facility" (PSAD-78-103, May 23, 1978).

we arbitrarily presented four mixed options indicating possible levels of efforts at the various laboratories.

Subsequent to receiving the comments, in April 1980 a subcommittee of DOE's High Energy Physics Advisory Panel reported on the funding support required to ensure a preeminent position. This report refers to the subcommittee report in chapter 5 and includes the entire report in appendix V.

We basically agree with Stanford's observation that most of the problems encountered by the field can be attributed to a shortfall in funds. While this shortfall could have been ameliorated by providing more funds, we continue to believe that the problems could also have been resolved by making difficult decisions to undertake fewer but better-funded efforts.

POLICY OPTIONS

Related to the concern over implied funding levels, the agencies and laboratories stated that the options do not reflect present program goals, lack scientific judgment, and place undue emphasis on competition, and that dominance was not a useful goal. As with the concern over implied funding levels, we added the discussion of DOE's High Energy Physics Advisory Committee's subcommittee report on a "preeminent" program. We also rephrased the dominant option in the draft to being the world leader at each laboratory.

The options we present in this report do not reflect present program goals. This was not our intent. We are presenting the options to illustrate the potential impact redefining the policy goals could have on the program strategy and costs.

In regard to comments that the options lack scientific judgment, the basic input was provided by laboratory and agency program officials using their best judgment. Thus, we believe scientific judgment was used with respect to the input data. As some agencies and laboratories pointed out and as indicated in our presentation, we basically summed up the input for the various options, except for the mixed options which arbitrarily present selected input from various laboratories. Under the assumptions made for the various options, we believe they generally reflect possible but not necessarily desirable courses of action. As we previously suggested, the most prudent course of action should be determined by a work group of experts, possibly using input from the physics community.

With respect to the concern that we placed undue emphasis on competition, perhaps this reflects a feeling that the program should no longer compete on each technological frontier and, instead, work toward an international mode. Competition has been a major factor in setting the pace of the program in recent years. If competition is not considered important, the desired physics might be attainable, from a worldwide perspective, through regional emphasis on specific, but different, accelerator technologies.

ALTERNATIVE METHODOLOGY

Stanford and Fermilab criticized the methodology we used in this study. Stanford said that we did not consider how the output of the U.S. program, as measured in terms of science produced, should be gauged, let alone optimized. Fermilab more specifically stated that the fundamental issue should be whether high energy physics, funded at a level of about $400 million in fiscal year 1980, will provide a return to society which justifies the effort. Fermilab also suggested addressing some questions designed to provide cost-benefit data on which to base judgments as to the health of the field.

We believe that much of Stanford's and Fermilab's concern over the methodology has been mitigated by revisions incorporated into this report. However, we discuss below our view of the merit of pursuing Fermilab's proposed questions. The questions were:

"1.) Given the inventory of accelerators in the U.S., using various indicators of effort e.g. European levels, historical records, economic utility of past discoveries and technological spin-off, population of practitioners, etc., what is the payoff versus cost of increasing utilization from, say 50% to 80%? How does this compare to capital investment?

"2.) What is the validity of the claim that HEP [high energy physics], in addition to its thrust to account for the basic structure of matter and energy, is an economic benefit to the U.S. due to its technological spin-offs alone? What is the ratio of technological breakthroughs originating in unmotivated research versus programmatic R&D? If it could be demonstrated (and who could do it better than the GAO staff?) that HEP contributes to the economy more than it absorbs, the entire thrust of the GAO report would be changed. Yet this was ignored."

"3.) What are the other benefits to the U.S. of a strong HEP program: i) In strengthening the scientific potential for solving shorter range problems ii) In prestige relative to other developed and developing countries iii) In influencing high technology industry with implications for balance of payments, better life for our citizens, etc.?

"4.) What are the bureaucratic obstacles to efficient management of the HEP program? What would be the cost effectiveness of a large reduction in numbers of reports required by DOE, in restrictions on operating versus R&D versus construction fund types? How can one reduce the time lag of the system to respond to new ideas? In short, how can one better carry out the basic will of Congress to do excellent science for minimal cost by simplifying the procedures?"

After receiving Fermilab's comments, we discussed the questions with the Director, Fermilab. He told us he or his laboratory had not explored the questions, but he proposed methodologies for addressing them.

The Director's proposed methodologies for the first three questions seek to quantify various aspects of program input and output. The overall result would be data indicating whether high energy physics provides more benefits than it costs. Although such data would have some utility, the broader question would still remain: "Do the relative cost-benefits of high energy physics warrant the current, larger, or smaller, level of funding when compared to other sciences?"

During the scoping phase of our review, we briefly considered conducting a study such as that outlined by the Director's proposed methodologies. However, several agency officials, as well as the experts advising us on our review, told us such a study would require that a number of subjective judgments and assumptions be made to fully quantify the benefits of high energy physics. Since varying opinions exist as to the value of the knowledge gained from the research, we were told that the results of such a study would not be credible. Furthermore, as previously indicated, we would still not know whether greater benefits might have been obtained by funding other scientific efforts at higher levels.

With respect to the fourth question, which seeks to determine the cost-benefits of management control procedures, we agree in principle that unneeded management controls should be eliminated. However, we do not believe a comprehensive cost-benefits study of the various controls is needed to quantify their usefulness. Instead, unneeded controls should simply be identified and eliminated. Our study did not seek to identify unneeded controls.

OTHER COMMENTS

A number of other comments were made with respect to specific statements or facts presented in the draft report. Some of the more notable concerns and our analysis and/or disposition of them are discussed below.

OSTP noted that we had not conducted work at its office. This is true because the responsible OSTP official (1) told us his office had little involvement in high energy physics, (2) discussed OSTP's involvement and views with us via the telephone, and (3) mailed us documents which we reviewed. Accordingly, we have revised our scope section to more accurately reflect the work we performed.

OSTP stated that attention to peak funding is inappropriate because it is largely a result of construction. While the peak funding occurred during the construction of the Fermilab facility, we believe that the attention we draw to it is appropriate under the context presented. We have drawn attention to it because Federal basic research funding is at or near its peak level while high energy physics constant dollar funding has leveled off below its peak.

OSTP also stated that mission-oriented basic research funding has increased, but not at the expense of fundamental science with intrinsic merit. We recognize that the President's fiscal year 1981 budget request placed greater emphasis on supporting basic research.

DOE, Brookhaven, Fermilab, and Lawrence Berkeley commented that although laboratory in-house research staff provides some support to users, physics research funds are primarily for in-house research and not for support to university researchers. We continue to point out the need for increased research support, but deleted the statistics from this report.

Argonne stated that developing additional physics capabilities was the primary goal of pursuing its accelerator R&D efforts while the impending shutdown of its accelerator was being considered. The laboratory also pointed out that the results of those efforts were or are successfully being used at Argonne or elsewhere. We apparently had misinterpreted Argonne officials' previous statements that averting the shutdown was a major reason for the continued R&D. Accordingly, we have revised our discussion to better reflect the physics goals of the efforts and the ultimate use.

Stanford pointed out some additional facts with respect to its construction projects. The laboratory pointed out that (1) a substantial part of the Positron Electron Project's costs were projected at the time of authorization and (2) the upgrade of the linear accelerator to 30 GeV was primarily for research, not for injection to the Positron Electron Project. We revised the report to reflect these points.

US MAJOR ACCOMPLISHMENTS IN HIGH ENERGY PHYSICS SINCE 1945

June 1979

Prepared by:

Division of High Energy Physics
US Department of Energy
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APPENDIX I

US Major Accomplishments in High Energy Physics Since 1945

1945 Synchrocyclotron principle (phase stability) suggested, removing the last technical barrier to unlimited particle energies attainable by orbital or linear accelerators.

1946 184" synchrocyclotron began operation with 350 MeV protons at what is now the Lawrence Berkeley Laboratory. This machine, which by 1957 had reached 720 MeV, was used to study in detail the pions which were discovered in cosmic rays in 1947.

1947 Muon identified as lepton, i.e., as a particle which does not experience the strong nuclear force. First observed in the late 1930's, the muon had been thought to be the meson carrier of the strong force postulated by Yukawa in 1935. The finding that it did not interact strongly led to a successful search for a heavier particle, the pion, which decayed into the muon. The pion was found to interact strongly and hence identified as the Yukawa meson.

1948 a Universality of weak interactions suggested, i.e., weak interaction recognized as a fundamental force. This force is responsible for the decays of radioactive nuclei of muons and many other particles and for neutrino interactions with matter, which have been extensively studied in the 1970's and are important in astrophysical processes.

1948 b Quantum electrodynamics renormalized. Stimulated by measurement of the Lamb shift, a very small splitting of two quantum states of the hydrogen atom which had been thought to have identical energies, new calculations showed how to resolve the long-standing divergence problems of QED and established it as the correct relativistic theory of the electromagnetic force. This theory has withstood 30 years of extremely rigorous experimental testing, and is still our most precise field theory, serving also as a model for other gauge theories presently being developed for the strong interaction. J. Schwinger shared the 1965 Nobel Prize for his contributions to QED.

1949 a Berkeley Synchrotron began operation with electrons of 330 MeV. This accelerator, which utilized the principle of phase stability, produced in its first year the discovery of the neutral pion and played an important role in determining the properties of pions.

1949 b Neutral pion discovered at the Berkeley Synchrotron, completing the triplet of charge states, $\pi+$, π^-, and π°, which mediate the strong force binding neutrons and protons in atomic nuclei.

1949 c Feynman diagram approach to field theory invented, and applied to quantum electrodynamics. This graphical way of describing interactions has been a powerful language for both quantitative and qualitative calculations in all areas of particle physics and in other fields of physics as well. Feynman shared the 1965 Nobel Prize for his work on quantum electrodynamics.

1950 High Power Klystron first used in the Stanford Mark II linear accelerator. These power amplifiers are the heart of modern electron accelerators, supplying radiofrequency power to the cavities which accelerate the electrons.

1951 Resonant beam extraction developed for accelerators. This technique of extracting the particle beam from the machine in a slow "spill" is crucial for many electronic experiments.

1952 a Associated production hypothesis proposed: that the Λ and K particles, which had been discovered in cosmic ray interactions, are produced only in association with each other. The hypothesis was experimentally confirmed and led to the discovery of a new property of certain particles called "strangeness" (see 1953 e).

1952 b Δ discovered at the Chicago Cyclotron. The delta was the first detected particle "resonance", or very short-lived particle formed as a resonant combination of two or more other particles, in this case the pion and proton. Many other resonances have since been discovered, leading physicists to seek a more fundamental set of building blocks of matter (see for example 1961 a and 1964 b).

1952 c Strong focusing (alternating gradient) principle discovered. This method of using magnetic field gradients alternating in direction to focus a beam of charged particles had two important consequences: it greatly reduced the cost of accelerators by allowing much smaller magnet apertures (since the beam could be focused to a small diameter), and it provided a means of transporting extracted or secondary beams (produced by interactions of the primary beam with a target) to experimental apparatus located far from the accelerator.

1952 d Cosmotron began operation at Brookhaven National Laboratory with 2.3 GeV protons, later raised to 3 GeV. The first multi-GeV accelerator, it had sufficient energy to produce the strange particles which had been observed in cosmic rays. The associated production hypothesis was confirmed, several additional strange particles were discovered and their properties studied.

1953 a CPT Theorem proved - any local field theory is invariant under the combined operation of particle-antiparticle exchange (C), mirror reflection (P), and time reversal (T), if the theory of relativity is correct. This very general invariance principle has important consequences, e.g., that a particle and its antiparticle have equal masses and lifetimes.

1953 b Bubble chamber invented at the University of Michigan. This device records the passage of a charged particle as a trail of small bubbles in a medium such as liquid hydrogen. Photographs of the tracks allow detailed studies of the interaction of an incident particle in the liquid and any subsequent decay processes. The bubble chamber became the dominant experimental high energy physics technique for many years and is still a powerful tool for exploratory research and very detailed studies of complex processes.

1953 c Σ^+ discovered in a cosmic ray experiment: the first member of a new family of "hyperons" similar to the lambda hyperon, a strange baryon. The other two members of the sigma family were subsequently discovered at the Cosmotron.

1953 d Experimental confirmation of associated production of strange particles, at the Cosmotron (see 1952 a).

1953 e Strangeness quantum number proposed to explain associated production. Hyperons and kaons have a charge-like property, called "strangeness" for the "strange" behavior of strong production and weak decay. Strangeness is conserved in strong interactions such as those which produce lambdas and kaons. However, these particles are not massive enough to decay to other strange particles, so their decays must proceed through the weak interaction which does not conserve strangeness. (Heavier strange particles have since been discovered which do decay via the strong interaction.)

1954 a	Bevatron began operation at Berkeley with 4.9 GeV protons, later increased to 6.2 GeV. The antiproton and many other particles and antiparticles were discovered with this accelerator.
1954 b	Ξ^- (Ksi) discovered: the first of a pair of hyperons with two units of strangeness.
1954 c	Yang-Mills theories - a method of constructing field theories invariant under a local gauge group was invented. This class of "gauge theories" has been important in theoretical developments such as recent advances in developing a theory of strong interactions (see 1973 b).
1954 d	Derivation of dispersion relations from quantum field theory. These relations provided support for field theory, independent of perturbation theory (the usual method of field theory calculations based on successive approximations), and formed a basis for a detailed understanding of pion physics.
1954 e	Cornell BeV Synchrotron began operation. This 1.3 GeV electron accelerator demonstrated the practical application of the strong focusing principle.
1955 a	Discovery of the antiproton at the Bevatron, confirming the 1928 prediction of relativistic quantum mechanics that for every basic particle there must be an antiparticle, i.e., a particle with all charge-like quantum numbers (such as electric charge, strangeness, baryon number) reversed in sign. E. Segre and O. Chamberlain received the 1959 Nobel Prize for the discovery.
1955 b	K_L^0 predicted. The neutral K meson was predicted to decay through two states, K_S^0 and K_L^0, with different decay modes and lifetimes. Each would be a quantum mechanical mixture of particle and antiparticle but a definite state of CP (see 1953 a).
1955 c	First indications of structure within the proton, now understood to be composed of quarks. Studies of large angle scattering of electrons by protons showed that the proton had a finite size and hence must have structure. Hofstadter shared the 1961 Nobel Prize for this and related work.
1956 a	Neutrino observed, confirming the hypothesis of its existence made by Pauli in 1933. The existence of an uncharged, massless particle having only weak interactions seemed essential to an understanding of nuclear beta decay, yet these properties made its experimental observation a very difficult matter until recent high energy accelerators were built.

1956 b Parity violation predicted to occur in weak interactions. One of the most challenging problems of the 1950's was the so-called τ-θ puzzle; there seemed to be two particles which were identical in every respect except that they decayed into states of opposite parity. A radical alternative was suggested, that they were in fact the same particle (the K^+) but parity was not conserved in the weak interaction which governed its decay. It had long been assumed that parity, the mirror reflection operation, was conserved in all physical processes. Parity violation was quickly confirmed in a nuclear physics experiment, and T. D. Lee and C. N. Yang won the 1957 Nobel Prize for their bold prediction.

1956 c K_L^0 (long-lived neutral kaon) discovered at the Cosmotron, confirming the prediction of 1955 that there be a long and a short-lived state. The short-lived state K_S^0 had long been known, from the earliest cosmic ray "vee" particles. These mixtures of particle and antiparticle are unique to the neutral kaons.

1958 a V-A theory of weak interactions proposed, explicitly incorporating parity violation. This theory, extended in 1963 to include strangeness-changing processes, has successfully explained a wide variety of weak interaction processes.

1958 b Neutrino helicity determined. The massless neutrino always has its spin directed opposite to its momentum; it is said to be "left-handed" or to have negative "helicity". (The anti-neutrino has positive helicity). The observation of the neutrino's helicity was an important confirmation of the V-A theory.

1959 72-inch bubble chamber began operation at the Bevatron with a liquid hydrogen fill. Luis Alvarez received the 1968 Nobel Prize for particle discoveries using this device and the extensive system developed to analyze the photographic data.

1960 a Brookhaven Alternating Gradient Synchrotron (AGS), a strong focusing accelerator, began operation with protons of more than 30 GeV, a factor of five higher than previous machines. Among the most important discoveries made at the AGS are the ν_μ, the Ω^- and the Ψ/J. (1962 a, 1964 a, 1974 a).

1960 b Y* (1385) discovered at the Bevatron, the first of a family of pion-hyperon resonances.

1960 c Spontaneous symmetry breaking proposed as the determining mechanism by which the strong interactions manifest themselves in nature. This work has had an influence on current thinking about quark interactions and on the efforts to unify all interactions within a single theory.

1961 a — SU(3) theory proposed independently by scientists in the US and in England. Known mesons and baryons were grouped into families, in a manner analogous to the periodic table of the elements. SU(3) led directly to the idea of quarks (1964 b). M. Gell-Mann received the 1969 Nobel Prize for this and other work.

1961 b — η (eta) meson discovered at the Bevatron, completing one of the SU(3) families (that of the spin 0, odd parity mesons) and thus giving important support to the theory.

1961 c — ρ (rho) meson discovered at the Cosmotron, the first pion resonance, and the first member of a new SU(3) family, the spin 1 nonet.

1961 d — ω (omega) meson discovered at the Bevatron, a pion resonance which occurs as a quantum mechanical mixture with the ϕ meson (1962 c).

1961 e — K* (890) discovered at the Bevatron, the first strange meson resonance, also a member of the spin 1 nonet.

1961 f — Matched long straight sections proposed for circular accelerators. A way was shown to insert straight sections without disturbing the operation of the machine, thus greatly enhancing its utility. Straight sections allow efficient systems for injection and extraction of the beam at high energy and are very important for colliding beam machines.

1962 a — ν_μ discovered at the AGS: a new type of neutrino, associated with the muon, The experiment showed that neutrinos produced in association with muons are distinct from those produced with electrons.

1962 b — On-line digital computer applied to data handling for a large array of scintillation counters, allowing much higher complexity of experimental apparatus and a higher rate of data acquisition. This technique and its further developments led to the present generation of very powerful electronic experimental facilities.

1962 c — ϕ (phi) meson discovered at the AGS, the first kaon resonance and a member of the spin 1 family, this meson mixes with the ω. The ϕ is now understood as an $s\bar{s}$ combination of quarks analogous to the $c\bar{c}$ quark combination which forms the J/Ψ (see 1964 b and 1974 a).

1962 d — Application of Regge theory to high energy physics: a sophisticated way of understanding scattering which has proved very powerful in explaining particle interactions at high energies.

1962 e — Proposal of current algebra, a set of relations which set the scale for weak interactions of hadrons, clarified the weak interactions of hadrons with leptons and provided support for the quark hypothesis. Current algebra is also playing a role in estimating quark masses and other modern topics.

1963 a — Argonne Zero Gradient Synchrotron (ZGS) began operation with protons of 12.7 GeV. Accurate and systematic studies using advanced experimental techniques such as superconducting analyzing magnets and polarized targets, have been the fortè of the ZGS program. Experiments in the 60's demonstrated the successes and limitations of Regge theory and confirmed duality (1968 a). More recently, the unique ZGS capability to accelerate polarized protons and deuterons (1973 a) has been exploited to study the spin dependence of proton and neutron interactions with complete spin information.

1963 b — Polarized proton target developed, allowing investigations of the spin dependence of the strong force. This type of target (with proton spins aligned) is widely used and is especially valuable at the ZGS, where experiments can now be done with both beam and target polarized (see 1973 a).

1964 a — Ω^- (omega) discovered at the AGS: a new hyperon with 3 units of strangeness which had been predicted by SU(3). The discovery of this particle completed the baryon decuplet and was a crucial confirmation of the theory.

1964 b — Quark hypothesis proposed: that the known hadrons (mesons and baryons) are composed of constituents called "quarks". At this stage, there were to be only 3 quarks, denoted by u, d and s. Mesons are made of a quark and an antiquark combination; baryons, of 3 quarks. During the 1970's, the quark hypothesis has been dramatically confirmed and extended to include two new kinds of quarks, the c and the b, and a sixth, the t, is predicted.

1964 c — CP violating $K_L^0 \rightarrow \pi^+\pi^-$ decays observed at the AGS, implying (by the CPT theorem) a violation of time reversal invariance and suggesting the possible existence of a new fundamental force.

1964 d — Charm hypothesis proposed: that there exists a fourth quark (c) with a new property called charm, a charge-like quantum number analogous to strangeness (such quantum numbers are now referred to by the generic term "flavors"). The basis for the charm hypothesis at this time was somewhat speculative; a stronger argument was offered in 1970. The existence of charmed quarks was demonstrated by the very exciting discoveries of 1974-76.

1965 a Color hypothesis proposed: that each "flavor" of quark (e.g., u, d, s, c), comes in three types or "colors". This distinction offered a way to place three identical fermions in an antisymmetric state inside a baryon in agreement with the Pauli exclusion principle which forbids such states. The color property is not interpreted as having a greater significance; the source of the strong force which binds the quarks into hadrons.

1965 b First application of a superconducting magnet to bubble chambers, using the 10" helium-filled chamber at Argonne National Laboratory. The use of the superconductors in magnets is of great importance in modern high energy physics facilities including accelerators and polarized targets as well as bubble chambers.

1966 Stanford Linear Accelerator (SLAC) began operation with electrons of 20 GeV. Discoveries at SLAC include ep scaling, the tau lepton, parity violation in neutral currents and, via SPEAR (1972 b) much of the new physics of charmed quarks.

1967 Weinberg-Salam theory proposed by scientists from the US and abroad with the aim of unifying wht weak and electromagnetic interactions. The theory has since been strongly supported by theoretical and experimental developments. It may be comparable in importance to Maxwell's unification of electricity and magnetism in 1865, and points the way to a possible unification of all the forces.

1968 a Duality: resonance formation and particle exchange models of hadron scattering were shown to be equivalent. This relationship has provided important insight into scattering processes.

1968 b ep scaling observed at SLAC: large angle electron-proton scattering behaves like that expected if the proton contained free (non-interacting) pointlike constituents.

1969 Parton model proposed to explain ep scaling as due to constituents of the proton called "partons". Partons are now interpreted as being quarks and "gluons" (carriers of the strong force between quarks).

1970 12-foot bubble chamber began operation at the ZGS, using a large superconducting magnet. This was the first of the very large hydrogen bubble chambers and was used especially to study neutrino interactions.

1972 a Fermi National Accelerator Laboratory (Fermilab) began operation with 200 GeV protons, reaching 400 GeV later in the year. This beam energy was considerably higher than that of any other accelerator. Major Fermilab discoveries include a wealth of information about neutrino and hadron interactions at very high energies, charm production, and the upsilon, made up of b quarks.

1972 b SPEAR began operation: an electron-positron colliding beam storage ring at SLAC with energies of 2.5 GeV in each beam, later raised to 4.1 GeV per beam. Charm was discovered and studied in great detail with this facility. SPEAR is the prototype of PEP at SLAC and PETRA at DESY, both higher energy e^+e^- colliding beam facilities.

1973 a ZGS accelerated polarized protons to 6 GeV (later increased to 12 GeV) a unique capability which allowed the study of the details of proton-proton scattering with both proton spins polarized.

1973 b Asymptotic freedom demonstrated as a property of Yang-Mills theories. This theoretical advance was important to the development of quantum chromodynamics (QCD), a gauge theory which treats the strong force that binds the quarks into hadrons as arising from the color "charge" of the quarks. The force decreases asympotically to zero as the momentum transferred in collisions increases without limit; thus the quarks behave as if they were free particles in very high energy collisions such as the ep scaling experiments (1968 b).

1974 a Ψ(psi)/J discovered: a new particle composed of a $c\bar{c}$ combination, i.e., a bound state of the predicted charmed quark (1964 d) and its antiquark. This dramatic discovery, which occurred independently at the AGS and at SPEAR, opened a new era in high energy physics. It put the quark model on a very solid foundation, confirmed the charm hypothesis, led to the discovery of a large family of charmed particles and encouraged a search for still other possible quarks, one of which has subsequently been found (the b; see 1977a). S. Ting and B. Richter shared the 1976 Nobel Prize for this revolutionary discovery.

