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HEPAP White Paper on

Planning for U.S. High-Energy Physics

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Division of High Energy Physics
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Dear Millie and Bob,

With this letter I am transmitting the High Energy Physics Advisory Panel's White Paper on Planning for U.S. High-Energy Physics, which was unanimously endorsed at the HEPAP meeting on October 30–31, 2000.

The White Paper takes the plan for the field presented by the 1998 Subpanel and updates it to provide an assessment of where we stand in a worldwide context. It calls for operation of the recently completed facilities at a level that will take advantage of their potential for a truly major breakthrough in the near-term. It also calls for increased support now for research and development of accelerator technology for an energy frontier facility that will allow the U.S. to remain a leader in the field over the long-term. Finally, it serves as a step in a long range planning process that will involve a new HEPAP subpanel and evaluation and input from the high-energy physics community in 2001.

Sincerely,

Fred Gilman

Frederick J. Gilman
Chair, HEPAP

EXECUTIVE SUMMARY

High-energy physicists seek to understand what the universe is made of, how it works, and where it has come from. They investigate 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 connection 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.

The 1998 Subpanel of the High Energy Physics Advisory Panel (HEPAP) laid out a strategy for U.S. high-energy physics for the next decade. That strategy balanced exciting near-term opportunities with preparations for the most important discovery possibilities in the longer-term. Difficult choices were made to end several highly productive programs and to reduce others. This year HEPAP was charged to take the plan given in the Subpanel's report, understand it in the context of worldwide progress, and update it. In response to that charge, this White Paper provides an assessment of where we stand, states the next steps to take in the intermediate term, and serves as input for a longer range planning process involving a new HEPAP subpanel and high-energy physics community evaluation in 2001.

Since the 1998 Subpanel, there have been important developments and a number of the Subpanel's recommendations have been implemented. Notably, construction of the B-factory at SLAC, the Main Injector at Fermilab, and the upgrade of CESR at Cornell have all been finished on schedule and on budget. We have gained great confidence in the performance of these accelerators and the associated detectors. The B-factory at SLAC is already operating above design luminosity and plans are in place to reach three times the design in the next few years. In addition, there have been major physics developments that lead us to believe that these completed projects are guaranteed to produce frontier physics results and have an enhanced potential for a truly major breakthrough. However, taking advantage of these facilities requires greater funding for operations than the significantly reduced level of the last several years.

- *The shortfall of funds for operating the recently completed facilities will severely hamper their utilization. The 1998 Subpanel's recommendation on optimum utilization of these facilities through funding their operations and supporting the groups extracting physics from them is reaffirmed as the highest priority need.*

Research at the energy frontier is essential for the sustained excellence of the U.S. program. The energy frontier, resident at Fermilab since 1985, will move to Europe in the middle of this decade with the completion of the Large Hadron Collider (LHC). Because of their scope in terms of both human and financial resources, future energy frontier facilities will need to be international in character in both their R&D and construction phases, with strong collaboration between the host and non-host regions.

Progress in particle physics has been driven by advances in accelerator technologies. A sustained accelerator R&D program has become ever more important with the long lead times for a future facility. It is essential to refine the new technologies employed and to drive the costs down. New accelerator technology has also been one of the main societal benefits from particle physics. The current expenditure on accelerator R&D for energy frontier facilities is inadequate to ensure a long-term future for the field, and the loss of this expertise and knowledge base will also be a loss for society.

• *Accelerator R&D is the lifeblood of our science, creating the tools that are needed to explore the physics of matter, space, and time. Current funding levels for R&D toward new accelerators are endangering the near and far term future of the field, and should be increased substantially*.

The possible options for a future energy frontier facility, identified by the 1998 Subpanel and the 1998 National Research Council decadal survey of elementary particle physics, are an electron-positron linear collider, a muon collider or a very large hadron collider. The 1998 Subpanel recommended appropriate levels of R&D for each of these possibilities, and significant progress has been made since then. The European and Asian communities are also conducting intensive R&D in support of long range planning for future accelerators and have identified similar possibilities. Driven by the recent evidence for neutrino masses, there has been a significant change in the R&D program for a muon collider, with the focus now being on a muon storage ring/neutrino source.

The timeline for decision points on these major facilities stretches over two decades or more. We expect that only one of each type of frontier facility will be built worldwide, and that they will be distributed in different regions. Work toward proposals for a 500 GeV scale electron-positron collider is well advanced in each of the three major scientific regions worldwide, with each region being a potential host for such a facility.

• *The study of the fundamental issues bearing on the nature of matter at the smallest scale, and the forces at work in shaping the universe, befit this nation. The U.S. should remain a leader in high energy physics. Maintaining the U.S. leadership and training new generations of scientists in this field demand an energy frontier facility at home.*

• *High energy colliders in addition to the LHC will be needed to understand the new physics now indicated from current experiments. There is a worldwide research and development effort for such energy frontier facilities, with a decision point on construction of an electron-positron collider coming in the next several years.*

I. Introduction

Experiments of the past decade at accelerators have resulted in a remarkable improvement in our understanding of the nature of the fundamental forces and the constituents of matter. The full set of particles expected to comprise matter in the Standard Model paradigm have now been observed. The rich pattern of masses of these particles has been delineated, and the agent for the generation of mass is now confidently predicted to be observable in near-future experiments. The case has been clearly made that new laws of physics associated with new fundamental particles or a new character of space-time should appear, to resolve flaws in the present picture. The potential for discovery of this new physics gives exceptional promise to the next stages of experimentation at high energy colliders.

