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# HEPAP Subpanel Report on

# PLANNING FOR THE FUTURE OF U.S. HIGH-ENERGY PHYSICS

February 1998

U.S. Department of Energy
Office of Energy Research
Division of High Energy Physics

UNIVERSITY OF CALIFORNIA, SANTA BARBARA

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DEPARTMENT OF PHYSICS | SANTA BARBARA, CALIFORNIA 93106-9530

February 20, 1998

Dr. Martha A. Krebs
Director
Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Martha:

With this letter I am transmitting the report of the "High Energy Physics Advisory Panel's Subpanel on Planning for the Future of U.S. High Energy Physics" chaired by Professor Fred Gilman. This Subpanel was formed in response to your charge letter dated March 11, 1997. The full Panel discussed the report at its meeting on February 18 and 19, 1998, and endorsed it unanimously and wholeheartedly.

On behalf of the entire community of high energy physicists we thank Professor Gilman and the members of the Subpanel for the good judgment they brought to their work. They set careful priorities in order to design an optimum program within the budget constraints detailed in the charge. The report outlines a plan for the future with an excellent balance among full utilization of laboratory facilities, the needed participation of university groups in the physics program, building up the LHC research effort, and R&D toward a new collider facility at the energy frontier. The report also lists the significant cuts needed in parts of the existing program in order to carry out the major recommendations. The plan recognizes the importance of global collaboration in the design, siting, and construction of large future facilities. The Subpanel gave special attention to the University HEP program, as you requested, and made several important recommendations to revitalize it.

The Report of the Subpanel makes a forceful argument that the investment being made in High Energy Physics continues to produce a rich return in scientific results. In recent months a great deal of attention has been focused on the many benefits to the nation from basic research and on the importance of federal support for that research. High Energy Physics will continue to be a central part of the overall enterprise of U.S. science. We hope that this report will help you communicate that High Energy Physics is one highlight of the excellent science program supported by the Office of Energy Research.

We look forward to working with you in the coming months and years to achieve the vision for the future that is laid out in the Subpanel's Report.

Sincerely,

Michael Witherell

Michael Witherell
Chairman
High Energy Physics Advisory Panel

Carnegie Mellon

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February 19, 1998

Professor Michael Witherell
Chair, High Energy Physics Advisory Panel
Department of Physics
University of California
Santa Barbara, CA 93106

Dear Mike,

I herewith submit to you and HEPAP the report of the Subpanel on "Planning for the Future of U.S. High Energy Physics." This Subpanel faced an unusually broad and difficult charge in recommending "a scenario for an optimal and balanced U.S. high-energy physics program over the next decade." We were asked to devote particular attention to the physics promise and feasibility of new accelerator facilities, the analysis and optimization of the university-based program, and the status of fixed-target experiments at Brookhaven after the AGS becomes primarily an injector for RHIC. Each of these topics might well have involved a separate subpanel.

This report answers the charge and is the plan developed by the Subpanel for the nation's high-energy physics program. It balances near-term scientific opportunities with preparations for the most important discovery possibilities in the long term. In developing this plan within a limited budget (at a constant level of effort in the central scenario), difficult choices were made to end or reduce some highly productive programs. The Subpanel's plan can be carried out within the budget by redirecting funding from these programs to those with the highest priority.

The Subpanel thanks the high-energy community for their valuable input in supplying us with both their thoughts and an enormous amount of data. We had excellent support and coöperation from the officials and staff of the Department of Energy's Division of High Energy Physics. Robert Diebold, our Executive Secretary, merits special thanks for all his work on our behalf. The clarity of the Subpanel's written words and the report as a whole benefitted greatly from the excellence of our editor, Kate Metropolis.

I personally thank the members of the Subpanel for their commitment and joining me in this difficult task. Every member of the Subpanel made a significant contribution to this report. I can truly say that while each member had their own expertise, we did not have single-issue members; all thought about the future of the field as a whole. They listened, debated, and worked on all aspects of the report, even though we had a portion of the Subpanel, headed by Abe Seiden (who gets my special thanks), that devoted special attention to the university issues.

I hope that HEPAP will endorse our plan for the future of U.S. high-energy physics, and that a continued U.S. role at the forefront of understanding the nature of matter at its most fundamental level will meet with the approval of the Administration and the Congress.

Sincerely,

Fred Gilman

Frederick J. Gilman
Chair, HEPAP Subpanel on
Planning for the Future of U.S. High
Energy Physics

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Planning for the Future of U.S. High Energy Physics

## EXECUTIVE SUMMARY

High-energy physicists seek to understand the universe by investigating the most basic particles and the forces between them. Experiments and theoretical insights over the past several decades have made it possible to see the deep connections between apparently unrelated phenomena and to piece together more of the story of how a rich and complex cosmos could evolve from just a few kinds of elementary particles.

Our nation's contributions to this remarkable achievement have been made possible by the federal government's support of basic research and the development of the state-of-the-art accelerators and detectors needed to investigate the physics of the elementary particles. This investment has been enormously successful: of the fifteen Nobel Prizes awarded for research in experimental and theoretical particle physics over the past forty years, physicists in the U.S. program won or shared in thirteen and account for twenty-four of the twenty-nine recipients. New high-energy physics facilities now under construction will allow us to take the next big steps toward understanding the origin of mass and the asymmetry between the behavior of matter and antimatter.

The U.S. Department of Energy has asked its High Energy Physics Advisory Panel (HEPAP) to recommend a scenario for an optimal and balanced U.S. high-energy physics program over the next decade. In response to this charge, a Subpanel was appointed in March 1997. The present report is the plan developed by the Subpanel for the nation's high-energy physics program. It balances near-term scientific opportunities with preparations for the most important discovery possibilities in the long term.

In the field of high-energy physics, the financial and intellectual scale of future large facilities means that international collaboration in their design and construction is increasingly necessary. This trend is exemplified by U.S. participation in the Large Hadron Collider at CERN, the accelerator that will begin to probe the high-energy frontier in the middle of the next decade.

To make possible the most important new research opportunities over the next decade within a constrained budget, the high-energy physics community and the Subpanel have had to make difficult choices to target a number of highly productive programs for termination. It will be possible to carry out the Subpanel's recommendations by redirecting funding and scientific manpower from programs that are ending.

In framing its plan for the next decade, the Subpanel used the following guiding principles:

- Maximize the potential for major discoveries by
  - utilizing existing U.S. facilities at the frontiers in energy and precision to capitalize on prior investments and
  - participating in experiments at unique facilities abroad.
- Position the U.S. program for a long-term leading role at the energy frontier through
  - vigorous research and development on possible future facilities and
  - international collaboration on future machines.
- Prepare the next generation of scientists through education and training at universities nd laboratories.

Guided by these principles and assuming a constant-level-of-effort budget, the Subpanel has developed a set of recommendations to enable the U.S. high-energy physics program to continue to play a leading role in the international effort

- to discover the underlying reasons for the observed masses of the elementary particles,
- to understand the observed difference between the behavior of particles of matter and anti-matter, and
- to seek a deeper understanding of the connection between the fundamental forces in nature.

**Recommendation on the Effective Utilization of Facilities**

The Subpanel places its highest priority on optimum utilization of the forefront facilities nearing completion. The Subpanel recommends that funding for Tevatron collider, PEP-II, and CESR operations, and for the physics groups using them, be at a level that ensures these facilities fulfill their physics potential.

**Recommendation on the LHC**

The Subpanel strongly endorses the physics goals of the LHC and U.S. participation in the accelerator project and the ATLAS and CMS experiments. The funding level and schedule contained in the CERN-U.S. LHC agreement should be followed. The Subpanel expresses its gratitude to the Congress, DOE, and NSF for making possible U.S. participation in the LHC.

**Recommendation on Planning for Future Facilities**

The Subpanel recommends that a new facility at the energy frontier be an integral part of the long-term national high-energy physics program.

**Recommendation on R&D for a Linear Collider**

The Subpanel recommends that SLAC continue R&D with Japan's KEK toward a common design for an electron-positron linear collider with a luminosity of at least $10^{34}$ $cm^{-2}$ $s^{-1}$ and an initial capability of 1 TeV in the center of mass, extendible to 1.5 TeV. The Subpanel recommends that SLAC be authorized to produce a Conceptual Design Report for this machine in close collaboration with KEK.

This is not a recommendation to proceed with construction. A decision on whether to construct a linear collider should only follow the recommendation of a future subpanel convened after the CDR is complete. The decision will depend on what is known about the technology of linear colliders and other potential facilities, costs, international support, and advances in our physics understanding.

**Recommendation on R&D for a Muon Collider**

The Subpanel recommends that an expanded program of R&D be carried out on a muon collider, involving both simulation and experiments. This R&D program should have central project management, involve both laboratory and university groups, and have the aim of resolving the question of whether this machine is feasible to build and operate for exploring the high-energy frontier. The scale and progress of this R&D program should be subject to additional review in about two years.

**Recommendation on R&D for a Very Large Hadron Collider**

The Subpanel recommends an expanded program of R&D on cost reduction strategies, enabling technologies, and accelerator physics issues for a VLHC. These efforts should be coordinated across laboratory and university groups with the aim of identifying design concepts for an economically and technically viable facility. The scale and progress of this R&D program should be subject to additional review in about two years.

## Recommendation on the Level of Funding for the University-Based Program

An important part of the charge concerned the university-based high-energy physics program and its optimization within the overall plan for the next decade. The Subpanel intensively examined the status of high-energy physics research at universities and makes a major recommendation:

The Subpanel recommends that, over a two-year period, the annual DOE operating funds for the university program be ramped up by a total of 10% above inflation. The Subpanel encourages the NSF to make a similar increase in its experimental and theoretical elementary particle physics programs. These increases should be used for activities judged to have the largest impact on physics goals and student training. This would partially restore the losses of the last five years and better prepare university groups to use the new facilities.

## Additional Recommendations

A number of additional recommendations that relate to specific aspects of the high-energy physics program, as well as consideration of the impact of modestly decreased and increased budgets, are found in chapter 7.

## Impact of Increased Support for the Program

Leaders of many scientific and engineering societies recently proposed that the nation's research budget be doubled over a ten-year period. Such an increase would enable the U.S. to maximize the scientific return on the facilities that are now being completed and would strengthen the U.S. program sufficiently for the U.S. to play a leading role in initiating the next major international collider in the coming decade. The Subpanel urges the Administration, the Congress, and the American people to make possible the opportunities envisioned in this proposal.

## Contents

# 1 What Is High-Energy Physics?

*The supreme task of the physicist is to arrive*
*at those universal elementary laws from which*
*the cosmos can be built up by pure deduction.*

—Albert Einstein, *Homage to Max Planck*

High-energy physics is the quest to uncover the nature of matter at its most fundamental level. With this knowledge, we strive to understand why the universe is the way it is.

A hundred years ago, the first of the basic particles, the electron, was discovered. So began a remarkable journey inward to smaller and smaller distance scales, from the atom composed of electrons and a nucleus, to the nucleus composed of protons and neutrons, to protons and neutrons composed of quarks. Much of modern technology is based on the deep understanding of matter that has developed over the last century.

Along with this discovery of the fundamental constituents came an understanding of the interactions between them. One of the great achievements of the last quarter century is our understanding that apparently different interactions are unified as different manifestations of a single force. Whether all interactions, including gravity, can be understood in terms of a single theory is one of our major pieces of unfinished business.

High-energy physics is intimately connected to cosmology. Experiments in the laboratory produce particle collisions similar to those that occurred just after the big bang. From high-energy physics experiments, we know that matter and antimatter behave differently at the fundamental level, and we seek to gain enough understanding to connect this to the universe, in which there is far more matter than antimatter.

The journey inward has made it possible to probe amazingly small distances, a realm governed by both relativity and quantum mechanics. If we were able to expand a single atom to be the size of the earth, a proton would be about the size of a football field, and our current experiments able to find the football. In the time frame we are considering in this report, we should be able to resolve the laces.

The "microscopes" with the resolution to investigate these tiny scales are provided by particle accelerators. The higher their energy, the smaller the distances probed, leading to the seemingly paradoxical situation that our study of the smallest objects requires constructing some of the largest and most complex scientific instruments ever built. Driven by the science to move the frontier to higher energies and smaller distances, we have learned to construct accelerators with ever higher energy. Over many decades, the effective collision energy has doubled every three years, on average. The cost per unit energy of accelerators has dropped steadily over this period due to advances in technology such as the development and production of superconducting cable and magnets on an industrial scale.

Particle accelerator technology developed for high-energy physics is used in many areas of science and technology. Applications include accelerators used for radiation therapy, neutron sources used for materials science, and synchrotron light sources used for research in many fields, including materials science, environmental chemistry, and structural biology. Synchrotron light sources are accelerators that provide intense X-ray and ultraviolet probes with high space and time resolution. The number of applications for synchrotron light sources has been growing steadily for over two decades, and researchers always need greater access to these facilities. This entire field grew out of work on high-energy accelerators, so it is not an accident that the forefront facilities for synchrotron radiation research in this country—Stanford, Cornell, Berkeley, Brookhaven, and Argonne—are all located at particle physics laboratories.

Detectors for high-energy physics have also grown larger and more complex. Each is proposed, designed, built, and operated by a large collaboration of scientists from universities and laboratories throughout the U.S. and around the world. The components of these detectors range from one-of-a-kind devices to those requiring mass production in lots of hundreds or thousands. Some pieces, including those that are too large to transport, are built at the laboratory where they will operate. The rest are fabricated at the many collaborating institutions and by private industry and brought together at the laboratory for assembly.

The experiments run twenty-four hours a day, seven days a week, staffed by rotating shifts of scientists and students drawn from the collaborations. Huge sets of data are recorded and analyzed using sophisticated computer systems. The need for global collaborations to exchange large amounts of data and other information led to the invention of the World Wide Web at CERN, the European Laboratory for Particle Physics.

The investment made in high-energy physics over the last fifty years has been enormously successful. The experimental discoveries and the theoretical advances made to explain those discoveries have changed the way we think about the natural world in a profound way. In the last forty years, fifteen Nobel Prizes have been awarded for research in experimental and theoretical particle physics. The strong role of the U.S. in this field is reflected in the fact that thirteen of these prizes were won or shared by physicists in the U.S. program. The scientific progress we are making today is the result of investments made in accelerator technology more than a decade ago. Similarly, the accelerator research being done today will bear fruit in the discoveries of decades to come.

The remarkably simple and beautiful synthesis of our understanding of the fundamental constituents of matter and their interactions is a tremendous scientific achievement. At the same time, this theory points toward the next big questions to be answered. For example, why are there six types of quarks? Does the difference in the behavior of matter and antimatter predicted by the theory actually describe the world? Why is the top quark more than ten thousand times heavier than the light quarks found in the proton and neutron? The pattern of quark masses makes all the difference in the properties and stability of ordinary matter, and life as we know it might not exist if their values were even slightly different than those observed.

We are on the threshold of another golden age of discovery about the fundamental nature of matter. Indeed, we know that the answers to our questions about the origin of mass are within sight of the next generation of accelerators. New experiments made possible by technological advances will lead to insights as surprising and remarkable as what we have learned so far.

# 2 The Subpanel

*We pass the word around; we ponder how the case is put by different people; we read the poetry; we meditate over the literature; we play the music; we change our minds; we reach an understanding. Society evolves this way, not by shouting each other down, but by the unique capacity of unique, individual human to comprehend each other.*

—Lewis Thomas, "On Committees"

## A. MOTIVATION AND CHARGE

The High-Energy Physics Advisory Panel (HEPAP) Subpanel on Planning for the Future of U.S. High-Energy Physics was formed in the spring of 1997 and given its charge by Martha Krebs, director of the Office of Energy Research in the U.S. Department of Energy (DOE). (Appendix A contains the charge to the Subpanel and appendix B contains the membership of the Subpanel.) The general charge was to "consider the potential scientific opportunities and recommend a scenario for an optimal and balanced U.S. HEP [high-energy physics] program over the next decade," assuming a budget that keeps up with inflation, i.e., a "constant level of effort." The charge also asked the Subpanel to "consider the sacrifices that would be implied by a modest decrease in funding and the opportunities that would be presented by a modest increase." The Subpanel was requested to pay particular attention to possible major future facilities, to the university-based high-energy physics program, and to the priority of high-energy fixed-target experiments at Brookhaven National Laboratory after the Alternating Gradient Synchrotron's operations become funded by the DOE's Nuclear Physics Program.

The report "Vision for the Future of High-Energy Physics," prepared by the 1994 subpanel chaired by Sidney D. Drell, and subsequent history provide the context of the present report. In the wake of the cancellation of the Superconducting Super Collider project, the Drell Subpanel recommended that the U.S. "continue to be among the leaders in the worldwide pursuit of the fundamental questions of particle physics" and continue its tradition of success in this field through a strong U.S. program that included "significant participation in the LHC [Large Hadron Collider] accelerator and detectors, both to provide research opportunities at the energy frontier and to ensure that U.S. physicists remain integrated in the international high-energy physics community." The international collaboration agreement for U.S. participation in the LHC project at CERN (the European Laboratory for Particle Physics) was signed in December 1997, realizing this key recommendation.

The Drell Subpanel also recommended that the high-energy physics budget of the DOE provide constant-level-of-effort funding plus a three-year bump of $50 million per year. In the absence of this temporary budget increase to revitalize the ongoing research program, the Drell Subpanel recommended that a new subpanel be formed "to recommend appropriate changes and sacrifices." Given that the LHC is proceeding with strong U.S. participation, but also that only a small fraction of the bump was actually funded, it is now appropriate to review the U.S. program, to see how it can best be positioned as part of the international program in the future, and to optimize the elements of the U.S. program within an overall plan for the next decade.

Though work at the LHC will continue for many years, the large scale of high-energy physics facilities requires that planning begin now for the era following the LHC. A future facility requires accelerator research and development in the present. The funding for such research and development comes at the expense of the rest of the current high-energy physics program, which seeks to make the best use of existing facilities, built at great cost and effort, as well as those about to come online. These are among the elements the Subpanel was asked to balance in defining the optimal program.

## B. COLLECTION OF INFORMATION

This Subpanel, chaired by Fred Gilman, consisted of twenty-two high-energy physicists. Most were from universities and laboratories in the U.S.; in addition, there were members from Europe and Japan. Meetings were attended by DOE representatives, notably John O'Fallon, director of the Division of High Energy Physics; Robert Diebold, who served as executive secretary to the Subpanel; and P. K. Williams, head of University Programs in Experimental and Theoretical High-Energy Physics. Patricia Rankin and Marvin Goldberg, program officers for Elementary-Particle Physics at the National Science Foundation (NSF), were also present.

All Subpanel members were responsible for addressing all aspects of the charge, but it was felt that the specific part of the charge dealing with the university-based program required specialized discussion and additional input. For that reason, a portion of the Subpanel devoted particular attention to these issues. This group, headed by Abraham Seiden, was made up of the members from universities and Charles Prescott of SLAC.

The Subpanel met initially in Washington, D.C., on April 20 and 21, 1997, to plan its activities. These were designed to inform the Subpanel thoroughly on the issues related to the charge and to give members of the high-energy physics community the maximum opportunity to be heard.

Information reached the Subpanel through four main channels. Two are identified in a letter to the community from Fred Gilman (appendix C): direct input to the Subpanel by letter or electronic mail, and a series of three meetings, held in the San Francisco, Chicago, and New York regions June 23–26, August 11–14, and September 17–21, 1997, respectively. The third source of information was responses to specific questions directed to various segments of the community. The fourth source was statistical information collected by others. Solicitations for input and open invitations to attend the three meetings were sent by electronic mail to the members of the Division of Particles and Fields (DPF) and the Division of Physics of Beams of the American Physical Society, as well as to the laboratory users groups. The same information was included in the DPF newsletter and posted on the Worldwide Web on the DOE Division of High Energy Physics homepage, http://www.hep.net/doe-hep/home.html.

The Subpanel received approximately 120 direct submissions from individuals and groups. These were roughly equally divided between letters on issues of particular concern to the authors and responses to specific inquiries from the Subpanel. Electronic mail was automatically and immediately distributed to each member; regular mail was forwarded periodically.

The agendas of the three fact-finding meetings are included as appendix D. Each began with a day devoted to university issues. These were held at the University of California, Berkeley; the University of Chicago; and the State University of New York at Stony Brook. A letter to the community from Abraham Seiden (appendix C) included an open invitation to participate. The Subpanel also requested several presentations to address specific portions of the charge, including meetings with spokespersons from several large experiments to discuss university participation in these efforts. Though organized by the portion of the Subpanel devoted to university issues (about half the full Subpanel), it was typical for other Subpanel members to be present at these meetings as well.

The rest of each multi-day meeting was spent at a national laboratory: SLAC, Fermilab, and Brookhaven. Two days at each laboratory were largely devoted to open presentations by representatives of the host laboratory and of another nearby laboratory, sessions devoted to significant aspects of the U.S. high-energy physics program not covered elsewhere, and a community forum. The laboratory representatives presented summaries of their upcoming programs, research and development for new facilities, and their priorities for the period of interest to the Subpanel. Special sessions were convened on the LBNL program and non-accelerator physics at the SLAC meeting; on the Argonne program at the Fermilab meeting; and on the CESR program, the LHC, and both the Brookhaven and Fermilab kaon programs at the Brookhaven meeting. Possible major future facilities were also discussed: a linear collider at the SLAC meeting, a muon collider at the Fermilab meeting, and a very large hadron collider (VLHC) at both the Fermilab and Brookhaven meetings. The Subpanel also heard presentations on DESY's future plans, especially regarding a linear collider, and on the work of the National Research Council's Committee on Elementary-Particle Physics, chaired by Bruce Winstein. Altogether, the Subpanel heard 150 presentations in open session.

The community forums were organized by Subpanel members and the laboratory users organizations. At Brookhaven, the CESR users organization and the AGS users organization presented a combined program. For these forums, the Subpanel made two requests: that the presentations have something to do with the current state of the field or its future directions (as opposed to being simply physics talks), and that younger physicists—graduate students, postdocs, and young faculty—be encouraged to make presentations. At the first of the forums, at SLAC, some of these young physicists made a special effort to discuss the state of the field with their peers and to report on what they heard. We found these presentations to be very informative and encouraged the Fermilab, BNL, and CESR users groups to arrange similar talks. This they did, and these, too, were thoughtful and thought-provoking.

The Subpanel devoted the beginning and end of each day, as well as a day or two at the end of each meeting, to discussions in executive session. We tried to summarize the presentations and to establish if we were missing any information important to our deliberations on the laboratory programs and priorities. Also at these sessions, the information presented at the university day at the beginning of the meeting was summarized and discussed by the full Subpanel.

Besides information obtained from the general request to the high-energy physics community, the Subpanel found that specific information was needed to address the issues in our charge. The Subpanel requested that each national laboratory answer a list of questions on staffing, priorities, and interactions with universities. Members of the Subpanel focusing on the university-based program issued several requests for information, including a letter soliciting responses to the idea of forming regional centers for engineering and technical support of the university groups. The national laboratories were also asked about this issue and about the suitability of the laboratories to serve in this capacity. A sample of senior university-based physicists working at the national laboratories was asked a number of questions involving their interactions with the laboratories. (All of these queries are set forth in appendix C.) A sample of physics department chairs (not, for the most part, high-energy physicists) were asked to comment on the role and status of high-energy physics in their departments. Individual Subpanel members naturally conducted many informal inquiries as well, and we benefited greatly from such contacts.

The final source of information for the Subpanel was statistical and financial data provided by the DOE, in presentations by John O'Fallon and P. K. Williams; by the NSF, in presentations by Patricia Rankin and Marvin Goldberg; and by other gatherers of information. Notable among these was a report by Michael Barnett on a 1997 DOE/NSF-commissioned survey of high-energy physics education and outreach programs and on a 1995 census and survey of the field carried out for the NSF, DOE, and DPF. Also of particular interest was a survey for HEPAP of the university high-energy physics groups taken over the summer of 1997 by Pier Oddone and co-workers at LBNL (see appendix E). We were given access to these data, and the LBNL staff used the database they had assembled to answer specific questions raised by the Subpanel.

In July 1997, Fred Gilman produced a newsletter (see appendix C) summarizing the Subpanel's activities and reminding the community of upcoming meetings. This newsletter was distributed through many of the same channels as the original announcements.

## C. THE REPORT

The Subpanel met in Reston, Virginia, November 5–9, 1997, to complete its deliberations and to produce a draft of this report. The report was finalized at a last meeting, January 5 and 6, 1998, in Gaithersburg, Maryland. All recommendations were adopted by consensus of the full Subpanel.

The report continues with a description of the scientific issues at the cutting edge of the field (chapter 3) and the U.S. high-energy physics program now in place (chapter 4). There are issues likely to remain beyond the reach of the current program, and possible major new facilities to address these issues are reviewed (chapter 5). The status of the university-based high-energy physics program and how it should be optimized within the overall program are assessed (chapter 6). Finally, the Subpanel's plan for the next decade of the U.S. high-energy physics program is presented in the form of a set of recommendations and the changes implied by a modest decrease or increase in funding (chapter 7).

## 3 The Physics Questions Before Us

*They say of nature that it conceals with a grand nonchalance, and they say of vision that it is a deliberate gift.*

—Annie Dillard, "Seeing"

In this section we present the forefront scientific issues facing high-energy physics, discuss crucial areas that will be explored in the next decade, and present the scientific rationale for a new major accelerator that will complement and extend the physics reach of the current set of facilities and of the LHC.

### A. WHAT DO WE KNOW NOW?

Experiments over the past thirty years have conclusively determined that the elementary particles and their interactions are described by the so-called Standard Model of particle physics. According to the Standard Model, the fundamental constituents of matter consist of three families of quarks and leptons. The quarks and leptons interact through the electroweak force, while the quarks alone feel the strong force.

The strong force, quantum chromodynamics (QCD), governs the binding of quarks into protons and neutrons and ultimately into nuclei. The electroweak force has two aspects. One results in electromagnetic interactions and gives rise to electromagnetic waves, such as radio, light, and X-rays; the other aspect results in the weak interactions, which govern radioactive decay and make possible the generation of energy in stars.

In the Standard Model, all forces are mediated by the exchange of particles known as gauge bosons. For QCD these are the gluons, while for the electroweak interaction they are the photon, the W, and the Z. Together with gravity, the interactions they mediate ultimately govern all of matter and energy.

The interactions between the quarks, leptons, and gauge bosons have been measured very accurately and agree with the predictions of the Standard Model. For example, experiments using protons and antiprotons at the Tevatron collider have verified the predictions of QCD, provided the most accurate measurement of the W mass, and discovered the top quark predicted by the Standard Model. Experiments using electrons and positrons (the antiparticle of an electron) at the LEP and SLC colliders have verified dozens of predictions about properties of the Z particle to a precision of a fraction of a percent. The results of these experiments, and many others, have established the validity of the Standard Model and severely constrained any possible extension to it.

## B. WHAT QUESTIONS REMAIN OPEN?

Even though present-day experiments have confirmed the Standard Model to tremendous accuracy, we know that it is necessarily and fundamentally incomplete. For example, the Standard Model predicts the scattering rate for W and Z gauge bosons. The prediction is mathematically inconsistent at energies above about 1 TeV, which tells us that new physics is waiting to be discovered—physics beyond that of the Standard Model. New particles must come into play, with masses less than a few TeV.

At present, we have a few tantalizing ideas of what these new particles might be. We know that they play a central role in generating masses for the W and Z bosons—a process known as electroweak-symmetry breaking. Similarly, the generation of quark and charged-lepton masses also requires the breaking of electroweak symmetry.

One possibility is that the symmetry breaking gives rise to an elementary scalar particle called the Higgs boson. The Higgs boson mediates a new force, which cures the inconsistency in W and Z scattering. It has precisely prescribed couplings to the electroweak gauge bosons, quarks, and leptons. Unfortunately, the theory allows the Higgs boson's mass to be anywhere below about 800 GeV (where the Higgs theory becomes inconsistent).

While the simplest Higgs theory can accommodate electroweak-symmetry breaking in a manner consistent with experimental data, it does not explain why such symmetry breaking occurs. Furthermore, quantum mechanical corrections, unless very finely tuned, drive the Higgs mass far beyond the TeV scale. Two approaches have been taken in constructing a theory that does not suffer from this instability: supersymmetry and strongly interacting symmetry breaking.

In a supersymmetric theory, one stabilizes the Higgs mass by doubling the number of particles. For every quark or lepton, one adds a new boson, and for every gauge boson, one introduces a new fermion. (Supersymmetry requires that the Higgs sector also be expanded.) The couplings of these new particles to each other and to the Standard-Model particles are fixed by supersymmetry. The quantum corrections to the particle masses cancel, provided the masses of the supersymmetric partners lie below a few TeV.

If supersymmetry is correct, the effort to understand the superparticles and their properties will be the focus of particle physics well into the next century.

In a strongly interacting symmetry breaking theory, one introduces a new gauge interaction that becomes strong at an energy scale of order 1 TeV. As in QCD at much lower energies, the strongly interacting gauge theory breaks the electroweak symmetry. Such a theory could give rise to “light” particles with masses in the few-hundred GeV range (called technipions), as well as to a large cross section for the scattering of W and Z bosons. In addition, the theory generally predicts a number of particles with masses in the few-hundred GeV to the TeV region, including analogs to the familiar spin-one resonances in QCD.

The experimental investigation of electroweak-symmetry breaking is the most pressing issue before us. There are two reasons for this. First, experiments at the LHC are guaranteed to observe new phenomena associated with the symmetry breaking. Second, an understanding of electroweak-symmetry breaking is essential for answering other, equally compelling questions that the Standard Model does not address. We briefly describe some of these questions below:

*1. What is the origin of flavor symmetry breaking? Why are there three families of quarks and leptons, and what explains their masses? Do neutrinos have mass, and do the neutrino flavors mix?*

Various theories have been constructed to address flavor physics, that is to say, the origin of the quark and lepton masses (and mixings). Most invoke new interactions that distinguish between the various quarks and leptons. Generically, they predict phenomena that are either absent or highly suppressed in the Standard Model, such as flavor-changing neutral currents, rare decays of mesons or muons, or muon-electron conversion. Observation of such phenomena at other than expected rates would signal the existence of new interactions beyond those contained in the Standard Model. Searches are planned or in progress at all U.S. accelerator facilities.

Many extensions to the Standard Model allow neutrinos to have non-zero mass. The presence of neutrino masses could give rise to a rich phenomenology, including the possibility of neutrinoless double beta decay and neutrino oscillations. Neutrino oscillations are currently the preferred explanation for the difference between the observed solar neutrino flux and that predicted by the standard solar model, as well as for the discrepancy in the ratio of muon-neutrinos to electron-neutrinos produced when cosmic rays strike the atmosphere. Searches at accelerator and non-accelerator facilities are planned or underway.

*2. What is the origin of CP violation?*

The electroweak interactions are maximally parity (P) and charge-conjugation (C) violating because of the chiral nature of the electroweak gauge interaction. In contrast, the violation of the combined symmetry, CP, has so far only been observed as a 0.2% effect in the mixing of neutral kaons. The Standard Model can accommodate the presence of CP violation through the phase in the quark mixing matrix. It has not, however, been conclusively demonstrated that this is the origin of the observed CP violation. Furthermore, the Standard-Model CP violation is not thought to be large enough to result in the observed domination of matter over antimatter in the universe.

If CP violation arises from quark mixing, it should also be seen in as-yet-unobserved effects in neutral K- and B-meson decays. Additional physics introduced to stabilize the electroweak scale, or to explain flavor symmetry breaking, could give rise to additional contributions to CP violation. If new CP-violating interactions are present, they may become evident in the results of K- and B-meson experiments in the coming decade.

In addition to CP violation in the electroweak sector, the Standard Model also allows for CP violation in QCD interactions. This asymmetry would give rise to a non-zero electric dipole moment of the neutron, but current limits from atomic physics show that the effect is very small. Why is this so?

*3. What is the origin of the gauge structure of the Standard Model?*

In the Standard Model, the three independent gauge couplings and the parity violation in the electroweak sector are unexplained. The simplest attempts to explain these facts invoke grand unification, in which a single gauge interaction breaks, at a high energy scale, to the gauge structure of the Standard Model—just as electroweak gauge symmetry is broken to electromagnetism and the weak interactions. At present, there is a tantalizing experimental hint for unification: the measured values of the three coupling constants are such that, in the context of a supersymmetric theory, they unify at an energy of order $10^{16}$ GeV. Any theory in which the strong and electroweak interactions unify into a single gauge group gives rise, at some level, to proton decay. Results from the current round of water Cherenkov detectors should either observe proton decay or constrain the viable theories.

Unification is not complete without gravity, which must presumably unify with the other forces at an energy near the Planck scale, of order $10^{19}$ GeV. Recent developments in string theory have shed light on the possible structure of gravity at this high scale. Indeed, the discovery of new duality symmetries has led to the hope that the gauge structure of the Standard Model can be understood in terms of the physics of the string vacuum.

*4. How did the cosmos originate and evolve?*

During recent years, particle physics has developed close connections to cosmology, astrophysics, and gravity. Pertinent questions range from the microscopic origin of the cosmic matter-antimatter asymmetry, to the formation of structure in the universe, to the nature of the dark matter that is thought to dominate the mass of the universe. Large-scale experiments are being proposed to search for particle dark matter and survey large redshift galaxies. Other experiments are being planned to observe gravity waves, to map the anisotropy of the cosmic background radiation, and to study the highest-energy cosmic-ray particles.

Substantial progress has recently been made in constructing candidate theories of quantum gravity based on string theory and its generalizations. These investigations promise a deeper understanding of black holes and perhaps even of the big bang itself. A final facet of gravity, for which there is ample observational evidence and no theoretical explanation, is the absence (or near absence) of a cosmological constant. A successful quantum theory of gravity must provide an explanation for the fact that the cosmological constant is over a hundred orders of magnitude smaller than naively expected. This issue is central to understanding the evolution of the universe.

## C. WHAT IS LIKELY TO BE EXPLORED?

The current U.S. experimental program is discussed in the next chapter. In brief, during the next ten years we can expect tighter and tighter tests of the Standard-Model and we will be searching for evidence of non-Standard Model behavior. The current and proposed K- and B-meson experiments at fixed-target facilities, at B factories, and at the Tevatron aim to confirm or disprove the consistency of the quark-mixing matrix explanation of CP violation. Rare meson and muon decay experiments might give evidence for physics beyond the Standard Model, or they will further constrain possible extensions to it.

Results from underground detectors are likely to determine whether neutrino oscillations are responsible for the deficit in the solar neutrino flux. Likewise, high-statistics data from current accelerator and non-accelerator experiments could establish whether neutrino oscillations are responsible for the atmospheric neutrino anomaly. The observation of proton decay could give clues to the structure of unified gauge interactions. Other projects in non-accelerator physics may have unique capability to address the open questions above, such as the nature of dark matter or the origin of the highest-energy cosmic rays.

Signs of the physics responsible for electroweak-symmetry breaking could begin to show up at LEP II or at the Tevatron. LEP II should ultimately be able to discover a Higgs boson with a mass up to approximately 95 GeV, while a high-intensity Run III at the Tevatron might be able to discover a Higgs boson with a mass up to about 125 GeV. Together, these two sets of experiments could cover most of the Higgs-boson mass range predicted in the simplest supersymmetric models. Other signatures of the physics associated with electroweak-symmetry breaking, such as supersymmetric partners or technipions, could also be discovered if they are light enough.

The raison d'etre for the LHC is to push the search for electroweak-symmetry breaking into the TeV region. The ATLAS and CMS detectors are designed to discover the Higgs boson if it has a mass below about 800 GeV. These detectors should also be able to discover supersymmetry if the superpartners have masses lighter than approximately 2 TeV, as expected if supersymmetry is the mechanism responsible for stabilizing the weak scale. With several years of running at the highest luminosity, the LHC should also be able to establish whether the symmetry-breaking sector is strongly interacting through the observation of an enhanced cross section for W boson scattering, although there might not be enough luminosity to distinguish between different models.

## D. WHAT WILL REMAIN UNANSWERED AFTER THE LHC?

Experiments at the LHC will shed light on the origin of electroweak-symmetry breaking. They will open the door to a host of new questions. For example, the Higgs boson must have very definite couplings to the W and Z gauge bosons, as well as to the quarks and leptons, if it is to give rise to their masses. One would like to measure these couplings to determine whether the Higgs is responsible for all of electroweak-symmetry breaking.

If, however, the LHC discovers a significant new extension to the Standard Model, such as supersymmetry or a strongly interacting symmetry-breaking sector, there will be an entirely new world of particles to study and analyze. For example, if supersymmetry were discovered, a detailed understanding of the masses and interactions of the supersymmetric particles will be essential to understanding the origin of the universe itself.