1974 b Ψ' discovered at SPEAR: an excited state of $c\bar{c}$. This and many other excited states form a detailed energy spectrum of "charmonium" which provides a rigorous test of detailed theoretical predictions and strikingly confirms the quark hypothesis.

1975 a Λ_c (lambda) observed at the AGS and at Fermilab: a charmed baryon, i.e., a 3-quark combination with one of the quarks being the c. Finding particles like this with "naked" charm (as opposed to the hidden charm of the $c\bar{c}$ states) was a crucial confirmation of the quark model.

1975 b τ(tau) discovered at SPEAR: a new charged lepton, the first since the muon. The existence of this lepton and its associated neutrino provide a strong suggestion that there must also be another pair of quarks called the b and t, since quarks and leptons have so far occurred in matched pairs, e.g. (u, d) and (e, ν_e)

1975 c	Particle jets observed in e^+e^- collisions at SPEAR. Jets are clusters of particles moving in the same general direction; their existence is evidence for parton-parton collisions. The existence of jets has subsequently been confirmed in hadron collisions.
1976	D, D* mesons discovered at SPEAR: charmed mesons, i.e., quark-antiquark combinations with one of the two being a c (or $\bar{c}$). These naked charm particles provided important support to the charm hypothesis and the quark model.
1977 a	T(upsilon) discovered at Fermilab: a new particle composed of $b\bar{b}$, where b is a fifth quark. The existence of this new quark had been suspected since the discovery of the τ lepton in 1975. Finding the b gives strong encouragement to search for its anticipated partner, the t quark.
1977 b	T' discovered at Fermilab: an excited state of the $b\bar{b}$ combination. This discovery provided important confirmation of the existence of the b quark and further tests of the quark theory.
1977 c	Strong spin dependence of proton-proton interaction discovered in ZGS experiments using polarized proton beam and target. At 1.5 GeV, protons with parallel spins interact far more strongly than protons with spins opposed. This effect, not yet fully understood, could be the first indication of a new kind of particle composed of six quarks.
1978	Parity violation in neutral currents observed in polarized electron-deuteron scattering at the SLAC linac. This observation of a very small scattering asymmetry provided one of the most striking confirmations of the Weinberg-Salam theory unifying the weak and electromagnetic interactions (1967).

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EXPERTS CONTRIBUTING TO GAO HIGH ENERGY PHYSICS REVIEW

PHASE I - SCOPE AND REVIEW APPROACH

Experts participating in the October 1978 meeting.

--Dr. Herbert L. Anderson, Distinguished Service Professor of Physics, Enrico Fermi Institute, University of Chicago.

--Dr. Lawrence Cranberg, President, TDN Inc.

--Dr. Sidney D. Drell, Chairman, DOE High Energy Physics Advisory Panel; Deputy Director, Stanford Linear Accelerator Center.

--Dr. Robert L. Hirsch, General Manager, Exploratory Research, Exxon Research and Engineering Company.

--Dr. David Z. Robinson, Vice President, Carnegie Corporation.

--Dr. Louis Rosen, Director, Los Alamos Meson Physics Facility, Los Alamos Scientific Laboratory; University of California.

--Dr. Robert G. Sachs, Director, Argonne National Laboratory.

--Dr. Robert C. Seamans, Jr., Dean of Engineering, Massachusetts Institute of Technology.

--Dr. Gerald F. Tape, President, Associated Universities, Inc.

--Dr. George H. Vineyard, Director, Brookhaven National Laboratory.

--Dr. Milton G. White, Professor Emeritus; former Director, Princeton-University of Pennsylvania Accelerator Laboratory.

--Dr. Robert R. Wilson, former Director, Fermi National Accelerator Laboratory.

Other experts providing comments

--Dr. Walter Gilbert, Professor of Molecular Biology, Harvard University.

--Dr. Gerald Holton, Professor of Physics and Professor of the History of Science, Harvard University.

--Dr. Daniel Kleitman, Professor of Mathematics, Massachusetts Institute of Technology.

--Dr. Wolfgang K.H. Panofsky, Director, Stanford Linear Accelerator Center.

--Dr. Alvin M. Weinberg, Director, Institute for Energy Analysis, Oak Ridge Associated Universities.

PHASE 2 - RESULTS OF GAO REVIEW

Experts participating in the February 1980 meeting.

--Dr. Lawrence Cranberg, President TDN, Inc.

--Dr. Sidney D. Drell, Chairman, DOE High Energy Physics Advisory Panel; Deputy Director, Stanford Linear Accelerator Center.

--Dr. David Z. Robinson, Vice President, Carnegie Corporation.

--Dr. Louis Rosen, Director, Los Alamos Meson Physics Facility, Los Alamos Scientific Laboratory.

--Dr. Robert G. Sachs, former Director, Argonne National Laboratory; Professor of Physics, Enrico Fermi Institute, University of Chicago.

--Dr. George H. Vineyard, Director, Brookhaven National Laboratory.

Other experts providing comments

--Dr. Raymond L. Bisplinghoff, Vice President for Research and Development, TYCO Laboratories, Inc.

--Dr. Robert C. Seamans, Jr., Dean of Engineering, Massachusetts Institute of Technology.

--Dr. Gerald F. Tape, President, Associated Universities, Inc.

--Dr. Robert R. Wilson, former Director, Fermi National Accelerator Laboratory; Professor of Physics, Columbia University.

1965 PLANNING GUIDES FOR LONG-RANGE PLANNING OF THE U.S. HIGH ENERGY PHYSICS PROGRAM

"1. It is the national interest to support vigorous advancement of high energy physics as a fundamental field of science.

2. The high energy physics program is a national program not related solely to the mission of any one agency. The AEC is appropriately serving as the executive agent. Although the implementation of policy in this field will rest primarily with the AEC, participation by the National Science Foundation, the Department of Defense, and other agencies of the Government, particularly in their support of high energy physics at the universities, is important for the maintenance of a truly national program.

3. The level of character of support for high energy physics should be determined and periodically reassessed in the context of the overall national science program (rather than in relation to the applied research and development programs of the AEC), advances and promise of advances in the field itself, and the then existing fiscal situation. At present, the high energy physics program requires increased financial support, especially for the provision of advanced accelerators and equipment.

4. Planning should proceed for advancement by two significant steps, at appropriate intervals, to an energy of the order of 1,000 BeV [GeV], the second accelerator to be available for experimentation by 1979-84, depending on developments in the science and the design studies.

5. The operation of existing accelerators and the associated research should be supported, including steps to increase their scientific productivity, where necessary to maintain a sound national program and to make significant contributions to science."

Source: "High Energy Physics: Report on National Policy and Background Information," Joint Committee on Atomic Energy, Congress of the United States, Feb. 1965.

"6. New accelerators should be constructed only to provide significant extension of parameters or a new order of scientific capability.

7. Accelerators which become unproductive, should be closed down or reduced in level of operation.

8. The productive research utilization of the major accelerator facilities is a primary consideration for an effective national high energy physics program. A large portion of the research conducted in existing and proposed accelerator centers should be performed by university user groups. The present high quality of the activities of the existing user groups should be strengthened and the formation of new university user groups should be vigorously supported.

9. Major new high energy physics facilities of the future will necessarily be very large and, consequently, quite expensive. This means that there can be only a few such centers. Therefore, their organization and location must be carefully planned so that they can serve effectively the entire national community of high energy physicists.

10. In view of the high costs of new very high energy accelerators, opportunities for international cooperation in accelerator construction and use should be actively explored."

AGENCY AND LABORATORY INPUT TO POLICY OPTIONS

To ascertain the strategies, costs, advantages, and disadvantages for various policy options for the U.S. high energy physics program, we asked Federal program and laboratory officials for information. We obtained data from six U.S. laboratories, the Department of Energy, and the National Science Foundation on the costs, advantages, and disadvantages of the following selected policy options for the 1980s:

--Regaining world leadership 1/ in all frontiers of high energy physics.

--Being competitive in all frontiers, but the world leader in none.

--Forgetting about world leadership and competition and working in unison with European counterparts through greater cooperation and collaboration.

--Pulling out of competition and reducing U.S. efforts to a derivative mode.

The instructions given to the laboratories, their responses, and some assumptions made to develop the options presented in chapter 4 are discussed below.

According to the Chairman of DOE's High Energy Physics Advisory Panel, the frontiers of high energy physics could be defined in terms of either the concepts to be explored or the accelerator technologies needed to explore them. To help laboratory officials identify the costs of carrying out each option, we asked them to focus on the accelerator technologies and thus on comparing U.S. and foreign accelerator capabilities, either existing or planned. We advised the laboratories that in providing input to the options they should consider new accelerators or improvements that would meet a clear research need and be technically feasible to begin constructing during the 1980s. In addition to amounts needed for new accelerators and equipment, we asked for amounts needed to

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1/In requesting laboratories and agencies input, we used the term "dominance" for this option. Because this may imply a subjugating effect on other nations, we have revised the caption. We did not, however, change the definition of this option.

maintain and operate existing accelerators and equipment and to carry out in-house research.

From program staff at DOE and the Foundation, we requested estimates of the funding support under each option that would be needed for universities. We also asked laboratory and program officials to comment on the merits of each option and the impacts the laboratories' proposed efforts would have on various aspects of the program. Since a selected frontier option could be derived from the various strategies and related costs proposed by each laboratory, we asked for input to only four options. We defined the four options in the following colloquial terms to help focus their input:

--World leadership: "We have a great country, let's win it all for the United States."

--Competition with other nations: "We should win some and lose some, but be sure to win our fair share."

--World unification: "Let's forget competition; there is one worldwide community of scientists."

--Derivative participation: "Let someone else make the big investments; we can learn from them."

For the selected frontier option, we asked DOE officials for input as to what a preeminent program would consist of. However, they pointed out that the development of such an approach would entail considerable scientific judgment and requested the advice of the High Energy Physics Advisory Panel. A subcommittee of this panel reported on the needs of a preeminent program in April 1980. A copy of that report is included as appendix V. In the absence of scientific judgments as to the composition of a preeminent program, we arbitrarily selected some options to present in chapter 5 for illustrative purposes.

A summary of the proposed efforts and the average annual budgets (in 1979 dollars) for each option follows for university support and proposed efforts at each of the six U.S. laboratories. For university support, program officials provided "ball park" estimates, while the laboratories based their estimates on more detailed analyses of existing plans in various stages of development.

UNIVERSITY SUPPORT

Universities, the traditional centers for the pursuit of basic knowledge, play an important role in the U.S. high energy physics program. University-based researchers perform about 75 percent of the experimental research at the accelerator laboratories. In addition, they invent experimental techniques and participate in the planning and design of facilities and equipment.

Financial support for university research in high energy physics comes principally from DOE (60 percent) and the National Science Foundation (25 percent). Universities provide the remainder (15 percent). DOE supports about 150 high energy research groups at some 60 universities across the country. The Foundation supports about 75 groups at about 50 universities. Together, these efforts support the work of about 2,300 people, including physicists, technicians, and students. In 1979, DOE provided $43.2 million and the Foundation, $18.9 million to universities for high energy physics research.

World leadership

DOE program staff noted that real-dollar funding of university support has declined since 1969. Coupled with European increases, they believe this decline has impaired the Nation's position of leadership in the field. For world leadership in each technological frontier, they say the United States would have to increase the 1979 level of support to universities by an estimated 25 percent to be achieved over a 3-year period.

According to program staff, DOE-supported universities' use of laboratories would increase, and the statistical precision of some experiments, which is currently compromised by fiscal constraints, may improve. Also, the universities would strengthen their detector development efforts, which have been badly neglected in recent years; refurbish their computing capabilities to meet the needs of more complex experiments; and increase their engineering and technical staff. About 10 university groups would expand research efforts, and 10 new groups would be added.

A Foundation program official said the United States is leading the world in achievements, but not in staff or instrumentation. He indicated that over the last 5 years, real U.S. funding has decreased about 10 percent. He estimated that annual increases of 10 percent over the 1979 level would be needed in each of the next 3 years for U.S. leadership in all frontiers.

For such a world leadership program, DOE's annual university support would average nearly $53 million and the Foundation's $24 million during the 1980s. The total Federal average annual support to universities would be $77 million, or an increase of 24 percent over the 1979 level.

Competition with other nations

DOE program staff pointed out that if the United States is to successfully compete with foreign high energy physics efforts, physicists will still have to work hard, undertaking major innovative experiments designed to make new discoveries and understand phenomena. Program staff believed a 10-percent increase in funding over 1979 levels would be needed, primarily for strengthening the universities' engineering and instrumentation development efforts. University research groups would maintain current staffing levels and use existing or presently planned facilities.

Foundation program officials advised us that to be competitive, Foundation-supported university groups would annually need an additional $2 million, or about 10 percent more than in 1979. These additional funds would primarily support major instrument development by six to eight research groups. Additional funds may also be needed to accommodate an influx of new, young physicists.

Under this option, the annual university budget for the 10-year period ending in 1989 would average $21 million for the Foundation and $47 million for DOE, or a total annual average of $68 million--about 10 percent more than the 1979 level.

World unification

According to DOE program staff, the world unification option would require a 10- to 15-percent increase over the 1979 level of support for universities. They assumed that research groups would make experimental proposals all over the world on a basis of scientific merit. They advised us that research costs can be expected to rise due to increased travel expenses and the additional bureaucratic effort involved in coordinating experiments. They concluded that pursuing this option would raise university groups' research costs without increasing their research productivity.

According to Foundation officials, an international effort to build the next generation of accelerators may become necessary because one nation may no longer be able to afford to build and operate them. However, they believed that coordinating joint efforts would be a slow process, and the number of U.S. facilities and technical staff would be reduced. As with DOE staff, they noted university research costs would increase about 10 to 15 percent because of increases in travel, research operating costs, living expenses, and administrative efforts.

With the 10- to 15-percent increases, the average annual university support for the world unification option from 1980 to 1989 would amount to between $67 million and $70 million, similar to the level needed for a competitive effort.

Derivative participation

Program officials at both Federal agencies told us that the level of funding applicable under this option was unclear. They believed the lack of accelerators with frontier physics capabilities would result in a major reduction in experimental efforts. They asserted that many outstanding American scientists would no longer be able to obtain research support, research in some frontiers would be abandoned, and the effect on physics education would be catastrophic.

DOE and Foundation officials said that only the stronger and larger university groups would be supported. These groups would perform some experiments at the European accelerator centers as long as the political climate permitted. If such a climate did not exist, DOE program officials predicted the U.S. program would lose its identity, and American physicists would be absorbed into the larger European groups.

DOE program officials also envisioned that without U.S. facilities needed for frontier experiments, theoretical physics would eventually become the leading element of the U.S. program. Experimental physics, historically the major contribution of the U.S. program, would be rendered inconsequential.

Based on program officials' comments, we projected the average annual university support for 1980-89 under this option at $42 million.

FERMI NATIONAL ACCELERATOR LABORATORY

Fermilab, the largest U.S. high energy physics facility, is located on 6,800 acres about 30 miles west of Chicago. Fermilab's main accelerator is a 400-GeV, fixed target, proton synchrotron, which is 2,044 meters, or 1.26 miles, in diameter. Fermilab was completed in 1972 at a cost of $243.5 million. It employs about 1,600 people, and its budget averaged about $77 million (in 1979 dollars) from 1975 to 1979.

Fermilab is constructing the world's first superconducting synchrotron, called the Energy Saver, estimated to cost $46.6 million. This project is to allow the accelerator to routinely run at 500 GeV, using less electrical power than is possible using conventional technologies. It is the first phase toward a larger project called Tevatron. The balance of this project, which is not yet authorized, would make 1-TeV fixed-target and colliding-beam experiments possible. Fermilab expects the Energy Saver to become operational in 1982 and the Tevatron, if authorized, to be completed in the mid-1980s.

World leadership

Under the world leadership option, Fermilab proposed a modified Tevatron, with three construction phases, at a total cost of $74 million. The first phase, assumed to become operational in 1982, was to increase the maximum energy capability of the fixed-target accelerator to 1 TeV and upgrade the existing experimental areas for this level. The second phase, to be completed in 1982, would provide proton-antiproton colliding beams of 1,000 GeV x 1,000 GeV. The third phase would add a second colliding-beam area and expand the facilities available for colliding-beam experiments. To carry out this project, Fermilab projected that an average annual budget of $124 to $152 million would be needed in the 1980s.

Fermilab officials predicted that the Tevatron would provide capabilities for the world's highest energy fixed-target and colliding beam experiments in the 1980s. Around 1988, however, a planned Soviet machine may surpass the Tevatron's capabilities.

Since the Tevatron may become a second-best machine less than 10 years after it begins operation, Fermilab also proposed a "Pentevac" project that would quintuple the Tevatron's maximum energy range. Such a machine is expected to yield the world's highest fixed-target and colliding proton-antiproton, proton-proton, and proton-electron beam physics through the early 2000s. While Fermilab officials believed that this machine would have to be built and operated as an international effort, given sufficient funds, they said the United States could undertake the project in a bid to lead the world.

Construction and operation of the Pentevac would require major advances in superconducting magnets, computer control technology, electronics, and many other technologies. The machine was projected to cost about $1 billion to construct and to start operations in 1988.

Under this option, Fermilab's average yearly budget for 1980 to 1989 would be $235 million to $251 million--more than three times its 1975 to 1979 average.

Competition with other nations

Fermilab included its Energy Saver project under the competitive option. The project is to upgrade Fermilab's existing 400-GeV experimental program to 500 GeV. Fermilab officials classified it under this option on the assumption that no fixed-target accelerator of higher energy or intensity will be available abroad before the late 1980s. However, they targeted the Energy Saver to become operational in 1981 and proposed to carry out a large amount of R&D for the 1-TeV fixed-target accelerator. Under this option, Fermilab's average annual costs for 1980 to 1989 would range between $97 million and $140 million.

World unification

Fermilab proposed the Pentevac as a project, which in all likelihood would require international cooperation and cost-sharing. Laboratory officials proposed that the United States fund one-third of the $1 billion construction cost, plus a third of the increases in operating and equipment costs attributable to Pentevac, starting in 1987. Such an arrangement would minimize increases in U.S. funding, but would still involve a substantial, long-term funding commitment.

Fermilab officials viewed the project as an American initiative, with the United States playing a leadership role in all phases of the project. They assumed that to commit resources to the project, foreign countries would require assurance that their scientists and technicians can participate at the new facility on an equal footing with Americans. With only the U.S. share of Pentevac costs included, Fermilab's average annual budget for the 1980s would range between $169 million and $197 million.

Derivative participation

Under this option, Fermilab assumed that its current neutrino research capabilities would be dropped in the immediate future, its currently approved proton experiments would be completed, only future experiments with small costs would be initiated, and only meson experiments would still be run by about 1983. Officials further assumed that by 1986, most of the laboratory personnel needed to keep the accelerator operating would have left for other positions, so it would be time to shut down the whole facility. They viewed this option as disastrous. Their average annual budget for 1980 to 1989, which would include no costs beyond 1986, would be $45 million.

STANFORD LINEAR ACCELERATOR CENTER

The Stanford Linear Accelerator Center covers 421 acres of Stanford University property near Palo Alto, California. The Center is a single-purpose facility, dedicated to high energy physics. It has a staff of about 1,200 people. Its average annual budget for 1975 to 1979 was about $50 million (in 1979 dollars).

The Center's 2-mile linear accelerator was completed in 1966 at a cost of about $114 million. As the world's largest and most powerful linear electron accelerator, it delivers electrons at energies of 30 GeV and positrons at 20 GeV to fixed targets.

The Center's other major machine is the Stanford Positron Electron Asymmetric Ring. This machine is a storage ring which permits colliding-beam experiments at energies of 4-GeV x 4-GeV in two interaction regions. The facility began operating in April 1972 at 5 GeV and was upgraded in 1974. The total cost was about $6 million.

A larger accelerator was completed in April 1980. This accelerator, called Positron Electron Project, cost about $78 million to construct. The new ring is about 710 meters in diameter and permits colliding-beam experiments at energies of up to 18-GeV x 18-GeV.

Stanford officials had two distinct elements in their input to the first three options. One element dealt with the levels of exploitation of their principal machines--the linear accelerator and the Positron Electron Project--at the current site. The other element dealt with the construction of a major new facility at a new site.

World leadership

Under the world leadership option, the linear accelerator and the Positron Electron Project would be operated at nearly full utilization, with the emphasis on innovative frontier physics. The Positron Electron Asymmetric Ring would be used on a half-time basis, but phased out as improvements are made to the Positron Electron Project. Also, significant increases would be made in personnel, research, and facility development to support innovation. Improvements to the Positron Electron Project would cost $187 million to $210 million, including a 200- to 300-GeV proton ring 1/ and an upgrade of the electron-positron beams to 25 to 30 GeV. The energies would depend on the results of the laboratory's superconducting R&D program. Under the minimal budget, the upgrade of the beams would be forgone for a less costly single-pass collider at 60-GeV x 60-GeV to be used in conjunction with the proton ring.

Stanford also proposed to construct in collaboration with one or more U.S. laboratories, a large new laboratory, starting in 1982. Stanford officials said the new facility would be a $500-million construction project which might involve linear colliding beam accelerators, perhaps using superconducting technology, and would be located at a new site. A staff of 2,600 to 3,200 new employees would be needed by fiscal year 1989 to maintain steady operation of the accelerators. Officials noted the new site would permit new and exciting physics for a greater number of domestic and foreign users.

Because the two elements of Stanford's input can stand alone, the average annual budget for each element for 1980 to 1989 are shown separately below.

	Existing site	New site	Total
	----------(in millions)--------		
Minimal budget	$109.0	$85.4	$194.4
Optimal budget	123.0	99.9	222.9

Competition with other nations

For this option, Stanford officials believed that about the same level of operations would be needed as for the world leadership option. However, they proposed fewer new improvements and delayed the timing of those to be pursued.

1/This proton ring, estimated to cost $150 million and intended to help provide proton-electron collisions, appears to duplicate a similar capability proposed by Fermilab for this option (see p. 136); therefore, we omitted it from the estimated average costs presented in chapter 5.

In its proposed optimal budget, Stanford included the 200- to 300-GeV proton ring and the upgrade of the Positron Electron Project's beams to 25 to 30 GeV. The proton ring, which is the most costly improvement, is forgone for the less costly single pass collider in the minimal budget. The improvements would cost $97 million to $210 million, spread out over a 7- to 8-year period, compared to the 4-year construction period proposed for the world leadership option.

Stanford also provided for construction of the new site on the basis of assumptions similar to those made under the world leadership option. However, construction would not begin until 1989. Average annual budgets for 1980 to 1989 would be:

	Existing site	New site	Total
	----------(in millions)--------		
Minimal budget	$ 83.8	$ 5.9	$ 89.7
Optimal budget	113.6	10.8	124.4

World unification

Under this option, Stanford officials would further delay or reduce improvements. One major difference is that the upgrade of the Positron Electron Project's beam is not included in either the minimal or optimal budget. The minimal budget includes the proposed single-pass collider, while the optimal budget includes the proposed proton ring. Such differences bring the cost range of the improvements to between $37 million and $150 million, depending on whether a minimal or optimal approach is pursued. Also, the improvements would be started later and in the case of the proton ring, would extend over 5 years, as opposed to the 4-year span in the world leadership option.

The utilization levels of the machines also varied. While the optimal level is close to the full utilization envisioned in both the leadership and competitive options, the minimal budget provides for reduced use of the Positron Electron Project and the linear accelerator. The proposed minimum budget also provides for less use of the Positron Electron Asymmetric Ring through 1987 at which time it would be shut down.

Stanford also included the new site in this option, but would build it with international collaboration. Construction is presumed to start in fiscal year 1989, after coordinated R&D beginning in 1985. Stanford did not indicate the amount of cost-sharing to be provided by other nations, but noted that most shared costs would be incurred in the 1990s.

Stanford officials characterized their minimal world unification budget for the existing site as close to the level of operations and improvements permitted under current Federal funding levels. They believe this budget underutilizes their existing facilities and provides for Positron Electron Project improvements relatively late, compared with expected European progress. The following chart shows estimated average annual budgets for the 1980s:

	Existing site	New site	Total
	----------(in millions)--------		
Minimal budget	$67.4	$ 5.9	$ 73.3
Optimal budget	$92.6	10.8	103.4

Derivative participation

Under this option, the Positron Electron Project would be exploited through its useful life without major improvements or modifications. The laboratory would also continue to use its linear accelerator at its current low rate until 1983, when it would begin a phased shutdown. The Positron Electron Project would begin a phased shutdown in 1985; there would be no new site.