Non-accelerator experiments also provide understanding of the fundamental particles and of the universe itself. In some of these experiments, the particles observed have come from the furthest reaches of the cosmos. Non-accelerator experiments often complement those at accelerators. Both look back to the earliest moments of the universe after the Big Bang. Those involving astroparticle studies see remnants from an earlier era, while those at accelerators recreate directly the conditions of the Big Bang in a controlled environment.

A companion document to this report has been prepared, intended as a briefing book for use in describing the long-range vision for High Energy Physics. That report, *"Interactions: the Physics of Matter, Space and Time"* gives the basic themes of research in the field, the major questions that guide its evolution over the next several decades, and some indications of the steps that need to be taken along the way to their achievement.

The 1998 HEPAP Subpanel on "Planning for the Future of U.S. High-Energy Physics" laid out a strategy for the next decade that balanced near-term scientific opportunities with preparations for the most important discovery possibilities for the long-term. In developing the 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.

It is worthwhile to recall both the primary recommendations of the 1998 report, and the context in which they were drawn. Following the termination of the SSC project in 1993, and to help guide the U.S. high energy physics community in recovering from the lost initiative, the 1994 Drell HEPAP Subpanel considered the future direction of the program. Its first recommendation was:

> *"As befitting a great nation with a rich and successful history of leadership in science and technology, the United States should continue to be among the leaders in the worldwide pursuit of the answers to fundamental questions of particle physics."*

Based on the conclusion that research at the energy frontier is an essential element of sustained excellence of the U.S. high-energy physics program, the Drell Subpanel went on in its second and third recommendations to advocate U.S. collaborative participation in the CERN Large Hadron Collider (LHC). Enhanced effort in accelerator R&D was recommended to prepare for future frontier accelerators in the U.S.

In 1998, the decadal study by the National Research Council Committee on Elementary Particle Physics surveyed the field, and made its first recommendations on accelerator-based experimentation at the energy frontier. The preamble to this set of recommendations began:

"At the present time, the Tevatron at Fermilab and the Large Electron-Positron collider (LEP II) in Geneva are the only machines operating at the energy frontier. In two years, LEP II will be dismantled, leaving the Tevatron alone at this frontier until completion of the LHC in the middle of the next decade. The LHC will dramatically extend the energy reach, pushing beyond the TeV scale, where we know that the physics of electroweak symmetry breaking must appear. However, this report concludes that in the future, another collider will be required to complement or extend the range of the LHC and to explore fully the physics of the TeV scale .."

The 1998 Gilman subpanel, convened after a strong U.S. commitment to the LHC was made and during the period that several new U.S. facilities were being completed, offered three primary recommendations:

> *"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 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."*

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

To make room for accomplishing these three major recommendations, the Subpanel advocated that significant reductions in highly productive activities of other sectors of the program be made. The 1998 report recommended that the almost forty-year old AGS fixed-target program at BNL be concluded, apart from one or two possible high priority experiments; this has now been accomplished. The 800 GeV fixed target program at Fermilab was also designated to end and the last such run took place in 1999. Finally, the SLC accelerator and SLD detector at SLAC were stopped after the 1998 run, in accord with the Subpanel's recommendation. In addition, a number of other experiments at each of the operating laboratories that had less than top priority had to be postponed or simply turned down because of the budget restrictions which the field faces.

The primary recommendations of the NRC and Gilman Subpanel reports on future directions were in very close agreement. Both reports identified three possible future accelerators on the worldwide scene that could extend the energy frontier beyond the LHC: a linear electron-positron collider operating at the TeV scale, a muon collider at several TeV, and a very large hadron collider in the 100 TeV range.

Since the unanimous adoption of the Gilman Subpanel's report by HEPAP and its submission to the Department of Energy, more than two years have elapsed and many of the Subpanel's recommendations have been implemented. Notable developments include:

1. Construction of the B-factory at SLAC, the Main Injector at Fermilab, and the upgrade of CESR at Cornell have all been finished on schedule and on budget. This exceptional record in successful management of high-energy physics projects by both DOE/SC and NSF is a strong source of pride for the agencies and the field.

2. There have been important physics discoveries from the ongoing program, including evidence that neutrinos have mass, direct observation of the tau neutrino, and the demonstration that there is a matter-antimatter asymmetry in the decays of K mesons.

3. Additional research programs were stopped or severely curtailed to make room for higher priority efforts in accord with the Subpanel's recommendations.

4. Significant progress has been made on research and development work for a future facility at the energy frontier.

We have also had budgets for two fiscal years for which the increase in DOE funding for high-energy physics averages 1.6% per year. This is below the constant-level-of-effort central scenario considered by the Subpanel, especially since the market-driven rate of increase in salaries of the technical personnel in the laboratories is well above the overall U.S. inflation rate.