If the symmetry-breaking sector is strongly interacting, the dynamics that transmit the symmetry breaking to the quarks and leptons—especially to the top quark—will be of great interest. A thorough understanding of the particles in the symmetry-breaking sector, and of their couplings to quarks and leptons, would cast light on this dynamics.

While the LHC (or possibly LEP II or the Tevatron) will discover new particles associated with the symmetry-breaking sector, a complete investigation of all particles associated with the symmetry-breaking dynamics will likely be beyond the LHC's reach, either because their masses are too large or because they are hard to distinguish from high backgrounds.

The history of particle physics shows that progress in understanding the basic rules by which the universe works—that is, determining the properties of the fundamental constituents of matter and the forces through which they interact—comes by doing experiments that explore high energies. This exploration is a continually evolving process: at successively higher energies, deeper layers of physical law emerge. There is no reason to believe we are at the end of this story.

If there is one thing of which we are certain, it is that the dynamics of electroweak-symmetry breaking requires new physics. This, in turn, will lead to new questions, which will be just as exciting as the questions that motivate us today. There is no doubt that many of long-standing questions about our world will remain unanswered, even after the LHC has completed its mission. New facilities will be required to address these questions, as discussed in chapter 5.

Experimental opportunities to probe the fundamental properties of matter come in many forms: colliders, fixed-target experiments, bottom and charm factories, and large non-accelerator detectors. But the direct approach of controlled collisions at high energies and luminosities is the most fruitful for producing new particles and elucidating their properties. We believe this will continue to be true in the years to come.

## 4 The U.S. High-Energy Physics Program

*If science is to progress, what we need*
*is the ability to experiment . . . .*

—Richard Feynman, *The Character of Physical Law*

### A. INTRODUCTION

The U.S. high-energy physics program seeks to advance understanding of the fundamental particles and the forces between them. Much of the research in this program is conducted with experiments at powerful accelerators, both within the U.S. and abroad. These accelerators provide beams of particles at precisely controlled energies, which can either be brought into head-on collision with other beams in a "collider," or with stationary targets in "fixed-target" experiments. Beams of electrons, protons, or other particles enable a wide variety of phenomena to be investigated under well-controlled, repeatable conditions. Hadron (proton or antiproton) colliders give the highest energies and thus allow frontier searches for new phenomena. Electron-positron machines provide high-energy, well-controlled probes, well suited for the precise study of several key particles. Electron-proton collisions are used to study the constituents of the proton itself. Some experiments are done without accelerators, such as studies using cosmic rays, decays of radioactive sources, or neutrinos from nuclear reactors. In all of these experiments, U.S. high-energy physicists work closely with physicists from around the world, often in large multinational collaborations.

In the past several years, the U.S. high-energy physics program has led to many important discoveries about the basic properties of matter. The Standard Model of particle physics has evolved through experimental and theoretical advances over the past three decades, and this Standard Model now serves as an invaluable template for predicting and correlating data in diverse experiments. Recent experiments led by U.S. physicists have uncovered the extraordinarily heavy top quark and have verified the character of the unified electroweak force to a highly accurate level. The nuclei of the atoms that constitute the ordinary world around us are made predominantly from the lightest quarks, and the interactions of these have been studied and compared with the theory of the strong force, quantum chromodynamics (QCD), at distance scales down to $10^{-18}$ meters. The heavier strange, charm, and bottom quarks have been studied in a wide variety of experiments. The study of K mesons has revealed the violation of CP symmetry (the lack of reflection symmetry when a physical process is viewed in a mirror and particles are transformed into antiparticles, called CP violation) whose origin remains mysterious. Although the Standard Model is an edifice of great beauty and has successfully withstood the test of experiment so far, we now know that it is but an approximation to a complete theory. The Standard Model must be augmented with new phenomena at energies within the reach of experiments planned now at current or new facilities. Indeed, many recent experiments have probed this region of departure and have helped to develop the experimental and theoretical tools for this next stage of investigation. The U.S. program is thus poised to capitalize upon great opportunities to advance our knowledge of the fundamental processes of nature in the coming decade.

The U.S. program is supported by the U.S. Department of Energy (DOE) and by the National Science Foundation (NSF). DOE operates several large facilities and supports the research of many university groups; its high-energy physics budget for FY98 is $678 million. The NSF operates one facility and supports the research of about one-quarter of the university investigators in this field, with a budget of just over $50 million. Proposals for new experiments and accelerator facilities are scrutinized by intensive external peer reviews, as are grant proposals by university researchers.

The U.S. accelerator laboratories include Fermi National Accelerator Laboratory (Fermilab), operating the 1.8 TeV Tevatron antiproton-proton collider and an 800 GeV fixed-target program; the Stanford Linear Accelerator Center (SLAC), operating the 91 GeV electron-positron linear collider (SLC) and a fixed-target program at energies up to 50 GeV; and Brookhaven National Laboratory (BNL), operating the 30 GeV high-intensity AGS proton accelerator, all funded through the High Energy Physics Program of the DOE. The Cornell Electron Storage Ring (CESR), which provides electron-positron collisions at about 10 GeV, is funded by the NSF. These accelerators provide the core facilities with which the U.S. high-energy research program is conducted and attract scientists from around the world to participate. Two laboratories that operated accelerator facilities in the past, Lawrence Berkeley National Laboratory (LBNL) and Argonne National Laboratory (ANL), have a large technical infrastructure that provides critical support of the program. Students are engaged in educational programs at the national laboratories to train them in the most advanced accelerator techniques.

U.S. physicists also participate in experiments at accelerator laboratories abroad, at CERN (Geneva, Switzerland), with both an 180 GeV electron-positron collider (LEP) and a fixed-target program in operation and a 14 TeV proton-proton collider (LHC) under construction; at DESY (Hamburg, Germany), with a 300 GeV electron (positron)-proton collider (HERA); at KEK (Tsukuba, Japan), with a broad program involving electron and proton beams at energies up to 25 GeV; and at BEPC (Beijing, China), with an electron-positron collider at 2-5 GeV.

About thirty-five hundred physicists participate in the U.S. high-energy physics program, including about a thousand graduate students. Roughly one-third are theoretical physicists. Over five hundred physicists are employed at the national laboratories; the rest hold positions in universities across the country. The university portion of the program involves about 135 universities nationwide. Approximately 14% of the DOE total funding, and two-thirds of the NSF funding, goes directly to support the university operations. The large collider detector collaborations (CDF, DØ, SLD, BaBar, and CLEO) are composed of about 50% U.S. university scientists and 30% scientists from abroad, with the remainder coming from U.S. laboratories.

University research groups are an integral part of this program. The important function of training students and guiding them in the development of their own research programs is primarily the responsibility of university physicists. University physicists work on experiments at accelerator laboratories in this country and abroad, and on a variety of nonaccelerator experiments, in many cases providing leadership for these efforts. Both laboratory and university physicists have made key contributions to the development of new experimental projects and innovative detector ideas. As the design and operation of the accelerators have become centered at the laboratories, the laboratories have taken the lead in research on new accelerator techniques, though even here, individuals in the universities have provided important innovative ideas. In the past, the universities have made advances necessary for developing new experimental techniques. A concern addressed in chapter 6 of this report is the serious erosion over the past several years of the university infrastructure necessary to continue the development of experimental techniques.

U.S. theoretical physicists have made crucial contributions to our understanding of nature. Theoretical research spans a wide range of topics. It ranges from the development of new formal mathematical tools and theories that encompass the particles and forces, to development of new models that extend our ability to correlate a wide range of phenomena and the detailed confrontation of these models with new data. The interplay of experiment and theory is vital to progress in the field; the two alternate in identifying the new directions that lead to deeper insights into the character of matter. The majority of theorists are at universities and together they address a very diverse range of issues. Each of the accelerator laboratories has a strong theory group, which focuses in part on explaining the results from experiments at that laboratory and correlating them with the wider body of knowledge. Laboratory-based theorists also pursue wider investigations, sometimes capitalizing upon special opportunities that exist in the labs, such as extensive computing expertise. Major theoretical work has recently been focused on understanding the mechanisms for electroweak-symmetry breaking, the analytic and computational study of quantum chromodynamics, the properties of hadrons containing heavy quarks, astroparticle physics, and the study of string theory, which could provide a unified description of gravity and the other fundamental interactions.

The character of research at the U.S. accelerator laboratories will change in the coming ten years. World leadership on the energy frontier will pass from the Fermilab collider to the LHC at CERN after approximately 2005. The LHC will be built in the existing LEP tunnel; the U.S. is participating in both the accelerator and the detectors. The SLAC linear collider will stop operating, and the 10 GeV asymmetric-energy PEP-II electron-positron collider will be brought into operation. The Brookhaven AGS will begin service as the injector for the Relativistic Heavy Ion Collider (RHIC) in 1999, and from that time the operation of the AGS will become the responsibility of the DOE's Nuclear Physics Division. The Fermilab collider program will be scaled down as the LHC comes into operation. Using its fixed-target mode of operation, Fermilab will begin new programs in neutrino physics. Fermilab and Brookhaven have opportunities to expand upon rare K decay and muon studies. Cornell is investigating new possibilities for a very high-luminosity electron-positron collider studying rare B decays.

U.S. physicists will undertake a major role in experiments at the LHC, working at the highest energy available in the world. Nevertheless, there is every indication that crucial experimentation will be necessary at still higher energies of the colliding elementary constituents, so in the coming years the U.S. must work to develop new opportunities to extend beyond the LHC.

Several possibilities for future facilities to complement and extend the physics reach of the present program are now in the research and development stage: an electron-positron linear collider at 1.5 TeV, a muon collider at energies up to 4 TeV, and a proton-proton collider at energies up to 100 TeV. (These accelerators are discussed in chapter 5.) Existing facilities and infrastructure must therefore serve the nation's needs both for near-term experimentation and for developing future opportunities to keep the U.S. at the forefront of the field. Each of the laboratories conducts research and development devoted to further future accelerator technology, and these efforts are closely interconnected so as to bring the expertise of each laboratory into a coherent effort.

The U.S. program has had notable achievements in the past several years. The rest of this chapter discusses, at a fairly technical level, the experimental investigations conducted at facilities here and abroad where U.S. physicists work. The development of upgraded and new facilities described below is of great importance for the near-term future of the field. A brief summary concludes the chapter.

## B. FERMI NATIONAL ACCELERATOR LABORATORY

Fermilab has operated the 1.8 TeV antiproton-proton Tevatron Collider since 1987 and has continued to provide a variety of beams of protons, pions, kaons, hyperons, muons, and neutrinos for studies with fixed targets. The Fermilab program has been rich with discoveries of new phenomena at high energies; high points include the discoveries of both third-generation quarks: the bottom (b) quark in the fixed-target program and the top quark at the collider. Experiments have helped to illuminate the electroweak force, CP violation, and the properties of charmed hadrons. Fermilab has conducted programs of accelerator research both for its existing machines and for future possibilities. Future programs include higher-intensity and higher-energy operation of the Tevatron collider using the new Main Injector/Recycler complex, and new beams from the Main Injector operating simultaneously with the collider for neutrino and rare K decay studies. Research and development efforts are underway to explore possible new very high energy colliders using muon and proton beams.

### *1. The Current Experimental Program*

The Fermilab collider has provided the highest-energy collisions in the world since it began operation in 1987. The discovery of the top quark in 1995 by the CDF and DØ collaborations marked the end of a twenty-year search for the partner to the b quark discovered at Fermilab in the late 1970s. The extraordinarily large mass of the top quark compared with all other quarks is peculiar and suggests that the top quark may be special. The higher-order corrections to the electroweak model have now been convincingly tested using the combination of the top mass measurement, the precision determination of the W boson mass to within about 0.1% by CDF and DØ, and the LEP and SLC precision studies of the Z boson. This allows the mass of the conventional Higgs boson to be inferred to within about 100 GeV (see figure 4.1). The Tevatron experiments have studied the production of two gauge bosons resulting from the basic trilinear coupling predicted in the Standard Model. The presence of the expected gauge couplings of the $SU(2)\times U(1)$ electroweak theory was first verified in these experiments, and limits have been set on anomalous couplings. CDF and DØ have also searched for a variety of new

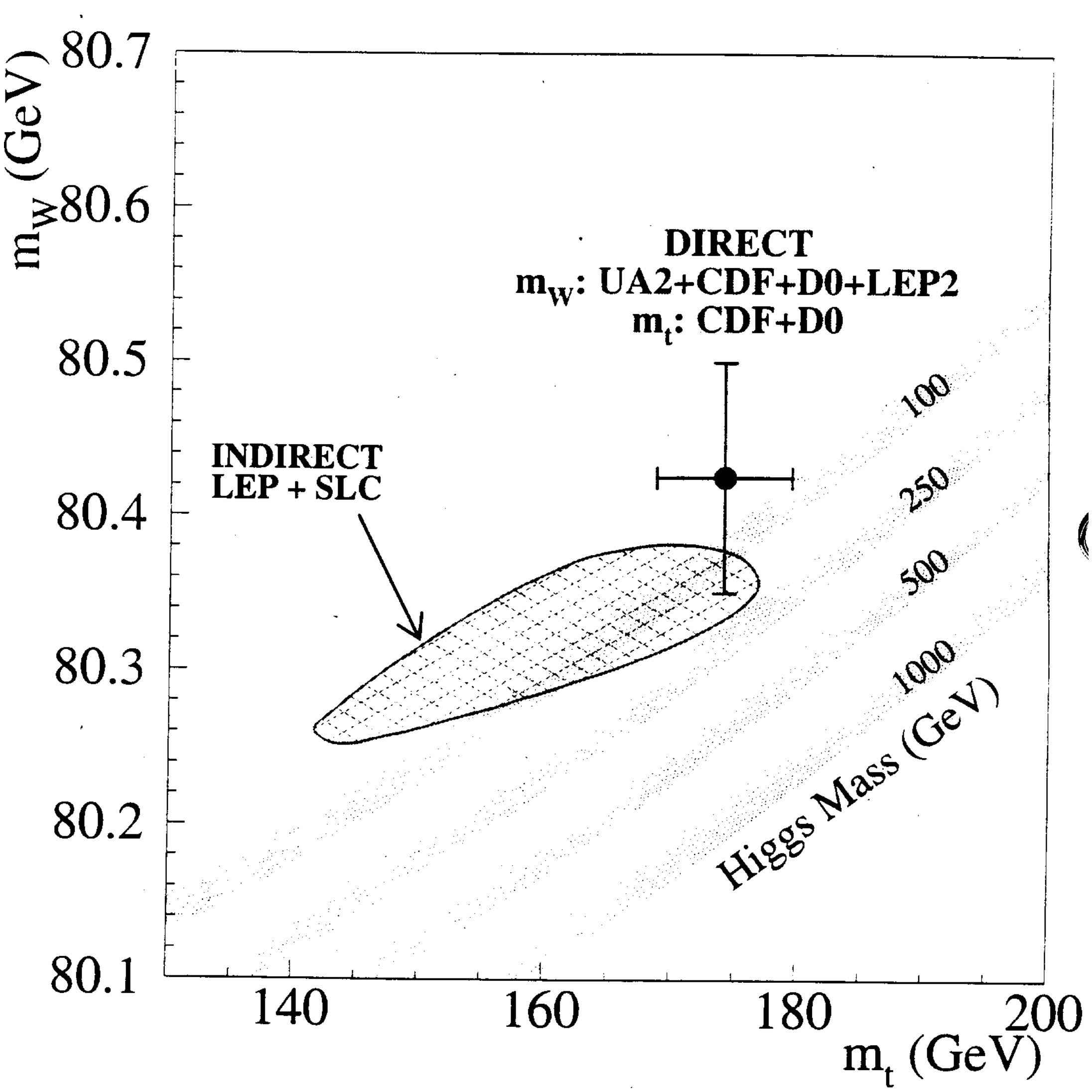


Figure 4.1 The Tevatron collider at Fermilab is the only accelerator in the world with sufficient energy to allow direct measurement of both the mass of the W boson and the mass of the top quark. The data point is the average of the direct experimental measurement of these masses, including data from Fermilab and from CERN. This information precisely tests the Standard Model and guides the search for the mechanism of electroweak-symmetry breaking, as shown by the shaded bands that give the predictions for specific Higgs boson masses. The cross-hatched area shows the region allowed at the 68% confidence level by the many precision measurements on Z boson properties made by experiments at CERN and SLAC.

particles suggested in various theoretical frameworks for extensions beyond the Standard Model. The masses of possible supersymmetric partners of the quarks and gluons are constrained to be above about 260 GeV (for equal-mass squarks and gluinos). The mass limits on first-generation leptoquarks, earlier suggested as an explanation of the recent excess of events in high-$Q^2$ electron-proton scattering at HERA, have been raised to about 240 GeV, ruling out this interpretation. The Tevatron experiments have studied b hadron production and decay, proving that these measurements can indeed be performed with high sensitivity in hadron colliders. There have been a variety of novel studies of QCD through the production of parton jets, W and Z bosons, and photons. Studies of events with jets and angular regions devoid of particle activity have shed light on the Pomeron that mediates particle interactions with no color flow.

Experiments in the fixed-target program have produced a series of impressive results, giving increasingly precise measurements of CP violation in K decay, rare K decays, the determination of $\sin^2\theta_W$ in $\nu$-nucleus scattering, studies of charmed hadrons and charmonium states, CPT conservation, deep-inelastic muon and neutrino scattering, and the search for the tau neutrino. These experiments have extended our knowledge significantly.

## *2. The Near-Term Experimental Program*

Now the laboratory is primed for another round of discovery, through upgrades of the accelerator complex and the detectors, together with several new initiatives. The approved facilities at Fermilab include the construction of the Main Injector, to be completed in 1999; the collider detector upgrades, with first operation in 2000; and the NUMI project, which will provide new beams and detectors (MINOS and COSMOS) for the study of neutrino oscillations. Other experiments have been proposed that could extend the studies at Fermilab in other areas. These include further upgrades to the existing collider detectors, a dedicated B detector at the collider, proposals to do K decay physics using the 120 GeV Main Injector beam, and some further neutrino experimentation. The Tevatron Collider will produce the highest energy collisions in the world until the turn-on of the LHC around 2005. The CDF and DØ detectors are being upgraded for the increased luminosities planned for the collider in the Main Injector era, both during Run II (the period before 2002) and beyond. The CDF detector is replacing its central drift chamber and forward calorimetry. DØ is adding a solenoid magnet and replacing its tracking detectors. Both experiments are implementing ambitious new silicon-strip vertex detectors and extensions of the trigger capabilities, as well as modernizing their software using an object-oriented methodology. An outstanding physics program is planned, taking aim at some of the most important issues of the field, such as the nature of the electroweak interaction, searches for phenomena beyond the Standard Model, and CP violation. It includes measurements of top quark properties; precision electroweak measurements based on comparison of the masses of the top quark and the W boson; searches for the Higgs boson; studies of CP violation and quark mixing in the B sector and rare B decays; and searches for new phenomena beyond the Standard Model.

The early years of operation with the Main Injector and Recycler in Run II should bring the luminosity to $2 \times 10^{32}$ cm$^{-2}$ s$^{-1}$ by about 2002. With 2–4 fb$^{-1}$ accumulated luminosity, both CDF and DØ should acquire over 1000 $t\bar{t}$ events in the low background sample with at least one b-quark identified. Studies of single top quark production will permit the measurement of its width and coupling parameters. The large samples of top quarks will allow sensitive searches for new phenomena in its decays, and for possible $t\bar{t}$ resonances. The large samples of W bosons (several hundred thousand) will allow the measurement of the W mass to about 50 MeV in each experiment. Studies of Z decays will give measurements of the weak mixing angle in the light quark sector with high accuracy. Taken together with LEP and SLC results, these measurements will give stringent tests of the electroweak model.

Beyond Run II, Fermilab proposes continued increases in luminosity in what is termed the TeV33 era. These upgrades in luminosity are partially motivated by the window of opportunity for discovery of the conventional Higgs boson above the region studied by LEP. The total luminosity accumulation required to establish a five-standard-deviation effect for the 125 GeV Higgs is estimated to be 20 fb$^{-1.}$ By 2005, with instantaneous luminosity reaching $5 \times 10^{32}$ cm$^{-2}$ s$^{-1}$ in the TeV33 era, this accumulation could be achieved.

Fermilab also has an opportunity at the Tevatron Collider to explore the properties of the b and c quarks. The studies of the states heavier than the $B_d$ mesons will be unique at Fermilab prior to the LHC. Measurements of lifetimes, $B_s$ mixing, the spectroscopy of heavy b-quark mesons and baryons, and the search for new phenomena inferred from very rare decays of the B states will be very important contributions. The study of CP violation in the decay $B_d \rightarrow J/\psi\ K_s$ in CDF and DØ should be possible with accuracy comparable to that from the electron-positron B factories. Studies of the other angles in the unitarity triangle may be accessible through the study of the decays $B_d \rightarrow \pi^+\pi^-$ and $B_s \rightarrow D_s K^+$ in a proposed dedicated B detector (BTeV).

The question of neutrino mass and neutrino oscillations can be explored in new regimes with the NUMI facility (for "neutrino beams with the Main Injector") and the new MINOS and COSMOS experiments. There are now somewhat contradictory indications for neutrino oscillations in atmospherically produced neutrinos observed in underground detectors and in one accelerator experiment (LSND). Fermilab can help resolve these issues. Located 1000 meters from the Main Injector beam, COSMOS would search for $\nu_\mu \rightarrow \nu_\tau$. It has sensitivity in the $\Delta m^2$ region above 1–10 $eV^2$ for very small mixing angles. MINOS, a magnetized tracking calorimeter with one detector at 730 kilometers from the Main Injector in the Soudan mine in Minnesota and an associated detector at Fermilab, 1250 meters from the Main Injector, will search for $\nu_\mu \rightarrow \nu_e$ and $\nu_\mu \rightarrow \nu_\tau$. MINOS could extend the search sensitivity for oscillations down to $\Delta m^2$ of $10^{-3}$ $eV^2$. MINOS now plans for an initial far detector of about 5 kilotons, to be increased eventually to 10 kilotons. The DONUT experiment, seeking the first direct detection of nt, recently ran in a fixed-target beam line and proposes a follow-up run in 1999. A proposed experiment, MINI-BOONE, would use neutrinos from the 8 GeV booster to re-examine the possible nm ®$n_e$ appearance signal reported by LSND.

The KTeV experiment, which studies CP violation in the neutral K system and seeks rare decays of the K meson, has proposed to run again in 1999 with the Main Injector. Extensions of KTeV and other new experiments would significantly extend our understanding of rare K decays, such as $K_L \rightarrow \pi^o \nu \bar{\nu}$ and $K^+ \rightarrow \pi^+ \nu \bar{\nu}$, affording unique and sensitive studies of CP violation complementary to those in the B-meson sector.

Fermilab plans to continue its contributions to experimental astrophysics, including participation in the Cryogenic Dark Matter Search (CDMS) and the Sloan Digital Sky Survey (SDSS). CDMS will search for WIMPS (weakly interacting massive particles) using a low-background cryogenic detector in the Soudan mine. SDSS is a dedicated telescope and data-acquisition system that will perform wide-angle surveys of the sky to study large-scale galactic structure. These efforts are both larger collaborations to which Fermilab contributes in areas of its special expertise.

Recently Fermilab has assumed the role of host laboratory for the LHC CMS experiment activities at U.S. institutions. In addition to project management for the U.S. CMS effort, Fermilab will be responsible for major CMS construction projects, including the end muon chambers and the tile scintillation hadron calorimeter. Playing this central role in LHC detectors is a natural continuation of the Tevatron collider program and is also a bridge to possible future initiatives for a VLHC (very large hadron collider).

*3. Near-Term Accelerator Plan*

Near-term effort on accelerator issues at Fermilab is focused on increases in luminosity and energy for the collider and on implementation of new intense fixed-target beams. Accelerator projects include further development of the existing antiproton-proton collider facilities for higher luminosity and a new proton source that would serve the needs of the existing experiments and become the springboard for future initiatives.

In Run II, starting in 2000, the Tevatron collider luminosity will increase by a factor of ten over the maximum luminosity achieved previously. This improvement will result primarily from an increase in the number of antiprotons. The increased antiproton intensity (a factor of 6–9) will be provided by the higher proton intensity and repetition rate of the Main Injector, improvements in antiproton transmission and coalescing, also a result of the Main Injector, and the beam recovery and cooling enhancements afforded by the Recycler Ring. Improvements in the antiproton-source debuncher and accumulator elements are being implemented to deliver the increased antiproton flux. The Tevatron energy will be increased to 2 TeV by using the existing subcoolers, together with some magnet replacements and rearrangements. At the start of Run II, a six-fold increase in the number of bunches will be used in the Tevatron to keep the number of interactions per crossing to about two. At higher luminosities, the number of bunches will need to be increased by another factor of three to keep the number of interactions per crossing fixed. This will require a crossing angle to avoid parasitic crossings. Crossing-angle operation results in a smaller interaction region, which is useful for the experiments, but requires a reduction in the bunch length to avoid loss of luminosity. Fermilab proposes to accomplish this with the development and installation of 200 MHz superconducting rf cavities. Finally, a new low-beta interaction region is proposed at the CØ interaction region that would be used for the BTeV experiment.

Plans are being developed for further increases in luminosity, up to $10^{33}$ cm$^{-2}$ s$^{-1}$ for TeV33, through improved injection into the Main Injector to increase the proton intensity on the antiproton production targets, aperture increases of the collection subsystem, and debuncher and bandwidth increases in the accumulator cooling system. A new beam line from the antiproton source to the Recycler would be required, and the stochastic cooling system in the Recycler would be supplemented with an electron cooling system.

A substantial upgrade to the linac and booster (the “proton source”) is also in the planning stages. There are several motivations: the need for high proton intensities for antiproton production for the Collider program, and benefits to fixed-target experiments using beams from the Main Injector and possibly from the booster. In addition, the improved proton source could serve as the driver for a muon collider or the first-stage injector for a VLHC. In the current plan, the existing linac and booster would first be moved to a new location. Subsequently, in three additional steps, the linac would be upgraded to 1 GeV, the booster to 16 GeV, and finally a 4.5 GeV pre-booster would be added. The final complex would be capable of producing $10^{14}$ protons of 16 GeV.

### *4. Accelerator Research and Development*

The Beams and Technical Divisions at Fermilab carry out research and development aimed at future initiatives. This effort now includes design studies for muon and hadron colliders and development of superconducting magnets, superconducting rf, and photo-injectors. Fermilab also collaborates with BNL and LBNL on the construction of the experimental insertion magnets for the LHC.

Fermilab continues its pioneering development of new superconducting magnets in collaboration with industries and universities. Superconducting materials are being developed for both conventional low-temperature compounds and high-temperature oxide superconductors. Efforts are underway to study innovative low-field magnet designs for a VLHC and to develop new designs and new materials for very high field (>12 T) magnets. These efforts are part of a five-year research and development plan to help narrow the choices among the various VLHC options, discussed in chapter 5.

Fermilab research on superconducting rf technology supports the needs of Run II noted above and anticipates the rf systems required for a muon collider. Fermilab collaborates with BNL and LBNL on design studies for a muon collider and is working to implement tests of critical muon collider systems using beams from the Tevatron complex.

Finally, Fermilab has built a laser-driven electron photo-injector, which is designed to produce a short bunch length 15 MeV electron beam for TESLA at DESY. This device will be used to study wakefields in superconducting cavitities, and it will also be used in a plasma wakefield experiment aimed at achieving accelerating gradients of 1 GeV/m.

## C. STANFORD LINEAR ACCELERATOR CENTER

SLAC has a long history in the research, development, and use of electron beams. The upgraded SLAC electron linear accelerator has continued to operate for fixed-target experiments probing new aspects of quantum electrodynamics (QED) and the structure of the proton and neutron. The linac provides both electrons and positrons that collide head on in the SLAC Linear Collider (SLC) with a total energy equal to the Z boson rest energy. This first linear collider has enabled incisive new studies of the electroweak force by the SLD detector, exploiting the polarization of the electron beam and the very small interaction region size. The old PEP electron-positron collider is being substantially modified to provide a new facility, PEP-II, in which B mesons can be produced copiously and CP-violation studies can be carried out. SLAC has conducted extensive research and development into future linear electron colliders that would extend the energy reach to the TeV scale. In a joint research and development effort with Japan, work is proceeding towards a future linear collider.

*1. The Current Experimental Program*

The SLC is an electron-positron collider operating at the Z boson mass (91 GeV) with the capability for electron beam polarizations of up to 80%. The polarization, combined with the luminosity achieved by colliding micron-sized beams, has allowed SLAC, with about 300,000 accumulated Z events, to compete effectively with LEP (with about 4 million Z events in each of four experiments) in the area of precision electroweak studies (see figure 4.2). With up to 500,000 Z's anticipated from the planned running, the SLD experiment would measure the left-right asymmetry, giving the single most precise measurement of the pivotal electroweak parameter, $\sin^2\theta_w$, which characterizes the breaking of electroweak symmetry. SLD has installed a new charged-coupled-device pixel vertex detector that has improved the tagging efficiency for b and c quarks considerably. With this improvement and the desired 500,000 Z events, the mixing of $B_S$ and anti-$B_S$ mesons could be studied with a precision superior to that obtained at LEP. Besides thesetwo key physics improvements, the SLD precision for the wide range of measurements for the b and c quarks would be comparable to that from the full set of LEP experiments.

Studies of the spin structure function of the proton and neutron, and of the role of the gluon in providing the spin of the nucleon, have been carried out in the SLAC linac electron beam. This series of experiments uses the End Station A spectrometer with polarized targets exposed to high-intensity polarized electron beams. These experiments have built on the long history of spin-averaged deep inelastic scattering experiments at SLAC and elsewhere, by probing the spin orientation of the constituents of the nucleon. This in turn provides new tests of QCD. Interest in this area has been great, since CERN and SLAC results suggest that quarks do not make the dominant contribution to the spin of the proton. Future experiments have been proposed for End Station A to study polarized open charm production and precision electroweak effects in electron-electron scattering.

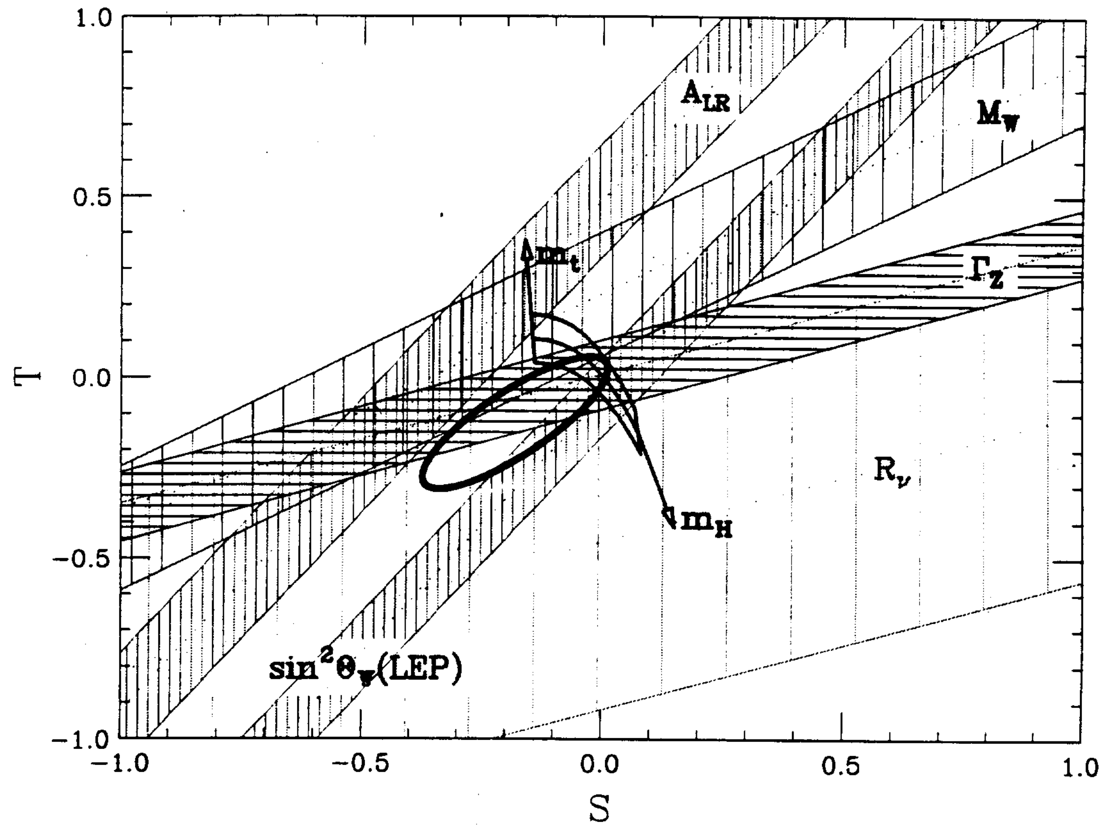


Figure 4.2 The SLAC SLC collider has made the only measurement of the left-right polarization asymmetry $A_{LR}$ using its unique polarized-beam capability. This is one of the most precise tests of the Standard Model. It can be compared with other precision electroweak measurements in terms of S and T characterizing the weak radiative corrections. S and T are nearly equal to zero in the minimal Standard Model; large differences from zero may signal departures from the minimal Standard Model. The bands shown from the experimental measurements of $A_{LR}$ (from SLC), $\Gamma_z$ (from LEP), $\sin^2\theta_w$ (from LEP), $M_W$ (from Fermilab and CERN), and Rν (from neutrino deep inelastic scattering experiments at CERN and Fermilab) indicate the 68% confidence allowed regions in S, T space. The half-chevron region encloses the Standard Model prediction for $m_t$ = 175.5 ''' 5.5 GeV and $m_H$ between 70 GeV and 1 TeV (and has S = T = 0 for $m_{top}$ = 175.5 GeV and $m_{Higgs}$ = 300 GeV). A fit to all electroweak data yields the 68% confidence region bounded by the ellipse and shows the consistency of the data and the agreement with the minimal Standard Model.

SLAC has encouraged and supported a number of smaller experiments. These bring an important breadth to the overall SLAC program and produce a variety of physics results for a small investment. This work includes both on-site activities and participation in the BES collaboration in China and the CLEO II collaboration at Cornell. For BES, SLAC provides laboratory infrastructure to support the work of nine U.S. universities at Beijing.

The on-site small experiments probe effects in several specialized regimes. A milli-charged particle search and an experiment that examines low-$Q^2$ QED processes in a regime never studied before are being conducted. A further experiment is studying high-field QED using a terawatt laser beam colliding against a 50 GeV electron beam; critical field strength non-linear processes are sought using electric field gradients of $10^{16}$ V/m. There is also a free quark search being conducted at SLAC.

SLAC is encouraging an astroparticle initiative, called GLAST, that would provide high-energy physics instrumentation for a satellite to study particles and gamma rays from deep space. This project would be supported jointly with NASA, NSF, and foreign agencies.

*2. The Near-Term Experimental Program*

The immediate future of the accelerator-based research program at SLAC will center on the asymmetric-energy electron-positron B-factory (PEP II) and the BaBar detector. PEP-II will produce copious B pairs in electron-positron collisions at the energy of the Upsilon(4S) resonance. The asymmetric energy results in both final state B's moving significantly in the laboratory frame, so that the finite decay times can be observed.

PEP-II, on which SLAC, LBNL, and Lawrence Livermore National Laboratory collaborate, is currently under construction in the existing PEP tunnel. It has a design luminosity of $3 \times 10^{33}$ cm$^{-2}$ s$^{-1}$ and electron and positron beams of 9.0 GeV and 3.1 GeV, respectively. PEP-II uses the existing linac for beam creation and acceleration, with additional elements for high- and low-energy beam extraction. The injection system fabrication is complete, as is the construction of the high-energy ring; stored beam was successfully achieved in the high-energy ring in June 1997. The low-energy ring is nearing completion and good progress has also been made on the interaction region. The PEP II schedule calls for the first electron-positron collisions in the summer of 1998, with collisions in BaBar in early 1999.

The BaBar physics program addresses the nature of CP violation in the b-quark sector and, in particular, the phase structure of the quark-mixing matrix. This requires the measurement of CP-violating asymmetries in neutral B decays to CP eigenstates. The final CP eigenstate decays must be reconstructed with a companion tagged B meson to determine the flavor of the first B meson. This tagging will be achieved by precision vertex reconstruction; flavor-tagging using electrons, muons, and kaons; and the full reconstruction and measurement of individual charged particles and photons. BABAR will also measure branching ratios for a number of $B^o$ and $B^+$ hadronic decays.