As the accelerators are phased out, the laboratory's physics research group would increase its activities at other laboratories. At the end of the Positron Electron Project's useful life in 1987, all accelerators would be shut down. Stanford would become an academic center for physics, whose experimenters would do their research at other accelerator laboratories. Continuing through the 1980s would be a strong user group with a responsible level of advanced detector development and fabrication. Under this option, the Center's annual budget would average between $43.5 million and $55 million during 1980 to 1989.

BROOKHAVEN NATIONAL LABORATORY

Brookhaven National Laboratory was established in 1946 on 5,400 acres in Upton, New York. The laboratory is a multiprogram center and traditionally has made a strong effort in high energy physics. About 935 employees, or approximately 25 percent of Brookhaven's total, are involved in high energy physics. The average annual high energy physics budget for 1975 to 1979 was about $47 million in 1979 dollars. Current efforts center around the Alternating Gradient Synchrotron and Isabelle construction. Total project funding requirements for Isabelle are over $423 million, including about $275 million for line-item construction. Upon completion in 1986, Isabelle will have two counter-rotating proton beams, each about 400 GeV, which will collide at six interaction regions.

The Alternating Gradient Synchrotron is a 33-GeV, fixed-target, proton synchrotron, which was completed in July 1960 and underwent a major conversion in August 1973. About a third of the machine's running time will be needed as an injection source of protons to Isabelle. The total investment in the synchrotron and related equipment in 1979 dollars was about $163 million.

World leadership

To help the United States lead the world in high energy physics, Brookhaven would complete construction of Isabelle, start operations in 1986, and keep it in the forefront of the field through the 1980s. The proposed construction would include two experimental areas not currently authorized. Later additions to Isabelle include an electron ring and a separate accumulator ring to produce and store antiprotons for colliding beam experiments in the main ring. 1/

The Alternating Gradient Synchrotron would be used as an injector to Isabelle and for experiments, including polarized and unpolarized proton beam experiments. Improvements to this machine would include a new target station with a primary proton transport, two new secondary beams, and an Isabelle test beam. Other additions would be an antiproton cooling ring and a spectrometer. Brookhaven's average annual budget for 1980 to 1989 would range from $101 million to $109 million.

1/These additions apparently duplicate capabilities envisioned by Fermilab under this option (see p. 136); therefore, the costs averaging about $10 million a year are omitted from the average annual cost estimate for the leadership program in chapter 5.

Competition with other nations

Under the competitive option, Brookhaven would include the two additional Isabelle experimental areas, but not the added rings. Brookhaven would use its present synchrotron as an injector to Isabelle and for experiments throughout the 1980s. Improvements oriented to Isabelle, most of which were authorized in 1979 and are to be completed in 1982, would be made to the Alternating Gradient Synchrotron. A competitive budget for Brookhaven would average $92 million to $100 milli ı a year through 1989.

World unification

Brookhaven officials believed the world unification option was extremely ambiguous when applied to Isabelle. The option envisioned (1) international collaborations in the construction and operation of facilities and (2) international agreements to construct future facilities in alternating locations. Such approaches would not apply to Isabelle, which is already authorized for full U.S. funding.

On the other hand, Brookhaven officials told us that Isabelle could fit into the definition of the world unification option. First, because of its size and cost, they did not believe a similar accelerator will be constructed elsewhere. Secondly, they recognized that Isabelle may be one of a series of large machines of different capabilities which would be available to researchers of all nations. Brookhaven officials noted that while Isabelle is a unique machine, other types of machines, either currently contemplated or under construction, will compete for discoveries of certain fundamental processes of nature. For this reason and the large U.S. investment, Brookhaven officials believed that Isabelle should be promptly exploited and extremely well-supported to ensure a reasonable national return.

Under both the leadership and competitive options, Brookhaven officials envisioned that international cooperation would be widespread. This cooperation would resemble existing collaboration, where researchers from other countries will be allowed access to Isabelle, based on the scientific merit of their experiments. They said a European research group has expressed interest in obtaining experimental time on Isabelle; however, no formal request has been submitted.

Based on the above comments, we assumed that the United States would complete construction of Isabelle and the cost estimates provided by Brookhaven for the world leadership option ($101.1 million to $109.0 million) would apply for this option.

Derivative participation

Brookhaven officials said that by definition this option was not pertinent to Isabelle. They believed retreating to this posture would destroy the U.S. frontier physics efforts and would send the most productive and brilliant researchers elsewhere.

Since the option posited that the United States would not make any more large investments in frontier machines, the construction of Isabelle, a large investment in a frontier machine would be inconsistent with the option. U.S. adoption of a derivative participation policy at this time would necessitate a reevaluation of the decision to build Isabelle. This could conceivably result in the complete cessation of Isabelle work.

Thus, Brookhaven's role in a program directed toward derivative participation, consistent with the other U.S. laboratories' approaches, would call for terminating Isabelle construction, phasing out the Alternating Gradient Synchrotron by 1986, and establishing a group of R&D personnel and experimentalists who would use accelerators elsewhere after 1986.

In estimating a derivative budget for Brookhaven, we assumed full funding of its competitive effort through 1982 to account for the costs committed to existing Isabelle construction contracts. We further assumed competitive funding for the Alternating Gradient Synchrotron until 1986, when it would be phased out, and estimated $8 million to $10 million per year for user group and R&D efforts during 1987 to 1989. These assumptions resulted in an estimated annual cost averaging from $47 million to $58 million during 1980 to 1989.

ARGONNE NATIONAL LABORATORY

Argonne National Laboratory is a multiprogram laboratory located on a 1,700 acre site 27 miles southwest of Chicago. Argonne's high energy physics program has been built around the Zero Gradient Synchrotron, a 12.5 GeV proton accelerator which began operations in 1963. The authorized cost of the basic facility was $51.4 million. Because of the desire to pursue new frontiers under a limited budget, the synchrotron was shut down at the end of fiscal year 1979 (after Argonne submitted its information for this discussion). About 10 percent of the laboratory's 5,200 employees were involved in high energy physics. Argonne's high energy physics budget during 1975 to 1979 averaged about $20 million a year.

Argonne officials expected to have an important role in high energy physics as a center for mounting experiments at other accelerators. In this mode of operation, the laboratory will work closely with university groups and develop and operate major detectors. Also, it plans to collaborate with Fermilab in accelerator and equipment R&D to explore new opportunities in colliding-beam and polarized-beam physics.

World leadership

Under the world leadership option, Argonne would continue its accelerator operations 10 months per year through 1989, with the physics divided equally between polarized and unpolarized proton beam experiments. 1/ Until it shut down, the synchrotron operated only with polarized protons. Improvements would be made to introduce secondary beams produced from unpolarized protons and increase the intensity of the beams. The total cost of the various improvements would be $14.5 million.

Argonne officials told us that important areas of frontier research are not found exclusively at higher energies, but also occur in areas of specialized capabilities. The Zero Gradient Synchrotron, they said, was neither approaching technical obsolescence, nor exhausting its scientific capability, and under this option, it could mount a systemmatic attack on physics not fully explored. The world leadership option was also viewed as preserving an independent center of accelerator R&D that had strong links with Argonne's efforts in applied technology and would therefore benefit the overall laboratory. Argonne's annual high energy physics budget for the 1980s would average from $20 million to $31 million.

Competition with other nations

To remain competitive, Argonne would operate its synchrotron for 6 months per year as a polarized-beam facility with only minimal improvements through fiscal year 1985. According to Argonne officials, its operation is needed if the United States wants to maintain its lead in studying spin effects at high energies. Renewed development of the polarized source would allow the use of higher intensity beams. Argonne proposed to fully exploit the accelerator over a 6-year period.

1/Because the Argonne accelerator's operation apparently duplicates capabilities envisioned by Brookhaven under this option (see p. 142), we excluded it from the average annual cost estimated for this option presented in chapter 5.

The synchrotron would then shut down, and the laboratory's research groups would continue their efforts at other laboratories. Also, accelerator and equipment R&D efforts would continue in concert with other laboratories. This option would involve an average annual budget for the 1980s of $7 million to $11 million.

World unification

Under an international mode, the synchrotron would operate as a jointly funded facility for polarized-beam operations for 6 months per year over a 6-year period. This proposal would be similar to the competitive option, except that laboratory officials envisioned that other nations would bear half the operating costs. In addition, the laboratory would continue experimental research and accelerator and equipment R&D activities. The average annual budget for the 1980s would be between $6 million and $9 million.

Derivative participation

According to Argonne officials, the shutdown of their accelerator essentially fit our definition of derivative participation. Its user group and R&D efforts would cost an average of $6 million to $9 million annually during 1980 to 1989.

LAWRENCE BERKELEY LABORATORY

The Lawrence Berkeley Laboratory in Berkeley, California, is a multi-program laboratory at which about 380 employees are directly involved in high energy physics. Since fiscal year 1974, when its 6-GeV synchrotron was phased out, the laboratory has not operated a high energy physics accelerator.

Berkeley, however, still has a strong experimental and theoretical research group, continues with accelerator and detector R&D, and has collaborated with Stanford in planning and building the Positron Electron Project. Within Berkeley's multipurpose laboratory, high energy physics is considered important as a training environment for graduate students and a source of techniques and instruments useful in other disciplines. The laboratory's average annual high energy physics budget for 1975 to 1979 was about $13 million in 1979 dollars.

World leadership

Berkeley's role in a world leadership effort would be to work with Stanford to develop and construct a large new laboratory and accelerator complex for advanced electron-positron collisions. Accelerator R&D would be pursued with the dual objectives of increasing the performance and reducing the costs of high energy accelerators.

Berkeley officials stated that to secure world leadership in electron-positron physics, the United States must start planning for a new laboratory even as the Positron Electron Project is beginning operations. They envisioned that new accelerator development and construction would take place during the last half of the 1980s. Staffing would expand 75 percent over present levels, bringing the Berkeley laboratory's average annual high energy physics costs for 1980 to 1989 into the range of $24 million to $27 million.

Competition with other nations

Competitive experimental research in the early part of the 1980s would thoroughly exploit Stanford's Positron Electron Project. In the latter part of the decade, more of the research would be at Fermilab and Brookhaven. Accelerator R&D would also be pursued. In collaboration with Stanford, improvements would be proposed for the Positron Electron Project in the 1980s.

Officials said the Berkeley laboratory has declined over a 20-year period from a position of world leadership to a collaborative, competitive level. For the laboratory to remain competitive would require a doubling of funding over this decade. A substantial funding increase in 1980, when the Positron Electron Project begins operations, and a 30 percent increase in employees through 1989 would be needed to maintain a competitive effort. The average annual budget for 1980 to 1989 would range from $16 million to $19 million.

World unification and derivative participation

Berkeley officials believed that neither of the two options constitute a viable or stable policy. They indicated physicists would press for a stronger posture, if either option is pursued. Failing this, the high energy physics community would move into other activities, and the Nation would lose prestige, opportunities to develop new technology, and a source of well trained physicists.

Berkeley would phase down its efforts to an observer status under both options, and it would maintain a group of physicists to stay in touch with the field by attending meetings, reading literature, and conducting experiments at other laboratories. Officials indicated the phase down would occur less rapidly under the world unification option but time schedules or detailed budgets were not provided. A $4 million annual budget would be needed to support the group they envisioned.

On the other hand, Berkeley's overall input indicated a close relationship with the other laboratories, especially Stanford. Since Stanford envisioned significant improvements under the world unification option, we assumed that Berkeley's budgets for the competitive option ($16.4 million to $19.0 million) could also apply to this option. For the derivative option, we assumed a phasedown parallel to Stanford's. Accordingly, we used Berkeley's minimal budget for the competitive option through 1985 and its estimate of the user group's needs for 1986 to 1989, yielding an average annual budget of $11 million to $12 million.

WILSON SYNCHROTRON LABORATORY

The Wilson Synchrotron Laboratory in Ithaca, New York, is the high energy physics facility of Cornell University's laboratory of nuclear studies. In March 1965, the Foundation and Cornell University entered an $11.3-million contract for the construction of the Wilson Laboratory with a 10-GeV electron synchrotron. Accelerator operations began in 1967, reached 10 GeV in 1968, and ceased at 12 GeV in 1977. In September 1977, the Foundation and the University began to convert the synchrotron accelerator into the Cornell Electron Storage Ring, an 8-GeV x 8-GeV electron-positron beam accelerator. The conversion project is estimated to cost about $20.3 million and uses the electron synchrotron as an injector. The accelerator began operations in the fall of 1979.

Annual funding for Cornell's high energy physics efforts ranged from $4.1 million in the year ending February 1976 to $12.9 million for the year ending October 1978. The earlier period included preconversion operations, and the latter included principally construction.

World leadership

Cornell proposed to build a more powerful accelerator, with 60-GeV electron and positron beams. 1/ It would be completed with four experimental areas and one major detector by 1987, about 7 years after the initial operation of the present ring's 8-GeV beams. An interim improvement would be to increase the present ring's beam energies to 10 GeV for 1981 operations. Laboratory officials believe the new ring would permit the United States to move toward world leadership in the electron-positron colliding beam field. The machine is expected to have the capability of reaching the energy at which the carrier of the weak interaction is thought to exist. They believe it can be built and begin operations before the large electron-positron ring currently under consideration by the European high energy physics community. Under this option, the laboratory's average annual budget from 1980 to 1989 would be $27 million to $32 million.

Competition with other nations

Cornell officials submitted two proposals for the competitive option. The first, or optimal, proposal called for increasing the present ring's beams to 10 GeV for 1982 operations, along with major improvements to the presently planned detector and the construction of a new experimental hall. Cornell officials believed such improvements would provide the best facility for studying electron-positron collisions in the center-of-mass energy range between 6 and 20 GeV. Laboratory officials expect the facility to remain competitive for about 10 years. Under an optimal budget, this proposal would cost an average of $8.6 million a year from 1980 to 1989.

The second, or minimal, proposal called for energy increases to the 8-GeV x 8-GeV ring and only minor improvements for the detector. Costs would be relatively stable through 1989, when the accelerator would be close to the end of its lifetime. Minimal budgets during 1980 to 1989 would average $6 million a year--close to the funding currently being provided by the Foundation.

1/Because this accelerator's capabilities apparently would be duplicated by Stanford under this option (see p. 139) we omitted it from the average annual cost estimate for this option presented in chapter 5.

World unification

Laboratory officials did not submit any proposals directly addressing this option. However, for the competitive option, laboratory officials indicated that they expect to host foreign scientists and pointed out that the accelerator may provide opportunities for experiments not achievable elsewhere. Accordingly, we assumed that the competitive option, costing $6 million to $8 million a year, was responsive to the general aims of the world unification option.

Derivative participation

Under this option, laboratory officials would fully utilize the storage ring, but make no attempt to improve it or the detector. They envisioned phasing the facility out of operation in 1986. However, they would maintain an experimental research staff and continue R&D on superconducting equipment. They believed that their efforts would be seriously reduced beginning in 1986, and they did not provide budget figures for 1986 or thereafter. For the 10-year period ending 1989, Cornell's average annual high energy physics budget would be $3 million.

APPENDIX V

THE UNITED STATES HIGH ENERGY PHYSICS PROGRAM

(1980 - 1990)

HEPAP Subcommittee Report

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April 1980

APPENDIX V

<u>The United States High Energy Physics Program--(1980-1990).</u>

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E. The U. S. High Energy Program (1986-1990).

F. Summary.

Charge.

Examine the health of the National High Energy Program in the United States and to assess the support required to assure a preeminent position. There should be special emphasis on the years 1980-1985, with a specific focus on funding in 1982, and an examination of possibilties for 1986-1990.

B. Introduction.

The record of accomplishments in High Energy Physics in the United States since the early 1950's has consisted of many major findings, both in the theoretical and experimental domains. This success is reflected by the large number of Nobel Awards granted to American physicists over the last 25 years, including the most recent award to Glashow and Weinberg (1979). Progress in this field arises from the intricate interplay between empirical discoveries and theoretical insights. Essential to this process are the experimental tools needed for carrying on the required research, namely the accelerators. The evolution of such machines is shown in Figure 1 where one notes that the attainable beam particle energy (whether proton or electron) increases by a factor of 10 every seven years. One also notes the emergence of new concepts in accelerator design every five to ten years (Electrostatic Generators giving way to Cyclotrons to Linacs to Synchrotrons to Storage Rings to...) which allows for the timely and cost effective construction of even higher energy machines. The discoveries that have been made utilizing these accelerators have been enumerated many times, the latest by the Division of High Energy Physics of the Department of Energy in June 1979. Nevertheless, it is important to briefly mention some of the more important results.

High energy physics is concerned with the study and decipherment of the fundamental constituents of matter as well as an understanding of their interactions, and it is precisely in these areas that major breakthroughs have been made. Ordinary atoms of matter consist of a nucleus, composed of neutrons and protons, surrounded by electrons. In fact the only stable particles are the electron and the proton, and there are now theories which would even have the proton decay. New particles with unexpected properties have been found in both the area associated with electrons (leptons) and with the proton (hadrons). These include the so-called strange particles, whose number eventually reached well over one hundred; charm particles, even more massive particles that lived an unexpectedly long time; and the more recently found Υ (upsilon) family, again more massive states, ten times heavier than the proton. These findings, coupled with studies of the interaction of electrons, neutrinos and nucleons with nucleons, have led to formulation of the quark model of matter, whereby all these particles (hadrons) are composites of more fundamental quantities, called quarks, which come in a more limited variety, of the order of five or six basic units. Very analogous developments have occurred in the lepton domain where a similar proliferation of fundamental particles has evolved. Starting from the well known electron and the elusive neutrino, the catalogue has been increased by the discovery of a heavier electron, the muon (μ); then a second neutrino and more recently an even heavier lepton the tau (τ) lepton. In both these areas, hadrons and leptons, we are confronted with a situation where the basic quantities no longer number 1, 2 or 3, but 5 or 6 and may even increase.

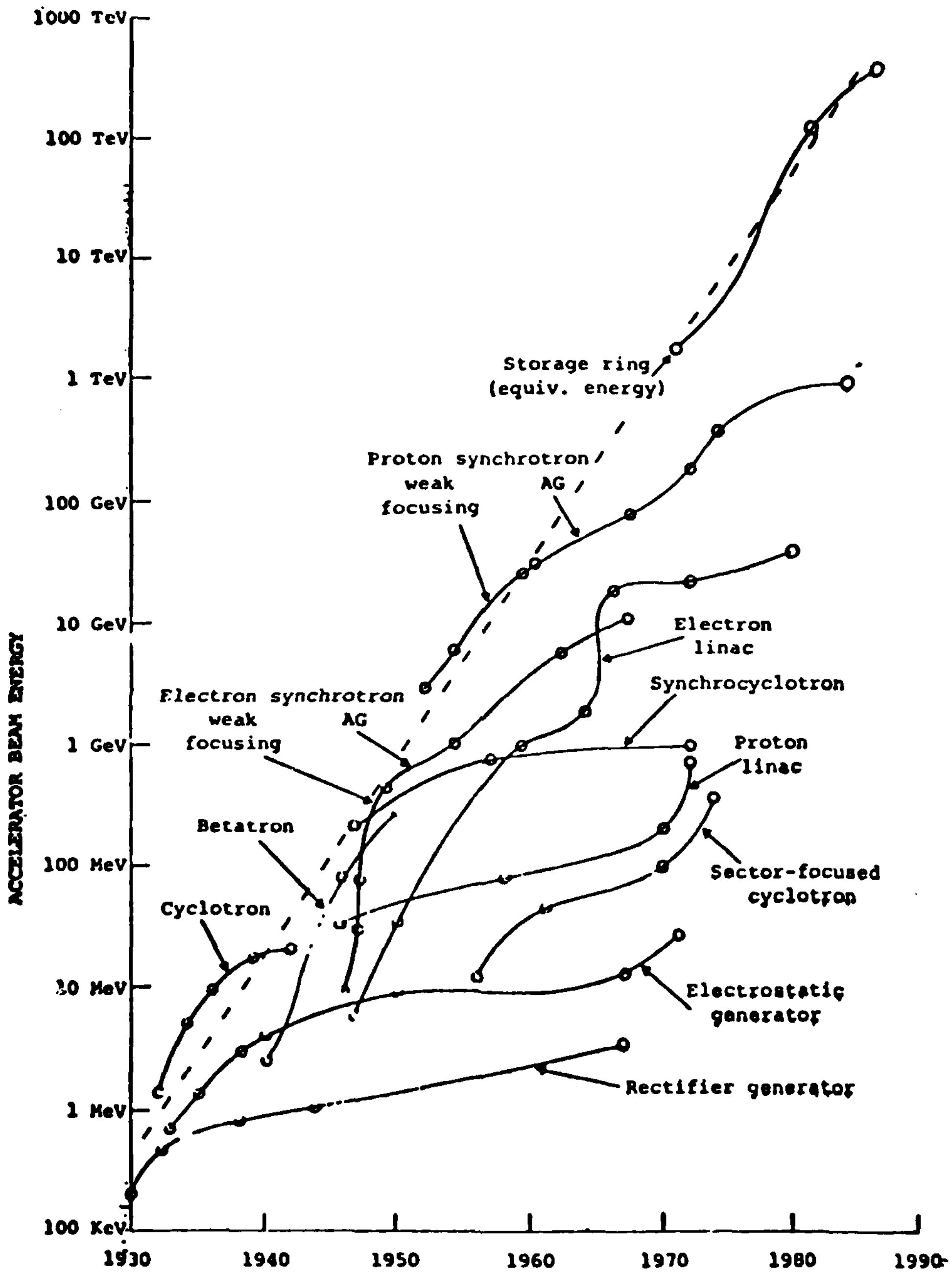


Figure 1
Energy growth of accelerators and storage rings

We are led to believe that nature is not as simple as we thought and indeed that there may be another layer of more fundamental quantities whose dimensions are less than 10^{-16}cm.

We have already alluded to the study of the mutual interaction of particles which occurs through the action of a variety of forces. A proper and exciting activity has been the investigation of the properties of these forces which, at present, we separate into four types. The gravitational force is the most familiar since it is what gives us weight and is responsible for preserving the stability of our planetary system. The next most familiar interaction is the electromagnetic force since it occurs between all charged particles and such is our sophistication in this area that we can predict effects to an accuracy of one part in 10^{10}. The third force, the strong interaction, is responsible for holding protons and neutrons together as well as causing the scattering of hadrons on hadrons. The weak force is the fourth and is the one responsible for the radioactive decay of many particles as well as the interaction of neutrinos with other particles. Indeed one recent exciting prospect is that the weak and electromagnetic forces may not be different but part of one unified force, in the same way that electricity and magnetism although appearing quite different are one electromagnetic force. If this is correct, then the carrier of this combined force is expected to be $\sim$100 times more massive than the proton and observable in the next generation of accelerators. A more ambitious hope is a grand unification of all the forces, gravitational, strong, electromagnetic and weak; but this awaits future Newtons or Maxwells and possibly experimental devices that have yet to be conceived.

In all of these activities, physicists in the United States have played a preeminent if not a dominant role. However, there is some doubt as to the continuation of this trend due to a variety of factors including funding levels. In this report we examine the present state of high energy physics in the United States and attempt to assure the requirements to assure a preeminent posture over the next five to ten years.

C. Historical Perspective.

It has been an accepted and effective procedure in high energy physics to periodically examine, via the establishment of a prominent panel of physicists, the health and future of the field. Among these have been the Ramsey Panel (1965), the Pake Panel (1966), and more recently the three Woods Hole Subcommittees of HEPAP (1974, 1975, 1977) chaired respectively by Weisskopf, Low and Sandweiss. In parallel with these latter studies, there was a report by the DOE staff entitled "Long Term Strategy for Construction and Operation of High Energy Physics Facilities" in 1976. At the same time, the high energy community has advised the government on a continuing basis via HEPAP. If one had to characterize the field of HEP over the past fifteeen years, above and beyond the major scientific discoveries discussed above, we would note the planned and systematic closing of the lower energy facilities in order to construct and utilize the new frontier machines. The accelerator centers which have fallen victim of this trend are the Cosmotron (1966), the Princeton-Pennsylvania Accelerator (1970), the Cambridge Electron Accelerator (1972), the Bevatron (1974), and most recently the Zero Gradient Synchrotron (1979).

