DRAFT REPORT

DOE/NSF HIGH-ENERGY PHYSICS ADVISORY PANEL

SUBPANEL ON LONG RANGE PLANNING FOR U.S. HIGH-ENERGY PHYSICS

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EXECUTIVE SUMMARY

Particle physics stands at the threshold of a new era of discovery. As experiments peer deeper and deeper into the heart of matter, they open strange new worlds and striking new vistas on the cosmos. They begin to address the most human of questions: Where did we come from? Where are we going? Particle physics is a grand adventure, a journey into the great unknown. It explores the frontiers of matter, energy, space and time, much like the early pioneers in a great new nation, 200 years ago.

Why should we study a world so removed, so different from our own? The reasons are the same as for the exploration of space, the sea, or any other new frontier. The journey is almost an end to itself: it is part of what defines our humanity. From Lewis and Clark to Shepard and Glenn, we have explored new territories because it is exciting and challenging – and because we learn so many new things along the way.

Today, perhaps even more important is the role that past investments in science and technology have played in creating the open and advanced society that we cherish. Future investments will help ensure our society's health and security in years to come. As a community, we stand ready to lend our skills and to support this critical effort.

We have learned much about particle physics, but much more remains to be found. We have discovered what we believe to be the basic constituents of matter, but we are just beginning to understand the fundamental principles that govern their behavior. During the past ten years, we discovered the top quark –the last quark, a quark as heavy as an atom of gold. We learned that neutrinos have mass, and that they change their identities over time. We confirmed electroweak unification to extraordinary accuracy, measured the matter-antimatter asymmetry in quark systems, and studied the interactions of quarks and gluons.

These discoveries were made by experiments in laboratories around the world. They were done by international collaborations that benefited from extensive cross-fertilization of ideas and techniques. They revealed a complex microphysical world, but one we can now describe by a surprisingly simple mathematical theory. These discoveries enabled the creation of a quantum theory of elementary particles that will stand as one of the lasting achievements of the twentieth century.

During the next few years, we will press our journey at the CERN LHC, an accelerator that will open a new era in particle physics. Its energy, almost ten times larger than the Fermilab Tevatron, will allow us to examine the very fabric of space and time. Theoretical developments suggest that the LHC could reveal entirely new dimensions of space. Where are they? What are their sizes and shapes? Why are they hidden? During the next decade, such questions will move from science fiction to science fact.

Experiments on the horizon will bring new revelations about the microphysical structure of elementary particles. Theories such as supersymmetry and superstrings suggest vast new worlds that will be accessible at the LHC. They point to the unification of forces and the realization of Einstein's dreams.

Astronomical observations provide clues to the Big Bang. They suggest that the universe is filled with dark matter and dark energy, unlike anything we have seen before. What is dark matter? What is dark energy? Particle accelerators hold the promise to create dark matter here on Earth. What is the fate of the universe? Dark matter pulls the universe together, but dark energy may drive it apart. Which way will it go?

Questions abound: Where is the antimatter? Why are there no antistars or antiplanets? Why do particles change their identities? Do the constants of nature change with time? Are protons forever?

Experiments in progress and under development offer the potential to answer these questions, and to reshape our view of matter and energy, space and time. Technological breakthroughs – superconductivity, nanotechnology, new accelerators, and information technology – offer the means to explore these frontiers. The future is very bright.

RECOMMENDATION 1:

> ***We recommend that the United States take steps to remain a world leader in the vital and exciting field of particle physics, through a broad program of research focused on the frontiers of matter, energy, space and time.***
>
> ***The U.S. has achieved its leadership position through the generous support of the American people. We renew and reaffirm our commitment to return full value for the considerable investment made by our fellow citizens. This commitment includes, but is not limited to, sharing our intellectual insights through education and outreach, providing highly trained scientific and technical manpower to help drive the economy, and developing new technologies that foster the health, wealth and security of society at large.***

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Our subpanel is charged with charting a twenty-year future for U.S. particle physics. We have had extensive discussions among ourselves, as well as with physicists at home and overseas. We received many thoughtful letters from our colleagues that were helpful in our deliberations. We found general agreement that our field has broadened to include overlapping areas of astrophysics, cosmology and nuclear physics, and that we should foster partnerships with scientists in these fields. There is a strong worldwide consensus that particle physics will require new frontier accelerators, as well as a carefully chosen set of initiatives using other techniques.