BABAR is a general-purpose detector with emphasis on good particle identification. Reliable pattern recognition should be attained from the five-layer silicon vertex tracker. The BABAR drift chamber consists of a low-mass axial-stereo design to provide good dE/dx resolution. Particle identification is provided by the DIRC system, which detects the Cherenkov ring produced in a quartz bar with photomultiplier tube readout. The electromagnetic calorimeter uses CsI crystals and will give significantly better electron energy resolution than previous systems. BABAR will have a 1.5T superconducting solenoid, whose instrumented magnetic flux return will permit muon identification with good efficiency for low-energy muons and allow $K_L^o$ identification with 70% efficiency above 2 GeV/c. The BABAR collaboration is a high-energy physics leader in the use of the new industry-standard object-oriented software and the C++ programming language, and an aggressive software development effort is in progress.

SLAC is considering upgrades for PEP II and BABAR to reach a luminosity of $10^{34}$ cm$^{-2}$ s$^{-1}$, allowing further incisive measurements of the properties of the b quark and CP violation.

### *3. Accelerator Research and Development*

SLAC operates the SLC, the world’s only electron-positron linear collider, and is the center of U.S. expertise on linear collider technologies. An active program is underway to develop the techniques that would be required for construction of a TeV-scale linear collider. This program has dominated accelerator research and development at SLAC for much of the past decade and is discussed in chapter 5.

Looking further into the future, one envisions the need for acceleration gradients of a factor of ten beyond the current state of the art. Recently, SLAC has established the Accelerator Research Department B (ARDB), devoted to research and development for the long-term future. Its goal is the development and application of targeted new technologies with emphasis on areas with potential for innovation.

The ARDB group is collaborating with a group from Stanford University on a design concept for a laser-based acceleration test using a high-intensity crossed-beam laser. The goal is to create an accelerating field of 900 MV/m. A demonstration experiment was begun in late 1997. In addition, a collaborative proposal with LBNL, UCLA, and USC has been prepared for a laser-driven plasma wakefield acceleration test at SLAC. The goal is the acceleration of a 30 GeV electron beam by 1 GeV over a 1 meter length.

The largest ARDB effort is invested in high-frequency rf acceleration with gradients of order 1 GV/m. A seven-cell, 90 GHz traveling-wave structure has been built and measured. A number of experiments are planned or underway to determine fundamental characteristics of such structures as they relate to use in a real accelerator. Near-term goals include procuring a 50-cell structure capable of producing 6 MV of acceleration.

## D. BROOKHAVEN NATIONAL LABORATORY

For more than thirty-five years, the Alternating Gradient Synchrotron (AGS) at Brookhaven National Laboratory (BNL) has provided proton, meson, muon, and neutrino beams for a vast array of experiments. Steadily increasing beam intensity, approaching $10^{14}$ protons per pulse, has provided excellent opportunities for precision determinations of Standard Model parameters, searches for rare decays, and studies of the properties of the quark and gluon constituents of hadrons. AGS experiments using intense K beams have carried out studies and searches for rare K decays sensitive to departures from the Standard Model predictions. The recently completed muon storage ring permits a precise measurement of the anomalous muon magnetic dipole moment (usually referred to as g–2), which is also sensitive to a wide range of possible new phenomena at very large mass scales. BNL proposes a restricted program, called AGS-2000, that would operate after the AGS begins service as the RHIC injector. At the same time, the laboratory is evolving to assume leading roles in projects at other laboratories.

### *1. The Current Experimental Program*

Several major milestones for the AGS physics program occurred in the past year. The detection of a candidate of the rare decay $K^+ \rightarrow \pi^+ \nu \bar{\nu}$, the successful first run of the g–2 muon storage ring, and new evidence for an exotic hadron represent major achievements of research programs of many years' duration. Precision tests of the Standard Model are the most prominent part of the current AGS program and would become the sole focus after RHIC turn-on. Rare kaon decays and the search for deviations from expected muon properties are the main topics. Current experiments, as well as those proposed for AGS-2000, rely on the intensity and flexibility of AGS beams, with an energy that is nearly optimal for producing intense secondary beams.

Experiment 787 is a continuing search for $K^+$ decays to several rare final states. Data collected so far have yielded first measurements of $K^+ \to \pi^+ \gamma\gamma$ and $K^+ \to \pi^+\mu^+\mu^-$ as well as the recent observation of one event consistent with the decay $K^+ \to \pi^+ \nu \bar{\nu}$ (see figure 4.3). A larger sample of these $K \to \pi \nu \bar{\nu}$ events would allow precise determination of the magnitude of the quark-mixing matrix parameter $V_{td}$. Experiment 865 is representative of AGS searches for decay modes that are forbidden in the Standard Model. It searches for the decay $K^+ \to \pi^+\mu^+e^-$, with an expected branching ratio sensitivity of $3 \times 10^{-12}$. Similarly, E871 is a search for $K_L \to \mu e$ and $K_L \to ee$, with a branching ratio goal of $10^{-12}$.

Tests of the Standard Model in the muon sector are the objective of E821, the muon g–2 experiment. After several years' construction, the first successful data-taking run of the muon storage ring has recently been completed. Preliminary results are encouraging, and runs in 1998 and 1999 should produce the world's best g–2 measurement. Running will continue for a few years after RHIC startup, giving an ultimate precision on g–2 of 0.35 ppm. Studies of hadronic physics with the AGS have used incident proton, meson, and nuclear beams to explore nonperturbative QCD.

Numerous hadron spectroscopy experiments have been conducted. There have also been many searches for exotic states, exclusive reactions in proton-nucleus collisions, and studies at the interface between high-energy and nuclear physics. These projects, which are either finished or near completion, are not expected to continue beyond RHIC startup. Experiment 852 uses an 18 GeV pion beam, the multiparticle spectrometer, and a large lead-glass array to search for hadronic hybrids (mixtures of quarks and gluons), glueballs, or four-quark states. Earlier this year, E852 presented evidence for a $J^{PC}=1^{-+}$ exotic meson in the reaction $\pi^- p \to \eta\pi^- p$. Experiments 913 and 914 use the Crystal Ball detector (previously at SLAC and DESY) to study the spectrum of nucleons and hyperons in the reactions $\pi^- p \to$ neutrals and $K^- p \to$ neutrals.

*2. Future Experimental Program*

BNL has proposed a modest continued program at the AGS after RHIC turn-on in 1999. The AGS will continue to operate as the injector for RHIC and could be used to deliver beams for high-energy physics. The proposed AGS-2000 program would focus on high-intensity experiments investigating rare K decays and precision muon measurements.

# $K^+ \to \pi^+ \nu\bar{\nu}$ Decay

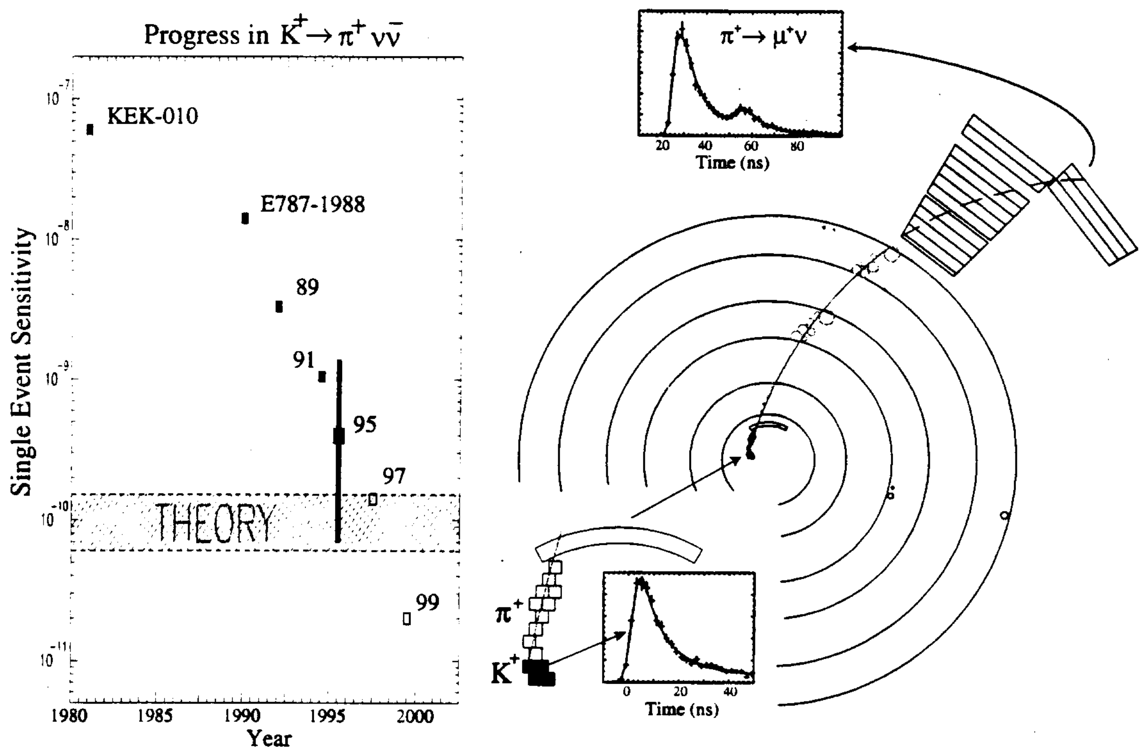


Figure 4.3 An experiment at Brookhaven National Laboratory achieved an important milestone with the first observation of the decay $K^+ \to \pi^+ \nu\bar{\nu}$. The measurement of this decay rate will yield valuable insights into possible new phenomena outside the Standard Model. The right side of the figure displays a cross-sectional view of the first event of this kind found in the E787 apparatus at BNL after searching through more than 1000 billion kaon decay events. A positive kaon (coming into the page) is observed to decay at rest (lower insert). The product of the decay is a positive pion, which curves in the magnetic field and comes to rest in a stack of scintillator plates. It is observed to decay into a muon as expected (upper insert). No gamma rays accompany the decay. The left side of the figure shows the sensitivity achieved for this decay mode over the past two decades. The E787 experiment should observe more such events with data from 1996–97 and the expected data in 1998–99, and thus refute or confirm the theoretical expectation.

Further studies of both charged and neutral $K \rightarrow \pi \nu \bar{\nu}$ decays would be possible at AGS-2000. The neutral decay $K_L \rightarrow \pi^o \nu \bar{\nu}$ would allow clean determination of the CP-violating parameter $\eta$. Another proposed test of fundamental symmetry would search for muon polarization perpendicular to the decay plane in $K^+ \rightarrow \pi^o \mu^+ \nu \mu$. Such polarization would be clear evidence for T violation outside the Standard Model. Combined measurements of CP violation and mixing angles in K decays will give complementary information on the physics of quark flavor to that obtained from B decays.

Extensions of the g–2 experimental apparatus have been discussed. Measurements of the muon electric dipole moment, muon lifetime, and muon neutrino mass are being evaluated. Another proposed experiment to exploit the very intense AGS beam in the RHIC era would search for muon to electron conversion with a sensitivity goal of $10^{-16}$.

*3. Experiments at Other Facilities*

After the transition of the AGS to be the injector for RHIC in 1999, much of the BNL staff's high-energy physics focus will be on efforts elsewhere and on research for future accelerators.

A BNL experimental group has made strong contributions to the DØ program at the Fermilab collider. They had the lead responsibility for the liquid argon central calorimeter, a crucial component of most DØ physics analyses. The group is now designing and building the forward pre-shower detector for the DØ upgrade, which will provide trigger and offline electron identification. The group was responsible for the online data-acquisition software and led the development of the offline reconstruction software. They are leading the effort to develop new object-oriented code for the upgraded DØ experiment. BNL physicists had key roles in the top quark discovery and the measurement of its mass.

Brookhaven is the host laboratory for the U.S. LHC ATLAS detector project. In addition to the project management for U.S. ATLAS, BNL has the lead responsibility for the U.S. calorimeter subprojects and will provide the calorimeter cryostats and front-end electronics. BNL will manage the overall U.S. ATLAS muon project and is specifically responsible for the cathode strip chamber construction for the forward muon measurement.

High-energy physics activities of BNL staff will continue at some level at RHIC, where there are some issues of interest to particle physics. For example, RHIC will operate some of the time as a proton-proton collider, with large proton polarizations.

Elastic polarized proton-proton cross sections and studies of chiral symmetry restoration in dense quark matter will occupy the attention of some of the BNL physicists.

### *4. Accelerator Research and Development*

BNL has an extensive history in developing very high intensity improvements for the AGS complex and has pioneered the heavy ion collider technology for RHIC. Current activities at BNL center on developing LHC accelerator systems, on research and development for future colliders, and on advanced acceleration techniques.

BNL is collaborating with Fermilab and LBNL on LHC magnets and on the design of the experimental lattice insertions. The fabrication of the beam separation dipoles and tests of critical current and quench stability for dipole and quadrupole superconducting cable are primary BNL responsibilities.

BNL is playing a leading role in the research and development for a muon collider, with tests proposed there for the primary pion production and capture techniques. Muon cooling experiments are being designed jointly with Fermilab.

There is a broad accelerator research and development effort at BNL. The user-based Accelerator Test Facility is used for experiments on laser acceleration of electrons, free electron lasers, and high-brightness electron sources. The AGS program has developed rf systems for high-intensity proton beams, and has conducted research and development for polarized proton beams. The development of high-intensity ion sources has been underway for several years. There are plans for development of new superconducting magnets based on high-temperature superconducting technology in the magnet facility.

## E. CORNELL ELECTRON STORAGE RING

The Cornell Electron Storage Ring (CESR), a symmetric electron-positron storage ring operating at the Upsilon (4S) resonance, produces large numbers of B meson pairs as well as charmed hadrons and tau leptons. These particles and their decays are analyzed by the versatile CLEO detector, which surrounds the collision point. CESR/CLEO has been the forefront facility for the study of b quark decays and quark mixing. Current and potential upgrades are aimed at studies of many phenomena related to the heavy quark hadrons, the quark-mixing matrix, and critical parameters related to CP violation. CESR operations and upgrades are primarily supported by the NSF; two-thirds of the CLEO collaborating institutions and much of the CLEO detector upgrade are funded by the DOE.

*1. The Current Experimental Program*

The CLEO experiment has accumulated more than 6 million B meson pairs. This data set has resulted in some very precise and elegant studies of b quark decays. For example, the most precise measurement of the coupling between b and u quarks, a fundamental parameter that is related to the phenomenon of CP violation, comes from CLEO. They have also found the first evidence for the flavor-changing neutral current decay $b \rightarrow s\gamma$, a measurement that is very sensitive to new heavy particles or interactions. Recently, CLEO has discovered many rare decays of B mesons to kaons and pions (figure 4.4). These reactions occur through complicated interactions of the top quark, W and Z particles, photons, and gluons. Understanding these reactions is important for testing our theories and planning for the physics program at the asymmetric-energy B factories.

For the past several years, CESR has been conducting a staged upgrade program to increase the yearly luminosity by a large factor. The CLEO detector is also being upgraded to allow analysis of rarer and more intricate processes. The first stage of this upgrade (Phase II) was completed in 1995 and resulted in a yearly luminosity of about 4 $fb^{-1}$ (about 2.5 million B pairs), an increase of a factor of 2.5 over the previous running. The new three-layer silicon vertex detector installed as part of this upgrade is expected to add significantly to the capabilities of the detector. Approximately half of all CLEO data currently under analysis is from the period after the Phase II improvements.

The next stage of the CESR-CLEO upgrade (Phase III) is in preparation for 1999. It should result in an increase of another factor of four in yearly luminosity. The detector will have a new silicon vertex detector, a new drift chamber, a ring-imaging Cherenkov detector for better particle identification, and a new data-acquisition system. With this major upgrade, CLEO should be analyzing over 10 million B pairs per year, as well as the largest sample of charm and tau decays in the world; these data will doubtless yield important new measurements and insights in the early part of the next decade.

*2. The Future Experimental Program*

The CESR/CLEO groups have begun discussion of potential further improvements that would raise the luminosity of the collider to $3 \times 10^{34}$ $cm^{-2}$ $s^{-1}$ in a possible Phase IV upgrade. This improvement is motivated by the desire to measure CP violation through the differing branching ratios of the $B^o$ and anti-$B^o$ to charge conjugate final states. Such measurements could give powerful additional constraints on the unitarity triangle and

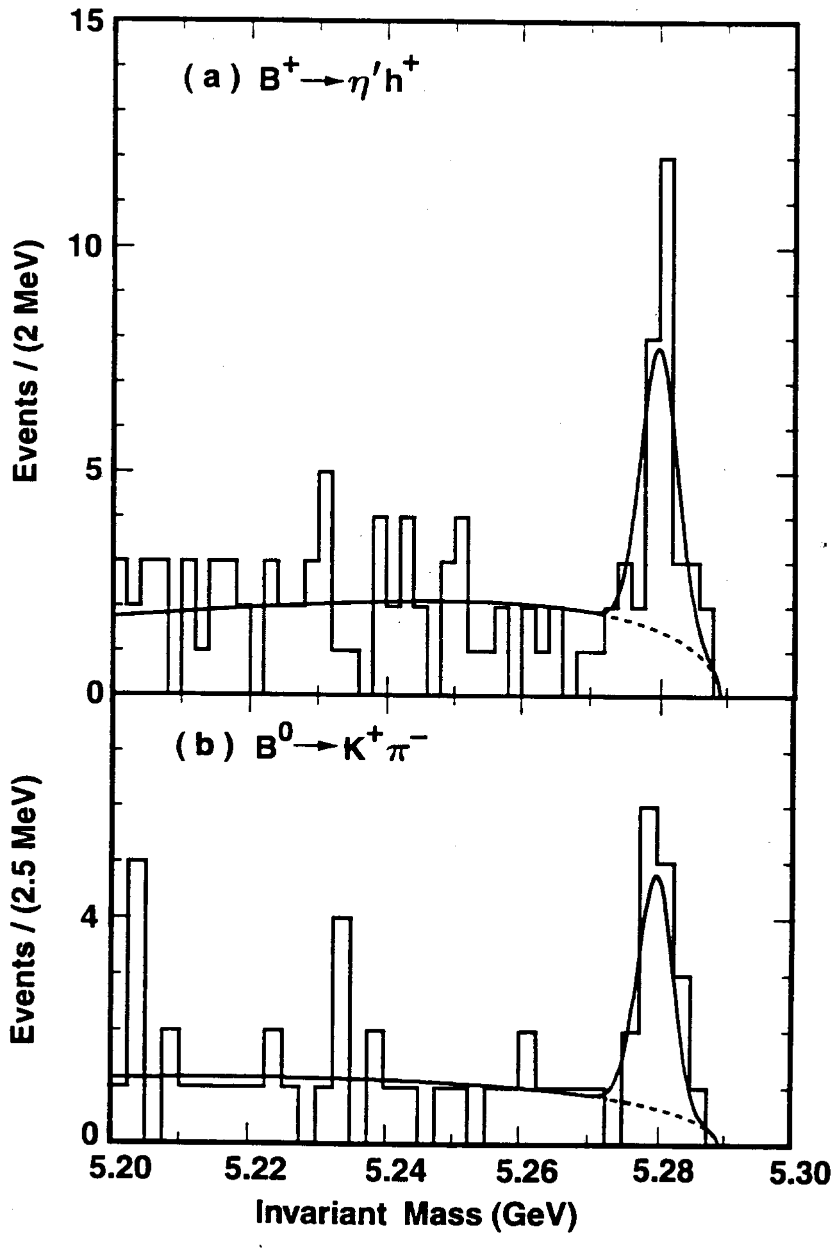


Figure 4.4 The world's highest luminosity electron-positron collider, CESR at Cornell, made it possible for the CLEO detector to observe very rare weak decays of B mesons. Such processes may also be a window for the study of CP violation. (a) Reconstructed invariant mass for $B^+ \rightarrow \eta' h^+$ candidate decays (and charge conjugate), where $h^+$ is a mixture of $K^+$ and $\pi^+$. (b) Reconstructed invariant mass for $B_d \rightarrow K^+\pi^-$ candidate decays (and charge conjugate). A peak at the B mass is apparent above the background in both plots. These decays signal the presence of loop processes involving the top quark and the vector bosons. The branching ratios of these processes are sensitive to the possible presence of new particles beyond the Standard Model.

would be sensitive to the presence of additional subprocesses. In addition, the high luminosity would enable sensitive study of rare processes such as $b \to s\ \gamma$, $b \to s\ \ell^+\ell^-$, and $B^+ \to \mu^+\nu$, which are sensitive to new physics at large mass scales. The goal of such an upgrade is to acquire 200 million B pairs per year. The current plan for the Phase IV upgrade to achieve this goal has new separate magnetic channels for electrons and positrons, a new vacuum chamber, and collisions with a small crossing angle.

*3. Accelerator Research and Development*

As the highest-luminosity electron-positron collider in the world, CESR is a major center for research in accelerator physics. Many important innovations have come from the upgrade program of this machine. For example, the ideas of non-zero collision angle and multi-bunch trains first implemented at Cornell were essential for achieving higher luminosity.

The CESR laboratory is also performing significant work on improving the gradients in superconducting rf cavities. A new rf cavity of Cornell design will be incorporated into CESR for the next upgrade. As part of this work, CESR staff members are also participating in the TESLA linear collider research and development program at DESY.

## F. LAWRENCE BERKELEY NATIONAL LABORATORY

Since the 1930s, LBNL (formerly LBL) has been highly productive in physics and an innovative source of technology. The main accelerator-based high-energy physics activities at LBNL are a hadron collider program, involving participation in the DØ, CDF, and ATLAS experiments, and a major effort on the BABAR experiment at SLAC.

LBNL provides vital technical resources for the high-energy physics community in instrumentation and computing. The Particle Data Group is a unique resource for the worldwide high-energy physics community and is expanding its outreach offerings.

*1. The Current Experimental Program*

LBNL physicists have made major contributions in the physics analyses of Tevatron data, particularly top quark studies, B physics, and electroweak measurements. This high level of expertise is now being applied to physics studies and simulations both for future Tevatron runs and for the LHC program.

LBNL has made significant contributions to the DØ and CDF detectors since their inception, specifically in tracking, calorimetry, and software for both detectors. LBNL built major calorimeter subsystems for both DØ and CDF. LBNL's expertise, particularly in tracking systems, is now being applied to the upgrades of DØ and CDF.

An important focus of the LBNL work for the Tevatron, and now for ATLAS, is the development and fabrication of silicon tracking systems and the associated electronics. Early development work culminated in the first use of a silicon strip vertex detector in a hadron collider experiment, followed by the first use of radiation-hard silicon strips. This work has greatly aided both top and b quark studies. A CDF/DØ collaboration with both LBNL and Fermilab will supply the SVX2 readout chip for the DØ silicon vertex tracker and the DØ fiber tracker and will be followed by the development of the new SVX3 radiation-hard deadtimeless readout chip for CDF.

For the ATLAS experiment at LHC, LBNL has undertaken a significant responsibility for the silicon tracking system. This system will consist of a combination of silicon strips and pixels and has demanding radiation-hardness requirements. Significant progress has been made in testing detectors and electronics irradiated at levels expected in the LHC operation.

LBNL has a well-established program of work on the BABAR detector at SLAC, following its involvement with the Mark II, PEP-4 (TPC), and SLD collaborations. Work on BABAR includes the vertex detector, the DIRC particle identification system, the trigger and data-acquisition systems, and online and offline software. LBNL has the responsibility for the design and integration of the ATOM silicon vertex chip and for the design and construction of the precision mechanical support for the silicon vertex tracker. LBNL is responsible for the design and construction of the central mechanical support tube for the BABAR DIRC system and has worked on the DIRC prototype. LBNL has focused on many areas of computing for BABAR. These include online software structure, detector control systems, offline reconstruction, databases, calibration systems, and event storage.

### *2. Accelerator Research and Development*

Accelerator research and development efforts, centered in the Accelerator and Fusion Research Division, exploit expertise in superconducting magnet technology, rf cavity design, feedback system design, ion sources, and laser-based technologies. LBNL has designed the PEP-II rf cavities and damping systems. With Fermilab and BNL collaboration, LBNL works on the LHC interaction region quadrupoles. For a possible future muon collider, LBNL is concentrating on the design of the collider ring magnets and ionization cooling. LBNL is pursuing very high field magnets for future hadron colliders and has achieved 10.1 T using NbTi, and 13.5 T using $Nb_3Sn$ in 50 mm aperture short magnets operated at 1.8 K. This program aims at developing a short magnet based on $Nb_3Sn$ over the next few years, operating at 16 T, with half the cost per T-m of today's technology.

In collaboration with SLAC, LBNL has worked on the design of the damping rings for a future electron-positron linear collider. They have also pursued applications of lasers to accelerator operations, including the design of a photon collider facility based on back-scattered laser light, and the use of lasers for optical stochastic cooling of high-energy hadron beams. An initial experiment aimed at detecting optical fluctuation signals in the LBNL Advanced Light Source has been proposed.

## G. ARGONNE NATIONAL LABORATORY

The Argonne High Energy Physics Division brings the resources of a multi-disciplinary lab to the service of high-energy physics, enabling collaborating university groups to gain access to unique facilities for detector construction and application of advanced techniques. The Argonne experimentalists collaborate on the CDF experiment at Fermilab, on ZEUS at HERA, on ATLAS at LHC, on STAR at RHIC, and on underground experiments on astrophysical and accelerator neutrino interactions in the Soudan mine in Minnesota. Argonne also has a facility devoted to the study of wakefield acceleration techniques.

### *1. The Current Experimental Program*

The experimental physics program at Argonne involves collider physics at CDF, ZEUS, ATLAS, and STAR, and underground and neutrino physics with Soudan 2 and MINOS. The CDF group has concentrated on the central electron and photon identification as appropriate for their focus on b quark, QCD, and electroweak physics. They have led the studies on the discovery potential for the Higgs boson at TeV33. The Argonne CDF group helped build the scintillator calorimeter and contributed to the outer tracker mechanical structure, the management of the muon upgrade, and the shower maximum and preshower detector front-end electronics. At ZEUS, the group has led analyses in jet physics, jet corrections, and high-x and $Q^2$ physics. They took a lead role in building the calorimeter and now are working on the calorimeter first-level trigger processors, the small-angle track trigger, calorimeter monitoring, and the barrel presampler upgrade. For ATLAS, Argonne has assumed major responsibilities for the mechanics of the tile calorimeter modules. Here they work closely with several U.S. universities, receiving components at Argonne for assembly and instrumentation prior to shipment to CERN. At RHIC, they are engaged in the STAR experiment, with particular interest in the measurement of the gluon and anti-quark contributions to the proton's spin.

Soudan 2 is an experiment using a 960-ton iron sampling and tracking calorimeter in the Soudan mine. Argonne took the lead role on Soudan 2, which has complemented water Cherenkov detectors in nucleon decay searches and has independently observed the atmospheric neutrino deficit observed in the water detectors. Having played a critical role in establishing the infrastructure at Soudan, Argonne now plans to collaborate on MINOS, whose far detector would be built in the Soudan Laboratory.

A common thread running through the collider experiments has been the development of scintillator calorimeters and studies of the physics enabled by calorimetry. Argonne has also made important contributions to detector development for Soudan 2 (long drift calorimeter), MINOS (proposed large-area aluminum proportional tubes), and in electronics and trigger systems (for CDF, ZEUS, and ATLAS).

### *2. Accelerator Research and Development*

The Argonne accelerator research program has focused on development of new techniques for acceleration using the transfer of energy from high-current, low-energy beams to a low-current, high-energy beam. The accelerator research and development group at ANL uses a very intense photo-injector (100 nC/bunch at 15 MeV) to study the physics of wakefield acceleration in dielectric waveguides, disk-loaded waveguides, and plasmas. Electron beam self-focusing in under-dense plasmas has also been observed and studied for the first time by the ANL group.

The ANL test facility provides a high-intensity drive bunch to generate wakefields that can then be probed by a witness bunch, whose delay relative to the drive bunch can be adjusted. Energy analysis of the witness bunch allows an investigation of the drive bunch wakefield. Future plans call for a "step-up transformer" experiment, in which the energy lost as the 15 MeV drive beam passes through a dielectric structure is coupled out to accelerate the witness beam; energy gains of 100 MeV/m for the witness beam are expected.

## H. THE LARGE HADRON COLLIDER

The LHC is a proton-proton collider that will be installed in the 27-kilometer tunnel that is now used by the LEP electron-positron collider at CERN. The machine is scheduled for completion in 2005. The collider is designed to run at a center-of-mass energy of 14 TeV (a factor of seven higher than the Fermilab Tevatron collider) and at a luminosity of up to $10^{34}$ $cm^{-2}$ $s^{-1}$. This energy and luminosity will give the LHC an effective energy reach approximately ten times that of the Tevatron.

Four experiments are planned for the LHC. Two, ATLAS and CMS, are large general-purpose detectors designed to study phenomena at the high-energy frontier. The other two are special-purpose detectors: ALICE will study heavy ion collisions and LHC-B is designed to explore B decays. A fifth experiment, FELIX, has recently been proposed for the study of forwardly produced particles. U.S. physicists are playing essential roles in the construction of the LHC collider and in the design and construction of the ATLAS and CMS detectors, building on the technology developed for the SSC.

The U.S. participation in the construction of the LHC builds on the strengths of the U.S. accelerator physics community. Areas of involvement include superconducting magnet technology, beam dynamics, and interaction region design. Approximately forty U.S. accelerator physicists are working on the project. Three national laboratories are involved: Fermilab, BNL, and LBNL (see figure 4.5).

Approximately five hundred U.S. physicists and engineers are working on the ATLAS and CMS detectors. The number of U.S. physicists in the two collaborations is about the same. U.S. contributions to ATLAS will include one-half to one-third of the silicon pixels, one-third to one-quarter of the silicon strips, the central transition radiation tracker, most of the readout electronics for the liquid argon calorimeter, the electromagnetic section of the forward calorimeters, about one-third of the scintillator tile calorimeter, the endcap muon system, and contributions to the trigger. The U.S. CMS contributions include the forward silicon pixels, the complete barrel and forward hadron calorimeter systems, the electromagnetic calorimeter front-end electronics, the endcap muon detectors, and part of the trigger system.

A formal US/CERN agreement on LHC participation has been negotiated, and signing ceremonies were held in December 1997. The agreement calls for U.S. high-energy physics funding of LHC projects totaling \$531 million. The DOE will provide \$450 million, of which \$200 million will go toward the LHC accelerator projects and the remainder to the ATLAS and CMS detectors. The NSF will contribute \$81 million to the detectors. The total U.S. funding for ATLAS and CMS is expected to be equal.

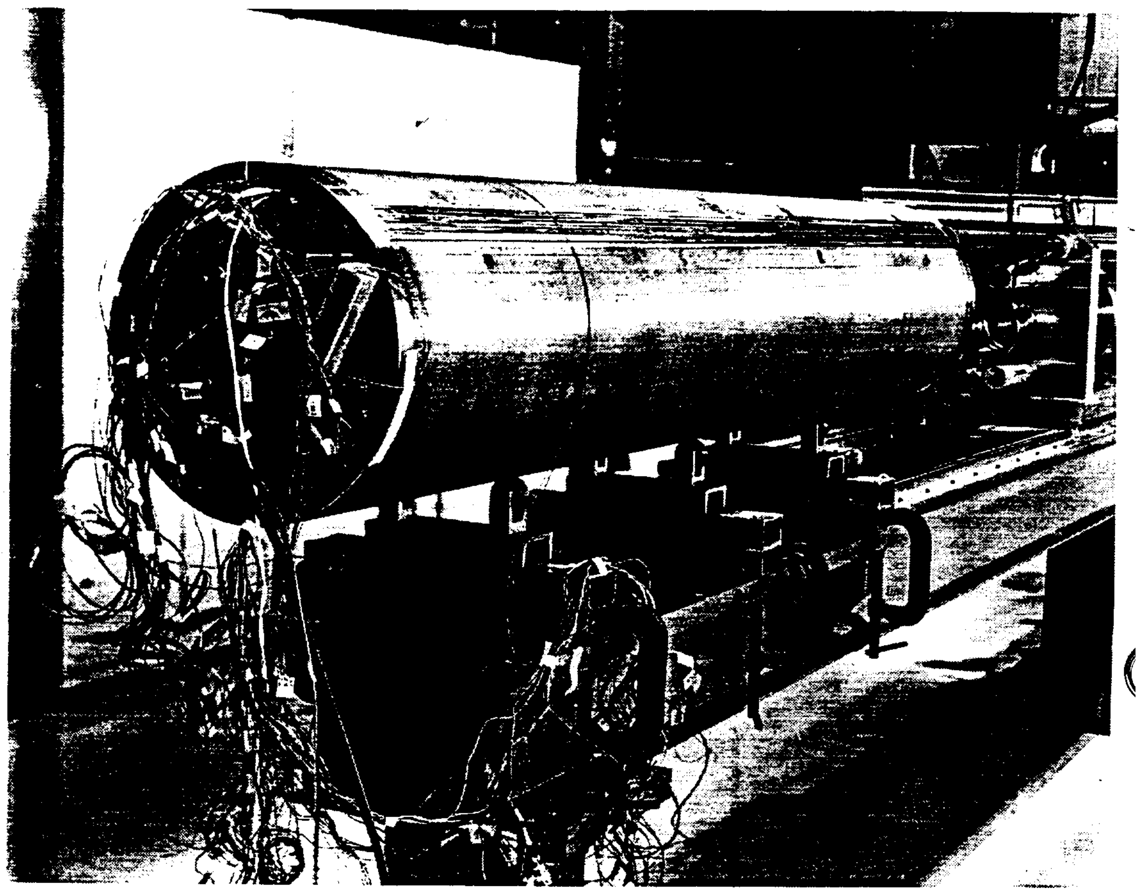

Figure 4.5 This model for the interaction region quadrupole magnets for the LHC at CERN is being tested at Fermilab. The magnet consists of four double layer coils of NbTi and will operate in superfluid liquid helium at 1.9 K. U.S. contributions to the LHC build on the strengths of accelerator physicists in the U.S. program. Three national laboratories are involved: Fermilab, BNL, and LBNL. Approximately five hundred U.S. physicists and engineers from universities and national laboratories around the country are working on the detectors for the LHC.

The primary physics goals of the general purpose experiments at the LHC include

- discovery or exclusion of the conventional Higgs boson and/or discovery of the multiple Higgs bosons as predicted in supersymmetric models,
- discovery or exclusion of supersymmetry over the entire mass range where supersymmetry would be relevant for electroweak-symmetry breaking, and
- discovery or exclusion of any new dynamics at the electroweak scale.

The LHC would also be sensitive to a variety of new particles or phenomena not predicted by the Standard Model, including the production of new W and Z bosons or additional quark or lepton species.

The LHC is unique among approved accelerator projects in having sufficient energy and luminosity to study the phenomena associated with electroweak-symmetry breaking, the mechanism by which elementary particles acquire mass. Detailed studies by the ATLAS and CMS collaborations demonstrate that these detectors can carry out the physics program described above. While it is not possible to predict what physicists will find at the LHC, there is no question that the results of this program will profoundly change our understanding of particle physics.

## I. OTHER ACCELERATOR EXPERIMENTS ABROAD

Of the university physicists presently doing experiments at accelerators, 28% are working at accelerators abroad. We estimate that this fraction will rise to 35% by 2002 as efforts shift to the LHC experiments. These endeavors are an important component of the U.S. program, as they capitalize upon opportunities not present in this country. U.S. physicists have held leadership positions in many of these experiments.

The LEP electron-positron collider at CERN operated for many years as a Z factory. It has now increased its energy by about a factor of two to 183 GeV, permitting the production of W boson pairs. Four large detectors, ALEPH, DELPHI, L3, and OPAL, are operating at LEP, all with U.S. university group collaboration. When operating at the Z mass, the LEP experiments made a long series of seminal measurements that refined our knowledge of both the electroweak and strong forces. These experiments also demonstrated that there are only three light neutrino families, bolstering the Standard Model assertion of three generations of quarks and leptons. The primary physics goals now are the study of W bosons and the search for the Higgs boson and for evidence of supersymmetry. After the full upgrade of the LEP energy, the LEP experiments should discover a Higgs boson if its mass is less than about 100 GeV.

U.S. groups also participate in fixed-target experiments at CERN. The NOMAD experiment seeks evidence for neutrino oscillations from $\nu_\mu$ to $\nu_\tau$. Experiment NA-47 is a study of role of the constituents of the proton and neutron in providing their intrinsic spin.

The HERA electron- or positron-proton collider at DESY in Hamburg is a unique facility that allows the study of the structure of the proton at very small distance scales. By colliding an electron or positron with a hadron, it is possible to seek new kinds of particles and to study fundamental interactions with techniques that complement both electron-positron and proton-(anti)proton colliders. Several U.S. groups participate in the ZEUS experiment and provide important leadership roles. There is also a small U.S. contingent on the H1 experiment. The U.S. ZEUS physicists have led the studies of the inelastic scattering of electrons and positrons from protons. These studies recently showed an excess of events at large $Q^2$ that, if verified, would signal some new physics beyond the Standard Model. The ZEUS results have also extended our knowledge on the quark and gluon constituents within the proton to much smaller momentum fractions than achieved before. The HERA experiments are expected to continue for at least six more years, with upgrades to the accelerator and detectors.