The funding pattern of the field during this period can be considered to have been viable during the period up through 1970, but was followed by a disastrous decreases until 1975. Furthermore, there were no new construction projects authorized during the nine year period following the authorization of the creation of the Fermi National Accelerator Laboratory at Chicago in 1968 until-the commitment to build the e^+e^- PEP machine at Stanford in 1977.

The crucial planning agreements for this period are the 1977 HEPAP Subpanel report and the "treaty" between Dr. J. Deutch and the Office of Management and Budget in 1978. The main components of these agreements are summarized below.

1. Woods Hole. The 1977 New Facilities Panel met and considered the proposals for new tools in the field, and made the following recommendations, which were endorsed by HEPAP:

   (a) We recommend as our first priority the authorization of the Brookhaven proposal for the construction of the proton-proton colliding beam facility ISABELLE, to operate with a maximum energy of about 400 GeV per beam and a peak luminosity of 10^{33} cm^{-2} sec^{-1}.

   (b1) We recommend authorization in FY 1979 of $12.8 million for Tevatron construction (sufficient apparatus and facilities to convert the completed Energy Doubler/Saver R/D project to an accelerator and to extract a 1 TeV beam to an appropriate beam dump or target). We also recommend authorization of ~$10 million in FY 1979 for the highest priority projects that will enable FNAL to begin the exploitation of the Tevatron for fixed target 1 TeV physics.

(b2) We recommend that funds be provided in FY79 for construction of the necessary facilities and apparatus needed in support of an accelerator R/D program on p-p and $\bar{p}p$ colliding beams at FNAL.

The committee was very enthusiastic about the opportunities that would be made available by the high energy and high luminosity machine at Brookhaven. They recommended that ISA be built as fast as possible.

The committee also felt that, in the context of the national program, the highest possible energy fixed-target physics at FNAL was of prime importance.

The committee was excited by the future potential for colliding bunched beams of very high energy p-p or $\bar{p}$-p at FNAL. Many unanswered questions vis-à-vis beam dynamics existed, and so support for R/D work to understand these problems was proposed. However, the R/D work should not interfere with the fixed target program.

The committee felt that high in its priorities for the national HEP program were to maintain balance and diversity among the separate laboratories because there was a real need for the different types of accelerators, each with its own unique capabilities. The main thrust would be at the highest energies, pushing the frontiers of the field, but much important work would also be done at low energies, work that would be vital to the progress of the field.

Geographical balance was also an important issue.

2. Funding Scenarios. The following budget scenarios were considered when reviewing the future: [include both NSF and ERDA (now DOE) support]

	FY78 Dollars	FY79 Equivalent
Base Budget.		
	$290M oper. & equip.	
	60M const.	
	$350M	($373M)
High Budget.		
	$305M oper. & equip.	
	70M const.	
	$375M	($400M)
Low Budget.		
	$256M oper. & equip.	$23M NSF
	47M const.	300M DOE
	$303M	$323M

For all scenarios the committee wished to retain work in all three main thrusts of our program: colliding e^+e, colliding proton ring, and highest energy fixed target collisions, and felt strongly that all are essential to maintain the cutting edge of this important area of science.

The consequences of the "Low" budget are spelled out: stretching out of construction, marginal utilization, and severe pressure on the lower energy program.

Indeed, they felt that on a long term basis this level is close to that which would require a drastic revision in the long range program and is probably not adequate to keep the United States in a world competitive situation in this field.

3. The Deutch Plan. The above sub-panel report, and its subsequent endorsement by HEPAP was followed by an agreement between Dr. J. Deutch of DOE and the OMB on the support level of HEP over the next five years. This agreement, or "treaty," was to provide a five year plan, a basis for long range planning in the field and give some stability to the field's funding. It reversed a very disturbing trend of falling operations support and no new construction for a period of nine years. The treaty spelled out the following points:

(a) ISA should be funded at $23M in FY79 as part of a balanced multi-year HEP program.

(b) This program is essential to preserve U.S. world leadership in HEP.

(c) It is based on three national labs: at BNL, SLAC, and FNAL.

(d) It is in the best interests of DOE and OMB to avoid large changes in total funding levels from year to year.

It was agreed that $300M DOE + 23M NSF in FY79 dollars, over the next several years would provide for programmatic stability, continued leadership in the field, and completion of ISA on a reasonable schedule.

The treaty allowed for HEP sharing in any real increase for basic research funding proposed by the government at same level.

4. Comments on the Impact of These Agreements.

The community accepted this response from DOE and OMB, and has tried to plan and live within the bounds of the "Deutch Treaty." It should be noted that this low Budget figure was viewed as being marginal, and if it were to persist the field would risk losing its pre-eminent position, and would have to revise, and drastically retrench, its program to remain viable.

The predicted hard times have indeed come about. The community had planned around the $323M level, and proceeded with the best HEP program that could be marshalled. What are the problems?

(a) The $323M was marginal as stated from the outset; it was given a "best efforts" chance, but as stated in the '77 HEPAP report, such a level, if maintained for a long period, may require drastic changes in the program within a few years.

(b) The European competition has been supported very well, both nationally, by the Germans at Hamburg, and internationally, at the joint European lab in Geneva. This vigorous and ambitious program in Europe challenges the U.S. leadership in high energy physics of the past thirty years.

(c) The cost of building the new super-conducting accelerators has been more than initially projected. These represent the first large scale application of super-conducting technology, and the real costs of making the final system have only emerged since the Wood's Hole studies. The enterprise is of major significance to United States engineering and industrial communities, and ensures the U.S. of technical leadership in the world in this area.

(d) The operations costs for the existing facilities have escalated somewhat faster than the standard cost of living index, due to special costs increases in energy and for accelerator components.

(e) In meeting the budget pressures and striving for the construction of the needed frontier tools, the utilization of the existing facilities has been drastically squeezed. All of the laboratories are now running in a mode with very high leverage quotient (i.e., the fractional increase in utilization for a given increase in operating support), and are operating well below the 50% exploitation level. This represents a poor use of our investment.

(f) In meeting the budget pressures, the R/D into new detection apparatus and into new technologies for accelerating have been severely reduced. This is not a healthy situation! Referring back to the famous and impressive Fig. 1 one sees that the impetus that has pushed the field to ever higher energy frontiers (which corresponds to looking at smaller and smaller distances) does not come from squeezing the last out of an existing technology, but derives from the invention and incorporation of exciting new ideas for the acceleration of charged particles. We need to spend some of our resources to continue to play our part in this impressive heritage.

Below we will review the present status of the U.S. high energy physics program, and look at the future with its problems and its promises. *However, before going on, it is important to note that despite the very real problems that this field is facing in terms of support, despite the strong challenge to U.S. leadership by an aggressive and well supported European program, despite the lost opportunity implicit in our under-utilization of our facilities--despite all of this--it is remarkable how close we have come to having a very healthy U.S. HEP program. In our review of the program, present and future, in the following sections we will argue that an increase is indeed required if the "NATIONAL TRUST" of a pre-eminent U.S. high energy physics program is to be honored, but that this increase should be very much along the minimum program of 1977 and would require 20% increase in funding level distributed over the categories of operations, equipment, new construction, and R/D. Such a support level would ensure the U.S. of a strong, healthy HEP program.*

D. The U.S. High Energy Physics Program (1980-1985).

1. Assumptions on Basic Policy.

As we discuss the U.S. HEP program over the next decade, first in some detail for the period '80-'85 and then rather more speculatively for the last half of the decade, we make the following assumptions as necessary ingredients for a healthy, viable, and competitive program.

(a) There should be three centers for HEP research funded by DOE--at Brookhaven, Fermilab, and Stanford, and that these centers will remain viable for the indefinite future. These laboratories will continue to support the research of the University user community, which currently involves 80 institutions.

The Wood's Hole panels felt strongly that the diverse and complementary HEP program afforded by these three national research centers working on their separate and unique frontiers was a very important ingredient in the national plan. Furthermore, it offers the University-based community a wide choice of which problems to attack, and how and where to pursue their experiments. This, and the different styles of physics and management at the different laboratories, has played a significant role in the past productivity and leadership of the U.S. national program.

(b) Utilization of major accelerators should not fall substantially below a level of about 50%.

There is little more to say. Clearly, if the exploitation of our major facilities falls below 50%, then we have a mis-match with our capital investment. The new facilities are required to keep our U.S. HEP program at the cutting edge of our science and competitive internationally. Currently we are in the position of under-utilizing or under-exploiting our powerful research centers--a matter of grave concern.

(c) Approximately 20-30% of total funding should be dedicated to the construction of new facilities which advance the frontier of our field.

This has been discussed at length in Wood's Hole panels and regular HEPAP meetings. The imperative of spending a fraction of the national support on innovative new facilities which allow experiments at higher energies or the study of new kinds of collisions is unanimously endorsed. It has been estimated that about one fourth is the optimum balance of the investment in new construction and in exploiting the existing facilities. If the fraction is much larger, then existing facilities will not be exhaustively exploited before most experimenters move on to the use of new installations. Conversely, if the number is much smaller, then the program will be headed slowly for extinction through obsolescence.

(d) *There should be an adequate pace of* accelerator research and development activities to support innovative future construction.

The importance of this assumption is dramatically portrayed in Fig. 1 , which demonstrates that the energy frontier in HEP has advanced by an order of magnitude every 7-8 years, with a corresponding decrease in the cost per unit of energy by a factor of a million over forty years. The advances have come about through imaginative new developments in accelerator technology and not by wringing the last, expensive "drop" out of existing techniques. This impressive historical heritage demands that a significant portion of our funds be invested in searching for clever new and economical ways to accelerate particles to high energies.

(e) There should be adequate support to assure that U.S. physicists and laboratories will continue to play a major leadership role.

The international position of the U.S. in this field is a question of national policy. The Department of Energy is explicitly charged with maintaining U.S. leadership in the field of high energy physics. Nearly everyone would find it unacceptable to permit this field, among the most basic of all the sciences, to become a purely derivative science in the U.S., with all new results generated at foreign installations. At the same time to demand complete U.S. dominance in the field would be unrealistic, considering the aspirations and competence of foreign scientific undertakings. The stated planning assumption thus reflects an intermediate position between these two extremes.

2. Present Facilities.

A brief description is given below of the present status and of the discernible future course of each laboratory.

(a) BNL

The major facility at BNL is the AGS, a 33 GeV, fixed target, proton accelerator which began operation in 1960. By present standards the energy of this machine is low and many, through certainly not all, of the experiments using its beams are no longer at the very forefront of exploratory physics. Important results of recent years include the discovery of the J particle, the charmed baryon Λ_c^+, and various measurements of neutrino interactions. The ACS will continue to have importance to the U.S. high energy physics program since, with the shut-down of the Argonne ZGS machine, it is the only remaining hadron facility with broad capability in the 0-30 GeV energy range. In particular, it seems likely that important physics results will be obtained with the AGS during the next five years involving the spin dependence of interacting hadronic particles. Such studies require a modest modification of the AGS which are presently under consideration.

The major authorized project for the future at BNL is the 400 on 400 GeV colliding proton machine ISABELLE. In colliding beam machines the energy available for the production of new particles is proportional to the beam energy, whereas for fixed target machines it varies only as the square root of the beam energy. ISABELLE will give us an order-of-magnitude increase in energy available for new-particle production and therefore represents a major expansion of the frontier of high energy physics. A very important attribute of ISABELLE is its high intensity which will allow rapid data accumulation as well as exploration of rare processes. ISABELLE will therefore allow for definitive searches for the W and Z particles, possible new members of the quark family, and most important for the completely unexpected which historically has accompanied the advent of nearly all new machines. At present it is foreseen that ISABELLE will commence operation in mid-1985.

(b) Fermilab

At Fermilab the major facility consists of a 400 GeV, fixed target, proton accelerator which has been in operation since 1973. With its large variety of secondary beams, Fermilab has provided the opportunity for many different types of experiments. Major results include the discovery of anomalous lepton production, the discovery of the upsilon particle, measurements of hadronic particle structure, and studies of weak interactions with neutrino beams. While the 400 GeV program is still at the forefront of high energy physics research, it has now reached a mature stage where some of the excitement has been replaced by a more systematic approach to various experimental questions. Looking to the future, Fermilab has been authorized to upgrade its accelerator by means of adding a string of superconducting magnets (~1,000) placed directly beneath the present accelerator ring. This allows for a variety of thrusts; large power cost savings can be realized by running the present accelerator up to 100 GeV and then transferring the beams into the superconducting ring for attaining 400-500 GeV (Saver Mode); the additional superconducting ring will also provide the opportunity to reach very high energies by means of colliding beams head on. This would involve a complex scheme of producing and accumulating antiprotons and then simultaneously accelerating these antiprotons with counter rotating protons to a center of mass energy of ~2,000 GeV (TEVATRON I); the superconducting second ring also allows for accelerating the primary protons up to 1,000 GeV, thereby doubling the energy of the fixed target program involving neutrino, muon and hadron collisions with high intensities and energies. (TEVATRON II).

(c) SLAC

The third major laboratory, SLAC, presently operates two distinct facilities and will soon add a third. They consist of the two-mile linear electron accelerator (LINAC) and the small electron-positron storage rings, SPEAR, to be joined in April-May by the large electron-positron storage ring, PEP. The LINAC began operations as a 20 GeV fixed target accelerator in 1965. Through an ongoing upgrading it is now running at 30 GeV and will slowly reach 35 GeV. SPEAR has been able to attain c.m. energies of approximately 8 GeV, while PEP will go to 36 GeV. Major results obtained at SLAC include detailed studies of nuclear structure, the discoveries of various particles containing charmed quarks, the discovery of the τ lepton, and a precise measurement of parity violations in weak neutral interactions. It is probably fair to say that SLAC has been a dominant force in high energy physics during the period 1974-79. In particular, SPEAR has shown that electron-positron annihilations provide an extremely clean method of producing new particle types. PEP hopefully will extend this kind of research to higher energies. Important results are expected on the relationship between hadrons and their constituent quarks and gluons, on weak-electromagnetic interference effects, and possible additional particle types.

Further expansion of the e^+e^- capabilities at Stanford are being studied. The most exciting possibility involves the creation of very tiny, high-current beams of e^+ and e^- which clash in a once-only mode, but allow study of e^+ e^- annihilation processes, with reasonable luminosities, up to energies of $\sim$100 GeV. This project has the double merit of allowing the possibility of the U.S. once again taking the leadership in e^+e^- studies with a very clever, innovative and "cheap" Z^o factory (i.e., cheap compared to the European LEP proposal for a Z^o factory with conventional technology), and also of allowing the accelerator R/D experience of such devices which are the field's only hope of continuing this clean, point-like probe of matter to even higher energies and, consequently, to even smaller and smaller distances.

In this section we have focused on the three major laboratories. However, it would be improper to exclude from this discussion the N.S.F. supported effort at Cornell University involving an electron-positron collider, CESR, with a top c.m. energy of 16 GeV. Operation of this facility has just begun and has already added important and unique contributions to our understanding of the quarks contained in the upsilon particle. In particular, three and possibly four excited states of the Υ have been observed and the energy and luminosity of this machine is extremely well matched to unraveling the mysteries of upsilonium physics. It should be noted that Cornell is also considering future possibilities of strengthening the study of e^+e^- collisions by going to higher energies, however their deliberations are not as far along as those at Stanford.

It should also be remembered that high energy research conducted by university physicists has been central to much of the progress of particle physics and has also provided the training of nearly all of the scientists entering the field. Continued support of the university community is therefore vital to the long-range future of high energy physics. It is foreseen in present planning that future support of this university community should be strongly maintained and strenghened.

When reviewing the facilities now in operation or under construction, it is difficult not to be struck by how well the predictions for this period made in the 1976 report--"Long Term Strategy for Construction and Operation of HEP Facilities"--have come to pass. See below for an excerpt from that report.

(1) Facility utilization levels of about 70 to 75 per cent.

(2) A 15 GeV x 15 GeV positron-electron colliding beams facility (PEP) jointly operated by SLAC and LBL, using the present SLAC linac as an injector. (SPEAR would be shut down when PEP comes into operation.)

(3) Higher energy electron (and positron) beams on fixed-targets at the SLAC linear accelerator as a result of improvements already underway (SLED). In 1985 the SLAC linac would be operating primarily in this higher energy (20-45 GeV) region for fixed target physics.

(4) A 200 GeV x 200 GeV proton-proton colliding beams facility (ISABELLE) in operation at BNL using the AGS as an injector. With authorization in FY 1978, initial operation would be about 1983.

(5) An AGS proton beam, fixed-target program, up to 33 GeV, with broadened emphasis to include the lower energies, compensating for the ZGS which might be shut down about FY 1981.

(6) A FERMILAB facility which would have had substantial growth in the experimental areas and in its capabilities. Some high energy proton beam, fixed-target capability up to perhaps 800-1000 GeV would be available based on the successful development of the Energy Saver/Doubler. This development would be an important expansion of the FERMILAB capability, hopefully having been successfully accomplished shortly after the operation of the 400 GeV SPS at CERN entered its second, heavily equipped stage in 1978.

(7) A multi-TeV proton fixed-target accelerator and/or colliding beams facility would be nearing completion of construction at FERMILAB.

It would make use of the superconducting magnet and cryogenic developments of the Energy Saver/Doubler program and would use the FERMILAB machine together with the Energy Saver/Doubler as an injector. Construction would have started about 1980 and be scheduled for completion about 1986.

APPENDIX V

In summary, for the five years under discussion we would have the following facilities:

FNAL The Tevatron, 1 TeV fixed target physics.

A 2 TeV Colliding $\bar{p}p$ Facility

BNL 30 GeV fixed target proton in AGS.

400 X 400 GeV p-p. high luminosity, (ISABELLE)

SLAC Linac operating up to 50 GeV - with dropping exploitation for HEP - down to 20% by 1985.

SPEAR - e^+e^- 3.5 X 3.5 GeV - stopping HEP by 1985

PEP - e^+e^- 17 X 17 GeV - running through the decade

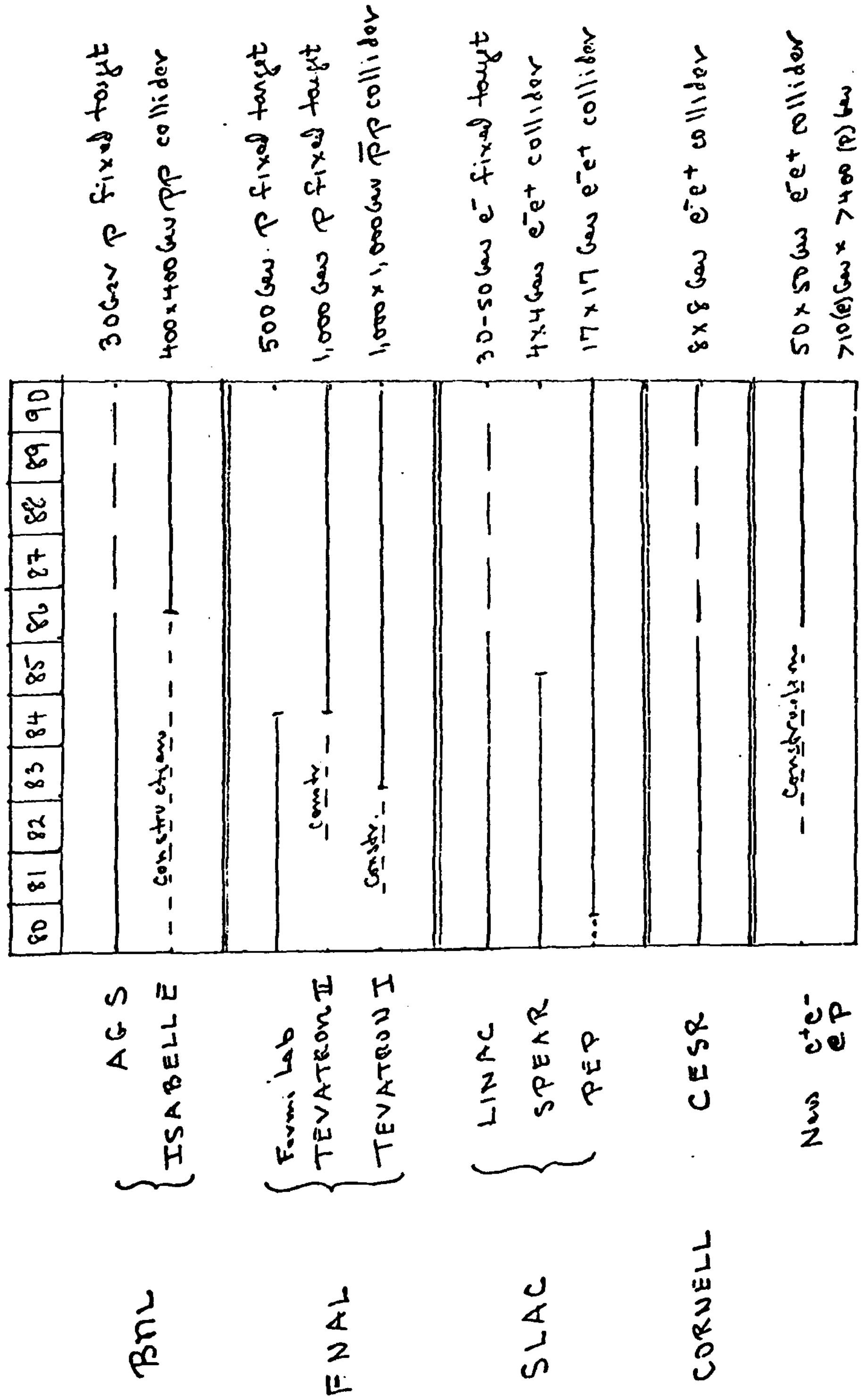
CORNELL CESR - e^+e^- 8 x 8 GeV.

e^+e^- New initiative in the 100 GeV energy region. The only existing proposal for such a machine is the Single Pass Collider from Stanford.
See Figure 2.

3. The International Scene.

Having described the facilities in the U.S., we now consider the international situation. In Figs. 3 and 4 are shown the various accelerator and colliding beam facilities around the world.

Considering only the larger machines in these plots, we observe that the operating machines in the U.S. and Europe are well matched in capability - the CERN P.S. and BNL AGS are comparable, as are the SPS and FNAL's 400 GeV machine and SPEAR/PEP with DORIS/PETRA. The only unique machines are the SLAC linac and the ISR at CERN. However, as we move into the coming generation, we see that successful completion of the Tevatron project will make FNAL a unique facility in that it will have twice the energy of the SPS. The $\bar{p}p$ collider will emerge as the highest energy proton -antiproton collider, being four times the energy of a similar project at CERN. The ISA at Brookhaven will also be unique in terms of the luminosities by roughly three orders of magnitude at such high energies. In the case of e^+e^- machines there are proposals for comparable machines to study the Z^o-- the European one an extrapolation of conventional storage ring technology to probably its last step, expensive and operating not before 1988, possibly 1986, while the U.S. discussion presently center on an exciting, novel idea which is about one-tenth the cost and could be built by 1985--but has only about one-hundredth the luminosity of the European machines. (The luminosities are, however, large enough by far to allow study of the Z^o particle.)