In this report we develop a roadmap for particle physics. This roadmap provides an overview of the field, as well as an outline of the steps we must take to reach our goals. The map is based on the best information available at the present time. It is built on fully exploiting our investment in the LHC and our ongoing program. It will need to be periodically updated to reflect new ideas, discoveries and technological developments in the worldwide scientific program.

Our roadmap reflects the fact that our scientific goals can be best achieved using a variety of scientific techniques. Our science requires forefront accelerators at the energy and luminosity frontiers. But it also requires innovative experiments in space, underground, and away from accelerators. It needs a balanced approach that capitalizes on our increasingly important links to astrophysics, cosmology and nuclear physics. A strong university program is fundamental to our field. Universities train the next generation of scientists. They provide breadth, leadership, a platform for education and outreach, and the opportunity to connect with scientists in other fields.

We recognize that this program demands sound management. The roadmap will help in this process because it highlights the tradeoffs and opportunity costs associated with the decisions we make. The scale of our science has grown to the point where we need a new mechanism to set priorities across the program. In this report we propose such a mechanism. It is important that we set priorities: our compact with the American people demands no less.

RECOMMENDATION 2:

> ***We recommend a twenty-year roadmap for our field to chart our steps on the frontiers of matter, energy, space and time. The map will evolve with time to reflect new scientific opportunities, as well as developments within the international community. It will drive our choice of the next major facility and allow us to craft a balanced program to maximize scientific opportunity.***
>
> ***We recommend a new mechanism to update the roadmap and set priorities across the program. We understand that this will require hard choices to select which projects to begin and which to phase out. Factors that must be considered include the potential scientific payoff, cost and technical feasibility, balance and diversity, and the way any proposed new initiative fits into the global structure of the field.***

* * * *

The roadmap begins with thorough exploration of the TeV energy scale. The exploration will begin, but not end, with the CERN LHC. There is now a worldwide consensus, reflected in recent reports by the Asian and European Committees on Future Accelerators, and by this subpanel, that a high-energy, high-luminosity, electron-positron linear collider is the most pressing new initiative for our field. The LHC and the linear collider are both essential to discover and understand the new physics at this scale. A coherent approach, exploiting the strengths of both machines, will maximize the scientific contribution of each.

For many years, vigorous R&D programs in Asia, Europe and the United States have been aimed at designing such a machine. The challenges were daunting: devising a way to accelerate electrons and positrons to enormous energies and then collide them in beams a thousand times smaller than a human hair. Recent successes have brought us to the point where we have confidence that this accelerator can be built. The linear collider is the next big step for particle physics.

Physicists in Germany and Japan are making serious efforts to have their countries host the linear collider. The project is so complex and costly, however, that just one should be built in the world. The project must be realized by an extensive international collaboration. Such a worldwide effort will require a partnership agreement that satisfies the participating governments and creates an organization capable of developing and managing the construction and operation of this challenging forefront scientific facility, as well as satisfying the scientific aspirations of all the participating countries.

RECOMMENDATION 3:

We recommend that the highest priority of the U.S. program be a high-energy, high-luminosity, electron-positron linear collider, wherever it is built in the world. This facility is the next major step in the field and should be designed, built and operated as a fully international effort.

We also recommend that the United States take a leadership position in forming the international collaboration needed to develop a final design, build and operate this machine. The U.S. participation should be undertaken as a partnership between DOE and NSF, with the full involvement of the entire particle physics community. We urge the immediate creation of a steering group to coordinate all U.S. efforts toward a linear collider.

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A linear collider will involve international participation and contributions, redirection of resources from the host country, and incremental funding, which will be greater if the facility is located in the United States. Nevertheless, there are strong reasons for hosting the linear collider in the U.S. This nation would become the center of activity for one of the greatest scientific projects of our time. The machine and its discoveries would excite the imaginations of our children and grandchildren, helping to produce a future generation of scientists and those who appreciate science. Moreover, the U.S. would recapture a greater portion of its economic investment through jobs and technological benefits.

The linear collider would be an exciting opportunity for the United States, and a flagship facility for the 21st century. It could be a centerpiece of a national effort to boost the physical sciences. In partnership with the broader scientific community, an X-ray free electron laser facility could be included in the project, providing a brilliant, coherent fourth-generation light source with femtosecond time resolution. Such a facility could open important new areas of research across many sciences, including the life and environmental sciences, as well as physics and chemistry.

Major scientific projects of the future must be increasingly international. A linear collider facility will require a new approach to planning, collaboration and management on a worldwide scale. Hosting this project would allow the United States to become a leader in forging this new way of doing science, and advancing international cooperation and progress. We fully expect that facilities directed at other parts of our roadmap will be developed abroad, and present reciprocal opportunities for realizing the goals of the U.S. program.