The HERA-B experiment at DESY, starting in 1999, will search for CP violation in the decays $B \rightarrow J/\psi K_S$, using the HERA proton beam and wire targets inserted close to the beams. Several U.S. groups participate in HERA-B.

The KEK laboratory in Japan is constructing an asymmetric-energy B factory, KEK-B, closely resembling the SLAC PEP-II collider. The BELLE detector is being built by a collaboration that includes several U.S. groups. BELLE will complement BABAR in seeking CP violation in neutral B decays; the two programs have chosen different solutions to many specific technical problems. KEK also operates several fixed-target experiments and participates in the kaon program at Brookhaven. KEK is planning a new long-baseline neutrino beam to study neutrino oscillations, using an on-site detector and the Super-Kamiokande detector; U.S. groups are helping lead this K2K project.

U.S. groups also have a small participation in programs at other laboratories, notably at Beijing, China; at Dubna, Novosibirsk, and Serpukhov in Russia; and at Frascati in Italy.

## J. NON-ACCELERATOR EXPERIMENTS

Non-accelerator physics experiments span a broad-based, multi-disciplinary venture at the interfaces between particle physics, nuclear physics, astrophysics, astronomy, and cosmology, and they allow the study of some of the most fundamental questions in particle physics. Currently, about 15% of experimental physicists funded through the university programs of DOE and NSF are associated with non-accelerator experiments. The national laboratories are also beginning to invest resources in this endeavor. The NSF supports the LIGO project that seeks evidence for classical gravitational radiation.

Until the 1950s, most of the discoveries in particle physics were made by non-accelerator experiments, and they continue to complement accelerator experiments today. Further progress will come from pushing both the high-energy and high-sensitivity limits at accelerators and from pursuing new non-accelerator experiments.

### *1. Proton Decay and Monopole Searches*

Some very fundamental properties of particles can only be probed by non-accelerator methods. For example, any theory in which a simple gauge group unifies the strong and electroweak interactions necessarily predicts proton decay and magnetic monopoles. A unification energy scale of order $10^{16}$ GeV implies proton lifetimes greater than $10^{30}$ years and extremely massive monopoles. In the 1980s and 1990s several non-accelerator experiments pursued these tests of grand unified theories. No proton decays were observed, but lower lifetime limits were established for forty-five possible decay modes. This work ruled out the simplest theory of unification. The ongoing proton decay experiments are Super-Kamiokande in Japan and Soudan 2 in a Minnesota mine; ICARUS is under construction at the Gran Sasso Laboratory in Italy. All experiments include U.S. participation. The question of proton decay is so fundamental to both particle physics and cosmology that any further evidence for grand unification would motivate additional proton decay experiments to the longest lifetimes experimentally accessible.

Several small-scale magnetic induction and scintillator searches for magnetic monopoles were performed in the 1980s. With stringent astrophysical constraints on the monopole flux, few induction experiments continue at this time. The largest ongoing effort is the U.S.-Italy MACRO experiment at Gran Sasso. Within a few years, this experiment will reach a sensitivity about a factor of ten below the astrophysical Parker Bound for the flux of magnetic monopoles in the galaxy.

If neutrinos have different masses, then, like the quarks, the different neutrino flavors can mix. The resultant non-conservation of lepton number would be outside the Standard Model. The search for neutrino mass and oscillation requires long baselines to reach the small mass differences expected. The studies of neutrinos produced in the Sun, in the earth's atmosphere, or in nuclear reactors gives this possibility.

Early measurements of solar neutrinos established that nuclear fusion powers the Sun, but showed fewer neutrinos than expected by standard solar models. Recent gallium-based experiments have confirmed the deficit by measuring the low-energy neutrinos from the dominant solar p-p reactions. Using neutrino-electron elastic scattering, the Kamiokande water Cherenkov experiment showed that the detected neutrinos were indeed coming from the Sun. Combined data from all experiments with their different energy regimes indicate a possible energy-dependent suppression consistent with matter-enhanced neutrino mixing within the Sun, although fine-tuned vacuum oscillations are still allowed. Experiments now planned or in progress will improve our understanding. The Super-Kamiokande experiment has confirmed the earlier results, with smaller uncertainties, in its first year of operation; it soon will be precise enough to decide which of the two solar matter-enhanced mixing solutions is favored, based on measurements of the day-night variations in $\nu_e$ flux and the low-energy neutrino spectrum. The SNO detector, being completed in Canada with U.S. participation, will measure the neutral-current interaction $\nu d \rightarrow \nu p n$, which will provide distinctive indications of neutrino oscillations. Other planned and proposed solar neutrino experiments have the aim of helping to resolve this puzzle. Furthest along is the Borexino experiment being constructed at the Gran Sasso Laboratory with U.S. participation, which will directly measure the $^7$Be monoenergetic neutrinos for which the energy-dependent matter-mixing suppression would be maximum. Within the next decade, the neutrino oscillation solution of the solar neutrino problem should be well established, or new light will be cast on unknown astrophysical or particle physics alternatives.

The IMB experiment, in an Ohio salt mine, and Kamiokande studied neutrinos produced in atmospheric cosmic ray showers and detected an apparent anomaly in the ratio of observed $\nu_e$ and $\nu_\mu$. No conventional explanations for the effect have been found. The possibility that the atmospheric neutrinos oscillate was strengthened by the apparent zenith angle distribution of the high-energy data from Kamiokande. The Soudan 2 experiment has confirmed the anomalous ratio of $\nu_e$ to $\nu_\mu$ rates. The Super-Kamiokande experiment, with twice the world's prior data, has shown preliminary data confirming the anomaly and its zenith-angle dependence (see figure 4.6). Experiments are now planned in the U.S., Japan, and Europe to search for oscillations using controlled, long-baseline neutrino beams from accelerators.

Low-energy anti-neutrino beams from nuclear reactors have been used to seek neutrino oscillations. In these, the disappearance channel $\nu_e \rightarrow \nu_x$ is studied over typical distances of 10 m to 1 km. Because of the low neutrino energy and the use of the disappearance method, these measurements are very sensitive to small mass differences but are not well suited for small mixing angles. U.S. physicists are engaged in the current CHOOZ experiment, in France, and the Palo Verde experiment, in Arizona, both located about a kilometer from high-power reactors. Results from the CHOOZ experiment have recently shown that $\nu_\mu \rightarrow \nu_e$ cannot be the source of the atmospheric neutrino anomaly. Next-generation experiments with detectors located about 100 km from the reactors are planned in Japan and may also be performed in Taiwan.

### *3. Dark Matter*

There is good astronomical evidence that most (~90%) of the mass of the universe is not directly observable. A wide variety of candidate objects have been suggested to explain this missing mass. Although baryonic macroscopic objects, such as dark stars, may account for some of the dark matter, current data from cosmology favor a strong non-baryonic component, implying a role for particle physics. Candidate particles include axions, WIMPs (weakly interacting massive particles), neutralinos from supersymmetry, and massive neutrinos. There are numerous experimental efforts employing diverse techniques such as cryogenic detectors, tuned cavities, germanium detectors, and short-baseline neutrino beams. These efforts are of considerable interest to both the astrophysical and particle physics communities.

### *4. Particle Astrophysics*

Particle astrophysics experiments study cosmic beams of photons, protons, and neutrinos with energies far exceeding those from accelerators. How and where nature accelerates particles to these energies is still unknown. Particle physics instrumentation is being applied in Earth- and space-based gamma ray telescopes, a space-based particle spectrometer, very large air shower arrays, and giant neutrino detectors to study this high-energy regime. These studies open new windows on the universe and investigate nature’s highest-energy interactions.

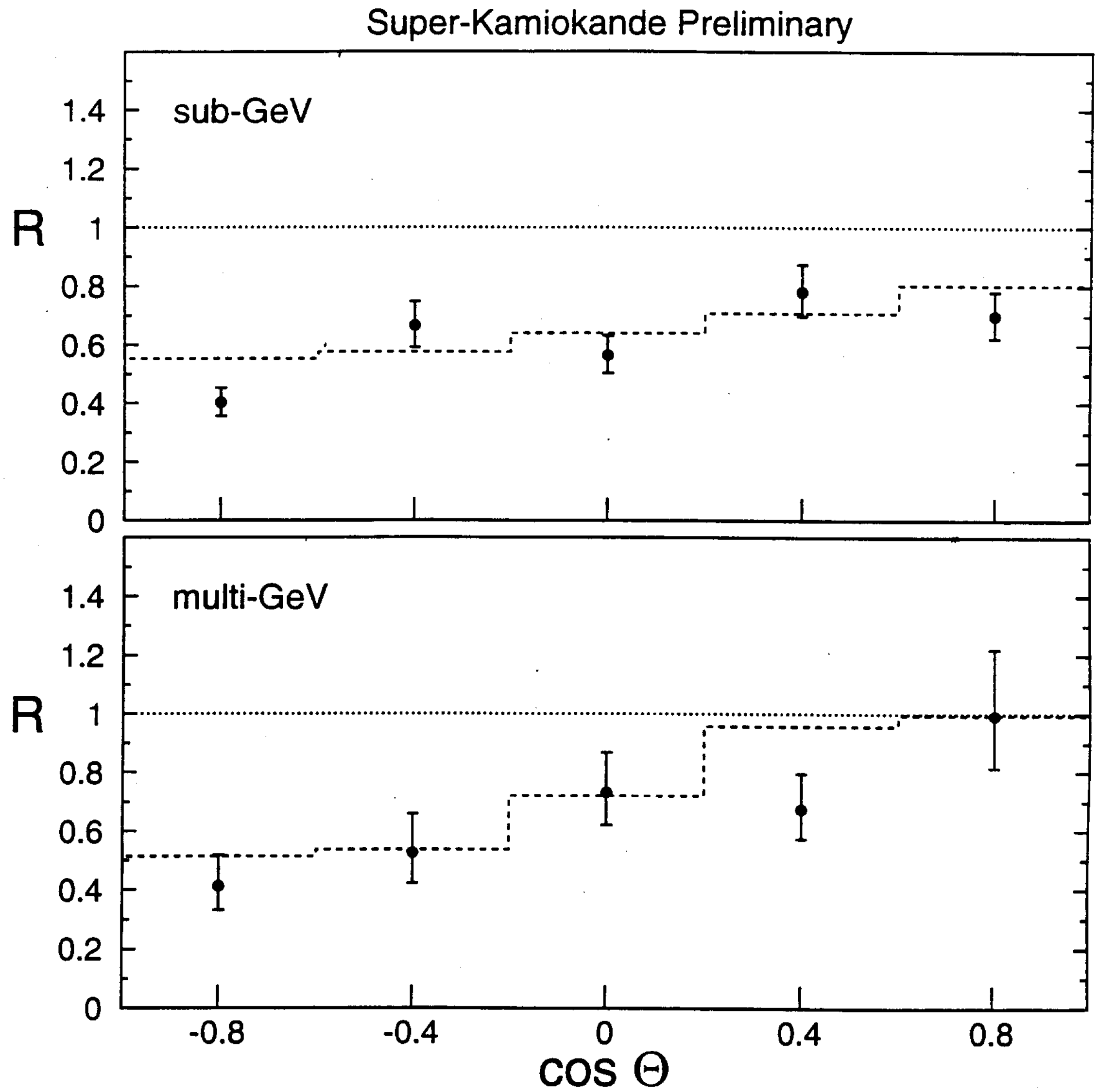


Figure 4.6 Currently, about 15% of university-based experimental physicists in the U.S. high-energy physics program probe the properties of elementary particles using non-accelerator experiments. In the Super-Kamiokande detector, for example, neutrinos produced in cosmic-ray interactions in the earth's atmosphere show indications of neutrino oscillations. The ratio of observed muon- to electron-neutrino interactions, divided by the ratio expected from Monte Carlo simulation assuming no neutrino oscillations, is shown as a function of the zenith angle ($\cos\theta = 1$ corresponds to neutrinos coming from above). The upper figure is for neutrinos below 1 GeV, and the lower figure is for neutrinos above 1 GeV. The data are inconsistent with $R = 1$ (no neutrino oscillations). The dashed histogram shows a sample prediction for $\nu_\mu \rightarrow \nu_\pi$ oscillations with $\sin^2 2\theta = 1$ ($\theta$ is the mixing angle) and $\Delta m^2 = 0.005\ \text{eV}^2$.

There are many ongoing and proposed experimental efforts in gamma ray astronomy, both on the ground and in space, involving particle physicists. Gamma rays produced by high-energy particle interactions in astrophysical sources can travel cosmological distances in straight lines, and so can be identified with discrete sources. In recent years, a large number of sources have been identified in the few MeV to few GeV range by spacecraft. Above 100 GeV, ground-based air Cherenkov experiments have identified a growing number of gamma ray sources. These observations now include enough events to study the properties of the Crab and other pulsars, and supernova remnants. A large number of active galactic nuclei were found to emit energetic gammas, and the multi-GeV emissions were discovered in association with enigmatic gamma ray bursts. Several gamma ray bursts have been observed in the tens of GeV range by spacecraft, and ground-based observatories hope to catch multi-TeV gamma bursts.

Existing experiments, such as the Fly's Eye, in Utah, have measured a few hundred examples of cosmic ray particles of energies greater than $10^{19}$ eV interacting with the earth's atmosphere. Particles of energies greater than $4\times10^{19}$ eV have a comparatively short mean free path in intergalactic space, due to their interactions with the 2.7 K cosmic microwave background. Such particles are extremely rare, so a new generation of giant arrays would be needed to detect a significant number of these ultra high energy cosmic particles. Very large ground-based detectors sensitive to extensive air showers have been proposed. The techniques used in these projects would borrow heavily from accelerator-based detector and data-collection technologies and would draw significant participation from particle physicists. The study of these very high energy particles and their interactions should provide a glimpse of physics far beyond the reach of current accelerators.

Many exciting results from the detection of cosmic neutrinos have emerged from non-accelerator experiments in the recent past. The observation of neutrinos from supernova SN1987A by IMB and Kamiokande confirmed the theory of stellar collapse to a neutron star, gave a limit on the mass of the electron neutrino similar to that obtained by laboratory experiments, and severely constrained the mass of the axion. From the astrophysical side, the high-energy neutrino sky is basically unexplored. There are experimental efforts at various stages to search for neutrinos from active galactic nuclei and for the neutrino component of gamma ray bursters. Projects are now being planned to observe neutrino interactions in the relatively transparent Antarctic ice and deep sea water. The study of cosmic neutrinos will be an important complement to those with gamma rays and particles, since ultra high energy neutrinos can reach us directly from the deepest reaches of the universe.

K. SUMMARY

Over the past several years, the U.S. has conducted a vigorous experimental and theoretical high-energy physics program. Several notable discoveries have advanced our understanding of the basic constituents of matter and the forces by which they interact. The last expected constituent of matter, the top quark, was discovered at Fermilab with a mass so much larger than its companions that it is regarded as a key for understanding the origin of mass itself. Experiments at SLAC, and at CERN with U.S. collaboration, have shown that there are indeed only three light lepton generations. Together with Fermilab, these facilities have made it possible for physicists to verify the basic characteristics of the electroweak and strong forces and to make precision measurements of the parameters of that theory. Experiments at Cornell, Brookhaven, SLAC, and Fermilab have explored the properties of hadrons containing the heavy quarks—strange, charm, and bottom—and have greatly improved our understanding of hadron structure, quark mixing, and CP violation. Experiments using fixed targets at SLAC and Fermilab, and at the DESY electron-proton collider in Germany, have yielded a precise understanding of the interplay of the constituents of the proton. Non-accelerator experiments have verified the deficit of neutrinos coming from the Sun, have found new indications of possible mixing of neutrino types from studies of neutrino oscillation, and have detected particles of extremely high energy from astrophysical sources.

This very successful program is possible because of earlier development of state-of-the-art accelerator facilities and detector techniques, and because of many insightful theoretical investigations. The new LHC collider at CERN and new facilities being completed at Fermilab, SLAC, Cornell, and CERN form the basis for the next round of experiments. It is clear that the research done at these new facilities will dramatically expand our knowledge of the fundamental construction of matter and make discoveries that lead us beyond the current Standard Model. The effective use of these facilities is of paramount importance. Research and development of the new techniques needed for the next stage of experimentation are likewise crucial for an effective U.S. high-energy physics program.

# 5 Possible Major Future Facilities

*If an experiment turns out precisely as predicted, this can be very nice, but it is only a great event if at the same time it is a surprise. . . . The surprise can be because it did turn out as predicted . . . , or it can be confoundment because the prediction was wrong and something totally unexpected turned up. . . . Either way, you win.*

—Lewis Thomas, *The Lives of a Cell*

## A. INTRODUCTION

The full exploration of the scientific issues discussed in chapter 3 will require a new high-energy collider facility that will complement and extend the physics reach of the current set of facilities and the LHC. In this chapter, we review the motivations for this new facility and describe the development status of three very different approaches currently under study in the U.S.: a second-generation electron-positron linear collider (a lepton collider), a high-energy muon collider (also a lepton collider), and a hadron collider with a beam energy several times higher than the LHC's, known as a VLHC (for "Very Large Hadron Collider"). Because of the many areas of technology that must be improved, the lead times associated with the development of major new facilities are typically measured in decades. This considerable time scale requires that we invest now in the development efforts that will lead to the high-energy physics facilities beyond the LHC.

Understanding electroweak-symmetry breaking is the most pressing issue before us. (Chapter 3 discusses three possibilities for the source of electroweak-symmetry breaking.) The LHC will search for Higgs particles over their entire allowed mass range. If these particles exist, the LHC will discover them and make initial measurements of some of their properties. Other properties will remain undetermined (such as the couplings to many of the fermions). A lepton collider with mass reach comparable to LHC, but with different initial states and less complicated backgrounds, would expand our vision of electroweak- symmetry breaking.

Should the origin of electroweak-symmetry breaking emerge from a supersymmetric theory with superparticles below the TeV mass scale, the LHC will discover this fact and uncover some of the properties of these particles. In this case, the full understanding of the particle spectrum is likely to remain open, as the complexity of the events observed at the LHC and the specific initial states will limit these measurements. Again, a new lepton collider that allows probes of supersymmetry with different couplings would expand the view and supplement the LHC discoveries.

Finally, there is the possibility that electroweak-symmetry breaking results from a new strongly interacting sector. In this case, a new gauge interaction would emerge at the TeV scale. Again, probes with either different couplings or higher energy will be necessary to fully decipher the LHC results.

This need for new windows on the unknown motivates the study of the three types of colliders discussed in this chapter. These colliders would either extend our understanding at the mass scale of the LHC (the electron-positron collider) or permit us to explore mass scales beyond the LHC (the muon collider and the VLHC).

The criteria for a new collider depend on the technology chosen; more precisely, on the particles that collide. The parameters of importance are the energy and the luminosity. A hadron collider needs substantially more beam energy than a lepton collider to probe a similar mass scale, because the particles used in hadron colliders (protons and/or antiprotons) are composites of three point-like quarks surrounded by a cloud of gluons and quark-antiquark pairs. These constituents must share the beam energy of the colliding hadrons; any individual constituent typically carries only a small fraction of the beam energy into the collision. A further consideration in defining the capabilities of a new collider is the extremely high luminosity needed to produce enough high-mass events to probe the most interesting phenomena with sufficient precision. The probability for head-on collisions of point-like particles is reduced as their collision energy is increased, but it is just such collisions that probe the highest mass scale accessible to a high-energy collider. Because of this, achievement of the design luminosity will be critical in future high-energy colliders.

The required capabilities of a new high-energy collider to follow the LHC have been the subject of extensive study by the high-energy physics community. In the summer of 1996, a meeting at Snowmass of several hundred high-energy physicists, including participants from abroad, addressed this issue comprehensively.

The Subpanel has based its criteria for a new collider primarily on the work done at Snowmass. An electron-positron collider with the capability of reaching 1.5 TeV center-of-mass energy and with a luminosity of $10^{34}$ cm$^{-2}$ s$^{-1}$ would have a reach comparable to the LHC. A muon collider with 3 TeV center-of-mass energy and a luminosity of $10^{35}$ $cm^{-2}$ $s^{-1}$ would extend the field's reach well beyond the LHC. A muon collider at a significantly lower energy could also be very interesting as a Higgs factory. Construction and operation of such a low-energy machine would be a natural step in the development of a high-energy muon collider. A hadron collider with 100 TeV in the center of mass (seven times higher than the LHC) and a luminosity of $10^{34}$ $cm^{-2}$ $s^{-1}$ would also extend the field's reach well beyond LHC.

The target capabilities of these potential colliders are given in table 5.1, along with a brief overview of their research and development status. Each represents a significant extension beyond current experience in both energy and luminosity.

The three approaches are in different stages of development. The second-generation linear collider is the most advanced. It builds on the experience gained in operating the SLC over the past decade and has an advanced research and development program. This effort is ready to proceed to a complete conceptual design and cost estimate.

The muon collider effort is younger, more speculative, and represents a completely new approach—no muon collider has ever been built. The muon collider concept makes it possible to use a storage ring to create a very high-energy lepton collider; such a possibility does not exist for electrons because of the enormous loss in energy to synchrotron radiation. However, the research and development effort has only been underway for a few years, and many technical issues need to be resolved before the feasibility of this approach can be validated.

The VLHC efforts build on recent experience with the Tevatron and with the design of the SSC and LHC. Two approaches, based on high-field (>10 T) and low-field (<2 T) superconducting magnets, are contemplated. For the low-field option, the next steps involve magnet prototyping, accelerator design studies, cost minimization studies, and possibly demonstration projects. For the high-field version, superconducting material and magnet development is the next required step.

Construction of any of these facilities would require a substantial investment. As a result, development of technologies with significant potential for cost reduction is a primary focus of all three research and development efforts. More detailed descriptions are provided in the subsections that follow.

POSSIBLE MAJOR FUTURE FACILITIES

| Facility | Energy (TeV) | Luminosity ($cm^{-2}s^{-1}$) | R&D Status |
|---|---|---|---|
| Second-Generation Electron-Position Linear Collider | 1.5 | $10^{34}$ | Complete design concept. Major subsystems prototyped. Engineering system cost minimization studies underway. Ready for complete conceptual design. |
| Muon Collider | 3 | $10^{35}$ | Design concepts require validation. through experiments and simulations. Demonstration projects and/or major subsystem prototyping now being discussed. |
| Very Large Hadron Collider | 100 | $10^{34}$ | Design concepts exist. Development of cost minimization strategies and overall systems integration required. Ready for some prototyping and modest demonstration projects (low field). Development of superconducting magnet technology required (high field). |

Table 5.1 Possible facilities beyond the LHC

## B. LINEAR COLLIDERS

A number of laboratories around the world are currently engaged in research and development on electron-positron linear colliders, with the goal of developing realizable, cost-effective designs for operating at an energy and luminosity significantly beyond the performance of the SLC.

SLAC is the center of U.S. linear collider expertise. An active research and development program there has been underway over the past decade. This work builds on the experience gained from operating the world's only linear collider, the SLC, and targets the development of technologies that will be required to construct and operate a second-generation linear collider (figure 5.1). The goal of the SLAC effort is the development of a 1 TeV linear collider with a luminosity of $10^{34}$ $cm^{-2}$ $s^{-1}$ upgradable to 1.5 TeV.

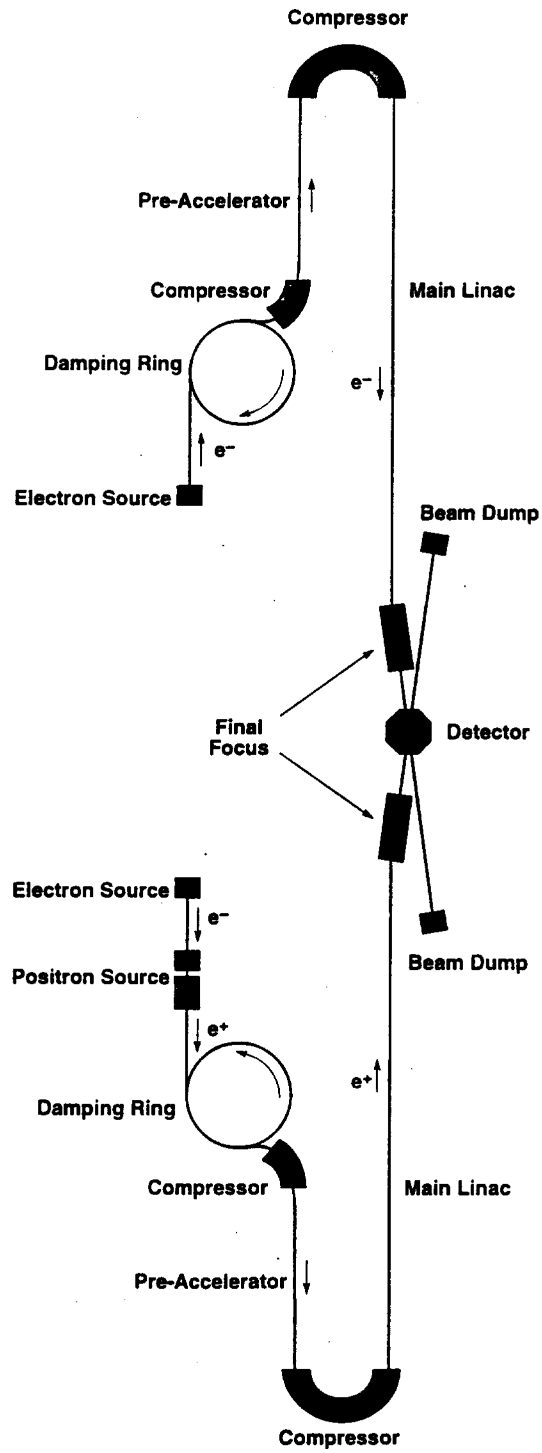


Figure 5.1 Schematic layout of a 1 TeV electron-positron linear collider (figure not to scale). The overall length is about 30 km. Linear colliders can be built and operated in stages of increasing energy. When extended to 1.5 TeV, this machine would reach mass scales roughly comparable to the LHC and provide a complementary approach to addressing physics issues, with relatively simple backgrounds. Its technology development is mature, and it is ready to enter the conceptual design phase.

The approach being pursued is based on a room-temperature, X-band (11 GHz) accelerating structure. Such an implementation represents a direct extrapolation of SLC experience. Major development efforts are also centered at KEK in Japan, and DESY in Germany. The KEK approach also uses room-temperature technology; DESY is pursuing a design based on superconducting accelerating structures operating at 1.3 GHz.

Extrapolation from current experience to a second-generation linear collider is significant, representing a factor of ten to fifteen in energy and nearly four orders of magnitude in luminosity. Critical technology issues associated with the development of a credible design include rf power systems, accelerating structures, final-focus optics, beam alignment, stability, emittance control, beam scraping and cleanup, and reliability. The SLAC/KEK X-band designs require very small beam sizes (a few nanometers high). Component fabrication and alignment tolerances, precision control of beam trajectories, and removal of optical aberrations to high order are especially critical in these designs. Many of the requirements can be relaxed if a superconducting accelerating structure is used, allowing an increase in the bunch train length. The trade-off is that superconducting accelerating structures are inherently more expensive and provide a lower accelerating gradient. The required facility is nearly twice as long as a room-temperature-based facility, which potentially limits energy expandability.

As important as technical issues is the cost. It is known that a 1 TeV linear collider will be a multi-billion dollar project. Given the possible resources that might be available for construction of such a facility, optimization of design parameters and configurations is extremely important throughout the early design stages.

SLAC issued a "zeroth-order" design report (ZDR) for a machine called the NLC (for "Next Linear Collider") in the spring of 1996. The NLC ZDR represents a relatively well-developed concept for a 0.5 TeV linear collider, intended as the initial phase of a 1 TeV facility. As conceived, the 0.5 TeV facility would be constructed with the accelerator configured to allow for doubling the energy by doubling the number of rf power sources. The final focus geometry is designed to accommodate an upgrade to 1.5 TeV. At present, the concept for producing a 1.5 TeV accelerator is to lengthen the facility, while keeping the accelerating gradient fixed. A number of research and development initiatives have been directed towards validation of performance requirements in several important underlying systems. The Final Focus Test Beam facility has produced a 70 nm spot size, demonstrating the required demagnification, although the spot size required for the NLC cannot be achieved due to the higher emittance of the SLAC linac. The Accelerator Structure Setup facility has demonstrated the viability of the damped/detuned structure concept for controlling wakefields.

The klystron development program has yielded a solenoid focused klystron capable of producing a 75 MW pulse and generating a gradient of 70 MV per meter in an unloaded accelerating structure. The corresponding requirements for 0.5(1) TeV operations are 50 (75) MW and 50 (85) MV per meter. These klystrons are being used to support the Next Linear Collider Test Accelerator facility, a 350 MeV prototype section of the NLC, currently in operation. However, these tubes would not be cost effective in the NLC because of the large power consumption in the solenoids. A first-generation periodic permanent magnet klystron has been constructed that overcomes this problem with a demonstrated output of 55 MW at 60% efficiency. A second-generation unit is being developed, and it appears likely that within a year a periodic permanent magnet klystron will exist that is capable of meeting the performance requirements of a 1 TeV NLC. For 0.5 TeV operation, 3300 such klystrons are required, and double that number for 1 TeV operation.

In parallel with efforts on the SLAC site, SLAC has entered into a collaboration with KEK to develop an advanced test facility in Japan for studying damping ring performance requirements. Commissioning of this facility began in January 1997.

The NLC ZDR was reviewed in the spring of 1996 by an international team of accelerator physicists. This panel concluded that a technical basis had been established to support performance goals for most major subsystems, and that primary outstanding issues were related to systems integration, operational stability, reliability, and reduction of costs. This assessment is still valid today. Since that review, significant effort has been invested in cost-reduction and integration of "design for manufacture" concepts into the design. Significant progress has been made in several areas; for example, the number of power systems required was reduced by 30% over the last year.

The scope of a second-generation linear collider appears to require an international approach to design, construction, and operation. To this end, SLAC has played a leading role in the creation of a world-wide effort to coordinate linear collider research and development activities. Recently, SLAC and KEK have negotiated an inter-laboratory Memorandum of Understanding that would form the basis of a research and development program towards a common design. The natural next step is the production of a Conceptual Design Report with a complete technical design and associated cost and schedule for specific sites. DESY is also aggressively pursuing the development of a technological base for a 500 GeV linear collider, called TESLA (for "TeV Electron Superconducting Linear Accelerator"). A design study, based on superconducting accelerating cavities, has been released and reviewed.

The critical issue in the TESLA approach is the development of low-cost superconducting rf cavities capable of supporting an accelerating gradient of 25 MV per meter with a quality factor in excess of $5\times10^{9}$. Because of the relaxed tolerances characteristic of the superconducting design, issues related to alignment tolerances, wakefield suppression, and beam orbit control are less severe than in the room-temperature approach. The concept for extending the energy to 800 GeV is based on improved (to 40 MV per meter) cavity performance, predicated on as-yet-unidentified methods of improving the purity of the Nb superconductor. Extension to 1.6 TeV would then be achieved by doubling the length.

The major activity at DESY is construction and operation of the TESLA Test Facility, a 500 MeV demonstration test representing a complete integrated accelerator system. Electrons have been accelerated to 120 MeV in the first (eight cell) acceleration module. Goals for the next year include installation of two more accelerating modules, leading to the demonstration of full energy, full bunch current operations.

The superconducting cavity development program is currently meeting the 15 MV per meter gradient and quality factor specification for the TESLA Test Facility. Considerable progress has been made in understanding the limits to production of high gradients, and at least one cavity has exceeded 25 MV per meter, at high quality factor, as is required for TESLA. Achievement of this performance has required stringent process control during fabrication and sophisticated surface processing techniques. Current effort is concentrated on raising the yield of acceptable cavities to 95% and on development of less expensive fabrication techniques.

As a result of the extensive research and development described above, both the SLAC/KEK and DESY efforts appear capable of developing complete conceptual designs and cost estimates for a 1 TeV electron-positron linear collider, extendible to 1.5 TeV. These designs could be completed early in the next decade, if given sufficient support.

## C. MUON COLLIDERS

The concept of a muon collider was first discussed in the 1960s, but only recently (since 1994) has it received a significant degree of attention. In the past three years, numerous workshops and conferences have been held; during and between these meetings, considerable progress has been made in the study of the formidable technical issues involved. At the workshop that took place in the summer of 1997, a collaboration devoted to the design of a muon collider was formally established, comprising about ninety scientists and engineers (about fifty-five are from the national laboratories; the rest are from universities).

The focus prior to 1997 was on a muon collider with an extensive physics capability: a center-of-mass energy of 4 TeV, and a luminosity of $10^{35}$ cm$^{-2}$ s$^{-1}$. The accelerator systems in this machine (figure 5.2) are a proton source, an ionization cooling channel, a series of muon accelerators, and a collider ring. To provide some context for the research and development issues involved in developing this machine, each of these systems is described briefly in the following paragraphs.

The proton source is a 15–30 GeV, high-intensity (5–10$\times$0$^{13}$ protons/pulse), rapid cycling (15 Hz) proton synchrotron, which serves as the driver for the muon source. The extracted proton bunch is required to be quite short in length (1 nsec rms) in order to allow subsequent momentum-spread reduction of the muons through rf phase rotation. The proton beam impinges on a heavy-metal target in the form of a liquid jet; the pions produced are collected in a strong (20 T) solenoidal field and enter a decay channel formed by a periodic array of superconducting solenoids. The resulting muons produced in the channel are phase-rotated using a 30–60 MHz linac. Momentum selection of the muons at the end of the channel allows some control over the polarization of the beam, at the price of a reduction in muon flux.

The muons then enter a 750 m ionization cooling channel. The basic structure of the cooling channel is a focusing lattice formed from alternating superconducting solenoids, containing LiH absorbers for transverse ionization cooling. Simultaneous momentum cooling is accomplished by the use of LiH wedge absorbers in dispersive regions. Linacs within the solenoids restore the energy lost in the absorbers. The entire cooling system is designed to reduce the transverse emittance by three orders of magnitude in both planes. In the final sections of the cooling channel, where very short focal lengths are required, current-carrying liquid Li lenses replace the solenoids as the focusing elements. The muon beam energy at the end of the channel is roughly 15 MeV.

The muons emerging from the channel must be rapidly accelerated to their final energy before they decay. This acceleration is accomplished in several stages: a conventional linac to 700 MeV; followed by a recirculating linac (with warm rf at low energy, superconducting rf at higher energies), to 100–200 GeV; followed by a series of rapid-cycling (0.2–1 msec period) pulsed synchrotrons, with hybrid (alternating resistive/superconducting) magnet systems and pulsed superconducting rf, to the final beam energy of the collider. Typically, 35% to 40% of the muons collected in the capture channel survive to be injected into the collider. Each muon bunch has an intensity of roughly $2\times10^{12}$.

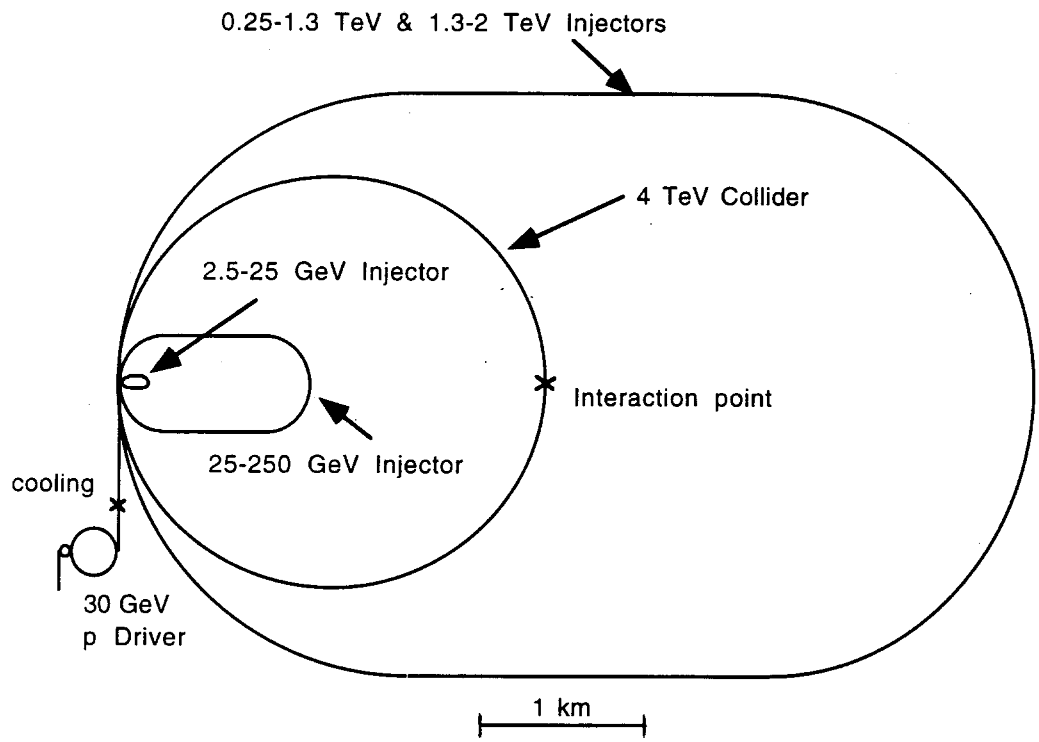


Figure 5.2 Schematic layout of a 4 TeV muon collider. This machine would reach mass scales two to three times that of the LHC. The muon collider concept is new and unproven, and considerable simulation and prototyping work will be required to determine its feasibility.