80
81
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86
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90
BNL
AGS
ISABELLE
FNAL
Fermi Lab
TEVATRON II
TEVATRON I
SLAC
LINAC
SPEAR
PEP
CORNELL
CESR
New e+e- e p
30 GeV p fixed target
400×400 GeV pp collider
500 GeV p fixed target
1,000 GeV p fixed target
1,000×1,000 GeV p̄p collider
30-50 GeV e- fixed target
4×4 GeV e-e+ collider
17×17 GeV e-e+ collider
8×8 GeV e-e+ collider
50×50 GeV e-e+ collider
710(e) GeV × 7400 (p) GeV
construction
constr
Constr.
Construction

Figure 2

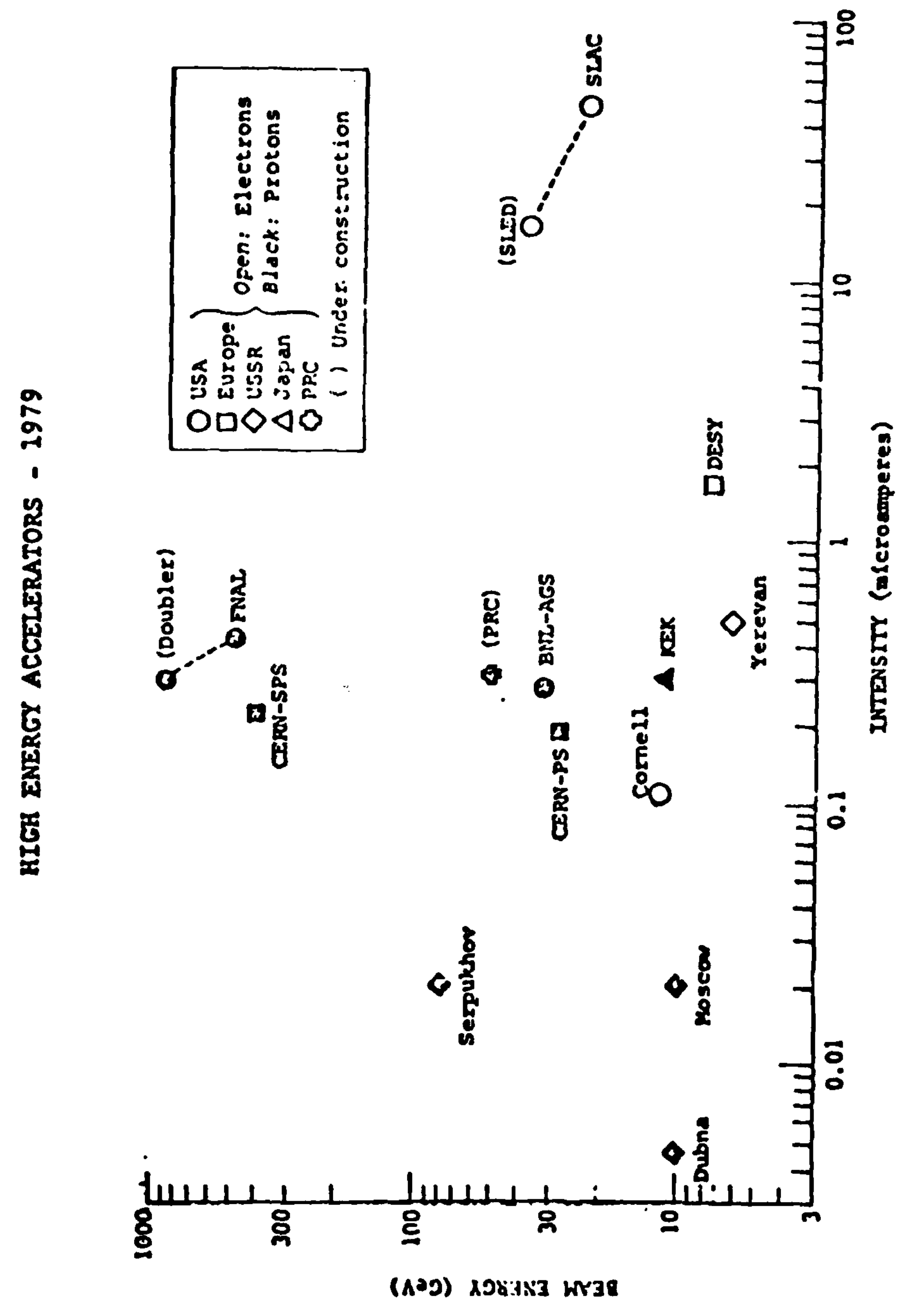

HIGH ENERGY ACCELERATORS - 1979
USA
Europe
USSR
Japan
PRC
Open: Electrons
Black: Protons
( ) Under construction
(Doubler)
FNAL
CERN-SPS
(PRC)
BNL-AGS
CERN-PS
Cornell
KEK
Yerevan
DESY
(SLED)
SLAC
Serpukhov
Moscow
Dubna
BEAM ENERGY (GeV)
1000
300
100
30
10
3
0.01
0.1
1
10
100
INTENSITY (microamperes)

Figure 3

APPENDIX V

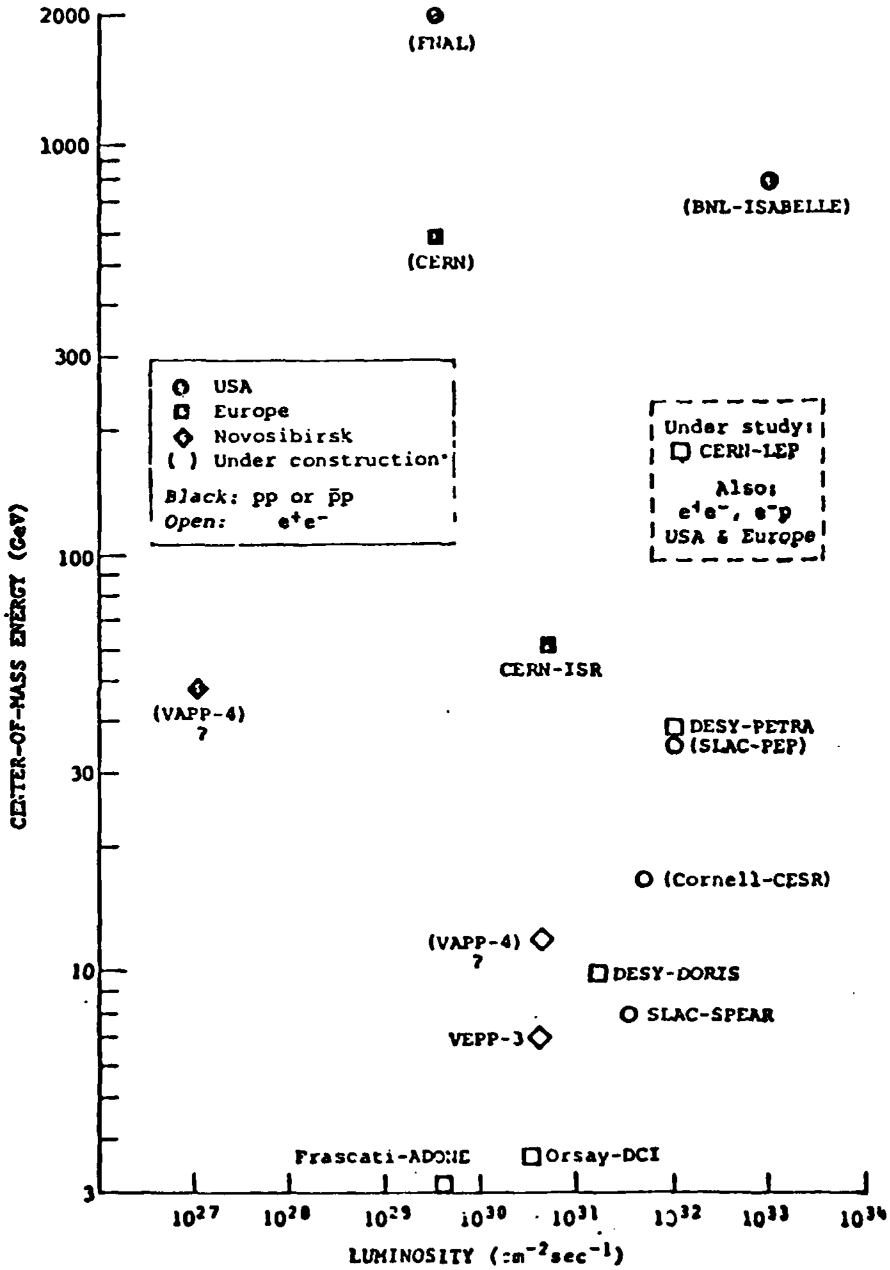


Figure 4
Colliding Beam Storage Rings - 1979

4. Competitiveness with the European Program.

In basic science to be first with a correct result is everything. It is therefore not just sufficient to review the intrinsic merit of the future high energy physics program but it is also necessary to compare it to that of our major competitor, Europe. The Europeans conduct research at very high energies at CERN, Switzerland and DESY, Germany. As noted earlier, CERN is a very large European laboratory whose major facilities consist of a 28 GeV, fixed target, proton accelerator (PS), a 400 GeV, fixed target proton accelerator (SPS), and a 32 on 32 GeV proton-proton collider (ISR). CERN's present capabilities are thus equal that of BNL and Fermilab with regard to fixed target accelerators. The ISR is a machine unique in the world, giving CERN an added capability. While Fermilab had a headstart of several years over the SPS, it is generally acknowledged that the present experimental utilization of the SPS is much stronger than that of the Fermilab 400 GeV machine.

Relevant to this comparison are the support levels for comparable activities. Here one can compare the support for the CERN SPS and its research program with that for the relevant FNAL program in the U.S. Support of the SPS program in Swiss Francs at CERN for comparable activities seems to be about four times that of support of the FNAL program in dollars. If the official exchange rate is used, this overstates the ratio in effort if the undervaluation of the dollar is considered. For purposes of discussion, an effort ratio of roughly 2-1 in favor of the CERN program would seem to describe the actual situation. That there is a very large difference in the level of support between the two institutions is obvious to anyone visiting the laboratories. Instrumentation at CERN is considerably more extensive; data analyses and computer facilities are more powerful; and the support staff available to physicists is about 1.6 times as large.

DESY is similar to SLAC in that the German laboratory has specialized in electron and electron-positron machines. Major facilities include DORIS which is similar to SPEAR but has recently been pushed to somewhat higher energies (10 GeV) for unique measurements of the upsilon particles, and PETRA which is very similar to the U.S. electron-positron collider PEP. Largely because of active governmental support, PETRA has commenced operation more than a year before PEP and has already mapped out many interesting results pertinent to high energy electron-positron annihilations. For the moment the U.S. has clearly lost leadership in a very important area of high energy physics.

The financial support and manpower of SLAC and DESY are roughly the same but there are large differences in program. The program at SLAC covers a considerably broader area, and the energy and intensity of the basic accelerator are substantially higher: 22 GeV (30 GeV with SLED I) and 50 microamperes average for SLAC, as compared to 7 GeV and about 3 microamperes average current for DESY. SLAC also supports an extensive electron scattering program, including the use of polarized beams, and a hadron interaction program using bubble chambers and spectrometers. Thus, while the SLAC and DESY support levels are comparable, SLAC has to pay for the salaries, services and materials for a substantially larger experimental program with a more powerful accelerator.

The future program of the Europeans is very interesting in that it exhibits at present a kind of aggressiveness once associated with the American style of science. One effort that is nearing completion consists of an ambitious program of colliding protons and antiprotons in the SPS for a total c.m. energy of 540 GeV. The expected completion date is ~1981. This collider will possibly provide mankind the first opportunity to observe the Z particle and the opportunity will occur in Europe and not the U.S. It is not a project without risk since the beam intensities will be somewhat marginal. Nevertheless, it is a marvelous technical effort which may give the first view of physics in this new higher energy regime.

Beyond the CERN proton-antiproton collider, the main emphasis in Europe will be on the construction of very large electron-positron colliding beam facilities. This is in marked constrast to the U.S. program with its present emphasis on high energy proton accelerators. The interesting consequence of this apparent divergence may be the emergence of two large research efforts which will be largely complementary rather than competitive. At this point it is difficult to know which of the two efforts will yield the more interesting results. The time scale and parameters of the new European initiatives are not yet fixed. At CERN one might expect a 150 GeV c.m. energy collider by around 1988. Similar efforts at DESY would be at lower energy (∿70 GeV) but at an earlier time (∿1985).

Although Western Europe is considered the main competition, the high energy activities in the Soviet Union must also be considered. In this country the main emphasis has been on fixed target proton machines although there has been some activity in the e^+e^- colliding beam domain (VEEP series of machines). The 80 GeV proton synchrotron at Serpukov is the main facility in the Soviet Union. There has been recent approval for the construction of a much larger proton fixed target machine, UNK, of approximately 3,000 GeV (3 TEV) laboratory energy to be built over the next eight years at Serpukov. This accelerator will compete with the Fermilab TEVATRON II and possible PENTAVAC (see section E).

The Japanese have also entered the high energy sphere by constructing and now utilizing a 12 GeV proton synchrotron (KEK). Future plans are now being crystalized, these consisting of a 30 x 30 GeV e^+e^- collider, with a two-three year construction schedule commencing in 1982, to be followed in the late 1980's by TRISTAN, an ep collider 25 GeV(e^-) x 600 GeV(p).

It should also be noted that the Peoples Republic of China has approved construction of a 50 GeV proton synchrotron to be built near Beijing the completion date being in the mid 1980's.

APPENDIX V

In conclusion one can summarize the international competitive situation as being id the main between the U.S. and Europe and that at the moment and for the next few years the Europeans will probably have an advantage in most areas of high energy physics. Whether or not this advantage translates into major discoveries remains to be seen. Over the longer term the two programs may diverge and become somewhat complementary. This may be of greater benefit to mankind as a whole, although reduced competition generally means a slower rate of discoveries.

5. Strengths and Weaknesses of U.S. High Energy Program.

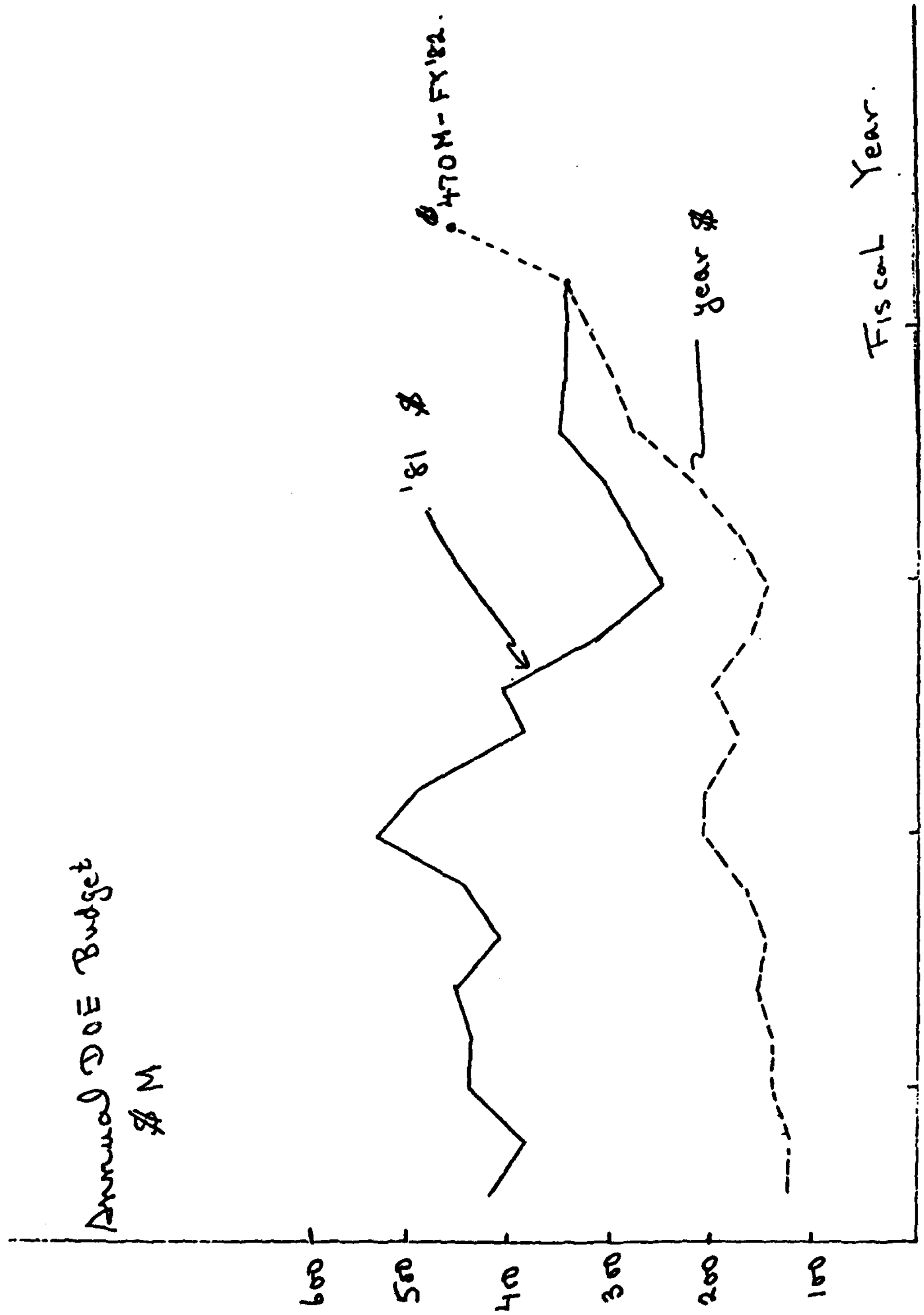
We now will briefly review what we consider are the strengths of the U.S. High Energy Program. With the constant monitoring of high energy activities via HEPAP and ad-hoc panels it is not surprising that the program is well balanced in a variety of ways. From a geographical point of view, there are major facilities in the west, mid-west and east coast of the U.S. The total funding is well distributed among these different regions of the country. Although by necessity a large portion of the funds have to go into operating, upgrading and constructing the large facilities, their utilization nationwide, as measured by nearly any reasonable yardstick is ~70% by University groups and 30% by Laboratory groups.

When one considers the type of facilities available and under construction, again there is a good balance. The whole domain of (e^+e^-) collisions from a few GeV to 30 GeV is covered by a combination of SPEAR (SLAC), CESR (Cornell) and the recently completed PEP (SLAC). This is augmented by the lepton fixed target efforts at the Linac at SLAC (up to 30 GeV) and the muon and photon beams at Fermilab (≈200 GeV). This variety of machines is complemented by several proton facilities. The fixed target domain is covered by the AGS (BNL) 0-30 GeV and the Fermilab complex up to 1,000 GeV with TEVETRON II. Very high energy collisions are provided by ISABELLE (pp, 400 GeV x 400 GeV) with high luminosities 10^{32}-10^{33}/cm^2/sec and by TEVATRON I, $\bar{p}p$ collider (1,000 x 1,000 GeV) with luminosities of up to 10^{30}/cm^2/sec. One therefore sees that nearly all the variety of reactions and energies are covered by this national program. New efforts are now aimed toward the next generation e^+e^- collider (approximately 50 x 50 GeV) and high energy ep collisions (10 - 20 GeV e^- on 400 - 1,000 GeV p's). The former would provide the U.S. response to the European LEP as well as initiate the development of techniques for reaching the high energy region (recall the Livingstone plot of Fig. 1) while the latter fills in an obvious gap in the type of available projectile and target.

The weaknesses of the program are to some extent a reflection of the noted strengths. In order to attain the very high energies in hadron collisions at reasonable cost, it is necessary to innovate and utilize superconducting technology. This implies certain risks in that both the TEVATRON and ISABELLE construction projects involve ≈1,000 superconducting magnets and their accompanying large cryogenic systems. This is the first such large scale deployment of such systems in high energy physics. The successful application of this technology can be the forerunner of future machines while the price exacted for this bold activity may be the longer time scale required to attain full operation. Similar remarks can be made concerning the proposals for high energy ep and e^+e^- colliders where one again is pushing the frontiers in beam bunch operation, manipulation and positioning. Again it clearly appears the way to go to assure the future. However, it is not without risks.

APPENDIX V

As one examines the present state of operations of the various accelerators, both in beam time, support for experiments and R&D efforts, they are clearly marginal. Running these accelerators ≈50% of the time, delaying or eliminating improvements, reducing maintenance, all conspire to produce a less productive and innovative program. It is therefore difficult under these circumstances to properly respond to exciting new physics prospects as well as to explore the many possible instrumental and machine innovations that are required for assuming the major physics breakthroughs. To put the importance of such activities into perspective, it should be noted that it required 10 years of intensive effort to develop the polarized electron source at SLAC which was crucial to the parity experiment verifying the Weinberg-Salam model. There are numerous similar activities at laboratories and universities that are either being postponed or delayed due to lack of support. This lack of flexibility, which can be tolerated for short periods of time, sooner or later strangles the program and leads to stagnation.


Annual DoE Budget
$M
'81 $
year $
$ 470M - FY'82.
Fiscal Year.
600
500
400
300
200
100
'65
'70
'75
'80

6. Conclusions and Recommendations.

The key question now is what are the actual funding levels required to assure that the U.S. high energy physics program is pre-eminent and competitive in the world arena over the next five to ten years.

The basis of these considerations is the "Deutch plan" which has attempted to provide stability and long range planning for the field with support at a level of $325M per year, in FY79 dollars, $300M from DOE and $25M from NSF. As stated in section C above, this level of support was considered marginal from the outset and could not continue for many years without serious implications for our program. The history of the DOE funding pattern since 1960 is shown in Fig. 5, in "then-year" dollars and also for FY81 dollars. One clearly sees the difficulty, with the sharp drop in the budget that occurred for successive years in the early 1970's and lasted until the Deutch restoration. The squeeze on the resources available to the program has been further aggravated by the inflation factors forced on the program in 1980 and 1981, which were quite unrepresentative of the real increase in material costs for our particular field. The net shortfall from the Deutch floor for these two years is in the neighborhood of 20-25 million dollars. This situation cannot be allowed to continue. The funding levels have to be increased to support a competitive, healthy, balanced High Energy research program being actively pursued at the national laboratories and Universities, exploring (and indeed creating) the frontiers of this most fundamental and basic field of science.

In section D 1 above, we reviewed the basic policy and necessary ingredients for achieving a pre-eminent HEP program. Following these guidelines, we evaluated the required changes in funding, specifically for FY82, and also generally for the next five years. The input for these evaluations was the budget information received at HEPAP meetings and HEPAP site visits over the past two years. We arrived at a figure which was a 20% increase over the Deutch plan for FY82. At the January HEPAP meeting, we were requested to go once more through the budget evaluation with input from each of the labs on their evaluation of what was required for a healthy program, and with the DOE and NSF Program Office help on the university users situation. As expected, the laboratories' requests were somewhat more urgent as to the need for increased operations support, and more optimistic in terms of their construction plans than we had reflected in our initial presentation. However, after some iteration and challenging exchanges, the two approaches resulted in quite fair agreement.

Specifically, our recommendations are that the Operations support be increased to raise the utilization of the three national accelerator centers to at least 50%, and to provide some support for R/D on both new accelerator ideas and for new detector developments. The university user group support was also increased substantially. Equipment support was increased substantially to recognize the need for new detectors at the accelerators now in construction (viz., ISA, $\bar{p}p$ Collider, and the Tevatron), and in an attempt to reduce the imbalance between the European instrumentation visible at PETRA, the SPS, and the CERN $\bar{p}p$ experiments, compared to our current experimental facilities. The construction budget allows the FNAL and ISA programs to proceed at a tolerable pace and makes it possible to begin work on a e^+e^- Collider, as a special opportunity.

Table I gives the detailed breakdown of the U.S. high energy physics budget for the period 1982-1986, for both DOE and NSF. The increase in the NSF support appears small, since the FY79 figure included "add-on" money for the construction of the CESR project at Cornell. Therefore, the $30M level for FY82 includes funds to restore the support of the user base program which has eroded somewhat in the last two years.

This level of funding would support a healthy, strong program at the three centers for HEP in the U.S. The operations of all three facilities would be improved allowing good utilization of new accelerators while continuing to exploit the effective programmatic physics of the more mature machines. It would also allow the building of the appropriate detector systems to competitively work in these unknown regions of new high energies. Finally, it provides the U.S. with excellent front line machines in each of the important frontiers in the field, and creates the basis of a truly exciting pre-eminent and timely high energy physics program.