As requested by the acting Director of the Office of Science, Jim Decker, in his charge letter to HEPAP (Appendix A), it is therefore most appropriate to take the plan given in the Subpanel's report, understand it in the context of worldwide progress, and update it. This report will provide an assessment of where we stand, state the next steps to take in the intermediate term, and serve as input for the longer range planning process of the next HEPAP Subpanel envisioned in the charge letter. In this regard, we view the joint sponsorship of HEPAP by both DOE and NSF as a very positive factor.

The high-energy community was informed of this planning process and their input requested through a message distributed by the Division of Particles and Fields (Appendix B). Input from the community at large was obtained through e-mail messages and letters, in sessions at the Fermilab and SLAC Users' meetings (Appendix C and D), and a Town Meeting at the Division of Particles and Fields Conference at Columbus (Appendix F). The "Writing Group" that was organized to draft the White Paper requested presentations on specific issues in its meetings at UCLA and Columbus (Appendices E and G). This large body of input material forms the consistent picture, within a world context, of the present and future of the U.S. high-energy physics program that follows. A review of the implementation of other recommendations of the 1998 Subpanel is contained in Appendix H.

The second part of the June 2000 charge to HEPAP outlined a process for more comprehensive evaluation of long-range future planning. This stage would be based on a broad discussion of physics priorities by the U.S. and international high energy physics community, and the institution of a new subpanel in 2001 reporting to both DOE and NSF. That subpanel will be informed by this White Paper and the community discussion during a three-week workshop in Snowmass in summer 2001.

II. High Energy Colliding Beam Accelerators

Particle accelerators are the "microscopes" that allow us to answer the basic questions regarding the structure of matter, and have been the primary source of the remarkable progress in our understanding of the fundamental constituents and forces. Seeing the structure of matter at increasingly finer resolution requires accelerator beams of higher energy. This leads to the seemingly paradoxical situation that our study of the smallest and most basic constituents of matter requires building some of the largest and most complex scientific instruments ever built.

The Tevatron collider at Fermilab is presently the highest energy accelerator in the world, and will remain so until the Large Hadron Collider (LHC) at CERN begins operation at mid-decade. With the Main Injector project now completed, the total amount of data collected at the Tevatron proton-antiproton collider should increase by a factor of 20 to 40 over the next 2 to 3 years. There are prospects for further increases of a factor of 5 to 10 prior to the LHC operation; these will be crucial for discovery of some portions of the new physics now expected.

The highest energy electron-positron colliders, SLC at SLAC and LEP at CERN, are concluding very successful programs. These colliders have lower beam energies than the Tevatron, but the masses of the particles they can create are nearly as large. The experiments at these machines have provided a wealth of information on the forces and particles that make up the Standard Model of fundamental particles. Electron-positron collisions, with their very precisely known properties, and with relatively small backgrounds, offer a very clean environment for precision measurements. The SLC and LEP data have led to a remarkable confirmation of the validity of the current model. A combination of electron-positron and hadron collider information has been essential for delineating the picture of particle interactions; together they now predict that major new discoveries related to the origin of particle masses should be made in the near-term future.

This complementarity of lepton and hadron colliders has characterized the field for decades.

The recently completed B-factories at SLAC and the KEK Laboratory in Japan, as well as the upgraded CESR machine at Cornell, are also electron-positron colliders. They operate with very high intensities, but at lower energies chosen to probe the differences in the behavior of matter and antimatter with great precision. The operating and planned neutrino beams from high intensity accelerators will take us further in understanding the puzzle of neutrino mass, and the connections among the three known species of neutrinos. Such facilities demonstrate the continued importance for conducting precision experiments at well-chosen energies below the energy frontier.

The LHC will begin operation at mid-decade. With a collision energy of 14 TeV, it is expected to have nearly certain opportunity to uncover evidence of what is responsible for the origin of mass. If the means by which this occurs includes supersymmetry, the LHC will surely discover some of the supersymmetric partners of the known particles and should delineate the main features of the supersymmetric world. If Nature has made other choices for the new physics, the LHC should see evidence for these as well. Thus, the LHC should fulfill long- standing goals set out for the next step at the energy frontier.

The LHC, however, will likely leave crucial aspects of our understanding of the new physics unexplored. It seems certain that we will need new accelerators besides the LHC to understand the physics discoveries of the near-term program. The world high-energy physics community has identified such potential accelerators as: a linear electron-positron collider in the TeV range; a muon storage ring serving as an intense source of neutrinos; a very large hadron collider in the 100 TeV range, and a multi-TeV collider employing electrons or muons.

III. Operation of the Recently Completed U.S. Facilities

The first element of the charge specific to the White Paper is a request to *"examine the issues of the discovery potential and optimum utilization of the facilities that have now been completed and upon which the Subpanel placed its highest priority"*. These facilities are the Main Injector to the Tevatron collider, the SLAC B-factory, and the CESR upgrade. Since the 1998 Subpanel report, all these projects have been completed on time and on budget.

However, funding for operating our existing facilities has eroded continually over the past decade, as discussed in Appendix I. Construction funding for the B-factory and the Main Injector came from reducing the equipment and operations funding of the field. Taking advantage of these facilities requires funding for facilities operations that is greater than the reduced level of the last few years. This lack of funds to adequately operate the new facilities is the most serious present problem for the field.

Fermilab is in the midst of final preparations for beginning the physics run of the Tevatron collider using the Main Injector. Commissioning of the chain of accelerators, including the Main Injector and that of the Tevatron collider ring itself, is finished and an engineering run is underway. The upgraded CDF and DØ detectors are now being commisioned, and are both on track to be complete for the scheduled start on March 1, 2001.