For a 4 TeV collider ring with an 8 km circumference, muon decay results in a luminosity lifetime of roughly 900 turns (about 25 msec). Two hundred thousand muon decays per meter deposit about 2 kW per meter into their surroundings; the superconducting dipoles of the ring need to be shielded with 12 cm diameter warm tungsten bore inserts. To achieve the stated luminosity, a $\beta^*$ of 3 mm is required; a commensurate bunch length is achieved through the use of a quasi-isochronous lattice. Maximum $\beta$ functions of several hundred kilometers appear in the final focus regions, and these regions will require local chromatic corrections. The high bunch intensity and small bunch length result in a very high peak bunch current (about 12 kA). This large current is accompanied by stability problems. The final focus quadrupoles require heavy shielding, a large aperture, and significant gradients. Because of the muon decays, the detector environment will be difficult to handle. Areas of concern include beam halo control, radiation damage to silicon vertex components, and occupancy issues in the tracking and calorimetry.

A feasibility study for the 4 TeV machine was published in July 1996, just before the 1996 Snowmass workshop on New Directions for High Energy Physics. It has been understood that the first muon collider to be built would likely be a much lower-energy machine. This is true not only because of the considerable technical risk involved in such a new, complex, and radically different approach, but also because the nature of the machine lends itself to a staged approach. The muon production system (proton driver, target and capture systems, muon ionization cooling channel), and the first stages of muon acceleration are essentially independent of the ultimate energy of the collider ring. Hence, an evolutionary development is possible, in which one builds the muon production system, followed by sufficient acceleration stages to reach a modest collision energy. After operation at this energy for some period, upgrading the energy requires addition of acceleration stages and a larger collider ring, but the muon source remains the same.

Since 1996, feasibility studies have focused on the parameters of the lower-energy machine, and on the research and development required to reach the point where a complete conceptual design and an engineering cost estimate could be begun. The energy has been selected to be about 100 GeV, primarily because of the possibility that this energy is appropriate for a Higgs factory. Work is continuing on issues related to the higher-energy machine, but the prospects for 4 TeV and higher energies have been dimmed somewhat by the recent realization of the neutrino radiation problem. To keep radiation at the surface from the muon decay neutrinos at a level of less than 10 mR/year, the collider ring at a normal site (that is, not an island or a mountain top, with unusual geometry) must be 300 m below the surface for 3 TeV, 1000 m below for 4 TeV, and correspondingly deeper for higher energies. In the light of this information, the most energetic machine currently under consideration is a 3 TeV collider.

The muon collaboration has outlined a five-to-six-year research and development program that addresses the critical issues associated with the muon source. The principal elements of that program are listed below.

1. Continued theoretical studies, and complete system simulations, are needed in many areas, principally simulations of the ionization and momentum cooling channel. Further studies are needed of the proton driver; of the target, capture, and rotation systems; of the acceleration rings; of the collider itself; and of backgrounds in the detector.

2. An experimental demonstration of ionization cooling is needed. A six-year experimental program has been outlined, in which the ionization cooling channel hardware is developed and demonstrated experimentally using muon beams from new beam lines at Fermilab or Brookhaven. The hardware to be developed includes high-gradient 1 m liquid Li lenses, a 10 m alternating solenoid FOFO channel with 6-cell special rf cavities (operating at $LN_2$ temperatures), and LiH absorbers. Instrumentation would be used to measure the performance of the prototype cooling channel in the muon beam line. The 10 m long demonstration experiment should provide a reduction in six-dimensional muon phase space density by a factor of two. Twenty to thirty such stages would be needed for the muon source in a muon collider.

3. Experimental work is needed on the target, capture, and phase rotation system. This work would involve construction of a prototype system, with a large bore, high-field (>20T) solenoid, a liquid jet target, and an rf cavity designed to sustain high-radiation fields. The target would be exposed to a high-power beam to study integrity issues, and the rf cavity would be subjected to high radiation fields to study its tolerance.

4. The rapid-cycling pulsed synchrotron, which accelerates the muons to the collision energy, will require a large number of pulsed magnets. Such magnets need to be prototyped to study issues such as eddy currents and fatigue lifetime. Pulsed superconducting rf will also be needed in the acceleration ring, and systems must be prototyped.

5. The superconducting magnets used in the collider ring have challenging requirements and can be expected to require substantial prototyping, due to the considerable heat deposition and radiation damage associated with muon decay. The final-focus superconducting quadrupoles, with their demanding apertures and gradients, will particularly need prototyping and development.

6. Studies will be required of the operation of proton synchrotrons with the very short bunches necessary for muon production. Some work has been done at the AGS on short bunches, but the beam intensity to date is well below what is required for a muon source driver.

After the completion of this research and development program, the collaboration believes that it would have either established the feasibility of a muon collider or determined that a muon collider with the luminosity needed is not feasible. The critical collider components would have been demonstrated, simulation models of all key areas of the complex would have been completed, and a clear idea would have been achieved of the further research and development necessary prior to preparing an actual proposal.

## D. VERY LARGE HADRON COLLIDERS

A VLHC (Very Large Hadron Collider) is a proton-proton collider with a physics reach to the 10 TeV mass scale well beyond the Tevatron and the LHC. Its energy and luminosity goals are a center-of-mass energy of 100 TeV and a luminosity of $10^{34}$ cm$^{-2}$ s$^{-1}$. Two approaches are being studied for the design of a VLHC. One is for a low-field superferric magnet approach, 1.8 T, based on existing technology. The other is for a very high-field magnet design (for example, 12.5 T) that might use new high-temperature superconductor technology (see figure 5.3). The two lead to fundamentally different accelerator designs (see table 5.2) that have different research and development requirements.

| | 50+50 TeV Collider (Low Field) | 50+50 TeV Collider (High Field) |
|---|---|---|
| Circumference (km) | 600 | 100 |
| Magnetic dipole field (T) | 1.8 | 12.5 |
| Protons per bunch | $1.7 \times 10^{10}$ | $1.2 \times 10^{10}$ |
| Number of bunches | 100,000 | 16,000 |
| Revolution frequency (kHz) | 0.5 | 3.0 |
| $\beta^*$ at interaction points (m) | 0.1 | 0.1 |
| Emittance (95%, $\pi$ mm-mrad) | 15 | 8 |
| Typical luminosity ($cm^{-2} s^{-1}$) | $10^{34}$ | $10^{34}$ |
| Integrated luminosity ($fb^{-1}$/year) | 100 | 100 |
| Interactions per crossing | 30 | 30 |
| Bunch spacing (ns) | 19 | 19 |

Table 5.2 The luminosity scenarios for the low- and high-field VLHC designs at 100 TeV center of mass

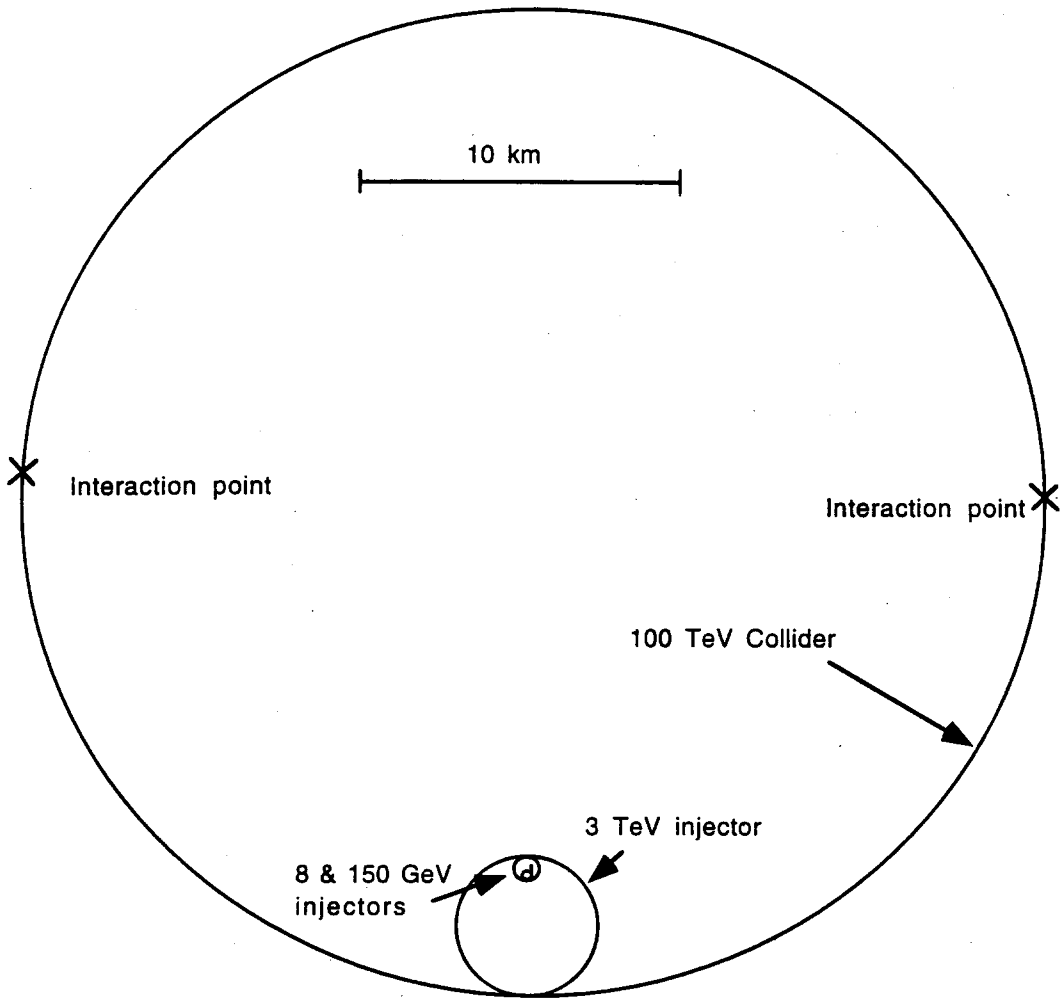


Figure 5.3 Schematic layout of a 100 TeV high-field hadron collider. The low-field variant of this machine would be six times larger. This machine would reach mass scales four times that of the LHC. The basic technology has been well tested in existing machines, but its large size means that considerable cost reductions of technical components must be realized for construction to be economically feasible.

The low-field approach uses a very large circumference (600 km) collider ring, for which the technology is a modest extrapolation from existing accelerators. The size means research and development must be aimed at cost reductions per meter of components and infrastructure. Work is being done at Fermilab on a 1.8 T combined-function magnet that could be used in a 3 TeV booster injector for either a low-field or a high-field collider. An innovative and economical design, called the double C or transmission line magnet, is a warm bore, warm iron magnet that uses a single 75 kA superconducting cable to energize two magnet gaps. The most economical approach is to use NbTi conductor cooled with liquid helium. Because the iron concentrates the field in the gaps where the beams circulate, the amount of superconductor required is small. The basic technology to build this magnet is in hand, and a 50 m long prototype is under development.

Fermilab is studying a phased approach to extending the energy frontier in the post-LHC era. The present thinking is to use the 150 GeV Main Injector to inject into a new 3 TeV booster and from there into a VLHC. The 3 TeV low-field booster/injector would be a machine slightly larger in circumference than LEP/LHC and would demonstrate all the necessary technologies and provide data for the associated costs of building a machine twenty times larger. Various physics options (fixed-target, proton-proton, proton-antiproton, electron-positron) for using this machine during the construction of the VLHC are being explored.

Considerable effort is needed to understand beam stability issues in such a large machine. The 2 cm high-vacuum chamber, which is 600 km long, presents a considerable impedance to the beam. This drives potentially serious instabilities, such as the mode-coupling single-bunch instability and the transverse coupled-bunch instability. A serious concern is that at low frequencies the electromagnetic fields of the beam penetrate the vacuum chamber walls. This could have a considerable impact on the value of the resistive wall impedance, in particular in the presence of iron pole pieces. Reliable estimates of vacuum chamber effective impedance need to be obtained. Studies of multibunch mode dampers and the tradeoffs between machine parameters, such as the number of bunches or the vacuum chamber aperture, need to be continued. Other effects that become important in these very large machines are sources of emittance growth, such as ground motion and equipment vibration.

If one assumes that affordable high-field magnets (for example, 12.5 T) will be developed sometime in the future, then the design of a VLHC is simplified. Not only is the machine smaller in circumference (100 km), but the synchrotron radiation can produce useful emittance damping with damping times of two hours. This then gives ten to twenty hours of integrated luminosity that is essentially independent of the initial emittance, and a higher peak luminosity for the same number of protons. With the same assumptions on interaction region optics, etc., the high-field VLHC achieves the same luminosity as the low-field version with approximately one-tenth the number of protons. The smaller number of circulating protons, smaller machine circumference, and emittance damping greatly relieve the beam dynamics issues compared with the low-field design. The major issues that require research and development for the high-field approach are technology development of a high-field magnet, the high heat load (5W/m from synchrotron radiation) into the cryogenic system, and gas desorption in the vacuum system by the synchrotron radiation.

In the high-field approach, magnetic fields as low as 10 T could also be used with similar benefits over the low-field designs. These magnets would be feasible with current NbTi technology at 1.8 K. However, the cryogenic loads and the complexity of the vacuum chamber would tend to make this approach very expensive.

General high-field superconducting magnet research and development is ongoing at BNL, Fermilab, LBNL, and Texas A&M. Overall, there is broad interest in high-field magnet development, as there are many applications in high-energy physics other than a VLHC. It is likely that 10 T magnets will be available soon, using NbTi or $Nb_3Sn$ superconductor. In fact, Fermilab is developing quadrupoles for the low-β insertions of the LHC that have fields up to 9.5 T. Using $Nb_3Sn$, the LBNL group has demonstrated a small model magnet with a field of 13 T at 4 K.

A real breakthrough would be the successful application to accelerator magnets of those high-temperature superconductors that have excellent high-field and high-current-density performance at a higher operating temperature. The latter, by increasing Carnot efficiencies in the cryogenic systems, reduces power consumption, simplifies cryostat designs, and makes it easier to handle the synchrotron radiation power.

At present, the most promising of the new materials include BSCCO-2212, with a critical temperature about 85 K, and BSCCO-2223, with a critical temperature about 110 K. Kilometer lengths of these materials have been made in the form of tapes that could be used in certain magnet designs. Another material, YBCO, shows even more promise of being useful at high fields and very high critical current densities. However, commercial production lags behind that of BSCCO. BNL materials scientists are working to produce samples that might be suitable for magnet development. Fermilab is considering the use of BSCCO to replace the 5 kA power leads to the Tevatron magnets to reduce the heat load on the 4 K system.

Some of the challenges to using high-temperature superconductor materials in high-field accelerator magnets are the relatively brittle character of these ceramic materials, their high manufacturing costs, and their relatively inferior ac characteristics. Their brittleness makes these materials considerably less resistant to strain than NbTi. New magnet designs that take account of both the mechanical difficulties and the potential high current densities must be explored. Some of the ideas include racetrack coils for twin bore magnets, current block designs, and magnets that use conventional NbTi or $Nb_3Sn$ outer coils with BSCCO or YBCO inner coils.

Efforts toward developing the design and technologies for a VLHC are centered at Fermilab and Brookhaven. A VLHC study group at Fermilab has proposed a detailed five-year research and development plan, which would bring the low-field design to a very advanced, if not complete, state. It also includes VLHC technology systems tests. For the high-field design, the aim is to have the first high-field magnet by FY 2002. At Brookhaven, researchers are concentrating on development of high-field magnets and on studies of the accelerator physics issues in the high-field design. The goal for both high-field and low-field designs is to lower the cost per TeV of such a hadron collider by about a factor of ten from the cost today.

## E. CONCLUSION

A new high-energy collider will be required to explore the energy frontier after the LHC. The extensive new technical systems needed for such a collider require a research and development program with a long lead time. Three different possibilities for such a facility are currently under intense investigation: a second-generation electron-positron linear collider, a first-generation muon collider, and a third-generation superconducting hadron collider. The linear collider is at the most advanced stage of development: work is ready to start on a complete conceptual design. The muon collider is a new concept, offering promise but requiring fundamental design concept validation. The hadron collider is technically the most conservative approach, but requires the development of new cost-minimization strategies to be economically feasible. All three approaches require increased research and development support if they are to be ready when needed.

# 6 The University-Based Program

*In science, teaching and research not only go hand in hand but are often the same hand: the pedagogical act an act of investigation, the investigatory act shared with students and associates who are also colleagues, the whole a splendid, ongoing instance of intellectual and human collaboration.*

—A. Bartlett Giamatti, "Science and the University"

## A. INTRODUCTION

With 80% of high-energy physics researchers in the United States based at universities, the strength and effectiveness of the university-based program is critically important to the success of the program as a whole. The charge that launched this Subpanel's work explicitly asked for analysis of the current state of the university-based research program and for advice on how to optimize that program in the context of the overall plan developed by the Subpanel. In this chapter, we examine the university-based program and the increasingly serious funding squeeze it has experienced over the past decade: an overall 22% reduction in buying power over the last five years alone. The experimental part of this program has suffered a 25% reduction.

We first describe the character and the scope of the university-based program. We review the contributions of university-based researchers to science and to education. We consider the relationship of university researchers to the national laboratories, and we identify some aspects that are of concern, including special problems faced by university scientists who carry out their research abroad. Some suggestions to the community at large, but outside our major charge, are indicated in italics in sections D, E, F, and G.

We next present and discuss statistical information on technical infrastructure, human resources, and funding. We evaluate the importance to the success of the overall program of having faculty, students, and technical staff working together on a daily basis. Although there is a general trend toward fewer, larger, and more complex detectors, we conclude that what is most needed for the health of the field are not new modes of operation, but the strengthening of groups within the traditional university setting.

Finally, in chapter 7 we make specific recommendations to the DOE to strengthen the university-based program, which will enable university researchers to execute future projects more efficiently, foster the development of sophisticated new research instruments, and enhance the education and research training of our nation's graduate and undergraduate students.

## B. SCOPE AND CHARACTER

The DOE and NSF support a diverse, broad-based program in particle physics at the universities. The program ranges from institutions having one or two faculty working primarily with undergraduate or master's degree students to those with groups of ten or more faculty, research and postdoctoral scientists, technical personnel, and graduate and undergraduate students. Typically, the larger groups involve twenty to twenty-five people, although several groups have more than seventy.

We can better characterize the high-energy physics community using statistical information presented to the Subpanel by P. K. Williams, head of University Programs in Experimental and Theoretical High-Energy Physics at the Department of Energy; Patricia Rankin, program officer for Elementary-Particle Physics at the National Science Foundation; Michael Barnett, Lawrence Berkeley National Laboratory, who reported on the results of recent surveys commissioned by NSF and DOE; and Pier Oddone, also of Lawrence Berkeley National Laboratory, who discussed the results of a 1997 survey of university infrastructure (see appendix E). The number of physicists in the U.S., including graduate students, who spend more than 50% of their research time on particle physics is about thirty-five hundred. Eighty percent are employed by universities. The university-based work force, including technical personnel, is distributed as follows:

| | |
|---|---|
| Faculty physicists | 27% |
| Staff physicists | 6% |
| Postdocs | 16% |
| Graduate students | 32% |
| Engineers | 5% |
| Technicians | 5% |
| Undergraduates | 9% |

The University Program of the DOE supports 230 research groups at 100 universities (FY97). These groups include 616 Ph.D. experimentalists, 375 experimental graduate students, 374 Ph.D. theorists, and 145 theoretical graduate students. All of these numbers have diminished in recent years. The NSF supports 66 groups at 43 universities (in addition to funding the CESR accelerator at Cornell). The university research salaries are supported approximately one-half by DOE grants, one-sixth by NSF grants, and one-third by non-federal sources (e.g., universities). The university contribution represents major cost sharing of the program.

The research supported by these funds encompasses a broad range of activities. University-based researchers generate new theoretical ideas about the basic particles and their interactions, identify experimental avenues that show the greatest promise for important discoveries, interpret existing data, and work on developing the next generation of instruments for research—including theoretical tools, hardware and software for detectors, and accelerators.

The ultimate criteria of success for any research enterprise are the quality and quantity of its scientific output. University scientists have played leading roles in generating the ideas and instruments that will be used to advance the science in the high-energy physics program of the coming decade.

## C. A BRIEF HISTORY OF UNIVERSITY CONTRIBUTIONS TO HIGH-ENERGY PHYSICS

The ways in which university-based researchers contribute to high-energy physics have evolved in response to the evolution of the instruments and intellectual collaborations needed to address the important questions of the field. Yet one feature of the university-based program has remained constant over time: individual faculty members retain significant control over their own work, while they also have the opportunity to benefit from and contribute to larger cooperative endeavors. The university-based program must be counted among the key elements of the nation’s success in high-energy physics, not merely because of its magnitude, but because of its flexibility to respond to promising new ideas, its proximity to researchers in other fields, and its responsibility to educate, train, and encourage young scientists.

The field of high-energy physics grew from roots in university research programs in nuclear physics in the 1930s and 1940s. A broad range of experimental and theoretical techniques were developed by university investigators. They swiftly opened up the new science and laid the framework within which we continue to work.

From early experiments conducted with cosmic radiation and radioactive decays of nuclei, which could be carried out in individual university laboratories, researchers recognized the need for accelerators that could supply high-energy beams of sufficient intensity to permit careful study of nucleons. Cyclotrons and betatrons providing versatile beams of protons and electrons were developed on several university campuses around the country. These instruments allowed rapid expansion of the fragmentary knowledge of the particle spectra and of the role of symmetry in understanding the subnuclear forces.

Striking results from these early studies showed that a rich field was being opened and that increasing beam energies would permit more incisive investigation of the fundamental properties of nature. The growing community of physicists made technical innovations in both accelerators and in detection methods.

However, as beam energies increased, costs rose as well, and the universities banded together to pool their resources to build larger accelerators. These regional and national labs arose from the already vigorous university program, and the management structures and scientific directions of the laboratory program were provided by universities.

The national laboratories now maintain a strong complement of experimental, theoretical, and particularly accelerator physicists as staff scientists. University physicists play a major role in formulating and executing research at the laboratories. The university community works in a fruitful partnership with the physicists and technical personnel at our national laboratories and with the analogous communities in other countries.

Complementary to experiments mounted at accelerators, experiments using particles from astronomical sources and from decays of nuclei in the laboratory have made crucial contributions to understanding particle properties. Historically, these investigations have been funded primarily by the university-based program.

The productivity and innovation of the university physicists show no sign of abating. The large experimental detector collaborations at colliders in this country have almost invariably been co-directed by university physicists, and university groups have shared the lead in producing the most interesting new physics. Examples of recent innovative projects initiated by university physicists include sensitive accelerator experiments to measure charge conjugation/parity violation effects and rare decays of K mesons; searches for oscillations between neutrino species, which would establish simultaneously that neutrinos have mass and that they violate lepton number conservation; plans for large detector arrays for ultra high energy cosmic rays; and underground and under-ice detectors to study solar, atmospheric, and cosmological neutrino sources. The ideas embodied in string theories, which unify the properties of space-time and internal symmetries, are exciting new theoretical developments.

## D. EDUCATION, OUTREACH, AND CAREER DEVELOPMENT

The intrinsic excitement of elementary particle physics makes it a wonderful vehicle for drawing young people into science, and for demonstrating to the general public the importance and value of fundamental research. For particle physicists who are based in universities, doing so is a special responsibility. The primary mission of faculty members is to educate, and the fulfillment of the educational mission both challenges and enriches their research.

University physicists' principal educational responsibility is teaching and encouraging graduate and undergraduate students. Graduate education is inextricably linked with research, and a hallmark of particle physics is the opportunities it offers young scientists to acquire broadly useful technical skills, problem-solving abilities, and experience working collaboratively in a group effort. As with students in all fields, graduate students in particle physics eventually apply their training in a great variety of careers. While young particle physicists have found their expertise to be in great demand, it can nevertheless be a difficult transition when it is first recognized that opportunities to pursue one's first passion are limited. The "export" of talented physicists is, however, one of our field's greatest continuing contributions to society, and facilitating career development is one of our most important responsibilities.

Particle physicists teach classes and develop curricula for undergraduate students planning careers in physics, as well as for those majoring in other sciences, engineering, and the liberal arts. They are enthusiastic participants in programs providing research opportunities for undergraduates and in collaborations with K–12 teachers and faculty from small colleges to enhance education in these settings. Particle physicists in universities have developed numerous outreach programs, providing exposure to research through hands-on experiences and through the World Wide Web, a highly visible byproduct of particle physics research at CERN.

### *1. Graduate Education in Particle Physics*

Elementary particle physics is the specialization for slightly more than one-eighth of the physicists who are awarded Ph.D.'s by U.S. universities each year. Approximately one thousand graduate students are currently engaged in particle physics. Slightly more than half are supported by DOE High-Energy Physics University Program grants, about one-fifth by NSF Elementary-Particle Physics and Theory Division grants, and the remainder by universities or by other sources. Two-thirds of these graduate students are involved in experimental research and one-third pursue theoretical studies, approximately the same proportion as university faculty. For both theory and experiment, graduate students are indispensable to the research program.

A typical course of graduate study begins with two years of class work beyond the bachelor's degree, followed by three or more years of research, culminating in a thesis and the award of a Ph.D. According to the 1995 Particle Data Group census and survey, the mean time to a Ph.D. in particle physics is currently 6.1 years; the average for all fields of physics is 6.5 years, as reported in the 1995 *AIP Graduate Student Report*.

Graduate education in theoretical and experimental particle physics provides effective training for a broad variety of technical and scientific careers. The experiences of attacking a complex problem in depth and of communicating and defending the results in a rigorous and competitive setting are invaluable in preparing for a career in academia or industry. The abilities to apply computers to solve challenging problems, to simulate complex systems, and to operate sophisticated equipment are skills prized in many settings. The availability of instruction and experience in advanced software techniques, like object-oriented programming, are recent enhancements in graduate training. Experimentalists can obtain specialized experience in electronics and the development of state-of-the-art detectors. There are also opportunities for working in and managing research or production teams, for interacting with engineers and industrial suppliers, and for gaining experience in international collaboration. Working within a large collaboration enhances communication and writing skills and emphasizes the importance of teamwork. These abilities have obvious application in the modern global economy.

In addition to the educational programs of the universities, high-energy physics funding agencies like DOE, NSF, and laboratories in Europe and Japan often provide special opportunities for students. Programs like the TASI, SLAC, and CTEQ schools, the seminar series at the laboratories, and support for students to attend conferences and group meetings nourish a great deal of supplementary advanced education.

While the current state of graduate education is healthy, challenges lie ahead. Data from the DOE show that the number of graduate students in high-energy physics supported by DOE grants has declined by approximately 20% in the past five years, from 467 to 375 experimentalists and from 183 to 145 theorists. The percentage decline in the number of students supported by NSF is even larger. While graduate education in our field has successfully adapted to research on large projects of several years' duration, there is concern that further lengthening of the time scale of experiments, and of each cycle of hardware upgrades and data taking, will lead to a more serious mismatch with the timing of graduate students' careers. This could lead to pressure to involve students in research earlier in their studies, compromising their course work.

In the recent high-energy physics infrastructure survey, 50% of responding institutions indicated that student involvement in high-energy physics is lower than it was five years ago. We believe that this is primarily due to declining undergraduate physics enrollments and to the perception of reduced opportunities in high-energy physics following the termination of the SSC in 1993. We expect that this trend will be reversed as new projects begin operation and as accelerator and detector research open new approaches to the high-energy frontier.

*2. Postdoctoral Training and Career Development*

Career issues are of great concern to current graduate students, especially as they near the completion of their Ph.D.'s. Statistics gathered for the 1995 and 1997 surveys suggest that 50% of Ph.D. recipients in particle physics continue their research with postdoctoral appointments. While it is difficult to obtain an unbiased measurement, there has been a clear trend toward longer periods of temporary research appointments. Particle physicists who received Ph.D.'s in the 1950s and have remained in the field typically spent two years in postdoctoral appointments before moving to permanent positions. In the past decade, this time has lengthened to about four years, with a long tail extending to more than ten years. While this practice may benefit the experimental program in the short term, the careers of long-term postdocs are often not well served by the current system.

Over the past few years there have been approximately twenty tenure-track or permanent staff positions in experimental particle physics and fifteen such positions in theory filled each year by physicists who did not previously have a permanent position. The recent surveys further suggest that between one-third and one-half of postdoctoral physicists ultimately obtain either a tenure-track position in a university or an equivalent position at a national laboratory, with the remainder obtaining positions outside of particle physics. Apart from short-term fluctuations, this average rate of graduation from our field to other careers seems to have remained quite stable and is approximately the same as for other physics subfields.

Today's large high-energy physics experiments are very reliant on the contributions of the young physicists, who carry much of the daily responsibility for construction and operation. The health of the field depends strongly upon nurturing these talents, although they often receive less recognition than is accorded accomplishment in physics analysis.

*We believe that steps should be taken to institute suitable recognition for young physicists who have made outstanding technical contributions. This could, for example, take the form of competitive awards recognizing such achievements. We would expect such awards to carry modest recompense but significant prestige, and we propose that they be arranged, administered, and awarded by the Division of Particles and Fields of the American Physical Society.*

### *3. Undergraduate Education and Particle Physics*

Particle physics provides unique opportunities for undergraduates to participate in research on campus and at national laboratories. The students employed are, in the majority of cases, physics majors, but many are also from related fields of engineering or computer science. They bring valuable expertise to particle physics projects, and they take away an understanding and appreciation of the research enterprise, and of physics, that serves them well in their own careers.

The recent high-energy physics infrastructure survey revealed that approximately three hundred undergraduate students are currently employed in university-based particle physics research. These students participate in detector research, development, and construction; in the operation and management of computing facilities; and in data analysis. The experience can significantly enhance their employability or readiness for graduate study. The value to the research effort is also enormous. Undergraduate students are an important asset for on-campus research.

Through the support of the NSF and the DOE, many programs have been established that use particle physics as a centerpiece for special educational opportunities for undergraduate students. The recent report, *Particle Physics: Education & Outreach*, sponsored by the NSF, the DOE, and the APS, lists dozens of university summer programs that seek to involve local undergraduates and those from other institutions in particle physics research. Many of these are supported under the auspices of the NSF's Research Experiences for Undergraduates (REU) Program, while others draw on laboratory or local resources.

While particle physics research is a proven tool for enhancing the education of physics majors, particle physicists make much broader contributions to undergraduate education. Effective teaching and forefront research are not only compatible, they are symbiotic. This is reflected by the role of particle physicists in initiatives to upgrade undergraduate education at many institutions. Some of these efforts involve new applications of technology; others develop new instructional techniques, especially in introductory physics courses. As an outgrowth of on-campus research and development programs, many particle physicists have started successful interdisciplinary programs within their universities.

### *4. Outreach*

Particle physicists are engaged in a great variety of activities designed to enhance the scientific literacy of the general population, to collaborate with K–12 teachers, and to share the excitement of physics and their own projects with potential future scientists. There are literally hundreds of such activities, occurring in essentially every university group and laboratory. Support for these programs is provided by the NSF, the DOE, universities, state and local governments, and private donations.

Many outreach activities have gone on for decades, including laboratory tours and open houses, public lecture series, school visits, museum programs, and encouragement of coverage by local and national media. In recent years, these efforts have become more focused, with many programs targeted at groups traditionally underrepresented in science. New and creative uses of technology, including the World Wide Web, are central components of many outreach activities.

Overall, more than a hundred specific outreach projects are described in the education and outreach report cited in the preceding section. Here we highlight a few examples. The Pathways Program at Boston University consists of two one-day workshops for young women from high schools in Massachusetts, with a planned enrollment this year of seven hundred. With enthusiastic participation of many women scientists, programs like Pathways aim to overcome traditional gender barriers. Similarly, programs targeted at minority students, such as that spearheaded by the particle physics group at Prairie View A&M, have been very successful in launching students on scientific career paths. World Wide Web sites, such as "The Particle Adventure" at Lawrence Berkeley National Laboratory, make very effective use of this far-reaching medium, with many thousands of accesses already recorded.

### *5. Prospects*

Conveying the value and the excitement of research in particle physics is a shared responsibility of everyone in the field. The use of particle physics to spark the enthusiasm for science among young men and women is both a dividend of the nation's investment in our research and an essential element of the program's continuing success. A public that is well informed about the challenges and shares in the triumphs of particle physics will be more likely to recognize its significance. The last few years have seen a great expansion in our efforts to achieve these goals, but much remains to be done.

There are more than a hundred universities engaged in particle physics research. They are at the forefront of education and outreach efforts, and success for the field requires their continued full participation. Declining support for the university program, as has been the case for the past five years, leads to reductions in the number of graduate students supported, to declines in the number of research and faculty positions, and to further deterioration in the on-campus infrastructure. These losses sap the vitality of the field and significantly compromise our ability to pursue compelling physics and provide outstanding educational opportunities.

*In addition to maintaining the health of university research groups, we must encourage continuing support of education and outreach through programs like REU and those that support opportunities for minorities and women, as well as outreach efforts to the public at large. Efforts to develop institutional, local, state, and private foundation support to help achieve these goals will be richly rewarded.*

## E. UNIVERSITY–LABORATORY RELATIONS

The majority of university groups participate in research programs at national high-energy physics laboratories. Cooperative relations between the university groups and the national laboratories are essential for a successful research program in high-energy physics. It is important to the field that this relationship remains healthy and serves to further the progress of the science.

During the fact-finding visits by this Subpanel, one-day trips to nearby major universities were included to hear about the university role in the national program. Representatives from the university groups were invited to discuss their impressions of the state of the field. In addition, a questionnaire was sent out by electronic mail to a sample of university researchers active at the laboratories (see appendix C). These presentations and responses to the questionnaire raised a number of issues that relate to university-laboratory relations. There are many positive elements in the relations between the universities and laboratories that could be highlighted. There are, however, areas of concern that deserve our attention. In the following paragraphs some of these problems and issues are discussed.

One major consequence of conducting research at a distant national laboratory is that time must be spent away from campus. Physics department chairs express concern over the periodic absence of their faculty, postdocs, and students in the experimental particle physics program. Postdocs and students can lose contact with campus academic life, and some professors are absent from teaching for long periods. Research at a distant laboratory is most often done at the cost of significant personal sacrifice. We must assist deans and department chairs to recognize the need to travel and to support the faculty, postdocs, and students who must be absent from campus.

Supplemental teaching of graduate level seminars and special courses for students stationed at a national laboratory has been a grassroots practice for some time. *This practice should be encouraged. The students and the scientific and academic nature of the laboratory community will all be well served.*

Some of the required travel could be reduced by expanded use of the telecommunication and video-conferencing technologies now becoming available. *Expanding the reach of these technologies into the smaller universities, and improving the performance of the communication tools, would greatly benefit university researchers and would reduce the need to travel often. Support for improving these services must remain a high priority of the U.S. program.*

There have been problems with health care for long-term visitors at the laboratories. In the era of HMOs, a university researcher sometimes finds that the university HMO does not cover charges incurred away from home. For students, postdocs, and young faculty members with limited financial resources, this situation is simply not acceptable. *The laboratories, working together with the university scientists, should take the initiative in obtaining affordable medical coverage for non-employee scientists who live and work at the laboratory for long periods of time.*

For university groups collaborating on construction projects at a national laboratory, construction funds are often transferred through the laboratory to the university. A common practice has been the transfer of funds from DOE to the laboratory by the standard financial plan change and from the lab to the university by memorandum purchase order (MPO) for work undertaken at the university. An alternative practice has been the transfer of funds by another path, from DOE to the university as a supplement to the university group's grant. Although the differences between an MPO and a grant supplement may appear to be relatively minor, the implications at the university can be significant. The grant supplement is recognized by the university administration as an addition to the university group's grant. This additional federal support is important to the high-energy physics group and is counted in the support they are able to generate from external sources. MPO's are not generally regarded by the university with the same importance as federal grant funds. This difference can be significant in the amount of support the group is able to gain as a university contribution. *Laboratory management, the DOE funding officers, and the leadership of the large construction projects should take into consideration this important difference when providing funds. Closely connected to this issue is the importance of project management control, which of course must be ensured, and the appropriate level of overhead for the university.*

## F. SPECIAL PROBLEMS WORKING ABROAD

University high-energy physics groups participate in experiments at such overseas laboratories as CERN, DESY, and KEK, as well as in smaller experiments and in numerous non-accelerator experiments abroad. These groups face special challenges. There are some common problems, even though some groups work in large international collaborations at established laboratories while others work at smaller experiments or at more remote sites.