TABLE I

U.S. High Energy Physics Budget ($millions)

		FY82	FY83	FY84	FY85	FY86
		(in FY82 dollars)				
DOE	Operations	280	290	300	305	320
DOE	Equipment	65	95	85	80	60
DOE	Construction	105	105	110	110	80
DOE	AIP/GPP	20	20	20	20	20
DOE	Sub-Total	$470	510	510	510	510
NSF		30	35	35	35	35
	U.S. HEP Total	500	545	545	545	545

E. The U.S. High Energy Program (1986-1990).

Forecasting a short range program is rather straightforward. However, as one attempts to predict the activities further in time it involves a greater uncertainty due to our lack of knowledge of technological advancements that (hopefully) will occur (see Fig. 1 again), and the new physics directions that will be revealed in the exciting frontier exploration to be undertaken in our near future HEP program. Nevertheless, having formed the basis of a healthy, preeminent program for the years up to 1986 it is logical to expand on it for the future years. The program in 1986 should be formidable. ISABELLE should be operational starting to give us a glimpse of high energy rare phenomena. The full TEVATRON should be producing physics in both the fixed target mode, up to 1,000 GeV proton lab energy and with the $\bar{p}p$ collider at a center of mass energy of 2,000 GeV; PEP should be on to a full complement of second generation experiments up to 35 GeV center of mass e^+e^- energies, and the new e^+e^- initiative at 100 GeV should be running or about to start operation; and finally an ep collider might be under construction. This represents a sophisticated array of facilities with an enormous potential for producing new and exciting physics in a variety of reactions. But future prospects are even stronger in that a technological base for even higher energies will have been set. This is due to the initiation and utilization of superconducting technology in magnets, large scale refrigerators and RF cavities as well as lepton collisions involving linacs. At the same time the physics results from the aforementioned machines should delineate the most auspicious path to follow. We now proceed to list the most likely future paths.

In the hadron domain the development of even higher field superconducting dipole magnets, magnetic fields of 10 tesla or more will allow for higher energy fixed target accelerators. In particular a machine at Fermilab of energy 5 TeV or more, the PENTAVAC, would be possible. Utilizing similar techniques pp colliders at energies of 5 TeV on 5 TeV with good luminosities, are also possible. Complementing these proton machines would be a real colliding e^+e^- linac laboratory where one could study with high luminosity the annihilation process up to energies approaching 1 TeV in the center of mass.

Although the present array of machines is providing a great deal of the know-how and experience needed for these higher energy forays, major R&D programs will be necessary during the intervening years. The scale of some of these new machine efforts may be such that consideration of inter-regional collaboration should be reviewed. Finally, we should recall the lessons of the Livingston plot and not preclude the evolution of a new technique or principle which will allow for the construction of higher energy machines at lower unit cost; in fact this may be the most likely possibility extrapolating from the history of accelerator development.

F. Summary

The directions being considered form the foundation of an excellent and exciting high energy program which would establish a preeminent role in this field for the United States. As noted numerous times its strength is in its balance and innovation. The balance is multifold; geographical, national laboratories and universities; electron and proton, and fixed target and colliders. It is innovative in exploiting new technologies and ideas, superconducting magnets and distributed cryogenic systems, colliding linacs and stochastic and electron cooling of beams. A truly remarkable and fruitful program.

The cost for retaining this commanding position for the U.S. in this forefront field of high energy physics is a total of $500M in 1982 in then year dollars. How does this relate to previous funding scenarios? It is precisely the Woods Hole Base Budget (page 4) extrapolated to Fiscal 1982 at an inflation rate of 10%/year. It also corresponds to ≈ 20% increase over the low budget of Woods Hole or the Deutch floor (these are the same) again extrapolated in the same manner to 1982. The conclusion is that a restoration of the budget to the base level and <u>not</u> the floor would allow for this preeminent program. The current level has provided a steadying influence in difficult years and reversed a previous disastrous trend. However, it is clear that in spite of hard decisions in cutting out programs, reducing operations, postponing essential R&D programs, the high energy program is in jeopardy. It can no longer function adequately at below the Deutch floor--a modest step corresponding to a level of $500M (combined DOE and NSF budget) in 1982 is required to restore the program to a healthy state.

Department of Energy
Washington, D.C. 20585

APR 1 1980

Mr. J. Dexter Peach
Director, Energy and Minerals Division
U.S. General Accounting Office
Washington, D.C. 20548

Dear Mr. Peach:

We appreciate the opportunity to review and comment on the General Accounting Office (GAO) report entitled "U.S. Efforts to Lead the World in High Energy Physics--Status and Problems." We believe the report contains a number of unwarranted recommendations and incorrect conclusions with respect to the Department of Energy (DOE). [See GAO note 1, p. 182.]

The GAO recommends that DOE and others form a working group to prepare a policy paper setting forth the program objectives, strategy and appropriate funding levels. We disagree. Although the Department has no objection to a new national study of priorities in basic science, it should be noted that adequate program planning and strategies already exist and, through the annual budget process, involve all concerned federal agencies, the Office of Management and Budget (OMB), the Office of Science and Technology Policy (OSTP) and the Congress. The problem is designating priorities among the federally funded basic research efforts; there currently is no basis from which to proceed on this aspect of the problem.

A policy and plan for the high energy physics program were developed late in 1977 and are now being followed. They were based on (1) a series of studies at Woods Hole in 1974, 1975 and 1977 by a subpanel of the High Energy Physics Advisory Panel (HEPAP), (2) numerous considerations by the HEPAP and (3) a study report, "Long Term Strategy for Construction and Operation of High Energy Facilities" issued by the Energy Research and Development Administration. The policy objective is to maintain U. S. leadership in high energy physics research. ("Leadership" is used in the sense of retaining the U. S. record of outstanding achievement in this area of basic research.) The plan is for a three-pronged approach to higher energy: three major U. S. accelerator centers focusing on (1) research in the areas of electron-positron colliding beam facilities, (2) proton-proton (proton-antiproton) colliding beam facilities, and (3) very high energy proton fixed target facilities. Studies already are underway through HEPAP to lay out an HEP strategy for the 1980's and beyond.

DOE and OMB have agreed on this plan. Extensive presentations on the plan have been made to OMB and the OSTP. It is the basis on which budget presentations to OMB and the Congress have been made. It forms the foundation for the understanding between DOE and OMB on the funding of high energy physics and the basis on which the high energy physics program operates. Program strategy and planning are continuously reviewed and updated. DOE and National Science Foundation (NSF) program managers meet with HEPAP approximately five times a year for this purpose.

DOE funding for high energy physics is based on the DOE/OMB understanding of support at the level of $300 million (M) per year for the total program in fiscal year 1979 dollars. The plan provides for annual adjustment of the $300 M base to take into account the effects of inflation. This was the minimum amount which HEPAP and DOE felt was required to carry out the strategy and plan to maintain a viable and productive national program. The understanding was based on the HEPAP agreement to this level as a floor level for high energy physics funding. Experience since that time (1977), together with the very high rate of inflation, the very high power-rate cost increases, and the fact that funding has not kept up with the inflationary effects, has strained the program greatly. Some increment in funding levels appears necessary to carry out the present plan and strategy--but not the large increases indicated in the report.

The GAO also recommends that DOE include accelerator development projects estimated to cost $5 million or more in its annual budget requests on a line-item basis. We disagree with this recommendation. It is impossible to know in advance whether a particular line of accelerator R&D is going to be successful and attractive. Some will succeed and others will turn out to be unattractive or of lesser priority. It would greatly restrict the essential flexibility for a basic research effort and cause long delays in the activity if such R&D efforts would have to be identified two to three years in advance as budgetary line items.

Another GAO recommendation is that DOE program managers provide, consistent with agreed upon strategy, guidance to laboratories on the amount of research support to be provided to university researchers. We believe that the recommendation is unwarranted. Primary support for university users comes from research contracts awarded directly to universities by DOE. The laboratory research subprogram funding, which GAO analyzes in Chapter 3 of the report, provides user-equivalent costs for the in-house laboratory research groups. The laboratory in-house staff provides some support assistance to users; however, this is a small part of the laboratory's assistance. The main support provided to users through the laboratory is the beam time and set-up of major apparatus, provided through the Facility Operations Subprogram. These services are provided to all approved experiments without regard to where researchers are located. It is true that user support has been less than optimal because of budgetary constraints. However, the program balance is proper within these constraints.

[See GAO note 2, p. 182.]

Finally, the GAO recommends that program managers consider the amount of equipment support needed for specific experiments, consistent with the agreed upon strategy. We disagree with the necessity for a recommendation on this point. The suggested consideration is a regular part of the DOE program review and funding allocation process. Overall funding constraints have made it necessary to fund some experiments at less than optimal levels. However, we believe that scientific priorities are being properly assessed in achieving a program balance.

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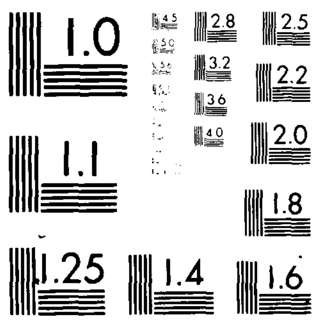

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In addition to the above comments regarding the recommendations for DOE action, we disagree with conclusions in the report concerning allegations of inadequate coordination in enhancing laboratory capabilities and R&D efforts, inadequate funding of physics research, and undue emphasis on scientific competition with Western Europe. We also believe planning scenarios used in the report are unrealistic. The report does not adequately recognize the importance of management flexibility in making judgemental trade-offs of limited fiscal resources in Federal sponsorship of individual R&D projects and the state-of-the-art prototype nature of accelerator facilities. We would be pleased to arrange for meetings between your staff and the program offices involved to discuss these differences.

We appreciate your consideration of these comments in the preparation of the final report and will be pleased to provide any additional information you may desire in this matter.

Sincerely,

for Jack E. Hobbs
Controller

GAO note 1: The report's title has been changed to "Increasing Costs, Competition May Hinder U.S. Position of Leadership in High Energy Physics."

GAO note 2: The subprogram funding which appeared in chapter 3 of the draft report has been deleted from this final report. (See p. 108.)

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

OFFICE OF THE DIRECTOR

March 17, 1980

Mr. Harry S. Havens
Director, Program Analysis Division
General Accounting Office
441 G Street, N.W.
Washington, D.C. 20548

Dear Mr. Havens:

This letter is in response to your invitation to comment on the draft report entitled "U.S. Efforts to Lead the World in High Energy Physics - Status and Problems." I will restrict my comments to issues raised in the draft which are directly related to the National Science Foundation projects and procedures. This does not imply agreement with any parts of the draft report upon which I do not comment. [See GAO note, p. 195.]

The NSF support of elementary particle physics (high energy physics) is about eight percent of the total federal support to this field, with the remainder supplied by the Department of Energy. The Foundation's program is entirely a university-based program. It supports one major accelerator, the Cornell Electron Storage Ring (CESR), which is available to outside users, and about one third of the university groups performing experiments on this and other facilities. NSF staff coordinates this program with DOE programs and participates in meetings of the DOE's High Energy Physics Advisory Panel (HEPAP) and in long range planning studies such as those held at Woods Hole in 1974, 1975, and 1977. In addition, the NSF's Advisory Committee for Physics performs oversight reviews of the NSF physics programs in a national perspective and advises on long range planning and balance among several areas of physics. The most recent oversight review of the NSF Elementary Particle Physics Program was conducted January 31 - February 2, 1980. In addition, the National Science Board must specifically authorize each project with an annual funding rate in excess of $500K per year before the project is undertaken.

My response will be kept as brief as seems reasonable in the light of the issues to be addressed.

the late 1940's with various accelerators developed and built by faculty and staff under the leadership first of Professor Robert Wilson and later of Dr. Boyce McDaniel. The last of these (until the storage ring project) was a 10 GeV (later upgraded to 12 GeV) synchrotron completed under NSF auspices in 1968. By the mid 1970's the operation of this facility and the associated in-house research program were being supported by NSF at an annual rate of approximately $4.5 million, about equally divided between operations and research.

By then it was becoming clear through reviews and site visits that consideration would shortly need to be given to the future of the laboratory. While the facility was operating at a utilization rate of the order of 80% and performing good quality research, it was possible to project the anticipated program a few years and to recognize that the synchrotron program had a limited useful and effective lifetime for elementary particle physics research.

Meanwhile, in late 1973, startling discoveries were being made at Brookhaven and at SLAC. Suddenly, the electron storage rings had become the premier machines to study and search for new quarks and states of matter by means of electron-positron collisions at particular energy ranges.

Cornell scientists, in looking to the future of their laboratory, considered moving into the promising electron storage ring area where higher priority and potentially interesting physics lay. They recognized further that a natural, straight forward, and economical way to achieve this lay in modifying the existing facility primarily through the addition of a ring of magnets and vacuum chambers which could store electrons and positrons injected from the existing synchrotron.

In early 1975, Cornell submitted a proposal to the NSF to add a storage ring to the laboratory's facilities. It was a construction proposal, including expansion of the north experimental hall, but not including major detectors or computer equipment for data analysis. The synchrotron program would be continued in part during the construction phase and perhaps beyond. The project as then proposed would have cost $16.8M plus funds for two major detectors, additional computer equipment, and continued operation of the synchrotron. This proposal was considered by the 1975 HEPAP Future Facilities Subpanel and was found meritorious in terms of science to be studied, technical soundness and economy, but was only included in an "optimum" funding level for the national elementary particle physics program. NSF then indicated to Cornell that it could not continue to consider the proposal in the form presented. Subsequently, Cornell reconsidered the project and in late 1975 submitted a revised proposal which eliminated almost all plant construction and suggested redirection of the entire effort of the laboratory into the three year conversion project. In addition to the storage ring itself, the plan called for additional computer capabilities and for a major detector for the south area. The total cost was $20.7M or about $5.7M incremental to the approximately $15M NSF would have provided to Cornell over the three year period for operations and research with the unmodified synchrotron. This proposal was enthusiastically endorsed by the vast majority of mail reviewers. All recommended funding it and all of those who checked an overall rating box checked "Excellent." The balance of the reviewers' opinion is incorrectly represented in the draft report by the single quoted capsule:

> "My opinion is that if the extra funds in question can be added to the program for that purpose, then I would unquestionably go ahead. If the funds have to be taken from the rest of the program then, from the national point of view, I believe they would be better spent elsewhere...."

Passages from other reviews are more representative of the majority, e.g.:

> "As described in the present proposal the Cornell Electron Synchrotron has been extremely fruitful in probing the electromagnetic phenomena of elementary particles. I am satisfied that the present staff is capable of accomplishing the feat they proposed in the proposal and I would support the construction of a colliding beam facility at Cornell enthusiastically. I am aware that the funds for this construction must come from a limited budget of NSF for High Energy Physics. I think this machine is promising enough for me to recommend approval of this proposal even if it means sacrificing some part of ongoing High Energy Physics programs to some extent."

> "Also for the modified proposal, the view of the Low panel* that 'the facility proposed by Cornell would provide a valuable addition to the U.S. HEP program in a field which has proven rich beyond all expectation,' is and remains my own. Even after the Woods Hole meeting, preliminary evidence strongly suggests that the structures to be uncovered are even richer than was known then. There is no doubt in my mind that a machine in the Cornell range is very much more than a 'double check facility' for other

machines. The amount of work to be done is so staggering that this machine will be a very welcome addition to a heavily burdened area of research. I may add that if there is one safe bet for creative new results, it is in the area of e^+e^- colliding beams."

* 1975 Future Facilities Subpanel of HEPAP, chaired by F. Low.

"In view of the fact that the datataking rate in storage rings is quite small, that a storage ring can only work at one distinct energy at a time no matter how many interaction regions it has, and in view of the very large energy range and physics range to be covered, there is no fear that this facility could present an unnecessary tripling up of effort even if both PEP and PETRA should be built. I conclude, therefore, that the physics potential and merit are amply proven. There is also no question about the competence of the investigators, who are internationally known to be among the finest in the field."

In addition to the mail review, a site visit with technical reviewers was conducted in May 1977, prior to requesting authorization from the National Science Board to proceed. One excerpt from the reports of the site visitors makes an important point about the need for an accelerator in the CESR energy range:

"The CESR project is designed for 8 GeV per beam with a luminosity of 10^{32}. Correspondingly, in the region of 4 to 8 GeV the luminosity is 4 times that which could be obtained at PEP or Petra. A priori it is not possible to anticipate where, in these new energy regimes of e^+e^- physics, the most interesting results will appear. It is important that the whole energy region be covered with machines which can produce the highest luminosity. The Cornell CESR fills a conspicuous gap especially since the most interesting physics is not necessarily associated with the highest energy."

The above quotes are included along with many others in the documentation (NSB-77-282) sent to the National Science Board. This material was reviewed by all levels of NSF management to assure that the balance of opinion stated in it accurately reflected the full content of the scientific and technical appraisal of the project.

HEPAP also strongly endorsed the Cornell proposal in letters from its Chairman to the NSF dated May 6 and August 3, 1976, the latter quoted here:

> "HEPAP wishes to go on record as endorsing strongly the Cornell proposal for the same reasons that I expressed in my May 6 letter, ... We are happy to see the support given so far by NSF for this project, and we enthusiastically urge that this support continue. We believe that this excellent proposal offers very much to the field of high energy physics, and we commend the Cornell staff for its imaginative contribution."

On the basis of these reviews, NSF made tentative plans to proceed with the revised project. The NSF intentions to fund the conversion project were explicitly included in the "FY 1978 Preliminary Plans and Estimates" submitted to OMB in May 1976 and in the NSF's "FY 1978 Budget to OMB." The latter document referenced the project in at least four places with language such as that used on page C-III-2:

> "The Elementary Particle Physics program will provide initial support for the conversion of the Cornell University synchrotron to colliding beam operation. The estimated first year cost of the conversion is $3.2 million above the laboratory's annual operating budget. The $2.08 million increase for this program will cover roughly two-thirds of the needed increment. The remaining $1.0 million will be generated at the expense of the base program in Elementary Particle Physics."

Thus, there is no basis for the claim that NSF "sneaked" the project by OMB. Similar language appeared in several places in the "FY 1978 Budget to Congress." For example, on page B-III-5 the NSF stated:

> "The Elementary Particle Physics program will provide initial support for the conversion of the Cornell University synchrotron to colliding beam operation. The estimated first year cost of the conversion is $3.2 million above the laboratory's annual operating budget. The $2.73 million increase for this program above the FY 1977 level of $19.37 million will cover roughly two-thirds of the increment needed for the Cornell conversion and will allow a start on instrumentation required by user groups for experiments at colliding beam facilities, especially at the Proton-Electron

> Positron (PEP) Colliding Beam Facility being constructed at the Stanford Linear Accelerator with ERDA funding. The remaining $1.0 million will be generated at the expense of the base program in Elementary Particle Physics."

Questions from congressional staff early in 1977 indicate they were aware of NSF's intentions regarding the conversion. No objections were raised either by the OMB or by the Congress in considering NSF's FY 1978 budget. Discovery of the upsilon particle at Fermilab in early 1977 greatly enhanced scientific excitement over the energy range to be covered by CESR. In July 1977, the National Science Board authorized proceeding with the conversion. The synchrotron was shut down in late September and the conversion began on October 1, 1977.

Current Status of the Conversion. The Cornell 12 GeV synchrotron has been converted to an electron-positron colliding beam facility and began operation for experiments in October 1979, six months ahead of the original schedule. The total cost of the conversion is estimated to be $20.3M, about $400K less than the original estimate of $20.7M, by October 31, 1980. This is remarkable in view of the unexpected inflation in this period. The task included installation of a storage ring in the existing synchrotron tunnel and fabrication of a major detector, CLEO, in the south experimental area. Experiments are also proceeding in the smaller, north experimental area. An active research program, involving Cornell physicists and users from seven outside universities, is being pursued in the energy range of the upsilon (Υ) family and other particles with the b quantum number. The Advisory Committee for Physics, in its recent 1980 report on the NSF's Elementary Particle Physics Program, concluded its discussion of the CESR facility with:

> "The Cornell staff are to be commended for an outstanding technical achievement in building the machine. Although there was not initial unanimity in the physics community about the desirability of building CESR, the outcome speaks well for the foresight of NSF and its program officers in deciding to go ahead with the construction. As a result, the United States has an opportunity to dominate studies of Υ physics for the next five years."

The NSF will provide $6.7M in FY 1980 to Cornell for the operation of CESR, for the Cornell in-house research program, and for minor modifications to the storage ring to complete the conversion ahead of the scheduled date of October 31, 1980. The funds provided in FY 1980 for completion of the conversion are included in the $20.3M estimated total cost given above.

NSF Management of the Conversion. The NSF and Cornell negotiated a cost reimbursement contract for "Conversion of the Cornell Electron Synchrotron into an Electron-Positron Colliding Beam Facility," NSF-C537, Amendment 35, effective November 1, 1977, with an expected completion date of October 31, 1980.

The Foundation has complied with the requirements of FPR 1-18 applicable to construction contracts and for cost type contracts. In regard to accountability, the Contractor has to comply with Allowable Cost and Payment clause, Limitation of Cost, Audit and Records, Examination of Records, Negotiated Overhead Rates - Predetermined, Davis Bacon Act (40 U.S.C. 276(a)-27a-7), Contract Work Hours and Safety Standards Act - Overtime Compensation (40 U.S.C. 327-333) Apprentices and Trainees, Payroll and Basic Record, Etc.

Once a contract is awarded, NSF monitors it through the NSF program director and the contracting officer in accordance with the terms of the contract. The contract contains a Technical Direction clause which designates the NSF program director as responsible for review of technical progress through site visits and evaluation of quarterly reports. In addition the contract requires that the contractor promptly notify the contracting officer in writing of any events which substantially alter the work statement and shall not proceed until written approval is obtained from the contracting officer. The contract also contains approval requirements for purchase of permanent equipment, subcontracts, and consultant costs. In addition to the above, the contractor is required to comply with the Limitation of Cost Provisions which protects NSF against any cost increase.

The contract details the requirements for financial and technical reporting. Cornell is required to submit:

Monthly financial reports in a detailed, specified format,
Quarterly technical reports,
Annual summary reports.

The financial reports clearly separate budgets and expenditures for the various components of the project into more than 100 elements. The required reports are being submitted by Cornell and are reviewed by NSF staff as they arrive.

In response to the statement that NSF does not place representatives at the site, it should be pointed out that NSF does not have the necessary staff to do this; thus NSF relies on the use of the program manager, site visits made up of experts in the field, close cooperation with the Contractor and enforcement of the terms and conditions of the contract, to resolve technical or cost problems. Again it must be pointed out that NSF does have agency procedures for the award and administration of contracts.

NSF took teams of technical reviewers to Cornell to assess the project in:

May 1977 (prior to the conversion),
May 1978 (during the conversion),
June 1979 (prior to seeking operating funds for CESR).

In addition, there were numerous site visits at other times during the period by NSF program officers to monitor progress.

Additional program staff was hired to assist the Elementary Particle Physics program director in the intensive monitoring and approval activities during early phases of the conversion. The combination of efforts of the program manager and of the contracting officer would have allowed early detection of problems which could lead to delays or cost increases.

In FY 1978, NSF speeded up the funding rate, enabling the contractor to purchase long lead items and resulting in an advance of the schedule for beginning experiments and an eventual cost savings to the government.

The draft report states "that the required fiscal controls, as set forth in contract are in place and they are working." The assertion that NSF has little management control over the construction activities is not correct. Thus, it is our position that the NSF's contracting and monitoring procedures are adequate and appropriate, even for projects as complex as the CESR conversion. We do not concur with the recommendation that additional contracting procedures are necessary for projects such as this.

GAO Report "Modernization of the Nevis Synchrocyclotron Facility" (PSAD-78-103)

On May 23, 1978, the GAO issued its report on the modernization of Columbia University's Nevis Synchrocyclotron Facility, which had supported some of the most illustrious discoveries in U.S. scientific research in the past quarter century. The draft report references this study as the basis for its concern over NSF's accountability and monitoring of construction projects. As stated in earlier responses to the report on the Nevis Synchrocylotron facility, the

Foundation did not accept certain conclusions of that report, accepted other suggestions, and noted that the GAO recommendation "that the Director of the National Science Foundation terminate funding for the synchrocyclotron project" was already being carried out independently by the NSF.

On November 14, 1977, Columbia University and National Science Foundation staff negotiated an orderly procedure for termination of the synchrocyclotron project by mid-1978. In accordance with this plan, no funding provision for operation of the Nevis synchrocyclotron was made in the National Science Foundation's FY 1979 Budget Request to the Congress.