The SLAC B-factory is off to a spectacular start. After operation for little more than a year, the peak luminosity is above the design value, as is the integrated luminosity per day on a consistent basis. The BaBar detector collaboration worked intensely to produce exciting first physics results at the International Conference in Osaka at the end of July. In many cases the results were already competitive in accuracy with the compilation of all the previous data up to now. With the end of the current run in October, they should have enough data to make an incisive measurement of CP violation (the difference between the behavior of matter and antimatter) for B mesons.

The upgrade of the electron-positron collider, CESR, and the associated detector, CLEO, is complete and also in commissioning mode. Data from an engineering run have been fully processed, and with the detector ready, the central effort now is in bringing the collider up to design luminosity.

With the passage of a few years since the Subpanel report, we have a better perspective on the physics potential of these facilities. If the Standard Model is correct, the implication of the precision data collected by LEP, SLC, and the Tevatron collider is that the Higgs particle has a comparatively low mass (less than 170 GeV at 95% confidence level). The present lower limit on the mass from direct searches at LEP is 113 GeV/c^2, eliminating the lower half of the preferred mass region. No evidence for supersymmetric particles has been found; LEP and the Tevatron have eliminated significant regions of parameter space. In the meantime, there has been further theoretical analysis and considerable sharpening of the experimental tools for finding a Higgs particle at the Tevatron. With the luminosity upgrades now planned at Fermilab, it will be possible to search for the Higgs particle with masses up to the current predicted limit, and to significantly extend the search for new physics beyond the Standard Model. The physics case remains clear for the forthcoming Tevatron run, and is more compelling than ever.

The last few years have also seen a great deal of theoretical effort in understanding multiple approaches for deducing the underlying physics in measurements of matter versus antimatter asymmetries for B mesons. In addition, the now proven performance of the BaBar detector at SLAC, the CLEO detector at CESR, and the Belle detector at KEK demonstrate that they can make the measurements for which they were designed. Consequently, the case for gaining crucial information on the difference between the behavior of matter and antimatter from the B-factories has gained in strength. Plans are already in place to triple the luminosity of the SLAC B-factory by the end of 2002. A further increase in luminosity by another factor of three is contemplated by mid-decade. A discussion of future options for CESR is underway at Cornell. Fermilab plans a new collider experiment, BTEV, in the latter part of the decade to study matter-antimatter asymmetries and rare decays in the B system, which will extend the study to states beyond those accessible at the B-factories, and will compete with the LHC-b experiment at the LHC.

The new long-baseline neutrino beam at Fermilab will provide the MINOS experiment the means to verify the indication for neutrino mass and mixing of the neutrino types recently seen in the underground detectors. The MiniBooNE experiment to confirm or reject the recent indication of neutrino oscillations in a Los Alamos experiment is essential for determining the number of neutrino species.

New experiments to study very rare decays of the K meson, and to explore the possible conversion of muons into electrons will provide new understanding of the mystery of the quark and lepton flavors. NSF is considering support for some of these experiments.

Therefore, with the benefit of increased confidence in the performance of the accelerators and detectors and enhanced discovery potential, there is a guarantee of a flow of frontier physics results and an increased likelihood of truly major discoveries at the Tevatron collider, SLAC B-factory, the CESR collider, or the Brookhaven AGS.

- *The shortfall of funds for operating the recently completed facilities will severely hamper their utilization. The 1998 Subpanel's recommendation on optimum utilization of these facilities through funding their operations and supporting the groups extracting physics from them is reaffirmed as the highest priority need.*

IV. The University High-Energy Physics Program

An important component of utilizing the existing facilities is a strong university physics program. The university-based high-energy physics program was the object of special attention by the 1998 Subpanel, and a set of recommendations was aimed at its improvement. The most significant of those recommendations in light of the erosion of funding of the previous five years was that "the annual DOE-operating funds for the university program be ramped up by a total of 10% above inflation" over a two-year period. The DOE has tried to follow the recommendation of the Subpanel, and there has been an increase in the program above inflation over the last couple of years. Given tight budgets, this has been below what the Subpanel recommended. *The core problems of university-based research remain, and there is need for continuing effort to achieve the full recommended increase.*

V. Research and Development for Major Future Facilities

Progress in particle physics research has been driven by advances in accelerator technologies. Starting with E. O. Lawrence's invention of the cyclotron, the progress of the field has been tied to a succession of increasingly higher energy particle accelerators that allowed us to probe deeper and deeper into the subatomic world. As one set of questions was answered and the structure of matter at that level understood, other questions arose that required looking at smaller distances and yet higher energies. By using a succession of technologies, accelerator physicists have been able to move the energy frontier upward decade after decade and have given us the tools to answer the next set of questions. At the same time, the cost per unit of collision energy decreased by a factor of about ten thousand.

A sustained accelerator R&D effort has become even more important as the lead times for any of the future frontier facilities are now up to 20 years. In addition, as the total cost of major facilities has risen, accelerator R&D has become essential in bringing the costs down. Money spent on R&D before a project starts can save many times that investment down the line in component costs. The current expenditure on accelerator R&D for energy frontier facilities is inadequate to ensure a long-term future for the field.