Full participation in an overseas experiment means added travel and additional expenses for infrastructure at the experiment. Of crucial importance has been the support of experienced senior physicists, either faculty or staff, who reside at the experiment for long periods. They make it possible for U.S. universities to take leadership roles in the construction and operation of large experiments based overseas. Groups also typically have to station students and postdocs at the remote site for extended periods. While video-conferencing and networking can make it easier for groups based in the U.S. to participate from their home institution, some presence at the experiment will always be essential.

Research groups require engineering, technical, computing, and clerical support, and it is often necessary for a group working abroad to establish technical and administrative infrastructure at the remote site. While some of the infrastructure may be provided by the host institution, it is not always sufficient, and the added expense must be covered by the U.S. group. In order to reduce the overall cost, essential infrastructure could be managed jointly by U.S. groups working on the same or similar projects.

*Frequent foreign travel for faculty and staff is an inevitable consequence of working abroad, and the costs of travel, not only in the financial sense, can be very large. To remain effective collaborators, university groups must have sufficient support to cover the travel expenses for faculty and staff. Currency fluctuations are an additional danger, and the cost of living differential for some cities can be a constant burden on a group's budget. Graduate students, postdocs, and others resident at the experiment should not be expected to bear this additional cost without adequate compensation.*

Postdocs and graduate students working abroad for several years often face increased difficulties when returning to the U.S. job market. Although it is desirable for younger physicists to reside at the experiment, it is important for their careers that they maintain close contact with their home institutions. *Adequate travel support is therefore essential to allow a balanced presence at both the experiment location and at the home institution.*

Networking and video-conferencing have become important tools for high-energy physics collaborations. A study by the ICFA Network Task Force has shown that the high-energy physics community will need substantially increased networking capacity by 2005. International network links are already heavily used, and demand is continually increasing. Plans for improved networking inside the U.S. academic community do not automatically include international connections. *It is imperative that university groups retain affordable access to networking and video-conferencing over the coming decade.*

## G. UNIVERSITY INFRASTRUCTURE

*Infrastructure*, as used here, refers to the technical resources necessary for university groups to participate effectively in the high-energy physics program. These technical resources include shops, laboratories, computers, and the technicians, engineers, technically skilled senior scientists, and computer specialists necessary to use them effectively. Adequate access to technical resources allows university groups to be major contributors to designing and building detectors and to analyzing data, and it is crucial for students to master the variety of technical skills they need to contribute to the high-energy physics program.

The amount and type of infrastructure varies greatly from institution to institution, as do the technical interests of various groups. Similarly, the sources of support for infrastructure also differ from university to university. Support generally comes from funding agency grants, university grants, or a combination of the two.

The 25% loss in buying power for the experimental part of the university program since 1992 has forced university groups to make significant reductions in all types of infrastructure. This funding squeeze comes at a time when institutional interest and participation in the field are high.

A look at the analogous community in Europe presented in appendix F makes the erosion in infrastructure much more striking. For example, in Europe, the number of physicists supported in the particle physics program has increased by 27% since 1988, and the number of students by more than 50%. The level of infrastructure, 0.82 technical support persons per Ph.D. experimental physicist, is very significantly greater than the U.S. average (for both large and small university groups) of about 0.23 technicians plus engineers per Ph.D. physicist.

University particle physics groups have responded to the reduction in support in a number of ways. To cope with funding losses, large groups typically have dramatically reduced their technical personnel. Smaller groups, already short on infrastructure, have tended to reduce the number of physicists to maintain a minimum of infrastructure needed for detector development.

Within this overall pattern, a few large groups have maintained a reasonable level of infrastructure exclusively (or almost exclusively) for the use of high-energy physics. This infrastructure is supported primarily by the groups' DOE or NSF grants, often by pooling resources over a number of particle physics projects. These groups are concerned that their personnel and equipment base remain at an appropriate level to remain effective. They often put a special emphasis on technical contributions.

Some institutions have made a transition to common shops. The university or department runs the shop and charges the particle physics groups based on the fraction of time personnel are used. This mechanism often makes it possible to keep available especially talented technicians and engineers with smaller base funding. Such groups then need periodic extra funding to pay for technical developments. This mechanism for sharing technical personnel requires a sufficiently large university or department that an appropriate shop can be maintained.

A third group of institutions, generally smaller, have virtually no on-site technical resources, or perhaps one technician. For these researchers to develop innovative ideas, the purchase of technical resources becomes essential. One possible approach is to allow the group to control financial resources for the duration of a given technical project, so that they can allocate funds with which to buy services elsewhere.

This Subpanel has examined various possible modes of operation for university groups to cope with losses in infrastructure, including the establishment of regional technical centers. *It has concluded that the ability of faculty, students, and technical personnel to work together on a daily basis at the university is essential to the research and educational missions of the university scientist. Keeping infrastructure as locally available as possible is therefore crucial for the field. For this reason, the Subpanel recommends that university groups be strengthened within their traditional university setting.*

## H. THE FUNDING SQUEEZE

Financial support for the university-based program is an important factor in determining its success. Over the past five years, funding of this program has declined. In FY1992 the DOE University Program budget was $106.4 million in then-year dollars. In FY1997 it was down to $95.1 million. As a fraction of the overall DOE budget for the field, this represents a drop from 17% to 14%. During this period, inflation has also taken a growing toll. The situation for the DOE University Program is shown in FY98 dollars in figure 6.1. The net result, including inflation, is a 14% drop in funding for operations (which excludes equipment) and a 22% drop overall. For the experimental program, which has suffered the loss of equipment as well as operating funds, the drop is 25%. The drop in the NSF support for particle physics at universities has been even more severe. In addition, the major sources of funds for detector R&D associated with the SSC project, including the Texas National Laboratory Research Commission, vanished as the DOE reductions began. Though the number of universities with DOE-supported high-energy groups has remained about 100 since 1992, in the last year or two 8% of the independent projects (tasks) have been dropped.

Other sources of funds for the university-based program, while important, have not changed the overall picture. The university-based groups receive some project-related support through the national laboratories for specific detector construction efforts, but not enough to cover the losses. Universities themselves have made contributions, such as increasing the number of high-energy physics students supported with teaching assistantships. This contribution has only partly offset the 20% drop since 1992 in the number of graduate students supported by DOE.

Faced with this squeeze, university groups and funding agencies have tried to keep the program as effective as possible. Funding levels for university groups are now well below the level of frugality.

The situation became painful early on for theorists, as funding per faculty member is smaller and thus less flexible than for experimental groups. University theorists report having to decide among hiring a postdoc, traveling to meetings, or receiving summer support. As an alternative to letting support for individual researchers drop below viable levels, both NSF and DOE are reducing the numbers of theorists they support. The number of DOE-supported theoretical faculty members has been reduced by 8% since 1992. Cuts in NSF support for theoretical high-energy physics have been even more severe, with a 17% reduction in the number of supported faculty. Even with these deeper

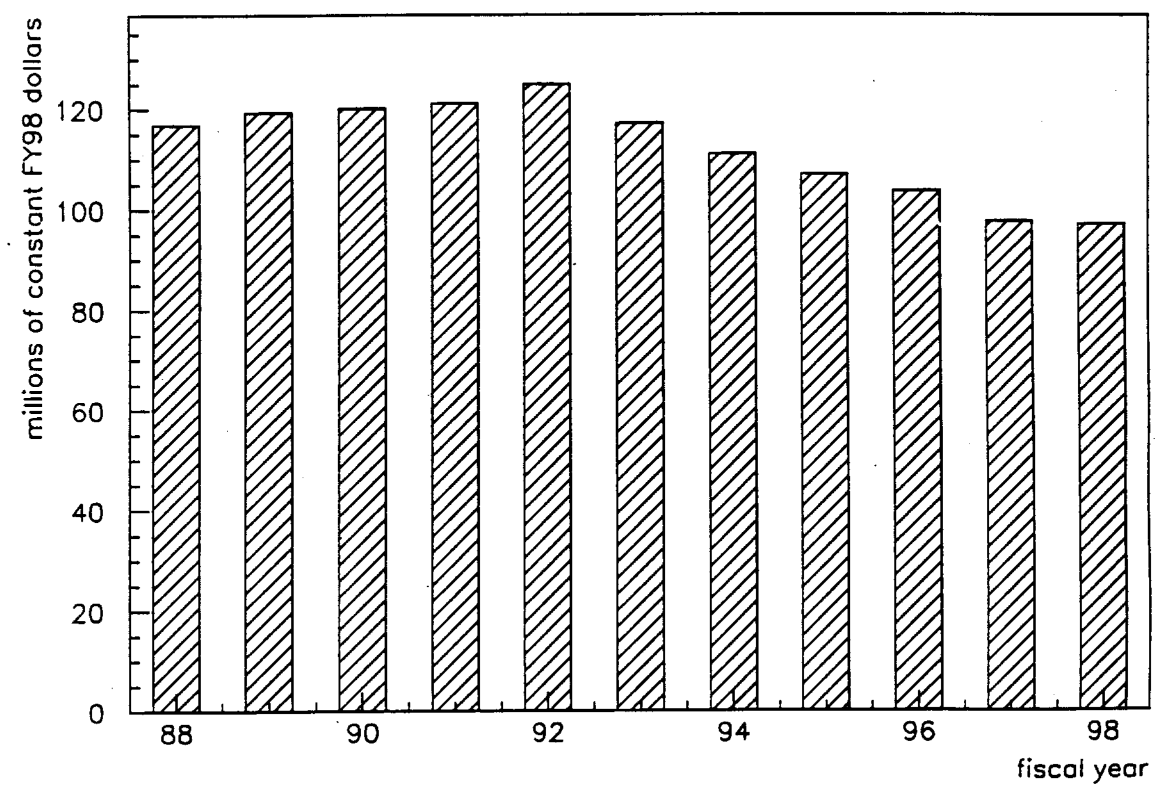


Figure 6.1 Decline of DOE funding of University Program

reductions, the situation for NSF-supported theorists is dire: average support is only about 70% of that received by their DOE-supported peers.

Experimental groups typically have faced the dilemma of choosing between retaining technical support personnel (engineers, technicians) or having a healthy cadre of postdocs and graduate students. In the Subpanel's meetings with representatives of university groups, many described the outcomes of such decisions. In a few cases, technical staff was retained, to ensure the continuance of traditional strength in electronics or detector development. More commonly, groups gave up technical support, reducing their capability to contribute to the design and construction of future experiments. Leading university groups gave examples of the reductions they were forced to make: 5.5 FTEs of engineers and technicians in 1983, zero now; 9 FTEs of technical support in 1984, 2.5 now. A frequently voiced concern was the "ratchet effect": engineering staff lost as a result of a short-term funding problem is very difficult to replace later.

A telling quantitative comparison can be made with the level of technical support for university groups ten years ago. The Treiman Subpanel report "On Future Modes of Experimental Research in High Energy Physics" (DOE/ER-0380, 1988) noted with alarm the deterioration made evident by a questionnaire that they circulated. Their report included a table showing the state of technical support in university groups in 1988. We have investigated how those groups have fared. Portions of the 1988 table are reproduced below, along with current data. In each case, historical data from the DOE were used to ascertain which universities were included in the 1988 categories. Then, data from the summer 1997 survey conducted by LBNL for HEPAP were used to tabulate the current status of those groups. (Due to differences in the questionnaires, it was necessary to aggregate some categories of personnel for the comparison.)

The 1988 report commented, "Further study of the questionnaires revealed a clear and worrisome trend in the makeup of even the healthiest groups: though the number of physicists, students, and even senior engineers has decreased only slightly over the past five years, the number of technicians has declined significantly." From our version of the table, we see that the shrinking of technical support in the small groups has bottomed out at minimal levels, and the loss of Ph.D. physicists and graduate students has become substantial. Our table also shows that the erosion in the large groups has accelerated, with further major losses of both engineering and technician support.

AVERAGES FOR 11 SMALL GROUPS ($0.3 MILLION–0.7 MILLION IN 1988):

| Year | Ph.D. Physicists | Grad Students | Engineers | Technicians |
|---|---|---|---|---|
| 1988 | 10 | 5 | 0.75 | 1 |
| 1997 | 7.8 | 4.0 | 0.77 | 1.1 |

AVERAGES FOR 22 LARGE GROUPS (>$1 MILLION IN 1988):

| Year | Ph.D. Physicists | Grad Students | Engineers | Technicians |
|---|---|---|---|---|
| 1988 | 20.5 | 13 | 4 | 6 |
| 1997 | 18.1 | 11.2 | 1.8 | 2.3 |

Increasing reliance on the technical support of the national labs is a natural and necessary response to the funding squeeze on the university experimental groups, but it has certain serious negative consequences. Collaborating effectively with an engineer or a technician often requires frequent, even daily, interaction in the laboratory. Having to travel to a national laboratory to meet with engineers and technicians means that university researchers are on campus even less, exacerbating some of the problems described earlier in this chapter. Besides interfering with university responsibilities, the shift of technical support off campus removes many opportunities for innovative technical and detector development, decreases opportunities for students to engage in exciting research, and severely lowers the profile of the high-energy enterprise on campus.

The squeeze on funding for the university groups has come in a period where DOE funding for high-energy physics has fallen modestly in real terms (about 6% in constant dollars in five years). The major cause of the university squeeze is the need to complete large construction projects in a period of flat budgets. While the construction projects are essential for the field, the universities are as well. University-based physicists not only lead research at the laboratories but also provide an independent point of view. As the main gateway into high-energy physics, the university-based program provides unique and invaluable opportunities for the students who will be the future members of the field. As the present series of construction projects is completed, the university groups must regain their strength for these new facilities to be used most effectively and for these groups to provide innovative ideas for the future.

In the next chapter, we provide four specific recommendations on funding and infrastructure aimed at strengthening the university-based program.

# 7 Plan for the Future of U.S. High-Energy Physics

*We will not be able to do everything, but what we choose to do we must do well....We cannot blink at the need to live within our means, but budgetary balance can and must be achieved in a way that enhances our quality, not in a way that sacrifices our quality.*

—A. Bartlett Giamatti, "A Free and Ordered Space"

## A. INTRODUCTION

In assessing the possibilities for the future program, the Subpanel considered the current and near-term program and the important open physics questions that have been described in previous chapters of this report. The Subpanel was informed by many members of the U.S. high-energy physics community, who responded both to direct, specific inquiries and to open invitations to comment on issues relevant to the charge.

In proceeding to develop a set of recommendations for the U.S. high-energy physics program over the next decade, the Subpanel was guided by a number of principles. These principles, and the recommendations to which they led, are presented here.

### *Guiding Principles*

- Maximize the potential for major discoveries by
  - utilizing existing U.S. facilities at the frontiers in energy and precision to capitalize on prior investments and
  - participating in experiments at unique facilities abroad.

- Position the U.S. program for a long-term leading role at the energy frontier through
  - vigorous research and development on possible future facilities and
  - international collaboration on future machines.

- Prepare the next generation of scientists through education and training at universities and laboratories.

## B. RECOMMENDATIONS

To balance near-term scientific opportunities with preparations for the most important investigations in the longer term, within a constant-level-of-effort budget, the high-energy physics community and the Subpanel have had to make difficult choices and to recommend that some highly productive programs be terminated. Only by doing so will there be sufficient funding and scientific manpower to carry out higher priority work.

The resulting plan, as expressed in the recommendations that follow, is intended to ensure that the U.S. high-energy physics community will continue to be a leader in both experimental and theoretical research that addresses the most important scientific issues in the field.

### Effective Utilization of New Facilities

The high-energy physics community is fortunate to have several facilities that will soon be coming into operation:

- The Main Injector project at Fermilab, to be completed in 1999, will enable the upgraded CDF and DØ detectors at the Tevatron collider to increase their total data by factors of 20–40 during 2000–2002. This program will bring new insights at the energy frontier.

- The asymmetric-energy B factory (PEP-II) at SLAC will be completed in 1998, and the BaBar detector will begin operation in 1999. This program will extensively explore CP violation in B meson decays.

- The CESR electron-positron collider (funded by NSF) at Cornell and the CLEO detector will have their upgrades completed by 1999, permitting a wealth of studies of rare B decays and charmed particles.

U.S. research groups at universities and laboratories have participated in the design and building of experiments at all of these facilities. These groups are poised to capitalize upon these opportunities. They train hundreds of students and postdoctoral researchers and generate the base for future innovations.

**Recommendation:**

The Subpanel places its highest priority on optimum utilization of the forefront facilities nearing completion. The Subpanel recommends that funding for Tevatron collider, PEP-II, and CESR operations, and for the physics groups using them, be at a level that ensures these facilities fulfill their physics potential.

**The LHC**

In 1994 the HEPAP Subpanel on Vision for the Future of High-Energy Physics, chaired by Sidney Drell, strongly supported U.S. participation in both the accelerator and the general purpose detectors of the Large Hadron Collider (LHC) project at CERN, the European Laboratory for Particle Physics. With strong leadership from the DOE and NSF, and effective help and guidance from the U.S. Congress, this has become a reality with the signing of the CERN-U.S. agreement in December 1997. This agreement enables the U.S. to play significant leadership roles in building the accelerator and the associated ATLAS and CMS detectors, and thus gives the U.S. high-energy physics community the opportunity to shape the exploration of particle physics at the energy frontier.

**Recommendation:**

The Subpanel strongly endorses the physics goals of the LHC and U.S. participation in the accelerator project and the ATLAS and CMS experiments. The funding level and schedule contained in the CERN-U.S. LHC agreement should be followed. The Subpanel expresses its gratitude to the Congress, DOE, and NSF for making possible U.S. participation in the LHC.

**Planning for Future Facilities**

Ultimately, to understand the fundamental particles that make up the universe and the forces between them, we want to reach energy, or equivalently mass, scales where we can produce these particles. Exploration of the high-energy frontier has always deepened our understanding of known phenomena and has often brought unexpected great discoveries. Discovery of heavier particles is an essential part of understanding the structure of the everyday world.

The energy frontier, which now reaches hundreds of GeV for the collisions of quarks, gluons, and leptons, will move to the TeV scale with the Large Hadron Collider (LHC) under construction at CERN. Access to that energy regime should reveal the origin of electroweak-symmetry breaking, most likely with the discovery of new fundamental particles with masses between about 100 GeV and 1 TeV.

New frontier machine possibilities are an electron-positron linear collider with a total energy reaching 1.5 TeV; a Very Large Hadron Collider (VLHC) with proton-proton total energy in the 100 TeV range; and a muon collider with up to several TeV total energy. An electron-positron linear collider would provide new and different scientific opportunities in the same mass range as the LHC, while a VLHC or muon collider would push the energy reach beyond that of the LHC.

Given their scope and cost, any of these new facilities would require the major part of a decade to build and should be an international effort. Development of the appropriate structures to coordinate R&D, decision-making, and management for these international cooperative projects is itself a daunting task. We urge that the U.S. take a leadership role in forging these intergovernmental structures.

Advanced accelerator R&D explores new technologies that might be used in very high energy accelerators far in the future. Given the long development time, it is crucial to invest appropriate resources in this effort now. The Subpanel encourages continued support of work in advanced accelerator R&D at a modestly increased level.

**Recommendation:**

The Subpanel recommends that a new facility at the energy frontier be an integral part of the long-term national high-energy physics program.

**Linear Collider**

The design of a linear collider is more developed than the design of a muon collider or that of a VLHC, and construction could potentially begin in the next decade. The SLC at SLAC, designed and built in the 1980s to study the Z boson, is the first and only example of a electron-positron linear collider and provides a test bed for further development of the linear collider concept. In the 1990s, an international collaboration was set up to study and develop technologies for the next step in energy and luminosity, with SLAC and Japan's KEK leading the R&D effort toward a machine that would use room-temperature rf cavities to accelerate the beams and Germany's DESY leading the corresponding effort for superconducting cavities. KEK and SLAC have recently signed a Memorandum of Understanding to work on R&D toward a common design. The next step is the production of a Conceptual Design Report (CDR) with a complete technical design and associated costs and schedules for specific sites. DESY plans to complete a CDR for a superconducting machine in the next several years as well.

**Recommendation:**

The Subpanel recommends that SLAC continue R&D with Japan's KEK toward a common design for an electron-positron linear collider with a luminosity of at least $10^{34}$ cm$^{-2}$ s$^{-1}$ and an initial capability of 1 TeV in the center of mass, extendible to 1.5 TeV. The Subpanel recommends that SLAC be authorized to produce a Conceptual Design Report for this machine in close collaboration with KEK.

This is not a recommendation to proceed with construction. A decision on whether to construct a linear collider should only follow the recommendation of a future subpanel convened after the CDR is complete. The decision will depend on what is known about the technology of linear colliders and other potential facilities, costs, international support, and advances in our physics understanding.

**Muon Collider and Very Large Hadron Collider**

A muon collider offers the possibility of using leptons to probe phenomena at mass scales that could exceed those at a 1.5 TeV electron-positron linear collider. At lower collision energies, Higgs bosons could be formed directly in muon-antimuon collisions. The idea of a muon collider has been seriously pursued only relatively recently, and the extensive R&D needed to establish the concept is just beginning. A collaboration has been formed to carry out a systematic study, by simulation and experiment, of issues such as muon production, trapping, cooling, acceleration, and detector backgrounds, all of which are needed before the feasibility of a muon collider as a frontier machine for high-energy physics can be demonstrated.

A VLHC would produce proton-proton collisions at center-of-mass energies of order 100 TeV, many times that of the LHC. For this reason, a decision to construct such a machine should await the new physics results from the LHC. Substantial reductions in the cost per TeV are required for any machine of this scale to become a viable option. In addition, such a machine would benefit from advances in technology (such as superconducting magnet technology for the "high-field" version) or better understanding of accelerator physics issues (for the "low-field" version).

**Recommendation:**

The Subpanel recommends that an expanded program of R&D be carried out on a muon collider, involving both simulation and experiments. This R&D program should have central project management, involve both laboratory and university groups, and have the aim of resolving the question of whether this machine is feasible to build and operate for exploring the high-energy frontier. The scale and progress of this R&D program should be subject to additional review in about two years.

**Recommendation:**

The Subpanel recommends an expanded program of R&D on cost reduction strategies, enabling technologies, and accelerator physics issues for a VLHC. These efforts should be coordinated across laboratory and university groups with the aim of identifying design concepts for an economically and technically viable facility. The scale and progress of this R&D program should be subject to additional review in about two years.

**Further Tevatron Improvements**

The Tevatron is the highest energy collider in the world and will remain so until the operation of the LHC. During the period 2001–2005 the Tevatron will be the only facility in the world that could begin to address the key question of the nature of electroweak- symmetry breaking. Completion of the Main Injector project and associated accelerator upgrades will improve the performance of the Tevatron collider to yield an integrated luminosity of 2 to 4 $fb^{-1}$ (per detector) by 2002.

Further incremental improvements could yield an additional 20 $fb^{-1}$ by the time the LHC is operational, extending the discovery potential of the Tevatron collider. For example, a Higgs boson with a mass below about 125 GeV could be found. In addition, such a data sample would yield 10,000 reconstructed top-antitop pairs and improve the precision on the top and W masses to 2 GeV and 20 MeV, respectively. In combination, these measurements would allow an improved prediction of the Higgs boson mass within the context of the Standard Model.

A number of upgrades to the accelerator and detector facilities at Fermilab are required to accumulate such a data sample. Budgetary constraints and the need to focus financial and human resources on other aspects of the future high-energy physics program make necessary our recommendation below to upgrade only one of the detectors.

**Recommendation:**

The Subpanel recommends that the Tevatron collider be upgraded in luminosity during the first part of the next decade with the goal of an integrated luminosity of 20 $fb^{-1}$ by the time the LHC is operational. One of the two large detectors should be upgraded to match the increased luminosity of the collider.

## The Study of CP Violation and the Physics of Quark and Lepton Flavors

Experiments that use intense beams of particles to provide large data samples allow rare processes to be studied. This approach is particularly useful in understanding CP violation and the physics of quark and lepton flavors. It complements experiments performed at the energy frontier. To best address the most important scientific issues, the Subpanel makes the following recommendations:

### B Physics

In the next few years, the B factories at SLAC and KEK will begin operation, and CESR at Cornell will have been upgraded through Phase III. These electron-positron colliders aim to operate with luminosities ranging from $10^{33}$ to $10^{34}$ $cm^{-2}$ $s^{-1}$ near the threshold for producing B mesons. In addition, B particles will be studied in hadron collisions at the Tevatron collider at Fermilab and in experiments abroad.

While there will be great advances in our knowledge of B physics and CP violation from the experiments turning on around 1999, we expect that there will remain compelling open questions that require much larger data samples to address. These laboratories will be considering proposals to further upgrade the luminosity or to build dedicated experiments to study CP violation in the B system at an enhanced level. Together with the work being done in K decays, this would permit incisive measurements to determine whether the Standard Model provides a consistent description of CP-violating effects or whether new physics at high mass scales is required to understand this fundamental property of nature. While approaches using different quark flavors are important in understanding these phenomena, it is important to avoid unnecessary duplication of efforts.

**Recommendation:**

The R&D being undertaken at Cornell (for a Phase IV upgrade of CESR) and at SLAC (for a PEP-II upgrade) aimed at substantially higher luminosity at electron-positron B factories, as well as at Fermilab on a dedicated B physics experiment at the Tevatron collider, should be actively pursued. Choices among the proposals for the upgrades of the electron-positron facilities and a dedicated hadron B physics experiment should be made after the currently approved experiments are operating.

## The Brookhaven AGS Fixed-Target Program

The Subpanel is directly charged with making a recommendation on the fixed-target high-energy physics program at BNL after the AGS becomes primarily an injector for RHIC in 1999. The Laboratory has discussed a small subset of the potential experiments, primarily involving the change of quark or lepton flavor, as candidates to be run after the startup of RHIC, when the base operating costs of the AGS are carried by the Nuclear Physics program of the Department of Energy and the incremental costs of doing such experiments are carried by the High Energy Physics program.

**Recommendation:**

Experiments E-821 to measure g–2 of the muon and E-787 to search for the decay $K^+ \rightarrow \pi^+ \nu \bar{\nu}$, both with sensitivity to effects at the level predicted from weak radiative corrections in the Standard Model, have represented major investments of resources as flagship, high-priority experiments at the AGS. The Subpanel recommends that E-787 be expeditiously completed by the time AGS base operations become supported by the Nuclear Physics program. That will conclude the AGS HEP base program except for E-821. We recommend that E-821 be completed by the end of FY2001.

**Recommendation:**

The Subpanel recommends that after the AGS becomes the injector for RHIC, the possibility be held open for running at most two concurrent experiments that compete within the national program and use the unique AGS beams to particular advantage. This level of AGS operation represents a major reduction and is one of the significant sacrifices required to meet budget constraints.

**Kaon and Muon Physics**

Beams of unprecedented intensity are available at Brookhaven and Fermilab. New experiments have been proposed to these laboratories that would use K beams to make precision tests of the Standard Model picture of the origin of CP violation and muon beams to study rare lepton flavor-changing processes. With competing experiments possible at Brookhaven and at the Main Injector at Fermilab in the same time frame, it is especially important in a time of tight budget constraints to avoid unnecessary duplication, even though these are difficult, exacting experiments that one might otherwise want to have done in different ways to obtain confirmatory results.

**Recommendation:**

Experiments with intense K and muon beams offer the possibility of adding greatly to our understanding of rare quark and lepton transitions and of CP violation. Some of these potential experiments might be carried out at either BNL or Fermilab. The Subpanel recommends that the decision on which, if any, of these competing experiments are approved should be made on the recommendation of the members of a single advisory body to the Division of High Energy Physics that is constituted to evaluate the physics and technical capabilities of such experiments when full proposals are available. Such an advisory body might be drawn from the Program Advisory Committees of BNL and Fermilab, plus several additional members with special expertise relevant to the experiments proposed.

**Neutrino Physics**

The observation of the solar neutrino deficit and the growing body of experimental evidence that the ratio of $\nu_\mu$ to $\nu_e$ produced in the earth's atmosphere does not conform to expectations are indications that neutrino oscillations may occur. If this interpretation is correct, these data would indicate that neutrinos have mass.

The patterns of oscillation with three neutrino types are complex, and the data are not wholly consistent. Current indications suggest that the difference in mass squared between $\nu_\mu$ and one of the other neutrino types may be smaller than thought when the Fermilab long-baseline NUMI/MINOS facility was proposed.

If any neutrino mass were in the eV range, to which the short-baseline COSMOS experiment could be sensitive, it could help resolve the puzzle of dark matter in the Universe. Experiments now underway are reducing the likelihood of this possibility. Thus COSMOS should be examined carefully to be sure that the potential scientific payoff is worth the expenditure.

**Recommendation:**

The Subpanel endorses the importance of the long-baseline neutrino oscillation program at Fermilab. The question of neutrino mass and flavor mixing is of fundamental interest for particle physics. The 1995 HEPAP Subpanel on Accelerator-Based Neutrino Oscillation Experiments recommended that "the Fermilab program should remain flexible to react to new information." Consistent with this, we recommend that Fermilab carefully evaluate the configuration of the NUMI/MINOS facility in the light of results becoming available from experiments elsewhere. We further recommend that the role of the short- baseline COSMOS experiment be reviewed.

**Non-Accelerator Experiments**

Non-accelerator experiments have historically played an important role in high-energy physics. While some projects are aimed at directly addressing particle physics questions, others are broad, interdisciplinary approaches to issues in astrophysics and cosmology. Forging partnerships with other disciplines expands opportunities for doing high-energy physics and maximizes the possibility of discoveries. The diversity and potential for new directions that non-accelerator experiments provide are important for the long-term health and vitality of high-energy physics.

The trend of strong growth in large-scale non-accelerator experiments is expected to continue in the coming decade. Large investments in this area have been made in Japan, Italy, and Canada, with significant U.S. participation in some experiments. Increased funding would be necessary for the U.S. to play a leading role in the next generation of experiments.

Recently, scientists at the national laboratories have become much more involved in non-accelerator projects. The national laboratory infrastructure and high-energy physics expertise could be beneficial in some of the proposed large-scale efforts.

In the past few years, the process of evaluating proposals in non-accelerator physics has included input from the Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP). Individuals in this group advise the funding agencies (DOE, NSF, NASA) on specific experiments. The SAGENAP process has worked well in making comparative evaluations and in setting priorities within the field. These comparative reviews of all new proposals are essential in order to ensure that the funds available for these experiments are optimally used.

**Recommendation:**

The Subpanel recommends that a balanced U.S. high-energy physics program include a strengthened non-accelerator component that is based on the quality and relative importance of the proposed projects within the overall program. The Subpanel recommends the continuation of SAGENAP, within which priorities are set and all experiments evaluated, including those with major national laboratory participation.

## The University-Based Program

The Subpanel was specially charged to examine the current state of the university-based high-energy physics program and to optimize it within the overall plan for the next decade. The Subpanel intensively examined the state of high-energy physics research at the universities, with a portion of the Subpanel devoting special attention to gathering data and input from the community on these issues. The recommendations that follow were developed as part of the optimally balanced plan for the whole program and to better allocate resources within the university program itself.

A vigorous program in high-energy physics requires dynamic experimental and theoretical research, the enthusiastic participation of students, cross-fertilization with other fields, and a diversity of scientific approaches. High-energy physics groups at universities can make unique and vital contributions toward the achievement of these goals. It is clear, therefore, that the 80% of the high-energy physics community that university groups represent cannot bear continuing budget reductions without serious damage to the field. After assessing the university-based program, the Subpanel has four specific recommendations regarding funding and infrastructure of university groups. These recommendations are intended to enlarge scientific understanding, to improve the training of young scientists, and to significantly increase the contributions of the U.S. high-energy physics program to resolving questions of scientific importance.

**The Level of Funding for the University-Based Program**

We are entering a period with many exciting new physics and technical opportunities supported by the DOE: unprecedented luminosity at the energy frontier at Fermilab, the new B factory at SLAC, the upgraded CLEO experiment at Cornell, and experiments at the LHC. The NSF, too, has recognized the importance of these fundamental science projects and is participating in a major way in several of the new construction projects, particularly the CESR upgrade and the LHC detectors.

However, construction of these new facilities has required sacrifices, including significant funding reductions for the high-energy physics program at universities. Cutbacks to the university program have significantly decreased the capability of university groups to participate in running experiments, to invent and develop techniques and instrumentation, and to attract and support students. Support for especially talented technical and scientific personnel, startup funds for new faculty, and support for graduate students are needed. Funds for equipment such as computers, CAD workstations, and general laboratory equipment are an additional need. The decline in support for high-energy theory must be reversed.

To cover most of these needs would require a very significant increase in the funding for the university program. Restoring support to the 1992 level would require a 25% increase. Given the current funding limitations, only a fraction of these needs can be met. However, the most pressing must be met to ensure the success of the physics program in the next decade. A stronger scientific program at the universities will also help to address better the fundamental needs of the nation through enhanced education and outreach.

**Recommendation:**

The Subpanel recommends that, over a two-year period, the annual DOE operating funds for the university program be ramped up by a total of 10% above inflation. The Subpanel encourages the NSF to make a similar increase in its experimental and theoretical elementary-particle physics programs. These increases should be used for activities judged to have the largest impact on physics goals and student training. This action would partially restore the losses of the last five years and better prepare university groups to use the new facilities.

**Establishing a University-Based Detector R&D Program**

The present generation of new experiments, as has been true historically, relies heavily on new techniques and technologies. Funds for generic R&D in especially promising areas are needed to develop the technical innovations for future experimentation. This was a particularly successful aspect of the SSC program, which laid the foundation for the collider upgrades at Fermilab and the LHC detector designs. Such funds are nearly absent from the present university program. However, if sufficient support were provided, one would have every expectation that novel university developments in electronics, software, and computing methodologies, as well as in development of particle detection devices, would continue for the next generation of experiments.

**Recommendation:**

The Subpanel recommends that a detector R&D program, funded at an annual level of $2 million, be initiated to support exceptionally promising projects for future experiments.

**Sharing of University Technical Resources**

Collaborative sharing of responsibilities, and hence resources, has long been a feature of the field. Interaction with others encourages the diversity of talents represented in the university community. Examples of such collaboration exist between universities and all of the national laboratories and also between groups of universities. With the decrease of technical resources locally available at universities compared with a decade ago (even after our recommended 10% ramp up in support), such collaboration can often be a cost-effective way to develop technical ideas or detector components that are funded through the university program.

**Recommendation:**

The Subpanel supports the arrangements that universities have made to share infrastructure with other universities and with the national laboratories. We encourage technical collaboration on innovative ideas. The Subpanel recommends that each national laboratory appoint a liaison who can be contacted by outside physicists wishing to explore the possibility of technical laboratory-university projects.

**Comparative Reviews of University Groups**

The data we have collected show that over the past five years the DOE high-energy physics university program has suffered a loss of 22% in purchasing power. This decrease has had a significant impact on the way university physicists carry out their research, even though the program has proven to be remarkably resilient, and DOE officials are to be commended for the skill with which they have allocated the limited resources.

In this context, however, it is particularly important that the distribution of DOE high-energy physics funds be optimized. Moreover, it is important that the community have confidence that the distribution is being driven by the present and expected future value to the physics program and not by historical precedent. Traditional mail peer reviews focus on a single institution, but the overall calibration of these reviews can be difficult because they do not allow a direct comparison of support across institutions.

In addition, as users of off-site accelerators, the physics groups associated with ANL and LBNL function in a manner similar to those at the larger universities. There is currently no mechanism in the review process to directly compare the contributions of these groups with those of the larger university groups.

**Recommendation**:

We recommend that, on a trial basis, the DOE external peer review of proposals be augmented by direct comparative review of the groups supported by the university program. The physics groups at ANL and LBNL, and eventually BNL, should be included in this review process on a periodic basis.

## C. SETTING PRIORITIES

The preceding recommendations are designed to provide an optimal and balanced high-energy physics program within the assumed constant-level-of-effort budget. This program requires both the effective use of existing facilities and those now under construction—where many of the new results of the coming decade will be obtained—and preparation for the long-term future. To develop the recommendations, the Subpanel had to set clear priorities. Support will be redirected from experimental programs that are ending to those that are essential to the future of the field, and scientists can direct their efforts appropriately.