Many of the questions posed in the GAO report on the Nevis Synchrocyclotron already were answered by the NSF on September 30, 1976, in oversight hearings before the House Subcommittee on Science, Research and Technology. The transcript of those hearings contains extensive additional information concerning the history of the project and particularly concerning some of the detailed management devices which we have adopted, some of them in response to problems encountered on the Nevis project and some of them evolving from experiences with our more successful projects.

The success of the CESR project attests to the efficacy of these new devices.

Recommendations Concerning the NSF Budget Structure

In connection with the recommendation that construction projects and accelerator development projects with total estimated costs over $5M be identified as line items in the NSF budget, we note (1) the Cornell conversion project was discussed explicitly, with budget numbers, in the text of relevant budgets to OMB and to the Congress, and that (2) the NSF's budget structure has been developed in discussion with staffs of Appropriation Committees which apparently find the present budget structure to be satisfactory. Therefore, in the absence of other guidance from Congress, NSF does not concur with this specific suggestion on the budget structure.

In 1978, the House Science and Technology Committee asked NSF to devise more specific procedures for examining so-called "big science" items before including them in the budget request to Congress. In February 1979 the Foundation notified the Committee of the policies and procedures it had adopted for reviewing the scientific justification, overall construction and operating costs, management structure and other aspects of "big science" projects--defined to include large capital investment items such as development of accelerators. These policies and procedures were very favorably received by the Committee and provide as much budget visibility and long-term cost information as the recommended line-item budgeting device. Line-item budgeting would complicate the budget process and reduce management flexibility without providing any additional information beyond what our budget documents already call out.

We also note that GAO Report PSAD-80-25 on "Financial States of Major Federal Acquisitions September 30, 1979" used a uniform threshold of $25 million to identify civil agencies' major acquisitions. We suggest that definitions for such thresholds which differ from one area of science to another would be confusing.

Other Recommendations in the Draft Report

The draft report also recommends that the program managers provide guidance to laboratories as to the amount of research support to be provided to university users. NSF already does provide such guidance to Cornell and the program officers attempt to work together with the managements of other laboratories, when appropriate, to provide for the needs of university users.

Likewise, the NSF believes itself to be as responsive as possible within funding constraints in providing equipment support to users for specific experiments. In elementary particle physics, NSF has provided equipment funds to users participating in experiments at CESR, at Stanford Linear Accelerator Center including experiments planned at PEP, at Fermilab, and at Brookhaven.

In conclusion, I summarize the NSF position as follows:

--NSF acted openly and properly in undertaking the CESR conversion project, being guided by the best scientific and technical advice available to the Foundation at the time the project was actually undertaken. The project was endorsed by mail reviews, by site visitors, and by HEPAP, and was approved by the National Science Board. The outstanding success of this project attests to the correctness of this decision. The CESR began operating for experiments in October 1979, six months ahead of the original schedule, and the total cost of the conversion is currently estimated at $20.3M, $400K less than the original estimate of $20.7M. This new facility is made available for research in high energy physics for about $5M more than NSF would have provided to Cornell for operation of the unmodified synchrotron during FY 1978-80.

--NSF views its contracting and monitoring procedures as appropriate and adequate to construction projects such as the CESR conversion. Concerns based on analogy with the GAO report on the Nevis Synchro-cyclotron project are unfounded.

--NSF does not concur in the specific suggestion that accelerator development or construction projects costing $5M or more need necessarily be identified as line items in the budget. The Foundation's policies require explicit long range planning in these areas and provide OMB and Congress with full and early information on major NSF projects without the complications inherent in line-item budgeting.

--NSF undertakes formalized long range planning, including for fields such as high energy physics, which are heavily dependent on centralized facilities. NSF program staff involve themselves strongly in the planning activities for the field of elementary particle physics. Any plans which are developed must continually be reviewed in the light of changing scientific, technological, and economic trends as well as overall Foundation and national priorities.

--NSF is continually concerned about the balance of funding among different fields of science. This is an extraordinarily difficult and challenging problem, involving all levels of NSF staff, the National Science Board, and the scientific community. Constant attention to this question is warranted.

--NSF feels itself to be in compliance with the recommendations on advice to Cornell regarding service to university users and on provision of equipment to university users for specific experiments.

If I can be of further assistance in your review of the contents of this draft, please do not hesitate to call on me.

Sincerely yours,

Richard Atkinson

Richard C. Atkinson
Director

GAO note: The report's title has been changed to "Increasing Costs, Competition May Hinder U.S. Position of Leadership in High Energy Physics."

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF MANAGEMENT AND BUDGET
WASHINGTON, D.C. 20503

MAY 15 1980

Mr. Allen R. Voss
Director
General Government Division
United States General Accounting Office
Washington, D.C. 20548

Dear Mr. Voss:

Thank you for the opportunity to comment upon the draft GAO report "U.S. Efforts to Lead the World in High Energy Physics---Status and Problems."
[See GAO note, p. 197.]

In general, we believe that the draft report is based upon an incorrect assumption, namely that the U.S. high energy physics program must be designed and supported with the goal of maintaining world leadership in all aspects of this field of science. We do not believe this is an appropriate assumption for a field in which cooperative international efforts are key.

In our view, the U.S. should be competitive in all fields of basic science, but not necessarily the leader in each and every one. Basic science by its very nature is an area of human endeavor in which countries should seek to cooperate and together advance the state of scientific knowledge. This is particularly true in "big science" where increasingly expensive facilities are needed to advance scientific knowledge and where, therefore, cooperative endeavors are called for to an increasing extent.

Despite this caveat, we would note that the U.S. still continues not only to provide a significant share of the world's budgetary resources devoted to high energy physics but also to produce important scientific results. In the view of the key agencies involved in support of this field, namely the Department of Energy and the National Science Foundation, the U.S., with the resources now being applied, is competitive with other countries in high energy physics.

With regard to the OMB agreement with the high energy physics community on a long-range level of funding, we would note that this agreement was not designed to hold down progress in the field but rather to provide needed stability in a field that previously had been subject to annual and somewhat unpredictable fluctuations in support. Stability in and commitment to support is particularly necessary in high energy physics because of the high equipment and facilities costs. The agreement fixed a reasonable funding profile for this program with provision for inflation escalation and for reconsideration of and adjustment to the overall level of support from year to year. Unfortunately the GAO report is clearly aimed at breaking that treaty on the upside. It would be extremely difficult for OMB to agree to a significant increase in funding level especially in times of severe budgetary constraints as we are now experiencing.

There is one further perspective to which I would call your attention. The GAO report calls forcefully for significantly larger funding for high energy physics per se. Certainly any field of basic science could effectively utilize significant budget augmentation. However, the government has the responsibility for maintaining a vital overall basic science effort of which high energy physics is but one part. The GAO dollar recommendation, without addressing corollary impacts or support of other fields of science, will simply be disruptive.

We feel the present funding for high energy physics is appropriate. We cannot support either the draft report's basic premise generally or the substance of the report that calls for significantly higher funding.

Sincerely,

John P. White
Deputy Director

GAO note: The report's title has been changed to "Increasing Costs, Competition May Hinder U.S. Position of Leadership in High Energy Physics."

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF SCIENCE AND TECHNOLOGY POLICY
WASHINGTON, D.C. 20500

March 20, 1980

Mr. Harry S. Havens
Director, United States General
Accounting Office
Program Analysis Division
Washington, D.C. 20548

Dear Mr. Havens:

Dr. Press asked me to respond to your letter, received on February 20, requesting OSTP comments on the draft report entitled, "U.S. Efforts to Lead the World in High Energy Physics -- Status and Problems." Because I recognize that the report is being reviewed by other agencies, I will focus my comments on matters of particular interest to OSTP. My failure to comment in some areas should not be construed as agreement with the report.

[See GAO note 1, p. 201.]

I.

There are a few basic premises that seem to underlie the report's approach to the subject matter. I believe it is important to address these premises explicitly, because I believe they are subject to question.

First, the report is founded on the notion that there is no current national strategy or plan for high energy physics. I disagree. The high energy physics community and the Government, largely because of the expensive nature of the machines and the need to plan research for years ahead, have long recognized the need to develop a strategic plan as to how the program should move forward. As a result, there is an extensive advisory and consultative mechanism, through the High Energy Physics Advisory Panel (HEPAP), that works with the physics community and with the program staffs at the NSF and DoE to develop a scientifically sound program. The strategy that has emerged from this process is clearly reflected in the three different major machines that operate in the United States, and in the long-range budgetary planning. The soundness of the approach is reflected in the remarkable advances that the program has continued to achieve. Indeed, the HEPAP mechanism and the DoE/OMB agreement are seen as models for planning that other disciplines, such as nuclear physics, are seeking to emulate.

Second, it is necessary to respond explicitly to the authors' sense that funding has "been stretched too thinly" (page 96) as a result of the agreement between OMB and DoE. It is important to recognize that,

[See GAO note 2, p. 201.]

for the first time, the agreement has provided needed predictability and stability in the funding situation, enabling the agencies and the physics community to plan ahead with confidence. In my view, these benefits are given too little weight. As a result of unexpectedly high inflation, it is possible that the program is now excessively tight. (Indeed, in recognition of high power costs, there was an adjustment in the FY 1981 budget to allocate some additional funds to high energy physics, beyond those that would arise normally from the agreement.) But high energy physics shares fiscal austerity with many other Federal programs. In these inflationary times, times in which the President and the Congress have emphasized the importance of balancing the budget, it may not be an opportune moment to press for substantial additional funds. Certainly the funding is nowhere near as inadequate as chapter four -- which seems to me to be a compilation of the laboratory directors' wildest dreams -- would lead one to believe. Moreover, like all other areas of research and development, there will be periodic reassessment to assure that the funding is adequate.

Third, the report is permeated with the view that our strategy for high energy physics should be completely governed by international competition. This notion first surfaces in the title and ultimately is reflected in the development of the various alternative strategies in Chapter 5. Although competition is an important element in science, and comparison with foreign efforts can be a benchmark for measuring our own programs, I doubt that "dominating" other countries' programs is, by itself, a useful objective. Such an approach is inherently unstable because of the mutual vulnerability of each country's programs to the initiatives of others. Perhaps our objective should be to maintain a program that systematically addresses the most important problems, and that adequately supports the efforts of our most productive scientists. Such an approach would not necessarily be controlled by budgetary comparisons with CERN. Thus, even if the Europeans and the Soviets were to conclude that funds previously allocated to high energy were best spent elsewhere -- if, by default, we were to "dominate" the world -- I would nonetheless argue that the U.S. should continue its program at its current level because of the importance of the research to our understanding of nature. In sum, even though foreign initiatives should be a factor in our planning, a sound program should be built on more meaningful foundations than keeping up with the "international Joneses."

II.

Let me turn now to the recommendation that is directed toward OSTP. The report suggests that OSTP should coordinate a study to set out a strategy for high energy physics, and to determine the appropriate level for funding the program, balancing its needs against other fields of science. We feel that such a study is unnecessary. As noted above, HEPAP has developed such a strategy in consultation with NSF and DoE, and the HEPAP advice has and will continue to guide our overall program.

A balancing of the needs of high energy physics against other scientific opportunities has already guided both DoE and OMB in determining the stable funding level. Indeed, the budget process routinely requires a balancing of various priorities in science.

III.

Let me conclude by drawing your attention to several other aspects of the report that are of concern to OSTP:

- The report indicates that GAO staff "conducted work at" a variety of American and European sites, including OSTP (page 14). To my knowledge, the only OSTP contact with this study was by way of a brief telephone conversation.

- The draft draws particular attention to the peak in funding in 1970, and the subsequent decline (pages 17-20). The emphasis on the peak year -- and the implication that our current commitment should be measured by comparison to that year -- is particularly inappropriate in the case of high energy physics because of the fluctuating nature of expenditures on facilities. As the chart on page 51 indicates, the peak in 1970 was largely the result of construction. Moreover, perhaps it is worthy of note that funding has increased since 1975 and the effect of the OMB/DoE agreement has been to smooth the previous fluctuations in funding.

- The report alleges on page 44 that U.S. policy toward basic science "emphasizes the funding of those sciences that may help resolve National problems." See also pages 45-46. The source for this allegation seems to be a single sentence from a 1978 memorandum. The Administration, however, has adopted a much broader strategy with regard to the funding of basic science. There has been substantial growth in the support of basic research in the mission agencies, because of the President's commitment to make investments in basic science that will underlie the achievement of agency missions. This strategy is in response to the decline in support over the decade starting in 1967. But this growth has not been at the expense of science with, to use your terminology, intrinsic merit. Thus, there has been continued growth in support for the National Science Foundation in the basic research directorates -- support that is provided to meet scientific opportunities, rather than necessarily to focus on National problems. Indeed, the January budget submission provided a nearly 17% increase in NSF funding of basic science over FY 1980, and over 40% growth in the period since FY 1978.

- The report is very critical of the NSF in supporting the Cornell facility. I understand, however, that the Cornell facility was given enthusiastic approval after the project was re-configured. It was completed for less money than was initially anticipated and will provide data in an important energy regime. This facility seems to be a success.

I appreciate the opportunity to comment on the report, and I hope these comments are helpful.

Sincerely,

Richard A. Meserve
Senior Policy Analyst

GAO note 1: The report's title has been changed to "Increasing Costs, Competition May Hinder U.S. Position of Leadership in High Energy Physics."

GAO note 2: Page and chapter references have been changed to reflect location in the final report.

U.S. Department of Energy
ARGONNE NATIONAL LABORATORY
9700 South Cass Avenue, Argonne, Illinois 60439 Telephone 312/972-5555

March 28, 1980

Mr. J. Dexter Peach
Director
United States General Accounting Office
441 G St. N.W.
Washington, D. C. 20548

Dear Mr. Peach:

Thank you for the opportunity to comment on the draft of the GAO report entitled "U.S. Efforts to Lead the World in High Energy Physics - Status and Problems." Many thoughtful and valid issues are raised in the report and should be discussed by various groups in the scientific community and government. While we agree with some of the conclusions reached in the report, in this letter we focus our comments on those items where our views differ from those given in the report. [See GAO note 1, p. 206.]

There is some confusion about the use of phrases such as world leadership and world dominance. High energy physics is research at the frontier of knowledge, and so the rewards are disproportionately weighted towards a situation where the principal discoveries are given great weight and the fill-in experiments, although important and interesting, are not heavily counted. There are no rewards for coming in in second place. In the breakthrough discoveries, however, it is naive to think that the United States can achieve a dominant position over the full range of options with a budget level that is about one-half that of Western Europe. We can, however, with the Deutch plan of $300M per year corrected for inflation, provide for an essential diversity in the field and also provide unique and frontier accelerators in some areas, and this is basically the strategy that is being followed.

The overall high energy physics options - world leadership, etc. are in the last analysis political matters and are properly decided by appropriate elected representatives in the Congress and by the Administration. It seems most unlikely that the proposed work group (p. 95) of science policy experts, etc. could do more than define various policy options, however, unless it also covered similar policy questions in all fields of basic research in physical sciences and life sciences, including such expensive and controversial areas as the war on cancer and space exploration. Comparisons between different areas of science are almost impossible to carry out in an objective way, and such studies are not likely to lead to results that are operationally useful. Furthermore, it is very implausible that the OMB or other governmental body would

[See GAO note 2, p. 206.]

The University of Chicago Argonne Universities Association

apportion the existing high energy physics resources in a more effective manner than do existing types of advisory and peer review mechanisms within the field.

Throughout the report there is an underlying theme that the field of high energy physics should be run with more central planning. It appears to us that additional central planning would in general have an adverse effect on the field. Indeed, the fact that the U.S. program has remained competitive with that of Western Europe is in large part due to the diversity of style and approach of the U.S. program. An important ingredient to this diversity has been the flexibility given the laboratories to develop their best and most competitive program. Clearly some overall coordination and direction must and is being given to the national program, but the problem of resource allocation in the field is difficult and complex. The community has input through national advisory bodies such as HEPAP, as well as through accelerator experiment selection panels and laboratory visiting committees and user groups, and in our view it is not advisable to provide detailed management from Washington for a research program. There is already general guidance about the amount of research support each laboratory provides to the users and a more detailed control would be counterproductive.

The report emphasizes the fact that the field at present is somewhat overextended with the simultaneous construction of several facilities. This situation is a fluctuation above the long-term average and represents a corrective action after a period of several years with no new construction. The present construction is an investment in the future of the field at the expense of near-term experimental results. It is not surprising that some of the users are unhappy with the inefficiencies this brings to their present programs. However, the decision to embark on major construction activities has been supported by the field through HEPAP, which considers both long-term and short-term aspects of the field. By contrast, the program in the Soviet Union, where new accelerator initiatives are not pushed, is moribund and not contributing to the field proportionally to the funds expended.

There is considerable criticism in the report connected with the construction of CESR at Cornell. Although this machine was indeed judged to be of lower priority than PEP, because of the existing tunnels and the 10-GeV electron accelerator that could be used as an injector, it was built in a cost-effective way and at a total cost comparable to a large experiment. The decision of NSF to proceed with this construction has been fully supported by the outcome. We now have a unique facility, well suited to explore the spectroscopy of the b-quark family and this facility has already produced results of great interest.

APPENDIX X

We agree that accountability through peer reviews is the best method for handling basic research. We do not agree, however, that the development, construction, and operation of accelerators should be treated as procurement items. By their very definition frontier accelerators must exceed and overcome the technical limitations of earlier machines. Because of this, frontier accelerator construction, as well as high energy physics research, is basically a risk-taking enterprise and one must maintain the flexibility to achieve the best results possible. The steady series of past U.S. success in this field were attained under special circumstances and are not guaranteed in the future, especially if the rules are changed. An attempt to decrease the risks by proliferating the governmental "oversight" and "accountability" mechanisms for high energy physics is very likely to be counterproductive.

Indeed, we have already seen this result in the relatively slow construction of PEP compared with the German machine PETRA, and the resulting delay has placed the U.S. program at a severe disadvantage in this area.

The overall sense of the paragraph on pp. 64-66 that accelerator R&D projects at Argonne (during, say, 1974-1979) were primarily aimed at averting the planned ZGS shutdown is grossly misleading. In fact, the primary goals of HEP accelerator R&D at Argonne during this period were:

1) To maximize the capabilities of the ZGS for carrying out the (widely reviewed) physics experiments during its final five years of operation. This included developing the world's first capability for accelerating polarized protons and polarized deuterons to high energy, as well as the world's first operational high energy superconducting beam line. These unique capabilities were essential in the extraction of a maximal amount of physics from the ZGS before its shutdown.

2) To further develop and demonstrate the technique of H^- injection into a synchrotron. After its successful use on both the ZGS and the "booster," this technique was adopted at Fermilab and is about to be implemented at Brookhaven. It also plays an important role in the design of next-generation accelerators, and was an important ingredient in our development of Heavy Ion Fusion concepts which could well lead to a solution of our long-range energy problems.

3) To develop the technology of superconducting magnets for accelerators. This is the Superconducting Stretcher Ring (SSR) project, which as the report pointed out was rejected by DOE three times. It was proposed, incidentally, about

midway through the productive life of the ZGS, not just before shutdown. However, the fourteen magnets were installed not in the accelerator (as stated in the report), but in the world's first (and highly successful) superconducting beam line as mentioned above. Some key issues about the SSR proposal are not mentioned in the report:

a) The proposal was made in explicit response to the recommendation of the 1971 HEPAP report that pilot projects be supported to develop new accelerator technologies.

b) The main goals of the SSR project have not yet been approached anywhere, namely, the achievement of practical experience with high intensity beams in rings of superconducting magnets, the achievement of power savings by avoiding "flat-top" operation of conventional magnets, and the achievement of 100% duty cycle beam for experiments. The power saving concept originated in the SSR proposal and was later incorporated as a basis for the Fermilab Energy Saver.

4) The booster project had three goals - development of new technology for future fast-cycling synchrotrons, use for injection into the ZGS, and long-term use as a pulsed neutron source at Argonne. The first and third of these goals were successfully achieved with broad impacts upon accelerator science and upon pulsed neutron physics experiments. The second was not achieved since the initial booster operation occurred about a year too late to be useful for unpolarized beam operations of the ZGS. It was, and is, Argonne's clearly stated position that the booster project was fully justified by its contributions to accelerator science quite apart from any use of this accelerator to produce beams for experiments. This position has been consistently supported in outside reviews of our program. In fact, at present (1980) this booster is a workhorse source of slow neutrons and has stimulated similar work in England, Germany, Japan, and Los Alamos. Several major research programs in solid state physics are carried out using the booster and it has replaced the CP-5 reactor for this purpose.

Please let us know if we can be of further help to you on this project.

Sincerely,

Walter E. Massey
Director

RED/WEM:jsd

GAO note 1: The report's title has been changed to "Increasing Costs, Competition May Hinder U.S. Position of Leadership in High Energy Physics."

GAO note 2: Page references have been changed to reflect location in the final report.

BROOKHAVEN NATIONAL LABORATORY
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March 18, 1980

Mr. J. Dexter Peach
Director Energy and Minerals Division
U.S. General Accounting Office
Washington, DC 20548

Dear Mr. Peach:

This is in response to your request for comments on the draft GAO report "U.S. Efforts to Lead the World in High Energy Physics - Status and Problems." I appreciated the opportunity to meet with you, Dr. Staats, and the key officials on February 26 to discuss the report. This letter is a follow up on that meeting. [See GAO note 1, p. 209.]

I am particularly concerned about two points that are made in the report, namely that more "accountability" is needed in the high energy program and that too much of the budget has been devoted to machine building in recent years.

As to the first point, accountability always gets presented as an o.k. thing of which we can never have enough. Nevertheless, over accountability results not in better programs but rigor mortis. Several of us tried to show that there is already an incredible amount of accountability in the high energy program through the various review and funding mechanisms that exist. No significant activity in the high energy program has taken place without advance knowledge by the program directors in Washington, by the principal planning entity, HEPAP, and by the community of high energy physicists. Each year extensive reporting of these plans to the Congress has taken place. It is my opinion that high energy physics in the United States is already burdened with too much accounting for itself before, during, and after the fact for the maximum health of the science, and I am surprised that the GAO has come out strongly on the other side. The consequence of their position could be still more centralized direction from Washington, which would be quite counter productive.

The second point, too much of the budget devoted to machine building, is played up strongly and repeatedly in the report but no evidence whatever is given in its support. Without the machines now operating, of course, experimental high energy physics would come to an immediate halt, but without support for new machines, high energy physics in the future would halt. The question of what is the best balance between the two needs in the face of limited budgets is highly technical. It has been hammered out in a series of meetings of HEPAP and its various panels of experts, and the present balance reflects that consensus, not the whims of laboratory directors. As a director of one of the three DOE high energy centers, I assure you that what we do is based on the broadest discussions within the high energy community and not just my personal preferences. The GAO is making a very serious charge of mismanagement here. How they arrived at their judgment I do not know, and the draft report does not explain. I am satisfied that the GAO could not back up their stance with the weight of expert opinion, and therefore I urge very careful reconsideration of this component of the report.

It may be worth restating some remarks I made at our meeting about the traditional handling of R&D funds in DOE and its predecessor agencies. The laboratories involved in accelerator development are all non-profit contractors. Each has had for many years an ongoing program of accelerator R&D supported by an operating budget (since they are non-profit, there is no other way for them to do this). This budget is meant to support generic research in the accelerator art and related matters. New concepts and new ideas for accelerators and experimental equipment arise from this research and can be tested and discarded, or developed up to the stage of implementation. Superconducting magnets, storage rings, colliding beams, new schemes for increasing beam intensities, energy saving, more effective detection systems, and a host of other ideas necessary for new frontiers are developed with these funds. From this work, it is intended that proposals for new machines will be developed, and from time to time each laboratory has done exactly this. These proposals go in as requests for line-item funding of new accelerator construction projects. Many proposals are not accepted or proceed only after repeated submission, while undergoing yearly modifications and updating according to the developments in the accelerator art and the needs of the science. After an accelerator construction proposal has been accepted by the DOE and Congress has authorized construction, the need for accelerator R&D to support the project continues through the construction years in order to build the most effective device, and afterward for the purpose of its upgrading and improvement. The GAO report makes this activity sound illicit and in need of tighter control when indeed it has been functioning exactly as intended and in an efficient and effective manner, in full view of the funding agency, the OMB, and Congress at every stage. This aspect of the report is, I believe, based on a misunderstanding of what is supposed to take place. If its thrust were to be accepted by the Congress, vital flexibility and the capacity of our accelerator builders to work in accordance with the very latest state-of-the-art instead of according to fossilized plans laid down years before would be lost. Also lost would be any hope for the United States to remain competitive on the world stage of high energy physics.