There is a further motivation for accelerator R&D support. Such research has had an enormous impact on many other fields of science and technology. For example, synchrotron light sources that evolved from the colliding-beam storage rings developed for high-energy physics are now essential instruments for material, biological, chemical, and environmental sciences. The specific impact of such facilities on structural biology has been great enough that the National Institute of Health is contributing more than half of the $58M needed to upgrade the synchrotron facility at SLAC. New technical breakthroughs in linear collider R&D have made it possible to design the next generation of these facilities, linear coherent light sources. These will make it possible to study the time evolution of chemical and molecular processes. The development of superconducting magnets for the Tevatron has created the industrial capacity for large-scale superconductor fabrication. Accelerators have also been introduced into hospitals for non-surgical cancer treatment, and in the U.S., one hundred thousand patients are treated daily with electron linear accelerators. Accelerators are also used for characterizing materials defects and for microlithography of integrated circuits.

Looking to the long-term future, the 1998 Subpanel recommended that *"a new facility at the energy frontier be an integral part of the long-term national program."* As already noted, the energy frontier, resident at Fermilab since 1985, will move to Europe in the middle of this decade with the completion of the LHC. With a proton-proton collision energy that is seven times that of the proton-antiproton Tevatron collider, a new realm of energies will be opened up at the LHC. This will allow us to point to the specific mechanism that gives matter the property of mass, and to discover the new physics expected in this regime. It is now time to consider the steps in addition to the LHC.

Corresponding to this, the second and third elements of the charge to HEPAP for the White Paper, asked for it to *"(2) identify the major scientific issues confronting high-energy physics worldwide, and outline a timeline for R&D, design, and possible decision points on the future frontier facilities that will be capable of addressing those scientific issues; and (3) indicate the appropriate next steps for each of these facilities."*

The major scientific issues confronting high-energy physics are described in the companion document to this report, *"Interactions: the Physics of Matter, Space and Time,"* and the 1998 Subpanel Report. As potential future frontier facilities, the 1998 Subpanel and the NRC Decadal Survey identified an electron-positron linear collider that is complementary in physics reach to the LHC, and a muon collider and Very Large Hadron Collider (VLHC) that would probe yet higher energy scales. The Subpanel made specific recommendations on R&D for each of these possible machines.

It is important to note that the European and Asian high energy physics communities are also conducting intensive R&D in support of long range planning activities for future accelerators. They have identified similar possibilities for future projects as in the U.S. The worldwide community, and each region separately, are presently engaged in setting the priorities for these facilities. Although the list of potential new accelerators is similar in all regions, we expect that at most one of each type of energy frontier facility would be built. Further, a balance in siting new accelerators worldwide is healthy for the field. As the Drell Subpanel affirmed, for the U.S. to remain a leader in high-energy physics, one of these facilities should be sited in the U.S. While R&D should be shared across the regions according to their particular expertise, the choices for a project proposal will depend on the specific priorities of each region. However, the recent pattern of collaborative engagement across regions to build a new accelerator and exploit its physics program, established with the LHC, is expected to continue.

We examine each potential new accelerator project in turn, with a timeline that looks out over the next two decades or so. Necessarily, the precision of our timeline becomes less certain as we look further into the future. We expect future subpanels to define this timeline further.

V.1 Electron-Positron Linear Collider

In developing our present understanding of the fundamental particles and interactions that is the Standard Model, both electron and hadron colliders have made major, distinct, and complementary contributions. In looking out over the next two decades on the world scene, it is not just natural, but essential, that both these types of facilities be considered as energy frontier facilities. The LHC is the proton-proton collider that has sufficiently high energy to give evidence or signatures of the new physics that is associated with the mechanism for giving mass to the fundamental particles. An electron-positron linear collider of appropriate energy would complement the LHC. We do not know what secrets nature will reveal in this new energy range when experiments start at mid-decade. But for a wide range of possible models for that new physics, exemplified by a Higgs particle or other phenomena which play the role of the Higgs, a linear collider will add crucial information to our understanding from the LHC. It would allow measurement of the detailed properties of the Higgs, including the couplings to different particles that demonstrate its role in giving mass. It would complement the LHC in understanding the character of the new physics, for example by finding new supersymmetric particles and measuring their properties, or by revealing aspects, not accessible at the LHC, of a new strong force that could replace a simple fundamental Higgs particle.

The international high-energy community has been considering such a machine for a number of years. Last year, the International Commission on Future Accelerators (ICFA) issued a statement recommending *"continued vigorous pursuit of accelerator research and development on a linear collider in the TeV energy range, with the goal of having designs complete with reliable cost estimates in a few years."* In addition to the planning and R&D going on in the U.S., groups of distinguished scientists in Europe and in Japan are also conducting intensive R&D and working to produce reports by next year on the future of high-energy physics in their respective regions of the world. A focus of these groups is on a linear collider as the next major frontier facility in their region. On the world road map of high-energy physics, the next energy frontier facility is likely to be an electron-positron linear collider.