Difficult decisions about how to allocate limited resources have been made at two levels. At the first level, each high-energy physics accelerator laboratory worked with its user community to develop plans for the next decade that were presented to the Subpanel. Many of the relevant experimental proposals had already undergone rigorous peer review by the advisory committees associated with the laboratories. In shaping their proposed programs, difficult but responsible decisions were made to ensure that only experiments and projects of the greatest importance to the future of the field were stressed in the presentations to the Subpanel. Despite this careful process, the Subpanel found it necessary to make a second level of decisions, removing additional items and paring down others to fit the assumed budget.

In the cumulative process of setting priorities, a number of difficult decisions were made, including the following:

- Fermilab will end the 800 GeV fixed-target program, a central part of the laboratory program since 1983, in which a significant fraction of the national high-energy physics community has participated. This diverse program now includes first-rate experiments on neutrino and charm quark physics that cannot be carried out anywhere else in the world.

- SLAC will terminate operation of the SLC collider for the SLD experiment after the 1998 run. This experiment already has made the single most precise measurement of the weak mixing angle, an important window to new physics, and could have significantly improved this measurement if it had continued.

- Brookhaven will dramatically reduce the high-energy physics program at the AGS after 1999. The Subpanel recommends that there be at most two concurrent experiments after that time. This program has been extremely productive since the early 1960s, with three experiments leading to Nobel Prizes.

- Only one of the two large collider detectors at Fermilab will be further upgraded in the next decade to accommodate the higher luminosity expected with planned accelerator improvements. These detectors discovered the top quark, and, starting in 2000 after the current upgrades, will be exploring the energy frontier.

- Of the excellent experiments on CP violation and the rare decays of quarks and leptons presented to the Subpanel, only a small number will be performed, and those may start later than planned.

- Several large non-accelerator initiatives to study particle astrophysics have been proposed, which could move the U.S. toward a world leadership role in this area. Only a few of these exciting projects will be realized.

The termination or reduction of these programs provides resources for the highest priority items: the effective use of the facilities nearing completion, preparation for very high energy physics at the LHC, accelerator R&D needed to build a future collider facility, and partial restoration of the strength of university groups. Investment in the cost-effective research program proposed here, although requiring considerable scientific sacrifice, will continue to yield important scientific returns and a world leadership role for the U.S. in high-energy physics.

## D. MODIFICATIONS TO THE RECOMMENDATIONS WITH A DECLINING BUDGET

In considering a declining budget scenario, the Subpanel's strategy was to protect, to the extent possible, support for operation of the new facilities, for the LHC effort, for R&D on the accelerator concepts most likely to result in a future collider at the energy frontier, and for the university-based program. Everything else in the program would be reduced or eliminated, and the prospects for new discoveries in the U.S. high-energy physics program would be much less bright.

The specific budgetary assumption was that funding for high-energy physics would decline by being held constant in then-year dollars until FY2002. After that point it was assumed that the budget would again keep pace with inflation. Aside from the impacts described previously for a constant-level-of-effort budget, the Subpanel projects that additional severe cuts to the program would be necessary:

- The funding at the laboratories would decline along with the overall HEP budget, although that portion needed for accelerator operations would be preserved at a constant level of effort. This would lead to fewer experiments, further reduction in laboratory staffs, and a reduction in the possibility of new discoveries at those laboratories.

- Most of the new experiments and upgrades designed to study CP violation and rare processes in B meson, kaon, and muon physics would be eliminated. Progress in this important area might stop with the present experiments, despite known opportunities for new discoveries.

- The neutrino oscillation program at Fermilab would be stretched out or canceled. This could make it impossible for the U.S. to follow up on this exciting experimental indication of physics beyond the Standard Model.

- The partial restoration of support for the university program would not happen. At best, a constant level of effort could be maintained for a few years. The detector R&D program, needed to support the development of innovative instrumentation, could not be started. Along with the reduction in research opportunities at the laboratories, this loss would mean a much less productive physics program and the training of fewer young scientists.

- Accelerator R&D on new approaches to a collider facility at the energy frontier would receive only a fraction of the increase discussed in our recommendations, and the U.S. might be forced to concentrate resources early on one option. This could lose the breakthrough in accelerator technology needed to extend dramatically the energy frontier.

- There would be no increase in funding for non-accelerator experiments, including those in the exciting area at the intersection of high-energy physics and astrophysics.

Taken together, these cuts mean that major new experiments could not be started for some years, and there would be significantly less discovery potential. There would be an inadequate scientific return on prior investments in facilities. Most important, the steady erosion of support and weakened high-energy physics community could make it impossible to start a new collider facility at the energy frontier in the next decade and would greatly compromise a leadership role for the U.S. in high-energy physics.

## E. BENEFITS OF INCREASED SUPPORT FOR THE U.S. HIGH-ENERGY PHYSICS PROGRAM

The challenge for the U.S. high-energy physics program in the early years of the next millennium is to position itself for a leading role in the next international collider at the energy frontier. Such a machine must inevitably follow the Large Hadron Collider. Meeting this challenge will require developing a strong base for the field in the coming decade and the resources to move decisively toward the next collider.

The Subpanel considered the effect of increasing the funding for high-energy physics by doubling it over a ten-year period, as the leaders of many scientific and engineering societies recently proposed for the nation's research budget. Such an increase would have dramatic consequences for the field of high-energy physics. With such a funding increase, the Subpanel envisions the following important improvements to the program foreseen under a constant-level-of-effort budget:

- Most important for the long-term, the U.S. would be able to move forward with full exploration and development of the technologies for the next major accelerator, making the innovations and long-range preparations necessary to explore deeper layers of physical law. A reinvigorated U.S. program would then be well positioned to lead in starting the next international facility at the energy frontier.

- Support for university-based research would be increased to the level appropriate to enable the high-energy physics community to reap the scientific benefits of the enhanced program. This would expand the opportunities for the training of young scientists and support a broad program to develop innovations in electronics, computing, and detection devices.

- The U.S. would move effectively toward a world leadership role in non-accelerator experiments. These experiments would address fundamental issues in particle physics, as well as crucial problems in astrophysics and cosmology.

- The discovery potential of the existing facilities would be more fully exploited. Important experiments could be restored to the program. For example, a broader, multifaceted attack on the mystery of CP violation could be undertaken usingkaons and B mesons, rather than the sharply restricted set of experiments allowed under the constant-level-of-effort scenario. Comparison of precision results obtained in each of these meson systems would incisively test if CP violation can be understood in the Standard Model framework; if not, it would provide multiple insights into the character of the new physics.

This enhanced program would enable the U.S. to maximize the scientific return on the facilities now being completed. It would allow the U.S. to have a leading role in initiating the next major international collider at the energy frontier in the coming decade.

Such an investment would pay additional valuable dividends. There is no question that it would lead to a deeper understanding of the fundamental building blocks of matter and enrich our ability to understand the origins of the universe. It would inspire the next generation of students, who will become the scientists and engineers driving our nation's economy. It would also advance our scientific and technical knowledge of related disciplines, such as magnetics, computation, and materials science. The nation and the world would benefit from the cascading effect of scientific innovations, many of which we cannot imagine today.

The Subpanel urges the Administration, the Congress, and the American people to make possible the opportunities envisioned in this proposal.

## APPENDIX A: Charge to the Subpanel

**Department of Energy**
Germantown. MD 20874-1290
March 11. 1997

Professor Michael S. Withereil
Department of Physics
University of California
Santa Barbara. California 93106

Dear Professor Witherell:

This letter is a request that the High Energy Physics Advisory Panel (HEPAP) conduct a study of scientific opportunities and priorities in the U.S. High Energy Physics (HEP) program. We are seeking your advice and recommendations on how to optimize the program in the coming years and how best to position the program for new facilities that may be needed to exploit scientific opportunities beyond those to be addressed by the Large Hadron Collider (LHC).

Background

The 1994 report of the HEPAP Subpanel on Vision for the Future of High Energy Physics, chaired by Professor Sidney D. Drell, has been most helpful to the Department. To the extent feasible, the Department attempted to follow the recommendations in this report as it proposed a path for a strong and vigorous scientific program in the wake of the demise of the Superconducting Super Collider project. In particular, with the initialing of the international cooperation agreement and associated protocols for U.S. participation in the LHC project at CERN, the Department, together with the National Science Foundation, is implementing the recommendations of the Drell report for "significant participation in the LHC accelerator and detectors, both to provide research opportunities at the energy frontier and to ensure that U.S. physicists remain integrated in the international high energy physics community."

Now that the LHC project is proceeding with strong U.S. participation, it is time to consider how the U.S. High Energy Physics program might best position itself as part of the world program, in particular with respect to future facilities beyond the LHC; long-range planning for such facilities is essential given the long period of R&D and conceptual design required. The feasibility and physics potential of several possible future accelerator facilities were discussed at the Snowmass workshop held this past summer. The facilities considered are at varying stages of development, and all need additional accelerator R&D and design work. This work is an important investment that must be made in the future of the field, and a proper balance is needed between it and exploitation of present facilities and those presently under construction, namely, the B-factory at SLAC and the Main Injector at Fermilab. Also in this time frame, the Alternating Gradient Synchrotron (AGS) facility at Brookhaven will change from primarily supplying beams for high energy (and nuclear physics) fixed-target experiments to being supported by the Nuclear Physics program as an injector for the Relativistic Heavy Ion Collider (RHIC), although it would be possible to carry out high energy fixed-target experiments, if justified by the physics.

The Drell Subpanel also recommended broad reviews of the program. An important element of such reviews is consideration of the optimal balance between different aspects of the program such as support of university-based research, operation of accelerator facilities, non-accelerator experiments, technology R&D and the construction of new or upgraded facilities. High energy physics has always relied on a strong partnership between the universities and the national laboratories. It is commonly perceived, however, that many factors, including funding declines and the increasing sophistication of the needed infrastructure, have affected the capability of the university groups to contribute to the experimental program. Because of the importance of the university program, including its educational role, it is highly appropriate to review the university effort within the context of the national program.

Charge to the Subpanel

- General Charge

  Consider the potential scientific opportunities and recommend a scenario for an optimal and balanced U.S. HEP program over the next decade. Recommend how the program can best move toward new facilities to address physics opportunities beyond the LHC. Assume a constant level of effort for the base U.S. HEP program over this time. Also consider the sacrifices that would be implied by a modest decrease in funding and the opportunities presented by a modest increase. In addition, discuss the possible need for a major new facility and the resulting budget and other changes implied by its construction. In developing these plans, pay particular attention to the topics listed below.

- Specific Topics

  1. Future Facilities

     (a) What will be the compelling physics topics in the next decade and beyond and what facilities will be needed to address them? In the context of the LHC and existing HEP facilities, review and discuss the feasibility and physics promise of possible new HEP accelerator facilities and recommend next steps.

     (b) Recommend the optimal program of U.S. accelerator R&D and design activities aimed at these possible new facilities within the context of similar work being done elsewhere and within the plans for the national program developed above.

     (c) Consider and recommend how best to integrate these efforts with those of other countries, so as to lead to an international framework for construction, operation, and utilization of large (exceeding the resources of any one region) future high energy physics research facilities in both the U.S. and abroad.

2. University Program

   (a) Analyze the current state of the university high energy physics research program, with special attention to the capability of university groups to contribute effectively to different facets of the overall program and to the partnership between universities and national laboratories in carrying out that program.

   (b) Recommend how to optimize the university-based high energy physics research program in the context of the plans developed above for the national effort, the field's evolution toward fewer, larger, and more complex detectors, the trend toward large international efforts, and the broad improvements in computer and communications technology.

3. AGS High Energy Physics Program

   Examine the opportunities for an HEP program of fixed-target experiments at the Brookhaven AGS after the transition to Nuclear Physics and make recommendations on the priority of this program, within the context of the plans developed above.

I encourage the Subpanel to work with the American Physical Society's Division of Particles and Fields and Division of Physics of Beams in soliciting the views of the scientific community. I also encourage you to seek the advice, support, and participation of the National Science Foundation in carrying out this charge.

I hope to be able to discuss this charge with HEPAP at its next meeting and with the Subpanel at its first meeting. The Subpanel report should be completed, reviewed by HEPAP, and transmitted to the Department of Energy by March 2, 1998.

Thank you for your help. I realize that this is a large task, but it is also very important. The dedication and hard work of HEPAP and its Subpanels has always been a major factor in maintaining a strong U.S. program in high energy physics. I greatly appreciate the contributions from you and colleagues.

Sincerely,

Martha Krebs

Martha A. Krebs
Director
Office of Energy Research

## APPENDIX B: Subpanel Members

### HEPAP Subpanel on Planning for the Future of U.S. High-Energy Physics

Frederick Gilman, Chairman
Carnegie-Mellon University

Jonathan Bagger
Johns Hopkins University

James Brau
University of Oregon

Sekhar Chivukula
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James Stone
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Andrew White
University of Texas at Arlington

Michael Witherell (ex-officio)
University of California, Santa Barbara

Executive Secretary

Robert Diebold
U.S. Department of Energy

# APPENDIX C: Subpanel Communications to the High-Energy Physics Community

## Letter to the High-Energy Physics Community Requesting Views

April 1997

Dear Colleagues,

A new Subpanel of HEPAP has been formed to plan for the future of the U.S. high energy physics program, and we need your help to have it succeed. The charge, subpanel membership, and other information can be found through the DOE HEP home page (www.hep.net/doe-hep/home.html). We have just met for the first time in Washington on April 20-21, 1997.

The general charge is to consider the compelling physics issues and recommend a scenario for an optimal and balanced U.S. HEP program over the next decade, including the possible need for a major new facility and the budget and other changes implied by its construction. Particular attention is to be paid to three topics: (1) to review and discuss the feasibility and physics promise of new accelerator facilities and recommend the next steps in accelerator R&D and design aimed at these facilities; (2) to analyze the university-based program and recommend how to optimize that program within the context of the overall program; and (3) to examine the opportunities and make a recommendation on high energy physics fixed-target experiments at Brookhaven after the AGS becomes primarily an injector for RHIC.

In addition to requesting your views on issues before the Subpanel generally, we will be sending out shortly a survey to the university groups to which we hope to get a complete response. This will allow us to understand much more fully the situation at universities and the issue of infrastructure that helps define the ability of university-based groups to contribute to increasingly large, technologically-demanding experiments. A portion of the Subpanel, led by Abe Seiden, will be concentrating on these issues. We expect to hear directly from the high energy physics community through community forums organized at a nearby university the day before our visits to Brookhaven, Fermilab, and SLAC, as well as during meetings with users at the labs themselves. The schedule for these events will be announced shortly.

In any case, I urge you to send us your written thoughts, as individuals or groups, on any of the interlocking and important issues facing us. Early input, which we could read before our next meeting in the latter part of June, would be particularly useful. This can be by letter (to me at the Department of Physics, Carnegie Mellon University, Pittsburgh, PA 15213) or, even better, by e-mail (to a mailbox, FUTUREHEP@hepnrc.hep.net, from which the message will be distributed to all members of the Subpanel) on any of the important issues facing us.

Regards,

Fred Gilman
Chair, HEPAP Subpanel on
Planning for the Future
of U.S. High Energy Physics

**Letter to the Community on University-Based Program**

May 1997

Dear Colleagues:

You should have recently received a letter regarding the formation of a new Subpanel of HEPAP chaired by Fred Gilman. An important part of the charge is the evaluation of the contributions of universities to the high energy physics program, the status of university infrastructure, and how well positioned university scientists are to initiate and contribute to future projects. As part of our information gathering procedure we will be spending a day at each of three universities, just before full subpanel visits to National Laboratories, to allow direct input from a broad spectrum of university physicists. We would like to hear about positive university contributions as well as problems. Ideas on how to optimize the impact of university scientists on the national program would be most useful.

Our visits will be:

June 23, 1997 U.C. Berkeley , Room 375 LeConte Building

August 11, 1997 University of Chicago, Kersten Physics Teaching Ctr, Room 102

September 17, 1997 SUNY Stony Brook, Graduate Physics Building, Room S240 (Basement Level)

We will begin discussions at 9:30 am and continue until about 4:00 pm with a lunch break from 12:00 to 1:30 pm. Included in the morning presentations will be some talks on the status of our information gathering as well as talks on the status of universities from a variety of perspectives.

Please register at least two weeks prior to the meeting if you plan to come and indicate your interest in speaking in order to facilitate our planning. Please bring along a copy of the material you present to leave with us. To register send e-mail to: ABS@SCIPP.UCSC.EDU

Further information (maps, parking, etc.) will be found through the DOE HEP home page (www.hep.net/doe-hep/home.html). Thanks for your help.

Sincerely,

Abe Seiden for the HEPAP Subpanel

**Sample Letter Requesting Information from Laboratories**

Carnegie Mellon

**Department of Physics**
Carnegie Mellon University
Pittsburgh, Pennsylvania 15213-3890
412-268-2740

June 9, 1997

Professor Burton Richter
Director, Stanford Linear Accelerator Center
P. O. Box 4349
Stanford, CA 94309

Dear Burt,

I am writing as chair of the HEPAP Subpanel on "Planning for the Future of U.S. High Energy Physics" and its upcoming visit to SLAC on June 24 - 26, 1997. There are a number of questions that arise from the charge and relate to the national laboratories, to which the Subpanel will be looking to get answers. These are listed below.

1. Within the context of the charge to the Subpanel of a constant level-of-effort over the next decade (and without a major new facility, other than those under construction), what are the present plans of your laboratory for its overall high energy physics program during the next five years? ten years? What additional opportunities exist with a modest budget increase and what is lost with a modest decrease?

2. If a new facility is to be begun during the next decade, what facility or facilities would your laboratory be looking toward? What is the match between physics goals and accelerator technology? What level of inter-laboratory and international collaboration would be involved in the various stages of such a project, and how is it, or would it be, fostered? What is your laboratory's role and what R&D level and associated funding profile would be required to get to the construction stage? When could construction realistically begin and what is the scale of the construction cost of such a facility?

3. The Subpanel is looking at the university-based HEP program in the context of the overall national plan. What are you doing and what can be done to further the university-laboratory partnership? Do you have other suggestions on how to optimize the university-based HEP program?

4. With a new survey of the technical infrastructure and personnel at universities as one part of the picture, could you provide the Subpanel a summary of the technical personnel (physicists, engineers, technicians) and total manpower at your laboratory over the last decade?

5. As part of understanding the support of university-based groups, what funds, typically for detector construction, has your laboratory sent to universities in the last ten years?

I expect that many of these questions would have been naturally answered during the course of the presentations that SLAC is planning to make to the Subpanel. Some of the later, more quantitative questions on manpower and funding may involve some time to dig out the numbers. In any case, we would hope to have the answers developed by the beginning of August, *i.e.*, before the Subpanel meets at Fermilab.

Sincerely,

Fred Gilman

Frederick J. Gilman
Chair, HEPAP Subpanel

**Sample Letter to the Laboratories on Regional Centers**

Carnegie Mellon

Department of Physics
Carnegie Mellon University
Pittsburgh, Pennsylvania 15213-3890
412-268-2740

July 3, 1997

Dr. John Peoples
Director, Fermilab
P. O. Box 500
Batavia, IL 60510

Dear John,

As a result of input to the HEPAP Subpanel on "Planning for the Future of U.S. High Energy Physics," consideration is being given to the concept of regional centers that would provide access for the community to high quality engineering and technical support. These centers could help meet an essential and continuing need both for standard engineering assistance and for help in the development of forefront technologies to be incorporated into HEP experiments, since the technical infrastructure at many individual universities is no longer capable of providing the necessary services.

The present national laboratories with their existing technical infrastructure are candidates for such regional centers. In some cases they already provide such support tied to experiments and associated users in the program of the particular laboratory, but we are considering something that goes beyond the present situation.

Please comment on your laboratory becoming one of these centers. What would be an appropriate scale for this activity? What technical/engineering capabilities could be provided? How would you envision working with customers/collaborators from university groups and what organizational changes or financial support would be needed to establish the centers, so that priorities are established and resources are best managed to meet the needs of the HEP community? How could the long-term viability of this resource be assured?

The issues related to costs and management are clearly going to be particularly important. What can be done to minimize costs, including overhead costs, for outside customers/collaborators, while maintaining the requirements of quality and responsiveness?

We would appreciate receiving at least a preliminary response to the idea of your laboratory being such a regional center by early August, so that the Subpanel could have some well-informed discussion by the time of our meetings at Chicago/Fermilab on August 11 - 14, 1997. Thanks in advance for your helpful input.

Sincerely,

Fred Gilman

Frederick J. Gilman
Chair, HEPAP Subpanel

**Letter to the Community on Regional Centers**

July 11, 1997

Dear Colleague:

One of the charges to the current HEPAP Subpanel, chaired by Fred Gilman, is to investigate infrastructure issues involving the university-based HEP groups. Input to the Subpanel shows that there is a range from groups that lack technical/engineering infrastructure to others who have such support, but are not confident about retaining that support through the next round of projects. There is a general concern, even if the funds existed, of keeping technical personnel at universities fully occupied through all cycles of experiments.

Suggestions have been made to form regional centers that would provide access for the community to high quality engineering and technical support. These centers could help meet an essential and continuing need both for standard engineering assistance and for help in the development of forefront technologies to be incorporated in HEP experiments. This would be a new departure for our field, and were we to proceed down this path, there are many issues that would need careful consideration. We would be interested to receive your suggestions and comments. Some of the important questions follow:

Are such centers a good idea?

Which of the following would be desirable sites for a center: national labs, existing university groups, new centers? Associated with this, how does this relate to the infrastructure currently within the university program - what should be retained there and what concentrated at regional centers?

What should be the source of funding within the HEP budget? (Note that the Subpanel is working within the context of an overall HEP budget that is at a constant level-of-effort, plus or minus modest changes).

What are the most important areas to cover - mechanical engineering, electrical engineering/electronics, computing/software support - and at what level of sophistication?

What management and decision making process for a center would you advocate?

Also, please indicate how your group would imagine using such centers. We look forward to receiving your comments, preferably sent to FUTUREHEP@hepnrc.hep.net .

Sincerely,

Abe Seiden
for the HEPAP Subpanel

# Letter Requesting Information on Laboratory/University Relations

Dear University Colleague:

The Gilman Subpanel is actively in its fact-finding and data-gathering phase now. As part of this process, we are soliciting your views and information on University-Laboratory Relations.

University issues are an important part of the Subpanel's work. Two of us (Charles Prescott and Andy White) have been asked to look into University - Laboratory relations. Because you are one of the University physicists active at a National Lab, we are asking for your views on matters relating to university groups working at the laboratories. If you could take some of your time to respond, your input will be appreciated and will help influence the future practices of the field.

The Subpanel is already focussing on the University infrastructure problem. The Subpanel has already begun looking at the engineering and technical support for university groups working at the laboratories. Although you may want to discuss this issue in more detail, we are specifically asking about other issues that may get lost in the debate over the infrastructure problems.

Please comment on the following or any other matters of concern regarding the laboratory programs:

(1) What major activities (experiments, collaborations, etc.) are you involved in at the National Labs?

(2) Do you have any specific issues to raise regarding research at the Laboratories? What issues need attention? What services are effective? What services are lacking?

(3) Please comment on the mechanisms whereby financial support flows from the funding agencies through the Labs to the universities. What problems do you have and how could things be better?

(4) Discuss computing and networking support for your Laboratory-based research. What balance should we place on teleconferencing and telecommunications versus travel/living at the Labs?

(5) What role do you play in the planning and policy for research activities at the Labs? Are you adequately represented by the User organization? Do you feel you have influence? Do you have any views on the governance of the Labs (role of URA...)?

(6) Should the national labs play a role in the future large scale HEP activities that are not located on the laboratory site (ie., non-accelerator or foreign-based activities)? If so, what recommendations do you have to make the research more effective for your university group?

Your response is welcome to these and/or other issues. It will be shared with the full Subpanel, but it will not be distributed beyond that.

Please return your reply by e-mail to:

prescott@slac.stanford.edu
and white@utahep.uta.edu

your Subpanel representatives for University-Laboratory Relations.

****************************************************************************
UTA>

**Newsletter on Subpanel Activities**

July 15, 1997

Dear Colleagues,

I am writing to update you on the activities of the HEPAP Subpanel on "Planning for the Future of U.S. High Energy Physics" and to urge you to send us your thoughts on the issues facing the HEP community. The subpanel charge, membership, schedule and other information can be found through the DOE HEP home page (www.hep.net/doe-hep/home.html).

**University Issues**

On June 23rd, the portion of the Subpanel headed by Abe Seiden that is concentrating on the university program met on the UC Berkeley campus and heard a number of presentations, comments and suggestions. Michael Barnett showed demographic results from the 1995 Survey of Particle Physics (available as part of the directory of U.S. high energy physicists at http://pdg.lbl.gov/us-hepfolk), and Pier Oddone showed the preliminary results from the new survey of university technical/engineering infrastructure, based on a 75% response. (If your university has not yet responded, please do so as soon as possible so that we have a complete picture).

While every HEP group is unique, a number of common themes emerged. The buying power of HEP university grants has been shrinking in recent years, making it more difficult to negotiate with university administrators for HEP faculty slots, infrastructure and other support. The Outstanding Junior Investigator (OJI) program was seen as very successful and important. In some cases, groups appear to be over-committed to a large number of experiments. The recommendations of the NSF Special Emphasis Panel were discussed, including the comparative review at one time of experimental programs. It was suggested that in addition to peer reviews of specific university proposals, groups of proposals might be comparatively reviewed.

Several speakers noted that the infrastructure available to them has undergone serious reduction over the years. Smaller groups have lost most or all of their infrastructure, while about 20% of the budgets of the larger groups typically go toward technical infrastructure. However, when asked what they would do with additional funding, a large majority in the survey would choose to hire postdocs (or graduate students) rather than engineers or technicians. It was stressed to the Subpanel that this didn't mean that technical/engineering infrastructure was not needed, but that the inability to hold on to engineers and technicians over the long run leads to hiring choices that solve a broader range of problems.

This led to a discussion of the idea of regional centers to provide technical help to nearby university groups. The national labs already play such a role to some extent, but normally limited to help on efforts directly related to the laboratory's program. The Subpanel decided to send letters to the management at the major HEP laboratories asking for their views on the subject and how they might see their laboratories playing such a role. A message was sent to the HEP community, listing some of the questions and issues regarding regional centers and asking for views and comments.

**SLAC**

On June 24th the Subpanel heard about the SLAC program and views of its future. The B-Factory and the BaBar detector are making considerable progress, with the High Energy Ring storing beam just before the Subpanel's arrival, and the Low Energy Ring well on its way to completion. A possible upgrade path for the B-Factory and detector were presented, as well as a brief discription of more advanced accelerator R&D. A very large effort in linear collider R&D has taken place at SLAC as part of an even broader international collaboration. Much development work remains to be done before a costed conceptual design can be established. The directors of KEK and SLAC have drafted a Memorandum of Understanding on an International Linear Collider Optimization Study Group to continue development and optimization and produce a Pre-Design Report. They hope that this would lead to the realization of a TeV-scale linear collider built in either Japan or the U.S. by an international collaboration.

**LBNL**

The HEP program at LBNL was described to the Subpanel on June 25th, starting with work on traditional high energy physics (CDF, D0, BaBar, and ATLAS), especially as it incorporates work on silicon detectors and associated electronics. A significant part of the presentation was also devoted to non-accelerator physics and advanced accelerator R&D. Burt Richter underlined the important contributions of LBNL and their long and highly successful cooperation with SLAC.

**Non-Accelerator Experiments**

The four-hour session on this topic was arranged by Jim Stone from the Subpanel and consisted of a set of mini-review talks and then a few talks on some of the larger experiments being proposed. In addition to the considerable intellectual fervor in this area, the proponents stressed both the fundamental particle physics questions that were being addressed and the diversity it adds to the program. Several of the speakers felt that the establishment of the Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP) was a healthy step forward. Individuals in this group advise the funding agencies (DOE, NSF, and NASA) on specific experiments, and thus play a role analogous to Program Advisory Committees at the accelerator laboratories, although it is

HEPAP and its subpanels that advise on the overall balance between non-accelerator physics and other areas of the HEP program.

**Future Meetings**

The next meetings will be in the Chicago area. The university issues will be addressed at the University of Chicago on August 11 in Room 102 in the Kersten Physics Teaching Center. The full Subpanel will meet in Wilson Hall at Fermilab on August 12-14. Open presentations will take most of the first two days and will include Fermilab views and plans for the future, as well as presentations from Argonne and from the Muon Collider Collaboration. In the late afternoon of August 13th, we again plan a Community Forum, followed by a social for more informal exchanges of information. The Forum is intended for the whole community and is being organized by the Fermilab Users Executive Committee; if you wish to make a presentation, please contact Patty McBride (mcbride@fnal.gov).

Meetings will be held on Long Island in September. University issues will be the subject of the meeting at SUNY Stony Brook in Room S240 of the Graduate Physics Building on September 17th. The Subpanel will then meet at Brookhaven National Laboratory on September 18-21, with open presentations the first two days on the HEP programs at Brookhaven and Cornell (CESR/CLEO), as well as possible kaon experiments at Fermilab and U.S. efforts abroad, particularly future work on the LHC. A Community Forum, again open to all, will be organized by the AGS and CLEO users organizations for September 19th in the late afternoon.

Members of the community who wish to present their views on university issues should contact Abe Seiden (abs@scipp.ucsc.edu). Those wanting to discuss other issues related to the Subpanel's charge should contact the users organization arranging the appropriate Community Forum.

So far, we have received about 25 messages with advice related to the Subpanel's charge. We would very much like tq hear from more of you in order to get a wider sampling of what you are thinking. A fast and efficient way to communicate your thoughts and advice to the Subpanel is to send electronic mail to futurehep@hepnrc.hep.net .

Regards,

Fred Gilman
Chair, HEPAP Subpanel on Planning for
the Future of U.S. High Energy Physics

# APPENDIX D: Meeting Agendas

**Agenda**
**University Issues**
**HEPAP Subpanel on Planning for the Future of U.S. High Energy Physics**
**University of California at Berkeley**
**June 23, 1997**

Monday, June 23, 1997:

| | | |
|---|---|---|
| 8:30 a.m. | Executive Session | |
| 9:30 a.m. | Recent Survey on University Infrastructure | P. Oddone |
| 10:00 a.m. | Demographics of Community, based on last year's survey | M. Barnett |
| 10:45 a.m. | Break | |
| 11:15 a.m. | Theoretical High Energy Physics at a University | J. Gunion |
| 12:00 noon | Lunch | |
| 1:30 p.m. | Process for the NSF Special Emphasis Panel | R. Cahn |
| 2:00 p.m. | Experimental University Program (Oregon) | R. Frey |
| 3:00 p.m. | Experimental University Program (U.C.-Santa Barbara) | H. Nelson |
| 3:30 p.m. | Experimental University Program (Caltech) | D. Hitlin |
| 4:30 p.m. | Views of a Principal Investigator and from Department Chairs | B. Cabrera |
| 5:30 p.m. | Executive Session | |
| 7:15 p.m. | Adjourn | |

**Agenda**
**HEPAP Subpanel on Planning for the Future of U.S. High Energy Physics**
**Stanford Linear Accelerator Center**
**June 24-26, 1997**

Tuesday, June 24, 1997:

| | | | |
|---|---|---|---|
| 8:30 a.m. | Executive Session of Subpanel | | Orange Room |

SLAC Program

| | | | |
|---|---|---|---|
| 9:30 a.m. | Strategic Issues | B. Richter | Auditorium |
| 10:00 a.m. | Overview of SLAC Program | D. Leith | Auditorium |
| 10:30 a.m. | Break | | |
| 10:50 a.m. | B-factory | J. Seeman | Auditorium |
| 11:30 a.m. | BaBar | D. Hitlin | Auditorium |
| 12:10 p.m. | Advanced Accelerator R&D | R. Siemann | Auditorium |
| 12:30 p.m. | Lunch & Executive Session | | Orange Room |
| 1:30 p.m. | NLC Status & Plans | D. Burke | Auditorium |
| 2:20 p.m. | NLC Theory Overview | M. Peskin | Auditorium |
| 2:50 p.m. | Experimental Opportunities with an NLC | C. Baltay | Auditorium |
| 3:30 p.m. | Break | | |
| 3:50 p.m. | International Review Committee on Linear Colliders | G. Loew | Auditorium |
| 4:00 p.m. | Summary | B. Richter | Auditorium |
| 4:15 p.m. | NLC Tour | | |
| 5:30 p.m. | Executive Session | | Orange Room |
| 7:00 p.m. | Social | | |
| 7:30 p.m. | Dinner | | |

Wednesday, June 25, 1997:

| | | | |
|---|---|---|---|
| 8:00 a.m. | Tour of B-factory & BaBar | | |
| 8:45 a.m. | Executive Session | | Orange Room |

LBNL Program

| | | | |
|---|---|---|---|
| 9:30 a.m. | View from LBNL | J. Siegrist | Auditorium |
| 9:45 a.m. | Hadron Collider Physics at LBNL | K. Einsweiler | Auditorium |
| 10:15 a.m. | $e^+e^-$ Collider Physics at LBNL | B. Jacobsen | Auditorium |
| 10:45 a.m. | Astrophysics at LBNL | S. Perlmutter | Auditorium |
| 11:00 a.m. | Break | | |
| 11:15 a.m. | Critical Technologies for Collider Performance | W. Barletta | Auditorium |
| 11:35 a.m. | Advanced Accelerator Techniques at LBNL | S. Chattopadhyay | Auditorium |
| 12:00 noon | Lunch & Executive Session | | Orange Room |

Non-Accelerator Physics Program

| | | | |
|---|---|---|---|
| 1:00 p.m. | Introduction | J. Stone | Auditorium |
| 1:10 p.m. | Theoretical Motivation | P. Langacker | Auditorium |
| 1:40 p.m. | Solar Neutrino Experiments | R.G.H. Robertson | Auditorium |
| 2:05 p.m. | Atmospheric & Reactor Neutrino Experiments | H. Sobel | Auditorium |
| 2:30 p.m. | High Energy Neutrino/Gamma Experiments | F. Halzen | Auditorium |
| 2:55 p.m. | Gravity Wave Experiments | B. Barish | Auditorium |
| 3:15 p.m. | Dark Matter Experiments | B. Sadoulet | Auditorium |
| 3:35 p.m. | Break | | |
| 4:00 p.m. | The GLAST Project | S. Ritz | Auditorium |

| | | | |
|---|---|---|---|
| 4:20 p.m. | The AUGER Project | J. Cronin<br>A. Dzierba | Auditorium |
| 4:40 p.m. | The Km**3 Project | D. Nygren | Auditorium |

Meeting with Community

| | | | |
|---|---|---|---|
| 5:00 p.m. | SLUO Introduction | S. Hertzbach<br>(U. of Mass.) | Auditorium |
| 5:10 p.m. | Views of a Junior Faculty Member | B. Schumm<br>(UCSC) | Auditorium |
| 5:20 p.m. | Perspectives of a Post Doc | S. Fahey<br>(U. of Colorado) | Auditorium |
| 5:30 p.m. | University Infrastructure Issues | W. Toki<br>(Colorado State) | Auditorium |
| 5:40 p.m. | Perspectives of a Small University Group | W. Bugg<br>(U. of Tennessee) | Auditorium |
| 5:50 p.m. | Perspectives from<br>"The Great White North" | J. McKenna<br>(UBC) | Auditorium |
| 6:00 p.m. | Fixed Target Experiments<br>at SLAC End Station A | P. Bosted<br>(American U.) | Auditorium |
| 6:15 p.m. | Vertexing and the Physics<br>Reach at Linear Colliders | D. Jackson<br>(Rutherford Labs) | Auditorium |
| 6:30 p.m. | Personal Observations | S. Hertzbach<br>(U. of Mass.) | Auditorium |
| 6:40 p.m. | Comments from Community | | |
| 7:00 p.m. | Social with Community | | Auditorium Lobby |
| 8:00 p.m. | Adjourn | | |

Thursday, June 26, 1997:

| | | | |
|---|---|---|---|
| 8:30 a.m. | Executive Session | | Orange Room |
| 3:00 p.m. | Adjourn | | |

**Agenda**
**University Issues**
**HEPAP Subpanel on Planning for the Future of U.S. High Energy Physics**
**University of Chicago**
**August 11, 1997**