Finally, I must point out a misunderstanding that occurs on page 81 of the draft report under the subtitle "Support from Laboratories." The table here lists certain sums purported to be for the support of university researchers at the three DOE laboratories. In fact, these funds are not that at all - they are the cost of physics research done by the in-house staff of each laboratory. The remainder of the operating budget (around 85 percent of the total in each laboratory) supports the whole high energy community, largely composed of university scientists, because it is this money that operates the existing facilities and performs R&D oriented toward future facilities. [See GAO note 2.]

I have read the comments already submitted by W.R. Panofsky, S.D. Drell, and G.F. Tape, and also strongly back what they have said.

Thank you for the opportunity to comment on the report.

Sincerely yours,

George H. Vineyard
George H. Vineyard
Director

mk

GAO note 1: The report's title has been changed to "Increasing Costs, Competition May Hinder U.S. Position of Leadership in High Energy Physics."

GAO note 2: The table which appeared on page 81 of the draft report has been deleted from this final report. (See p. 108.)

APPENDIX XII

Cornell University
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March 5, 1980

Mr. Dexter Peach, Director
U. S. General Accounting Office
Washington, DC 20548

Dear Mr. Peach:

Enclosed you will find comments in response to your draft report entitled, "U. S. Efforts to Lead the World in High Energy Physics--Status and Problems."

I feel that the report contains a number of serious inaccuracies and misunderstandings. It also draws unjustified conclusions as a result of a lack of technical perspective and knowledge of the field.

Since I cannot reasonably hope to address all of my objections to the report, I have limited my comments to a relatively few points. These fall into two classes: (a) those relevant to the Cornell Electron Storage Ring, and (b) more general comments.

I hope that you find my comments useful and I urge you to make major revisions in the draft before it is published.

Because of the great weight which is attached to public documents originating from your office, I feel it is very important that your final report be correct in all basic aspects. For this reason, I request the opportunity to discuss relevant parts of the report with a cognizant member of your staff. I will come to your offices for such a discussion at any reasonable time in the near future. I very much hope you will find it possible to arrange such a meeting. I await your response.

Very sincerely,

B D McDaniel

B. D. McDaniel
Director

pb
Enc.

GAO note: The report's title has been changed to "Increasing Costs, Competition May Hinder U.S. Position of Leadership in High Energy Physics."

Fermilab

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Directors Office

March 12, 1980

Mr. J. Dexter Peach, Director
Energy and Minerals Division
U. S. General Accounting Office
Washington, D. C. 20548

Dear Mr. Peach:

I thank you for the opportunity to comment on the GAO report on High Energy Physics.

I recognize five substantive conclusions/recommendations in this report, briefly paraphrased:

1. The benefits of world leadership are not clear.
2. There has been an overemphasis on development and construction of accelerators.
3. Policy objectives (presumably measured only by a competitive stance) should be evaluated and implemented only if sufficient funds are available.
4. Not enough accountability (!). Accelerator R&D at the $5M level should be line item.
5. Accelerator utilization levels and laboratory assistance levels to University users should be established by Panels.

I will make my comments on each of these very brief since I'm sure others will comment at length. My numbering:

1. The GAO adopted a set of options which measure competitive status as a criteria for establishing a funding level, rather than scientific merit. Presumably they felt more comfortable with what seemed a more readily quantifiable property. Having done this, they find that the "benefits" of leadership are difficult to evaluate. We would rather defend a program on scientific merit but benefits of world leadership are obvious and historically documented. One cannot ask a gifted scientist to spend many years of his life working with second rate equipment. He is in demand and will go where the facilities are best. Thus was the origin of the Brain Drain of the 1950's and a factor in the strenuous efforts of Europe and Japan (and most recently, the PRC) to catch up.

2. If we have overemphasized construction, which of the present inventory is excessive? Cornell, the only example cited, happens to be in a unique position to do Upsilon spectroscopy and, at this writing, is the only e^+e^- machine in the U. S. getting excellent data. On a cost effectiveness basis it was an inspired investment. A comparison of European (ISR, SPS, DORIS, PETRA) and U. S. (FNAL, SPEAR, Cornell, PEP) accelerator projects in the 1970 - 1980 decade seems relevant. We have, in the past, retired CEA, PPA, Bevatron, Cosmotron and ZGS from HEP.

3. Traditionally policy was formulated by the science community and based upon scientific opportunities. Objectives were established in close communication with the agencies and formulators of national science policy. Community pressure, resisted by the watchers of the purse strings, set the pace. Today we have foreign yardsticks by which we can gauge our efforts. However, many of our Congressional statesmen (witness the Joint Committee on Atomic Energy) gave enthusiastic support to HEP before there was any European or Soviet activity. Competitiveness as a criteria for level of effort would benefit U. S. HEP today when European spending is twice as high. However, the GAO emphasis on this obscured the fundamental issues i.e. the validity of the assertion that HEP, funded at a level of about $400M (1980) will provide a return to society which justifies the effort. To compound the problem, the GAO, by summing the responses of all laboratories, arrived at a cost of "world domination" which is unreasonable.

4. Accountability. The track record is extraordinarily good here. The report just doesn't make a case for increasing the accountability. The only anecdote cited in the GAO report is the case of the Nevis Cyclotron - a medium energy facility which in fact did run into long delays due to technical problems but which was continuously monitored by NSF technical review panels. Contrary to the impression given in the GAO report, it was terminated after completion because of the existence of more powerful facilities at Los Alamos and Zurich. We also plead with you to delete the recommendation on placing accelerator R&D in a line item category.

5. The GAO misunderstood the data of its Table on page 81. The budget allocations for physics research listed are for <u>in-house</u> physicists. The remainder of the lab budgets are for the facility operation and outside user support. Thus Fermilab on the basis of its small in-house efforts does <u>most</u> for the University users. The decisions as to the allocation of Laboratory resources to users and the imposition of utilization levels by an outside Panel would limit Laboratory flexibility and most surely result in less good science.

In summary U. S. HEP has, in the years 1950 - 1970, by any measure of scientific productivity, achieved a great success. Using the European yardstick it is economical. There are problems but they are not addressed by the GAO study. Its major recommendations threaten the success of the discipline by increasing the already impressive problems of getting maximum science for the dollar.

There was an opportunity for the GAO to provide data upon which to base judgements as to the health of the field: I regret that the study did not address the following questions:

1.) Given the inventory of accelerators in the U. S., using various indicators of effort e.g. European levels, historical records, economic utility of past discoveries and technological spin-off, population of practitioners, etc., what is the pay-off versus cost of increasing utilization from, say 50% to 80%? How does this compare to capital investment?

2.) What is the validity of the claim that HEP, in addition to its thrust to account for the basic structure of matter and energy, is an economic benefit to the U. S. due to its technological spin-offs alone? What is the ratio of technological breakthroughs originating in unmotivated research versus programmatic R&D? If it could be demonstrated (and who could do it better than the GAO staff?) that HEP contributes to the economy more than it absorbs, the entire thrust of the GAO report would be changed. Yet this was ignored.

3.) What are the other benefits to the U. S. of a strong HEP program: i) In strengthening the scientific potential for solving shorter range problems ii) In prestige relative to other developed and developing countries iii) In influencing high technology industry with implications for balance of payments, better life for our citizens, etc.?

4.) What are the bureaucratic obstacles to efficient management of the HEP program? What would be the cost effectiveness of a large reduction in numbers of reports required by DOE, in restrictions on operating versus R&D versus construction fund types? How can one reduce the time lag of the system to respond to new ideas? In short, how can one better carry out the basic will of Congress to do excellent science for minimal cost by simplifying the procedures?

Sincerely,

Leon M. Lederman

LML:jw

GAO note: The table which appeared on page 81 of the draft report has been deleted from this final report. (See p. 108.)

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March 19, 1980

Mr. J. Dexter Peach, Director
Energy and Minerals Division
U.S. General Accounting Office
Washington, D.C. 20548

Dear Mr. Peach:

Your letter of February 13th to Dr. Sessler was referred to us for comment.

We have been privileged to review the letter sent to you by W.K.H. Panofsky dated February 27th and agree with the sentiments expressed therein. Further supporting comments fall into the following categories:

1) Accelerator Development

   It is strongly suggested in the report that such work should be a line item if the estimate for the project is $5M or more.

   This procedure would make it difficult to impossible to do Accelerator R&D. A good deal of this work is ongoing fundamental research and development and only becomes directed when a major project is in the offing. Progress reports and reviews of such activities are necessary and useful.

2) Regarding the funding priorities for H.E.P. the report recommends that the OSTP review the program and make recommendations. But this is exactly what HEPAP has been doing well. Many other panels have made many reports. Such studies which might set objectives for the program and allow for accountability over the program can never be more than educated guesses as to the future unknown discoveries, yet to be made, which can radically change the direction of the work.

3) Research Support for University User Groups

   The implication in the report is that University Users get their support from Accelerator Lab Research budgets. This is not only wrong but very misleading. Those research funds are for inhouse groups and depending on whether they are strong groups or not they get more or less money. The percent of the laboratory operating budget is irrelevant and it is unrelated to the number of experiments being done at that laboratory.

4) Lastly there seems to be little note taken of the fantastic results obtained in HEP the past few years and how those results have validated the decisions taken earlier to proceed with the projects that were built. Critical comments were made about those decisions in the report without regard for the physics output. The auditors notions about accountability would insure that the money would be spent unwisely.

Sincerely,

Robert W. Birge
Associate Director
Physics, Computer Science & Mathematics

Hermann Grunder
Division Head
Accelerator & Fusion Research Division

RWB:HG:jad

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February 27, 1980

Mr. J. Dexter Peach, Director
Energy and Minerals Division
U. S. General Accounting Office
Washington, D. C. 20548

Dear Mr. Peach:

Thank you very much for your letter of February 13, 1980, which I received on February 19. In that letter you transmitted the draft of a GAO report entitled "U.S. Efforts to Lead the World in High Energy Physics - Status and Problems," and requested that I transmit comments within 30 days after receipt. I am giving here some comments on the general methodology and conclusions of the report; if requested I would be pleased to furnish more detailed, page-by-page comments.

A. General Remarks [See GAO note 1, p. 222.]

The report aims to examine the factors which appear to threaten the world leadership in high energy physics which the support and creativity of the field has yielded until quite recently. The first part of the Draft Report is descriptive and historical in nature and also very informative; I have no overall comments until page 48. However I have problems with the material starting at that point. [See GAO note 2, p. 222.]

The Draft Report (p. 48) refers to the funding level of $300 million (in FY'79 dollars) negotiated between DOE and OMB. The Report refers to that level with the comment that "DOE concluded that a $300 million funding level would provide continued leadership (sic) in the field.", and "This level . . . was negotiated between DOE and OMB; endorsed by HEPAP and concurred by OSTP." I do not see how DOE could have certified this level as assuring leadership since HEPAP in its preceding study at Woods Hole had referred to that level as the minimum viable program. It is therefore also misleading to state that HEPAP had "endorsed" that level; "acquiesced to" would be a better term.

B. Innovation vs. Exploitation

The root of most of my problems with the Draft Report is that it does not in any way consider how the output of the U.S. program, as measured in terms of the Science produced, should be gauged, let alone optimized. This lack of output measurement notwithstanding, the Draft Report repeatedly criticizes that "Federal agencies and laboratories have been emphasizing the development and construction of accelerators believed to be needed to provide the capability for further research. Such emphasis appears to have resulted in overemphasis on developing and constructing accelerators." Although this criticism is repeated in numerous sections of the Report (ii ff, 50, 62, 75, 76, 77, 84, 110), I find no substantiation in support of this finding. As a practical matter, if total funding is limited (as it necessarily is), those responsible for the management of the nation's high energy physics program must seek a proper balance between innovating the basic facilities required in support of the program and exploiting the facilities constructed. It would be destructive to give absolute priority to exploitation over innovation, that is to insist that existing facilities be exploited to their fullest possible extent while ignoring the obsolescence of such facilities. Yet this appears to be the value scale which the GAO report implies. [See GAO note 3, p. 222.]

The Draft Report correctly identifies numerous institutions where development and construction of new facilities has been carried out to the detriment of the ongoing operations of the research program using existing facilities. Yet had such new construction not been initiated, the program would have faced certain obsolescence and consequent loss of leadership and productivity. It can easily be shown, based on the analysis of the time cycles involved, that a long range viable program which is a precondition to world leadership demands that about 25% of total funds be allocated to new construction. The failure of the program to initiate any new construction for nine years after authorization of Fermilab is partially responsible for the current world leadership crisis to which the Draft Report is addressed.

C. The SLAC Construction Projects

I am restricting my comments on the reviews in the Draft Report of specific construction projects to those pertaining to SLAC. Nevertheless, similar comments could be made on the analyses in the Draft Report of projects at other laboratories as well.

Let me specifically illustrate my earlier observations by reference to Stanford's construction of the *Stanford Positron-Electron Asymmetric Ring* (SPEAR) which is referenced in the Draft Report on page 68. The paragraph on that page points out that "In an effort to improve the frontier physics capabilities of these less productive facilities DOE Laboratory Directors have been emphasizing development and construction of accelerators." And later on, "Stanford completed the Stanford Positron-Electron Asymmetric Ring without making it a line item construction project . . ." The story is continued on page 73 giving the history and fiscal details in constructing SPEAR largely from Equipment funds. The Draft Report correctly quotes "Laboratory Officials" that SPEAR "was built at the expense of continued deterioration of general equipment, tools, and electronics and the further reduction of support for the construction of secondary beam lines and new particle detectors." [See GAO note 4, p. 222.]

This recital is used as one of several examples supporting the Draft Report's criticism that Laboratory Directors using their discretion have given undue emphasis to new construction. Yet the events described in the Draft Report through which SPEAR was added to the arsenal of the nation's accelerating and beam storage devices is by any judgment a major success story. Construction was spectacularly inexpensive and initiated what can only be described as a revolution in physics insight through the discoveries of new phenomena beginning in November 1974. A whole new set of particle states unfolded whose understanding has been the underpinning of our current concept of the existence of quarks as a fundamental building block of matter. Moreover, the success of SPEAR has given the principal impetus to the construction of follow-on devices, in particular in Western Europe (PETRA, now followed by plans for the Electron-Positron Ring HERA at DESY and LEP at CERN) and in the United States (PEP). It is no exaggeration to state that the success of SPEAR, the lack of authorization as a construction line item notwithstanding, has initiated at low cost both great new insights into high energy physics and also a new, and more efficient technology for obtaining such insights. Then what is the basis of the criticism? Why is this undue emphasis on construction?

The Draft Report continues (page 63) commenting on the cost of Stanford's Positron-Electron Project (PEP). The report itemizes the construction cost, the associated cost for experimental equipment being built to exploit the new devices, and projected operating costs. The implication of this listing appears critical, yet the report fails to state that PEP, similar to its predecessor 2-mile accelerator at SLAC, is one of the rare construction line items in these inflationary times which is being built within estimated cost and within the schedule originally projected. The Report also fails to state that very substantial operating costs and associated equipment costs were originally projected at the time of authorization.

The Draft Report mentions (page 63) that $5.3 million of AIP funds will be used to increase the energy of SLAC's linear accelerator, which will be used as an injector for PEP. The Draft Report then continues in a critical vein, "Although this improvement was needed for the Positron-Electron Project to attain its design energy, the costs were not reported as being related to the project." This criticism is unwarranted. The energy upgrade referred to was initiated and fully justified without reference to PEP. It yields an energy upgrade of the original Stanford linear Accelerator by 50%, from 20 to 30 GeV, at less than 5% of the original cost of the project. The increased energy is being exploited in elementary particle research on its own right. It is indeed fortunate, but fortuitous, that this increased energy also improves the injection efficiency into PEP, but this increased energy is in no way required for PEP, as the Draft Report states.

D. The Record of High Energy Physics Construction

In view of the foregoing the criticism on accelerator development and construction in the Draft Report is not well founded. On the contrary, it would be helpful to emphasize that the record of construction performance within the high energy physics program, both within DOE (and formerly ERDA and AEC) and

NSF has been outstanding when compared to the record of construction projects in the Defense, Space, and Nuclear Power programs. Specifically, the High Energy Physics Program has had a superb record in terms of completion of projects within budget, on schedule, and within specifications. It would be good for a GAO report to take note of this fact when examining control practices. It appears that the Draft Report might lead to the imposition on the High Energy Physics Program (highly successful in terms of construction accomplishments) of practices which have been used in Defense and Space projects where performance has been poorer!

As applied to DOE construction projects the Draft Report (page 57) is incomplete in describing the control practices of the agency. There are elaborate controls requiring submittals of management plans and identification of milestones. A large number of construction directives and subdirectives are issued by DOE to the laboratories at many stages of work. In fact a merited criticism is that such controls are over-elaborate, in particular as they apply to "conventional" construction. Generally one-third or less of the cost of a high energy physics construction project is "conventional," that is site development, construction of buildings, housings, utilities, etc., while the balance accomplishes high technology undertakings. Yet a large preponderance of DOE controls devolve on the former rather than the latter. It is indeed true that NSF construction controls are fewer, but it is not at all established, and in fact I seriously doubt, that the more extensive DOE controls lead to better performance, although I reiterate that construction performance, both within the DOE and NSF programs in High Energy Physics, have been good. I would, however, like to emphasize that DOE construction controls have led to extensive delays in accomplishment of the U.S. High Energy Physics Program, and it is these delays which are one of the factors which endanger the competitiveness on an international scale of the U.S. program. I am attaching a chart showing the construction schedule of the two parallel storage ring installations PETRA in Germany and PEP in the United States. It will be noted from this chart that the conventional construction of the American project suffered both from the initial delay caused by excessive controls during the Architect-Engineer selection process, and that the actual accomplishment of conventional construction was also slower. I would therefore urge that in a report dedicated to examining U.S. efforts to lead the world in high energy physics, considerable caution should be used in recommending increased controls on construction when it has been demonstrated that such controls are already more onerous in the United States than abroad, and where such controls have contributed to the loss of leadership.

E. <u>Policy vs. Funding</u> [See GAO note 5, p. 222.]

The above discussions illustrate that in my view the emphasis on excessive construction and deficient construction controls is misplaced. Rather, the principal problem affecting the U.S. High Energy Physics program derives from the mismatch between policy assumptions and funding. The current program aims to balance exploitation and innovation, and as I mentioned above, such balance requires that about 25% of funds be dedicated to construction. The current program is based on the continuing viability of three national centers supported by DOE and a fourth center sponsored by NSF. Moreover, these four centers are to serve a National community of approximately 80 academic institutions. It is current policy that this number of centers and institutions is to be adequately supported, while at the same time the longevity and competitiveness of the program are to be maintained by the appropriate balance between new construction and exploitation of existing facilities. These policy assumptions imply a certain level of funding. The minimum level of funding conforming to this policy underlies the negotiated level between DOE and OMB which the Draft Report refers to on page 48. Yet due to the ravages of inflation actual funding has fallen behind the amount negotiated by, perhaps, $20-25 million total for FY'80 and '81; it has been recognized that the negotiated level constituted a floor compatible with the policy assumptions. Similarly, for the NSF program, which is heavily criticized in the Draft Report, the base funding level for high energy physics was to be restored after construction of the Cornell Storage Ring; yet actual NSF funding has fallen below that assumption.

In other words, to put it bluntly, almost all the problems identified in the Draft Report are caused by a shortfall of funding levels below those consistent with long-range policy assumptions. I strongly believe this matter should be corrected. On the other hand, the Draft Report is incorrect in faulting the subsequent actions in response to these funding shortfalls; these have indeed resulted in stretch-out of experimental schedules (page 70), insufficient maintenance, underutilization of existing facilities (page 68), and many of the other problems which have endangered the competitiveness of the program. Those unavoidable responses by and large represent the best balance among the various requirements which could have been made under the circumstances.

F. Comments on Some Specific Recommendations in the Draft Report

I am not in agreement with the recommendation of the Draft Report that accelerator development activities which might lead or are expected to lead to construction projects costing $5 million or more be authorized on a line item basis. There is a continuum of activities ranging from fundamental research and development in the evolving accelerator arts to more specific conceptual study and development which might lead to a construction proposal. Part of the purpose of such study and development is to firm up the estimated cost of the construction project. If special line item authorization were required for such a development effort several difficulties would arise. One is that the boundary line between general research and development leading to advanced, and generally more economical accelerators and the R&D supporting specific future construction projects would be difficult to define. In essence a new accelerator construction project would have to be authorized by the Congress on a line item basis twice - once for its development and once for its construction phase. This would increase the lead time for construction activities even further and thereby disadvantage the U.S. program relative to European undertakings even more; I have noted above that Western European administrative procedures related to construction tends to be substantially more expeditious than U.S. procedures under DOE. Finally, the requirement for line item authorization of development and construction separately would generate pressures for seeking such authorization before estimated construction costs are reliably determined. Thus the risk for cost overruns would be enhanced rather than reduced.

I am also not in concurrence with the recommendation in the Draft Report that OSTP should convene a working group to conduct a study and to determine the appropriate level of funding for the U.S. High Energy Physics Program. High energy physics as a discipline has probably undergone more reviews by panels and committees than any other discipline in basic science and adding one more study would accomplish little. Moreover the Department of Energy, following the mandate given to its predecessor agencies ERDA and AEC, has been officially designated the executive agent of the National program in high energy physics and is as such explicitly charged with formulation of overall policy. I note that the High Energy Physics Advisory Panel (HEPAP) which is charged to advise the Director of Research of DOE, has been a very effective study and coordinating mechanism, and that its meetings are regularly attended by representatives of the NSF program office also. Therefore, requesting OSTP to carry out the proposed study would lead to justified requests that other fields of basic science competing for federal funds should be similarly examined. This, in turn, would generate an additional level of study and management on the OSTP level, which that office is not charged with undertaking and is probably inadequately staffed to carry out.

I recognize that the GAO staff has undertaken a very difficult task in trying to appraise U.S. efforts in high energy physics which is probably the most fundamental major scientific undertaking sponsored by the U.S. Government. I caution that in its concern with accountability the GAO not stifle the program flexibility on which the past successes and continued competitiveness of the U.S. program in this rapidly moving field so critically depends.

I hope you will find these comments on this study constructive. I will be happy to furnish any additional material should you find it useful.

Sincerely yours,

Wolfgang K. H. Panofsky
Director

enc.

GAO note 1: The report's title has been changed to "Increasing Costs, Competition May Hinder U.S. Position of Leadership in High Energy Physics."

GAO note 2: Page references have been changed to reflect location in the final report.

GAO note 3: Our conclusion of overemphasis which appeared in the draft report on the pages cited has been deleted from the final report. (See pp. 100-102.)

GAO note 4: The discussion of the construction of the Stanford Positron Asymmetric Ring primarily with equipment funds which appeared on pages 68 and 73 of the draft report has been deleted from the final report. (See pp. 102-104.)

GAO note 5: The discussion of DOE control practices, which appeared on pages 57 to 62 of the draft report has been deleted from the final report. (See pp. 102-104.)

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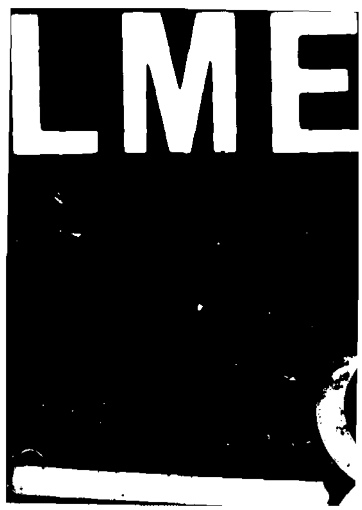
☆ U.S. GOVERNMENT PRINTING OFFICE : 1980- 620-386/245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