Work on linear colliders extends back more than fifteen years at SLAC, where the SLC was the first and only example of an electron-positron linear collider. It has provided a test bed for further development of the concept and its extension to much higher energies. An international collaboration in R&D for a future machine has been underway for a number of years. SLAC and KEK in Japan have led the R&D effort toward a machine that would use room-temperature rf cavities to accelerate the beams. Fermilab has now joined SLAC, LBNL, and LLNL in the U.S. as a major partner in carrying out the R&D effort for such a collider. Germany's DESY Laboratory has led the corresponding effort for superconducting cavities. DESY plans to have a technical design report ready for their proposed TESLA machine by Spring 2001. Japan is updating its design, and has begun the studies of cost and site.

The 1998 Subpanel saw the design of an electron-positron linear collider as much more developed than that of other possible energy frontier facilities. It recommended continued R&D with Japan of a machine with an initial capability of 1 TeV in the center of mass, extendible to 1.5 TeV, and that SLAC be authorized to produce a Conceptual Design Report (CDR) in collaboration with KEK. While the CDR was not authorized by the DOE, R&D has continued and many of the issues that were identified at the time of the Subpanel are being addressed. Test facilities have been constructed and operated, both in the U.S. and abroad, that are helping identify technical issues and solutions. Significant improvements have been made in klystrons and modulators. A design luminosity that is four times higher than in 1998 has been proposed based on new understanding of fabrication and alignment of the disk-loaded accelerating structures. A new, more compact, design for the final focus region would reduce the cost. Much of the work has concentrated on cost reductions that involve modifications to the design, use of different technology, or scope reduction for the initial machine. Potential cost reductions have been identified that could total 30% relative to the 1999 DOE review. Further R&D remains to test the full power delivery system, to examine the sustainability of very high electric field gradients on the structures and to fully integrate into the design the potential for energy upgrades. This is exactly what R&D is all about. In the immediate future, an aggressive program of R&D should be supported in the U.S. to improve the performance and reduce the cost of such an energy frontier facility.

Along with the cost reduction efforts, a different strategy for the collider's initial energy and subsequent upgrades has been presented by the proponents. In part, this is motivated by the cost of a higher energy machine. It is also argued that there is now a stronger physics case for a machine with an initial center-of-mass energy of about 500 GeV. The revised physics case is made on the basis of new results in the past two years. Measurements of the Z boson properties at LEP I and SLC have become much more refined. The W boson mass uncertainty has been reduced by a factor of two to three from new data of the LEP II and Tevatron experiments. The resulting indirect limits on the Higgs mass are now lower than two years ago, and are considerably more robust in the sense that, even when subsets of the data are ignored, the lower limit persists. Precision data also give strong restrictions on possible new physics models generally, and for many such models, signatures for new physics would be accessible with a linear collider of about 500 GeV. An increased design luminosity would strengthen the physics case as well. For a wide range of postulated new physics models, an upgradable 500 GeV linear collider is an excellent complement to the LHC to make fundamental, incisive measurements that are particular to an electron-positron collider. Rather than aiming at an initial capability of 1 TeV in the center of mass as envisioned in the 1998 report, many members of the community propose a linear collider that begins at about 500 GeV. This proposal is a pressing question for the community and a future subpanel to consider.

It is likely that the physics will require a future upgrade in energy and luminosity. This is true for most scenarios of the new physics, but the details of the energy steps will depend on what is found. It is important that a linear collider accommodate upgrades in energy. Some progress can be made by relatively straightforward enhancements to the original collider. There has been considerable recent progress towards an acceleration scheme using a second low energy, high intensity drive beam, which may allow a substantially higher energy collider. Increased R&D on this possibility is needed.

Given the research and development now underway in the U.S., it seems likely that a decision point on construction of a linear electron-positron collider would come in the 2003-2004 time frame. However, the proposals being developed in Europe and Asia may well force U.S. consideration of a linear collider sooner, and the U.S. must be prepared to decide what role it wishes to play. As part of the worldwide community, we should begin to explore how a choice of accelerator technology can be made, and how the world high-energy physics community will approach a construction decision on what will be a machine with major international participation. At the same time, the U.S. high-energy community needs to vigorously engage itself in defining the projected physics program. This includes not only the initial science objectives in the light of current physics developments, but the nature and importance of upgradability, and the option that has been recently suggested of an early "low energy" interaction region. For this, as well as understanding how the U.S. community can come to grips with a decision in a few years, Snowmass 2001 and a future HEPAP Subpanel will play a very important role.

V.2 *Muon Collider and Muon Storage Ring/Neutrino Source*

At the time of the 1998 Subpanel, the concept of a muon collider that would allow exploration of multi-TeV center-of- mass energies was still in a state of rapid change. The Subpanel's report recommended that *"an expanded program of R&D be carried out on a muon collider, involving 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."* In accord with this recommendation, the Muon Collaboration has been established with multi-laboratory coordination and has embarked on an expanded R&D program.

Since 1998, there has been a dramatic change in the physics goals and the machine that provides the central focus of that R&D. With the recent evidence for neutrino oscillations and neutrino masses, it was quickly realized that a muon storage ring could potentially make an intense and very well understood source of neutrinos as the unstable muons in the beam decayed. The energy of the stored beam would be far less than for an energy frontier collider aiming for multi-TeV physics; only a single beam need be stored at a time; and the requirement for "cooling" the muons would be orders of magnitude less than for a muon collider. A key limiting factor on the luminosity of a muon collider, the rapid decay of the beam particles, now becomes the very source of the beam of neutrinos. It is also important to note that such a muon storage ring/neutrino source could become an intermediate step that could deal with some of the technical issues that have to be solved before a decision could be made on a multi-TeV muon collider. The Muon Collaboration has consequently changed its focus considerably, and now plans for its R&D to focus primarily on the muon storage ring/neutrino source.