Monday, August 11, 1997:

| | | |
|---|---|---|
| 9:30 a.m. | Thoughts on U.S. University HEP Program | S. Wojcicki |
| 9:50 a.m. | University of Chicago Experimental High Energy Physics NSF Grant | M. Shochet |
| 10:10 a.m. | Trends in Formal Theory | J. Harvey |
| 10:40 a.m. | Issues in Universities-based Research in High Energy Theory | A. El-Khadra |
| 11:10 a.m. | Break | |
| 11:30 a.m. | Particle Astrophysics Opportunities | J. Cronin |
| 11:45 a.m. | Technical Capabilities at Universities | H. Williams |
| 12:00 noon | Comments to HEPAP Subpanel | M. Marshak |
| 12:15 p.m. | Lunch | |
| 1:40 p.m. | A Dean's Perspective on High Energy Physics | R. Peccei |
| 2:00 p.m. | U.S. ZEUS Program | W. Smith |
| 2:15 p.m. | Comments on Regional Centers for Engineering | G. Gollin |
| 2:30 p.m. | Top 10 Myths about HEP Research at Universities | K. De |
| 2:45 p.m. | High Energy Physics at SMU | R. Stroynowski |
| 3:00 p.m. | High Energy Physics at Iowa State University | E. Rosenberg |

| | | |
|---|---|---|
| 3:15 p.m. | Break | |
| 3:30 p.m. | The Need to Maintain Strong Infrastructure at the Universities for High Energy Physics Research | A. Dzierba |
| 3:45 p.m. | Purdue High Energy Physics | D. Miller |
| 4:00 p.m. | Accelerator/High Energy Physics Interface Wayne State University | D. Cinabro |
| 4:30 p.m. | Executive Session | |
| 7:00 p.m. | Adjourn | |

**Agenda**
**HEPAP Subpanel on Planning for the Future of U.S. High Energy Physics**
**Fermi National Accelerator Laboratory**
**Wilson Hall - 1 West**
**(Executive Sessions in 1 East)**
**August 12-14, 1997**

Tuesday, August 12, 1997:

| | | | |
|---|---|---|---|
| 8:30 a.m. | Executive Session of Subpanel | | (1 East) |

Fermilab Program (1 West)

| | | |
|---|---|---|
| 9:30 a.m. | Introduction to Fermilab Program | J. Peoples |
| 9:50 a.m. | Collider Physics at the Tevatron | P. Tipton |
| 10:30 a.m. | Break | |
| 10:50 a.m. | Collider Preparations for Run II and Beyond | J. Marriner |
| 11:30 a.m. | Upgrade of the DO Detector for Run II and Beyond | J. Butler |
| 12:00 noon | Upgrade of the CDF Detector for Run II and Beyond | D. Amidei |
| 12:30 p.m. | Lunch and Executive Session | (1 East) |
| 1:30 p.m. | New Proton Source | P. Martin |
| 1:50 p.m. | Opportunity for a Dedicated B Physics Program at the Tevatron | S. Stone |
| 2:20 p.m. | Neutrino Oscillation Experiments and Reach | B. Bernstein |
| 2:50 p.m. | The NUMI Project (Neutrinos at the Main Injector) | G. Rameika |
| 3:10 p.m. | Expected Physics Measurements with NUMI and Future Evolution | D. Michael |
| 3:30 p.m. | Break | |
| 4:00 p.m. | Accelerator R&D | P. Limon |
| 4:30 p.m. | Budgets | K. Stanfield |
| 5:00 p.m. | Conclusions and Summary | J. Peoples |

| | | |
|---|---|---|
| 5:30 p.m. | Executive Session | (1 East) |
| 7:00 p.m. | Adjourn | |

Wednesday, August 13, 1997:

| | | |
|---|---|---|
| 8:00 a.m. | Tour | |

Argonne Program

| | | |
|---|---|---|
| 9:15 a.m. | Argonne HEP Overview | L. Price |
| 9:35 a.m. | ANL Advanced Accelerator R&D | J. Power |
| 9:50 a.m. | ANL Collider Physics | S. Kuhlmann |
| 10:20 a.m. | Neutrino and Other Physics at ANL | D. Ayres |
| 10:40 a.m. | Summary | L. Price |
| 10:45 a.m | Break | |
| 11:00 a.m. | VLHC (Very Large Hadron Collider) | M. Albrow<br>S. Mishra<br>W. Foster<br>E. Malamud |
| 12:40 p.m. | Lunch and Executive Session | (1 East) |

Muon Collider Program

| | | |
|---|---|---|
| 1:40 p.m. | Organization of the Collaboration | A. Tollestrup |
| 1:45 p.m. | Introduction to the Muon Collider | R. Palmer |
| 1:55 p.m. | Physics Opportunities | J. Gunion |
| 2:25 p.m. | Machine Overview | R. Palmer |
| 2:55 p.m. | Break | |
| 3:05 p.m. | Detector & Background | A. Tollestrup |
| 3:35 p.m. | R&D Plan | J. Wurtele |
| 4:05 p.m. | Cooling Experiment | S. Geer |
| 4:35 p.m. | Summary | R. Palmer |

| | | |
|---|---|---|
| 4:50 p.m. | Break | |

Meeting with Community

| | | |
|---|---|---|
| 5:00 p.m. | Introduction/General Remarks | F. Gilman<br>P. McBride |
| 5:05 p.m. | Young Faculty Concerns | D. Gerdes |
| 5:20 p.m. | Graduate Students and the Short Term Future | M. Begel |
| 5:35 p.m. | Graduate Students and the Long Term Future | T. Joffe-Minor |
| 5:50 p.m. | View of a Postdoc | W. Cobau |
| 6:05 p.m. | An Englishman Visits Fermilab | V. Smith |
| 6:20 p.m. | Perspective of Smaller University Groups | D. Hedin |
| 6:30 p.m. | Fermilab - Crown Jewel of the U.S. Program | T. Liss |
| 6:40 p.m. | Support for R&D on Rad-Hard Materials for Micro-Vertex Detectors | T. Devlin |
| 6:50 p.m. | Discussion | |
| 7:00 p.m. | Social with Community in the Atrium | |
| 8:00 p.m. | Adjourn | |

Thursday, August 14, 1997:

| | | |
|---|---|---|
| 8:30 a.m. | Executive Session | (1 East) |
| 4:00 p.m. | Adjourn | |

**Agenda**
**University Issues**
**HEPAP Subpanel on Planning for the Future of U.S. High Energy Physics**
**State University of New York at Stony Brook**
**September 17, 1997**

Wednesday, September 17, 1997

| | | |
|---|---|---|
| 8:30 a.m. | Executive Session | |
| 9:30 a.m. | Perspective on University Program | S. Smith |
| 9:50 a.m. | University Infrastructure: Possibilities using a Collaborative Development Program | M. Shaevitz |
| 10:05 a.m. | Regional "Centers" The BMC Experience | J. Bensinger |
| 10:20 a.m. | Attracting Students into Physics | P. Fisher |
| 10:35 a.m. | Break | |
| 10:55 a.m. | Funding Issues in Theoretical Particle Physics | A. Guth |
| 11:10 a.m. | The Future of "Small" Experiments | C. Taylor |
| 11:25 a.m. | Laboratories and Universities in HEP | R. Lander |
| 11:40 a.m. | Accelerator R&D at Universities | P. McIntyre |
| 12:00 | Lunch | |
| 1:30 p.m. | The Stony Brook HEP Group, University Infrastructure for HEP Research, and the K2K Experiment | C. Jung |
| 2:05 p.m. | The CLEO Collaboration and Physics | G. Brandenburg<br>E. Thorndike |
| 2:45 p.m. | Break | |
| 3:10 p.m. | Theory/Experiment Interface | G. Sterman and<br>W. Tung |
| 3:30 p.m. | HEP Infrastructure at Universities | C. Baltay |

| | | |
|---|---|---|
| 3:50 p.m. | HEP from a Dean's Point of View | T. Appelquist |
| 4:10 p.m. | Supersymmetry, the Next Collider and the University HEP Program | K. Lane |
| 4:30 p.m. | Physics at Universities - A View as Chairman and CMS User | L. Sulak |
| 4:50 p.m. | NSF Theory Program, University Research Infrastructure, and Resources for Lattice Gauge Theory | B. Sugar |
| 5:20 p.m. | Executive Session | |
| 7:00 p.m. | Adjourn | |

**Agenda**
**HEPAP Subpanel on Planning for the Future of U.S. High Energy Physics**
**Brookhaven National Laboratory**
**Physics Building 510**
**Open Presentations - Large Seminar Room**
**Executive Sessions - Small Seminar Room**
**September 18-21, 1997**

Thursday, September 18, 1997:

| | | |
|---|---|---|
| 8:30 a.m. | Executive Session | |
| 9:30 a.m. | Welcome | P. Bond |
| 9:40 a.m. | Introduction and Physics Motivation | W. Marciano |
| 10:30 a.m. | Break | |

Fermilab Kaon Program at the Main Injector

| | | |
|---|---|---|
| 10:45 a.m. | KTeV and the Transition to KaMI | B. Winstein |
| 11:05 a.m. | KL -> pi0 nu nubar at KaMI | K. Arisaka |
| 11:20 a.m. | K+ -> pi+ nu nubar: the CKM Experiment | J. Ritchie |
| 11:40 a.m. | The CPT Experiment | G. Thomson |
| 12:00 noon | Facilities for 120 GeV Fixed Target at Fermilab | G. Bock |
| 12:15 p.m. | Lunch and Executive Session | |

Brookhaven AGS Program

| | | |
|---|---|---|
| 1:15 p.m. | K mu 3 T-Violation Experiment | H. Ma |
| 1:45 p.m. | K+ -> pi+ nu nubar and Ko -> pi0 nu nubar Experiments | A. Konaka |
| 2:30 p.m. | Rare Kaons Using the AGS | M. Zeller |
| 2:45 p.m. | Muon g-2 Experiment | L. Roberts |
| 3:15 p.m. | Break | |
| 3:45 p.m. | Muon Conversion Experiment | W. Molzon |

| | | |
|---|---|---|
| 4:15 p.m. | AGS-2000 Plan at BNL | T. Kirk |
| 4:45 p.m. | Executive Session | |
| 5:55 p.m. | Adjourn | |
| 6:15 p.m. | Reception | (Berkner Hall) |
| 7:00 p.m. | Dinner | (Berkner Hall) |

Friday, September 19, 1997:

| | | |
|---|---|---|
| 8:00 a.m. | Brookhaven Tour | |

Cornell Program

| | | |
|---|---|---|
| 9:00 a.m. | Cornell Introduction | K. Berkelman |
| 9:30 a.m. | The Case for Very High Luminosity | J. Rosner |
| 10:00 a.m. | Increasing CESR Luminosity | D. Rubin |
| 10:30 a.m. | Break | |
| 11:00 a.m. | CLEO Capabilities | A. Weinstein |
| 11:30 a.m. | Cornell Summary | K. Berkelman |
| 12:00 noon | Lunch and Executive Session | |

LHC Program

| | | |
|---|---|---|
| 12:45 p.m. | LHC Physics | I. Hinchliffe |
| 1:15 p.m. | ATLAS Plans | W. Willis |
| 1:35 p.m. | U.S. CMS Plans | D. Reeder |
| 1:55 p.m. | U.S. LHC Accelerator Plans | J. Strait |

Very Large Hadron Collider

| | | |
|---|---|---|
| 2:15 p.m. | VLHC (High Field) | M. Harrison |

BNL's HEP Role

| | | |
|---|---|---|
| 3:00 p.m. | BNL's Role in the National HEP Program | T. Kirk |
| 3:45 p.m. | Break | |

Meeting with Community

| | | |
|---|---|---|
| 4:00 p.m. | Nurturing High Energy Physics in the U.S. | G. Farrar |
| | Comments on BNL (AGS) Beyond 1999 | J. Sandweiss |
| | Perspectives from a Small Experiment | R. Carey |
| | Physics at the Feynman Limit | S. Adler |
| | Introduction to Young Scientists from CLEO | N. Menon |
| | Perspectives on the Experimental HEP Field | J. O'Neill |
| | HEP Outreach: Public Relations and Our Affiliation with Industry | J. Hinson |
| | Becoming an Experimental High Energy Physicist | V. Boisvert |
| | CLEO PostDoc's Perspectives | K. Ecklund |
| | Doing Particle Physics with Undergraduates-Only | P. Rubin |
| | Light Hadron Spectroscopy as a Problem/Opportunity for HEP | D. Peaslee |
| 6:15 p.m. | Discussion with Community | |
| 6:30 p.m. | Social Hour with Community | Berkner Hall |
| 8:00 p.m. | Adjourn | |

Saturday, September 20, 1997:

| | | |
|---|---|---|
| 8:30 a.m. | Executive Session | |
| 6:00 p.m. | Adjourn | |

Sunday, September 21, 1997:

| | |
|---|---|
| 8:30 a.m. | Executive Session |
| 3:00 p.m. | Adjourn |

## APPENDIX E: Questionnaire for HEPAP Survey of Support at U.S. Universities

**Questionnaire Used by P. Oddone and D. Vaughan for their "Survey of High-Energy Physics Support at U.S. Universities," LBNL Report PUB-806 (October 1997)**

# SURVEY OF HIGH-ENERGY PHYSICS SUPPORT AT U.S. UNIVERSITIES

The High Energy Physics Advisory Panel advises both the Department of Energy and the National Science Foundation on the conduct of high-energy physics research. The following survey is an effort by HEPAP to assess trends in the funding and staffing of high-energy physics projects at U.S. universities, and in particular, the supporting technical and engineering infrastructure. We are asking you, as the correspondent for your institution, to provide information not only for projects in which you are involved, but also for other high-energy physics projects at your institution.

**Please answer the questions as completely as you can, summarizing <u>all</u> high-energy physics efforts at your institution. Please write neatly. Again, you are the *<u>only</u>* person at your institution receiving this questionnaire.**

For further information or clarification, please call Douglas Vaughan at Lawrence Berkeley National Laboratory, phone 510/486-5698, e-mail *gdvaughan@lbl.gov*.

Your name ______________________________

Institution ______________________________

Phone number ______________________________

E-mail address ______________________________

## GENERAL INSTRUCTIONS: QUESTIONS 1 AND 2

The first two questions request information on the distribution of high-energy physics effort at your institution. Most of the answers are to be given in terms of *full-time equivalents*, or FTEs, where 1 FTE is equal to one calendar year's effort by a full-time staff member. Some examples follow.

*Example 1*

The following staff configuration is represented in the table entries below:

- 1 faculty theoretical physicist, supported for two months during the summer by an NSF grant (note that each full-time faculty should be counted as 1 FTE, regardless of the time spent teaching—in this example, time is therefore apportioned 2/12 NSF, 10/12 nonfederal)
- 2 faculty experimental physicists, both supported for two months during the summer by an NSF grant
- 1 retired faculty experimental physicist, supported one-quarter time by an NSF grant (note that retired faculty are shown as "other senior physicists")
- 1 nonfaculty accelerator physicist, supported one-half time by DOE base funding

| | No. of HEP staff | No. of high energy physics FTEs supported by | | | | | |
|---|---|---|---|---|---|---|---|
| | | **DOE grants** | **DOE xfers** | **POs** | **NSF grants** | **NSF xfers** | **Non-federal** |
| *Faculty physicists* | | | | | | | |
| Theoretical | 1 | | | | 0.17 | | 0.83 |
| Experimental | 2 | | | | 0.33 | | 1.67 |
| Accelerator design | | | | | | | |
| *Other senior physicists* | | | | | | | |
| Theoretical | | | | | | | |
| Experimental | 1 | | | | 0.25 | | |
| Accelerator design | 1 | 0.50 | | | | | |

*Example 2*

A second example:

- 3 grad students (2 theoretical, 1 experimental), supported full-time by NSF grants
- 1 grad student (experimental), supported by the institution for four months as a teaching assistant, the rest of the time by a DOE grant (note that, for grad students, the time spent teaching does not appear in the survey)
- 2 undergraduate students, each supported one-quarter time for nine months by a DOE grant (note that each therefore counts as $0.25 \times 0.75$ FTE)

| | No. of HEP staff | No. of high energy physics FTEs supported by | | | | | |
|---|---|---|---|---|---|---|---|
| | | **DOE grants** | **DOE xfers** | **POs** | **NSF grants** | **NSF xfers** | **Non-federal** |
| *Graduate students* | | | | | | | |
| Theoretical | 2 | | | | 2.00 | | |
| Experimental | 2 | 0.67 | | | 1.00 | | |
| Accelerator design | | | | | | | |
| Undergrad students | 2 | 0.38 | | | | | |

*Example 3*

- 1 mechanical engineer, supported for three months by a DOE grant
- 2 electronics engineers, each supported full-time by a purchase order from Fermilab for work on CDF
- 2 electronics engineers, each supported for six months by the transfer of DOE funds from SLAC for BABAR support
- 4 electronics technicians, supported for a total of six person-months of effort by a DOE grant

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| *Prof support staff* | | | | | | | |
| Mech engineers | 1 | 0.25 | | | | | |
| Elec engineers | 4 | | 1.00 | 2.00 | | | |
| Computer pgmmers | | | | | | | |
| Mech techs/machnsts | | | | | | | |
| Elec technicians | 4 | 0.50 | | | | | |
| Other (pls specify) ________ | | | | | | | |

*Questions 1 and 2 follow*

1. *Personnel Engaged in High-Energy Physics Research*

- Please provide for your institution a breakdown of the staff engaged in all facets of high-energy physics research during fiscal 1997 (Oct 1996–Sep 1997). Indicate the total number of staff, as well as the number of full-time equivalents supported by
  i. DOE High Energy Physics grants (base funding)
  ii. The transfer of DOE funds from other institutions (usually DOE national labs), typically earmarked for detector work
  iii. Purchase orders from national labs to build equipment
  iv. NSF grants (exclude the amount of any funds transferred to another institution)
  v. The transfer of NSF funds from other universities
  vi. Nonfederal sources of support, including state and university funds

  One FTE reflects one calendar year's effort by a full-time staff member; compute each full-time faculty member as 1 FTE, regardless of nonresearch teaching responsibilities.

| | No. of HEP staff | No. of high-energy physics FTEs supported by | | | | | |
|---|---|---|---|---|---|---|---|
| | | DOE grants | DOE xfers | POs | NSF grants | NSF xfers | Non-federal |
| *Faculty physicists* | | | | | | | |
| Theoretical | | | | | | | |
| Experimental | | | | | | | |
| Accelerator design | | | | | | | |
| *Other senior physicists* | | | | | | | |
| Theoretical | | | | | | | |
| Experimental | | | | | | | |
| Accelerator design | | | | | | | |
| *Postdoctoral fellows* | | | | | | | |
| Theoretical | | | | | | | |
| Experimental | | | | | | | |
| Accelerator design | | | | | | | |
| *Graduate students* | | | | | | | |
| Theoretical | | | | | | | |
| Experimental | | | | | | | |
| Accelerator design | | | | | | | |
| Undergrad students | | | | | | | |
| *Prof support staff* | | | | | | | |
| Mech engineers | | | | | | | |
| Elec engineers | | | | | | | |
| Computer pgmmers | | | | | | | |
| Mech techs/machnsts | | | | | | | |
| Elec technicians | | | | | | | |
| Other (pls specify) ________ | | | | | | | |

2. *Apportionment of Current Effort*

- In the current fiscal year, how is the total effort of high-energy physicists (faculty and other senior physicists, postdocs, and grad students) at your institution apportioned among the field's major projects? How do you foresee effort being apportioned in the year 2002, assuming a constant level of effort over the next five years? Please indicate levels of effort in full-time equivalents.

| | No. of FTEs (physicists only) | |
|---|---|---|
| | Current | Projected 2002 |
| *Specific experiments* | | |
| Brookhaven—AGS | | |
| Cornell—CESR | | |
| Fermilab—CDF | | |
| —D0 | | |
| —Fixed-target expts | | |
| SLAC—BABAR | | |
| —SLD | | |
| —Other | | |
| Other U.S. accelerators | | |
| CERN—LEP | | |
| —ATLAS | | |
| —CMS | | |
| —Other | | |
| DESY | | |
| KEK—BELLE | | |
| IHEP—BES | | |
| Other non-U.S. accelerators | | |
| Nonaccelerator expts | | |
| Nonspecific experimental research | | |
| Accelerator R&D/design | | |
| *Theoretical research* | | |
| String theory | | |
| Field theory | | |
| Phenomenology | | |
| Particle astrophysics theory | | |
| Other theory | | |
| *Other* | | |
| ______________ | | |
| ______________ | | |
| ______________ | | |

## GENERAL INSTRUCTIONS: QUESTION 3

Question 3 seeks to identify some of the important resources at your institution, together with the costs of using those resources. In answering the first part of the question, provide reasonable detail about current capabilities and facilities (including, for example, design expertise, unique experience in detector fabrication, state-of-the-art shop facilities, etc.).

In the final part of the question, provide the fully burdened cost to federal agencies for projects (of the three indicated sizes) done by engineers and technicians.

*Example*

The following situation is reflected in the table entries below:

- First $50,000 of effort by mechanical engineers (500 hours) or mechanical technicians (667 hours) fully subsidized by the university (no cost to DOE or NSF)
- 1 electronics engineer fully supported (1840 hours) by a DOE grant (base funding)
- Additional engineering effort charged to specific projects at $100/hr; additional technical support charged at $75/hr

| | Hourly cost for a project requiring an annual expenditure of effort equal to | | |
|---|---|---|---|
| | **250 person-hrs** | **1000 person-hrs** | **3000 person-hrs** |
| Mechanical engineers | $0 | $50 | $83 |
| Electronics engineers | 100 | 100 | 100 |
| Mechanical technicians | 0 | 25 | 58 |
| Electronics technicians | 75 | 75 | 75 |

Note that the cost of electronics engineering is $100/hr, regardless of whether support comes from base funding or a specific project. For mechanical engineers and mechanical technicians, the hours costs vary with the size of the project, owing to the university subsidy. For example, the hourly cost for a 3000-hour effort by mechanical engineers is

$$(3000 - 500) \times \$100 / 3000 = \$83$$

*Questions 3 and 4 follow*

3. *Current Engineering and Technical Capabilities and Costs*

- **Briefly summarize the most important technical capabilities and facilities at your institution.**

  ______________________________________________

  ______________________________________________

  ______________________________________________

  ______________________________________________

- **Briefly describe the high-energy physics equipment now being constructed or assembled at your institution. How is the engineering and technical effort being paid for?**

  ______________________________________________

- **What are the most significant high-energy physics construction or assembly projects your institution has completed in the past five years? Do you still have the capabilities to undertake such tasks?**

  ______________________________________________

  ______________________________________________

- **What are the <u>approximate</u> fully burdened hourly costs to DOE or NSF for high-energy physics jobs undertaken by engineers or technicians at your institution? For each box, assume a single job, to be completed within one year by the indicated engineers or technicians. If such a job is too large for your institution, so indicate with an "x" in the corresponding box.**

| | Hourly cost for a project requiring an annual expenditure of effort equal to | | |
|---|---|---|---|
| | 250 person-hrs | 1000 person-hrs | 3000 person-hrs |
| Mechanical engineers | | | |
| Electronics engineers | | | |
| Mechanical technicians | | | |
| Electronics technicians | | | |

*4. Demographics*

- Please indicate the **number of high-energy physics graduate students** currently enrolled at your institution (regardless of source of support), by current year of study.

| | No. of grad students | |
|---|---|---|
| | **Experiment** | **Theory** |
| 3rd year | | |
| 4th year | | |
| 5th year | | |
| 6th year and above | | |

- How many students received **Ph.D.'s in high-energy physics** last year? How many to you expect to receive them this year?

| | No. of Ph.D.'s awarded | |
|---|---|---|
| | **Experiment** | **Theory** |
| Last year | | |
| This year (est) | | |

- Indicate your general impression of **student interest in high-energy physics**, as compared with five and ten years ago.

**Compared with five years ago:**

- ○ Much higher
- ○ Somewhat higher
- ○ About the same
- ○ Somewhat lower
- ○ Much lower

**Compared with ten years ago:**

- ○ Much higher
- ○ Somewhat higher
- ○ About the same
- ○ Somewhat lower
- ○ Much lower

- How many **new, full-time, tenured and tenure-track high-energy physics faculty** do you expect (or guess) your institution will hire over the next three years? Include new hires to replace retiring faculty or faculty not granted tenure, and assume a constant level of DOE/NSF support (in FY97 dollars).

| | **No. of projected new hires** |
|---|---|
| Theoretical physicists | |
| Experimental physicists | |
| Accelerator physicists | |

- Indicate the **additional high-energy physics staff needs** at your institution by assigning a priority order (1 highest, 7 lowest) to the following choices. Assume that additional funding would be available to support your staff choices.

| | **Priority** |
|---|---|
| Two postdoctoral fellows | |
| One mechanical engineer | |
| One electronics engineer | |
| One software systems engineer | |
| One postdoc and one technician | |
| Two technicians | |
| Four graduate students | |

---

Please return this questionnaire to

Douglas Vaughan
Lawrence Berkeley National Laboratory
Building 50A-4119
1 Cyclotron Road
Berkeley, California 94720

# APPENDIX F: High-Energy Physics Programs in Europe and Japan

## A. High-Energy Physics in Europe

Particle physics experiments in Europe are carried out mainly at two laboratories, CERN and DESY, where high-energy accelerators are in operation. Approximately 84% of the community of experimental particle physicists in Europe is involved in these activities. The remaining 16% takes part primarily in experiments which do not use particle accelerators.

CERN, the European Laboratory for Particle Physics, was created in 1954 by a group of countries in Western Europe. It is located near Geneva, Switzerland. There are presently nineteen CERN Member States with a population of 444 million people. Their contribution to the CERN budget is proportional to their respective GNP. The 1997 CERN budget is 870.1 M Swiss francs, which is about $600 million at today's exchange rate. This budget includes the salaries of 2875 staff members, plus 178 fellows and 182 paid associates. For comparison, the CERN budget in 1988 was 787.9 M Swiss francs, corresponding to a buying power 8% higher than today.

CERN operates a number of accelerators: the PS, a 28-GeV proton synchrotron which is mainly used today as an injector to the higher energy machines; the SPS, a 450-GeV proton synchrotron that was also used as a proton-antiproton collider between 1981 and 1990 and can also accelerate heavy ions; and LEP, an electron-positron circular collider with a circumference of 27 km that began operation in 1989 and has reached a center-of-mass energy of 184 GeV.

LEP will be shut down at the end of the year 2000 and will be replaced by the LHC (Large Hadron Collider), a superconducting proton-proton collider with a total center-of-mass energy of 14 TeV. The LHC is expected to start operation in the year 2005.

The total number of CERN users on December 31, 1996, was 6895 physicists, coming from 308 institutions in the CERN Member States and 213 institutions in other countries. The number of U.S. physicists who were registered as CERN users at that date was 600.

In order to obtain the funding required by the construction of new facilities without increasing the yearly budget, CERN has shut down existing facilities and has been authorized to borrow money to cover peaks in expenditure. This was the case for LEP construction, which required closing down the Intersecting Storage Rings, the first proton-proton collider ever built. The same procedure will be applied for LHC construction. LEAR, the low-energy antiproton storage ring, has already been shut down, and no new fixed-target experiments at the SPS will be approved unless funded by outside sources.

DESY is the German National Laboratory for high-energy physics. It has two sites, the main one in Hamburg and a smaller one in Zeuthen near Berlin. The 1997 budget of DESY-Hamburg is 250 M German Marks (about $144 million at today's exchange rate). This budget includes the salaries of 1050 staff members. DESY-Zeuthen has a budget of 25 million German marks and a staff of 135.

The main accelerator presently in operation at DESY-Hamburg is HERA, the only existing collider that can collide electrons or positrons with protons. In HERA, a 29-GeV electron (or positron) beam collides with an 800-GeV proton beam, these collisions are studied in two large detectors.

Other experiments at DESY study the collisions of polarized electrons with a fixed, polarized proton target and B physics using the B particles produced by the collisions of 800-GeV protons against a fixed target.

Another important facility in Europe is the Gran Sasso National Laboratory, 150 km east of Rome, Italy, with three very large underground halls where non-accelerator experiments are carried out.

It is not easy to obtain a unified picture of funding and resources for high-energy physics in Europe, because of the many countries involved and their different administrative structures and budget definitions. However, some useful information is available through ECFA, the European Committee for Future Accelerators, which is the forum of European particle physicists in the CERN Member States discuss future accelerators, the use of existing facilities, and the resources required to support experiments.

Over the last decade, ECFA has made two surveys, a very complete one in 1988 and one in 1995 that was limited to a survey of the particle physics community.

The last survey of domestic expenditures for high-energy physics was made in 1988. Four categories of expenditure have been used: (I) equipment, (ii) recurrent expenditures within the Institutes (e.g. computer maintenance, supplies, etc.); (iii) costs of running experiments, and (iv) travel and subsistence. It is important to note that salaries, overhead costs, and technical and administrative support are not included in the 1988 ECFA survey.

The survey found that the sum of the four categories of domestic expenditures listed above for the years 1985-88 was, on average, 26% of the CERN budget, varying between 10% in the five smallest countries and 29% in the four largest ones. Under the reasonable assumption that this ratio has not changed over the last ten years (it had not appreciably changed over the previous ten years), we obtain an estimate of 226 million Swiss francs (about U.S. $160 million) per year for university funding in the CERN Member States, excluding salaries.

According to the latest ECFA survey, in 1995 there were 2775 experimental particle physicists in the CERN Member States, 20% more than the corresponding number in 1988. Of these, 76% had tenured positions. In addition, there were also 1208 Ph.D. students, 55%

more than in 1988. The support personnel for this community in 1995 consisted of 816 engineers and 1470 technicians, or 0.82 technical support persons per physicist. We note that 9% of this community is involved in experiments with heavy ion beams, which in the U.S. are supported by separate funds.

The community of theoretical particle physicists in the CERN Member States in 1995 amounted to 1436 (74% with tenure), 47% more than in 1988. In addition, the number of Ph.D. students was 704, 44% more than in 1988.

B. High-Energy Physics in Japan

The Japanese high-energy physics community comprises about 280 experimentalists and 260 theorists. As Table 1 shows, almost all accelerator physicists, engineers, and technicians are at the national high-energy physics laboratory, KEK, where most accelerator-based particle physics experiments are carried out. Japanese experimentalists also work abroad, at accelerator laboratories in Europe and the U.S., and carry out non-accelerator-based research.

Basically all Japanese university positions are permanent: the most junior position is instructor, then associate professor, followed by professor. Until recently, the ratio among the three ranks was kept to 2:1:1.

A new policy has been adapted recently by Monbusho (the Ministry of Education, Science, and Culture) and the major national universities to strengthen graduate education. In implementing this policy, instructorships were traded in to create new professors. Monbusho is even proposing to make the instructorship non-permanent. As an outcome of this policy, some physics departments have almost no stable posts for new Ph.D.'s while the number of graduate students. In a few years, Japanese academia will begin to suffer from a shortage of young talented scientists to replace retiring professors.

The main source of funds for basic research in Japan is Monbusho. The largest grants are awarded directly by Monbusho, the second largest are Grants-in-Aid, followed by fellowships and international exchange funds provided by the Japanese Society for the Promotion of Science (JSPS).

Table 2 shows funds directly provided by Monbusho. Per capita funds are allotted to professors in the Japanese national laboratories and national universities. Monbusho does not grant these funds to professors in private universities, but most private universities provide similar funds. The table does not include salaries, health-care, social security, etc., which amount to about 25% of the research funds for KEK, more than 50% for university laboratories, and nearly 300% for university groups. Civil construction costs for university-operated laboratories are also excluded.

Researchers from all fields of science compete for Grants-in-Aid, totaling 110 billion yen/year or nearly $1 billion/year. No quota is reserved to any branch of physics nor to any field of science; research related to high-energy physics receives roughly 1%. The grants range from small one-year grants for young scientists to five-year grants at about $3 million/year. When a new interesting topic appears in an interdisciplinary area, e.g., in non-accelerator particle physics, many new proposals are submitted from several fields, resulting in many more grants in the area. Depending on the total amount requested, proposals are reviewed by 3 to 20 peers, and on average about 20-25% are funded.

In addition to direct grants, Monbusho also awards fellowships and grants to international programs through the JSPS. Pre-doctoral fellowships (approximately 170 thousand yen/month) are typically given to about 20% of third- and fourth-year graduate students on a competitive basis. JSPS post-doctoral fellowships (approximately 270 thousand yen/month) now support a good fraction of new Ph.D.'s, but the maximum term of appointment is strictly limited to three years. Predoctoral and postdoctoral fellowships are distributed more or less evenly over major laboratories and universities. The total JSPS funding of HEP-related activities is roughly 500 million yen (over $4 million) per year.

Most domestic accelerator projects and experiments are financially supported through KEK, while most non-accelerator experiments are supported through the Institute of Cosmic Ray Research (ICRR) or through various Grants-in-Aid. A rough estimate of the total funds the Japanese high-energy physics community receives from the various sources is summarized below. Note that the salaries, benefits, etc., for permanent staff are *not* included.

- Accelerator-based experiments and accelerator R&D:

  about 16 billion yen/year for Japanese projects plus 2 billion yen/year for non-Japanese projects about 30 Postdoctoral and Pregraduate fellowships (90 million yen/year) plus 60 million yen/year for travel

- Non-accelerator experiments:

  about 1.5 billion yen/year for Japanese projects plus 20 million yen/year for non-Japanese projects about 30 Postdoctoral & Pregraduate fellowships (90 million yen/year) plus 30 million yen/year for travel

- Theory:

  about 300 million yen/year (3 billion yen if Supercomputer support is included.)
  about 80 Postdoctoral and Pregraduate fellowships (240 million yen/year) plus 20 million yen/year for travel

- Total funds for high-energy physics (not including salaries and benefits for staff)

  about 20 billion yen/year
  about 90% to accelerator experiments and accelerator R&D
  about 10% to non-accelerator experiments

The largest uncertainty facing the present Japanese high-energy physics community is the schedule for completion of the Japan Hadron Facility. Construction of a linear collider would presumably not begin until that time. Another concern is the flat 15% reduction announced for JFY 1998 in all operation budgets. This will hit the community, including KEK, quite hard.

**Table 1: Distribution of Physicists and Engineering/Technical Staff in Japanese High-Energy Physics Research Institutions**

| Institutes | Experiment | Accelerator | Theory | Eng./Tech |
|---|---|---|---|---|
| KEK and INS | 95 | 137 | 15 | 160 |
| Tohoku University | 10 | | 11 | |
| Hokkaido University | | | 5 | |
| Niigata University | 3 | | 6 | |
| Tsukuba University | 14 | | 12 | |
| U. of Tokyo (Hongo Campus) | 18 | 4 | 6 | 1 |
| U. of Tokyo (Komaba Campus) | | | 5 | |
| U. of Tokyo (ICRR) | 25 | | 3 | 12 |
| Tokyo Inst. of Tech. | 4 | | 3 | |
| Waseda University | 2 | | 2 | |
| Toho University | 3 | | | |
| Tokyo Metro University | 7 | | 4 | |
| Nagoya University | 14 | | 7 | |
| Nara Women's University | 4 | | 3 | |
| Kyoto Univ. (Dept. Phys.) | 7 | | 5 | |
| Kyoto U. (Yoshida Campus) | | | 5 | |
| Kyoto Univ. (Yukawa Inst.) | | | 9 | 2 |
| Osaka University | 4 | | 8 | |
| Osaka City University | 5 | | 4 | |
| Kobe University | 7 | | 4 | 1 |
| Hiroshima University | 7 | 1 | 5 | |
| Kyushu University | | | 3 | |
| Saga University | 3 | | 6 | |
| Other Universities | ~ 50 | ~ 5 | ~ 120 | ~ 5 |
| Total | ~ 280 | ~ 140 | ~ 260 | ~ 180 |

**Table 2: Funding by Monbusho to Particle and Cosmic-Ray Physics**

| Type of Funding | Details /Explanation | Annual Budget |
|---|---|---|
| Direct to Laboratories | | |
| National Labs. (KEK)[1] | Inst. Part. Nucl. Study | ~ 16 billion yen |
| | Accl. Research Center | (incl. in the above) |
| | Computer | (2.7 billion yen incl. above) |
| Inter-Univ. Labs. [2] | ICRR (U. of Tokyo) | ~ 0.7 billion yen |
| Inter-Univ. Centers [3] | ICEPP (U. of Tokyo) | ~ 110 million yen |
| Univ. Research Centers [3] | Bubble Ch.P L (Tohoku U.) | ~ 233 million yen |
| Int'l Coll. Projects [4] | | |
| U.S.-Japan | Exps. at DOE Labs. | ~ 1.3 billion yen |
| CERN-LEP2 | Through ICEPP | <100 million yen |
| CERN-LHC | Through KEK | ~ 5-7 billion yen total |
| CERN-ATLAS | Through KEK | ~ 3-5 billion yen total |
| DESY-ZEUS | Through KEK | 60 million yen |
| CERN-pbar | Grant-in-Aid to U. of Tokyo | ~ 2 billion yen total |
| Per capita Funds to Professors [4] | Each prof. in lab/univ. | ~ 2 Myen/prof. |
| Grants-in-Aid | Total to HEP | ~ 0.5-0.7 billion yen |

(1) JFY 1997; Division among Institutes and centers is often arbitrary.
(2) JFY 1994 Super-K Civil Construction not included.
(3) JFY 1994.
(4) Overhead varies among universities/laboratories.