The "long baseline" neutrino oscillation experiments presently operating or under construction involve sending the beam from a source at an accelerator laboratory (such as Fermilab) over hundreds of kilometers through the earth to a large underground detector (e.g., at the Soudan Mine in Minnesota). Such second-generation experiments will be conducted in Japan, the U.S., and Europe, and will help quantify the neutrino mass differences and mixing angles relating the three types of known neutrinos. Depending on what they find, third-generation experiments could measure the remaining parameters that characterize the mixing and perhaps see evidence for an asymmetry in the behavior of matter and antimatter in the neutrino sector. The muon storage ring/neutrino source would be used in such third generation neutrino experiments where the beams travel thousands of kilometers across a continent or even between continents. With the accelerator and neutrino source on one continent and the detector on another, this would necessarily be an international experiment from the start.

The accelerator R&D issues for a neutrino source, while less daunting than those for a muon collider, are still very challenging. There is more work to be done in simulation. An upgraded proton source, necessary for an intense neutrino source or a muon collider, is also of interest for other uses involving high flux secondary beams, including upgrades of the current neutrino experiments. This source requires further R&D. Cooling of the muon beam, while essential for a muon collider, remains an important issue for a neutrino source that needs to be better understood. Aside from questions of accelerator technology, the amount of cooling needed, if any, is directly linked to the required intensity of the neutrino source and thus to the proposed physics program.

Development of a viable proposal for a muon storage ring/neutrino source entails a multi-year program of R&D that goes into the latter part of this decade. Furthermore, until we know the results from the second generation neutrino experiments in the next five years or so, we will not know the physics questions that will be open to study in a third generation experiment. This sets the timeline for a comprehensive review of the physics and technology, and a decision point on constructing a possible muon storage ring/neutrino source then would come toward the end of this decade. In the near future, the U.S. high-energy community needs to further develop the potential physics program of such a facility. Again, the Snowmass 2001 discussion will be important.

Finally, we return to the multi-TeV muon collider itself. The 1998 Subpanel saw many years of intense R&D needed to even establish its feasibility, and the progress in understanding the issues made in the last few years have reinforced that conclusion. Moreover, within the Muon Collaboration the neutrino source is now seen as an intermediate step that comes before a muon collider. While some important technical issues for a muon collider will be dealt with or better understood in the course of work on a neutrino source, others will remain. Given the amount of needed R&D and the timescale for a possible neutrino source, it seems that a decision on whether to construct a muon collider is one the world high-energy community may face around 2020.

V.3 *Very Large Hadron Collider*

A Very Large Hadron Collider (VLHC) is the name given to a proton-proton collider that operates at a center-of-mass energy of roughly 100 TeV or more, well beyond the 14 TeV of the LHC. Such a machine would be 50-100 km in diameter. Building superconducting magnets is the key enabling technology. Both “low-field” and “high-field” prototype magnets are being explored. Reduction in tunneling costs is also an essential need. In the past two years, increased attention has been given to an evolutionary possibility for the VLHC, starting with an energy of tens of TeV and progressing to hundreds of TeV as technology improves.

In accord with the recommendations of the 1998 Subpanel, a national collaboration to carry out R&D for a VLHC coordinated across both laboratory and university groups has been organized. Through that collaboration, progress has been made on the technical issues of constructing both low-field and high-field magnets. The long-term goals of this R&D program are identification and development of design concepts for an economically and technically viable accelerator. This R&D carries wider implications, as such magnets would find use beyond high energy physics. The course of presently planned R&D is appropriately aimed to address these issues, with the next steps involving the testing of a series of coils and model superconducting magnets.

The physics potential for the VLHC has been explored further in the past few years. There are specific scenarios for new physics that could motivate the much increased energy beyond the LHC. If there is a new strong interaction involving new massive counterparts to the quarks, it will be important to create them and to study their possible role in giving mass to all particles and altering the known interactions of W and Z bosons. Should there be extra spatial dimensions that are confined to distances of femtometers or larger, the high energy of the VLHC could be needed to directly explore this new domain. Perhaps the most compelling argument is the historical observation that through exploration at hitherto uncharted energies, we have made our most significant discoveries. As stated by the 1998 Subpanel, understanding the implications of physics from the LHC is a necessary precursor for a VLHC, and therefore from a physics standpoint, the decision point on whether to build a VLHC lies somewhere in the 2010 to 2015 period.

V.4 *The time-line for future colliders*

New energy frontier accelerators will be needed to explore the nature of the new physics casting its shadow on the results of current experiments. The LHC will almost surely discover some elements of the new physics, no matter what its origin is. But we do not expect that the LHC will fully delineate the character of the new sector. Though it should discover the existence of new physics clearly, the LHC will likely not be capable of making a detailed study of the properties of all its new particles. Other facilities will be needed to carry the quest onwards and to answer the overarching questions. Thus, long-range R&D on these facilities is crucial for the future development of the field.

In the sections above, we have reviewed the progress and needed R&D for the three candidates for new energy frontier facilities. The character of that R&D differs for the different machines. For the electron-positron linear collider, the focus now should be upon proving the technological choices with large scale test facilities, demonstrating the reliability of the major components, and in finding cost-saving techniques for construction. Additional R&D is needed for a multi-TeV electron-positron collider. The muon collider and the muon storage ring are on different timescales. For both, the R&D should be focused on developing the enabling technologies and demonstrations of proofs of principle. For a hadron collider with significant reach beyond the LHC, the emphasis is long range and should be focused on technology development aimed at cost efficiencies in construction and operation.

There is good reason to believe that the physics needs will demand, in time, more than one of these candidate new accelerators. Important R&D for these facilities is being conducted in the U.S., Europe and Japan. It is likely that at most one of each type of major facility will be built in the world. It is desirable that they would be deployed in different regions. However, it is important that each region participate in the R&D phase, and in the discussion of the physics potential. The U.S. has specific expertise related to each potential collider. The only linear collider ever built is the SLC at SLAC, and unique test facilities have been built there. Cornell has played a central role in the development of superconducting rf cavities. The U.S. has led the technology development for a muon storage ring/collider. The development of superconducting magnets was pioneered for the Fermilab Tevatron, and FNAL, BNL, and LBNL continue to lead new magnet development. The global context of all future frontier facilities makes it important for the U.S. to continue to contribute to each of these branches of R&D.

As noted in past HEPAP Subpanel reports, and most notably in the 1994 and 1998 recommendations, the U.S. leadership in this branch of fundamental research has been key for the evolution of the field since its inception. The country is well served by the continuation of this leadership and a vital U.S. program is essential for the world effort in high-energy physics. The requirement for playing such a leading role is that a facility operating at the energy frontier must exist in the U.S. The next phase of experimentation at the LHC will mark the passage of the energy frontier to Europe, and the U.S. has staked out an important role in that international research program. However, given the need for future accelerators, and the need to have the U.S. operate at the energy frontier, it is not only natural but also necessary that one of the future machines be sited in this country. It behooves the U.S. to work constructively with its partners worldwide to develop a plan that provides new opportunities here and abroad, satisfying the needs of each region.

This report is a precursor to the 2001 deliberations at Snowmass and the future HEPAP Subpanel, and provides input to the options to be discussed in the next year. With a variety of possible frontier accelerators, and the differences of the specific physics questions they would directly address, the priorities for the future of the field will be developed through this process. This process will build upon the foundation laid by the 1998 Subpanel report and the 1998 NRC decadal study. The natural timescales of the several new projects also help define the road map.

The linear collider concept is the most well developed, and the physics case for its construction is better understood than those for the other facilities. Moreover, the worldwide motion towards proposals from each of the three major scientific regions is now well advanced. It is highly likely that there will be full-scale proposals for linear colliders at the 500 GeV scale in the next few years. The issues of whether, where, and how, to proceed with such a collider will need to be confronted. *For the U.S., the fundamental question is whether this machine is the desired candidate project in this country that will restore the U.S. to the energy frontier. Making this decision is thus the most pressing issue before our community.*

The time-line for the major new facilities has been indicated in the subsections above and stretches over two decades or more. To summarize these, we foresee a decision on the linear collider by about 2003-2004. The decision on a muon storage ring is paced by the ongoing R&D program and on the round of planned neutrino experiments, and should be appropriate toward the end of this decade. The VLHC is paced by physics results from the LHC and also requires R&D aimed at the enabling technology of superconducting magnets and at reducing costs; its decision point might occur in the 2010 to 2015 period. A multi-TeV lepton collider (muon or electron) involves very significant R&D and proof of principle for new technologies; these might become ready for a decision around 2020, though the comparison between these options could be appropriate somewhat earlier.

While the time-line indicates some sense of priorities for the R&D efforts, it is worth re-emphasizing that, in time, each of the potential new accelerators may be necessary, and the R&D to make them possible is needed now. Our conclusions on the next steps in the development of new facilities then are:

- *The study of the fundamental issues bearing on the nature of matter at the smallest scale, and the forces at work in shaping the universe, befit this nation. The U.S. should remain a leader in high energy physics. Maintaining the U.S. leadership and training new generations of scientists in this field demand an energy frontier facility at home.*

- *Accelerator R&D is the lifeblood of our science, creating the tools that are needed to explore the physics of matter space and time. Current funding levels for R&D toward new accelerators are endangering the near and far term future of the field, and should be increased substantially.*

- *High energy colliders in addition to the LHC will be needed to understand the new physics now indicated from current experiments. There is a worldwide research and development effort for such energy frontier facilities, with a decision point on construction of an electron-positron collider coming in the next several years.*

Appendix A - Charge Letter from Jim Decker

Appendix B - Letter to the Community from Fred Gilman

Appendix C - Agenda for the Fermilab Users Meeting

Appendix D - Agenda for the SLAC Users Meeting

Appendix E - Writing Group Agenda at UCLA

Appendix F - Agenda for the DPF Town Meeting

Appendix G - Writing Group Agenda at the DPF Meeting

Appendix H – Implementation of Other 1998 Subpanel Recommendations

Appendix I – DOE Funding for High Energy Physics