RECOMMENDATION 4:

We recommend that the United States prepare to bid to host the linear collider, in a facility that is international from the inception, with a broad mandate in fundamental physics research and accelerator development. We believe that the intellectual, educational and societal benefits make this a wise investment of our nation's resources.

We envision financing the linear collider through a combination of international partnership, use of existing resources, and incremental project support. If it is built in the U.S., the linear collider should be sited to take full advantage of the resources and infrastructure available at SLAC and Fermilab.

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The long-range future of particle physics will certainly require pushing the energy frontier beyond the LHC and the linear collider. This appears feasible if the enabling R&D is carried out on a worldwide basis. A large hadron collider, well beyond LHC energies, is a long-term goal that will require new ideas and technological developments. A multi-TeV lepton collider is even more challenging technically. To ensure future discoveries, the field must increase its effort on long-term accelerator and detector R&D, as well as in information technology and tools.

While research on accelerators, detectors and information technology is critical to progress in the field, it also has broader benefits for society. Medical technology routinely uses particles and particle detectors to see inside patients and diagnose their ailments. Particle beams themselves can effectively treat certain types of cancer. The World Wide Web was conceived at CERN to facilitate particle physics collaboration across the globe. In general, particle physics projects push technology to the state-of-the-art and beyond; this helps industry improve its capabilities, which later leads to advances in commercial products.

RECOMMENDATION 5:

We recommend that vigorous long-term R&D aimed toward future high-energy accelerators be carried out at high priority within our program. It is also important to continue our development of particle detectors and information technology. These investments are valuable for their broader benefits and crucial to the long-range future of our field.

* * * *

The twentieth century can be characterized by an increasingly global economic interdependence, as well as by many shared problems, including the health of the human race and of the Earth itself. It is becoming increasingly important to find successful international models for solving such problems. Particle physics represents one of the most successful areas of international cooperation. From the pivotal role of CERN in postwar Europe to the global collaborations of today, particle physicists have worked together with great success on problems of common interest. The construction of a linear collider will break new ground as an international partnership and provide a useful model for other areas of human endeavor.

At the beginning of the last century, few understood how scientific research would fundamentally change the world. But continued and consistent investments in science helped make the United States what it is today. As we head into the new millennium, few doubt that scientific research will remake our world yet again. It is our choice whether we want to help make this world – or retreat from it. We think the choice is clear.

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1. EXPLORING NEW WORLDS

We recommend that the United States take steps to remain a world leader in the vital and exciting field of particle physics, through a broad program of research focused on the frontiers of matter, energy, space and time.

The U.S. has achieved its leadership position through the generous support of the American people. We renew and reaffirm our commitment to return full value for the considerable investment made by our fellow citizens. This commitment includes, but is not limited to, sharing our intellectual insights through education and outreach, providing highly trained scientific and technical manpower to help drive the economy, and developing new technologies that foster the health, wealth and security of society at large.

1.1 Introduction

In this report, we present a roadmap for particle physics over the next two decades. We motivate our field by its scientific goals, including a new and deeper understanding of the universe through the structure of matter, energy, space and time. Our quest is to explore the worlds we know, to discover new ones, and to bring the public along on our journey. We are part of a long term and broad-based American investment in science and technology that has made us one of the most advanced societies on Earth, and has helped ensure our security and our way of life.

Particle physics and the technologies it fosters are very much a part of the overall fabric of science, drawing on advances in other areas and developing technologies that enable progress across the board. The U.S. Commission on National Security/21st Century recognized the importance of these relationships. The Commission's report concludes that national security rests on the strength of our scientific and technological base. It is not a single area or technology that must be fostered, but the entire portfolio that must be maintained at a healthy level to ensure the health, welfare and security of the nation in years to come.

Particle physics evolved out of nuclear physics and studies of cosmic rays. For over the past half-century, particle accelerators have been cornerstones of our field. Accelerators with higher and higher energies have enabled spectacular advances in our understanding of the subatomic world. The scope of particle physics is much broader than experiments based on accelerators, but as we look to the future of our field, we fully expect that a new generation of particle accelerators will again lead the way. For that reason, we use the terms particle physics and high-energy physics interchangeably throughout this report.

A confluence of experimental discoveries, theoretical insights, and technological advances has positioned our field at a threshold of discovery. In the late 19th century, a series of rapid advances sparked a scientific revolution that led to relativity, quantum mechanics, and a totally new view of nature. At the dawn of the 21st century, similar

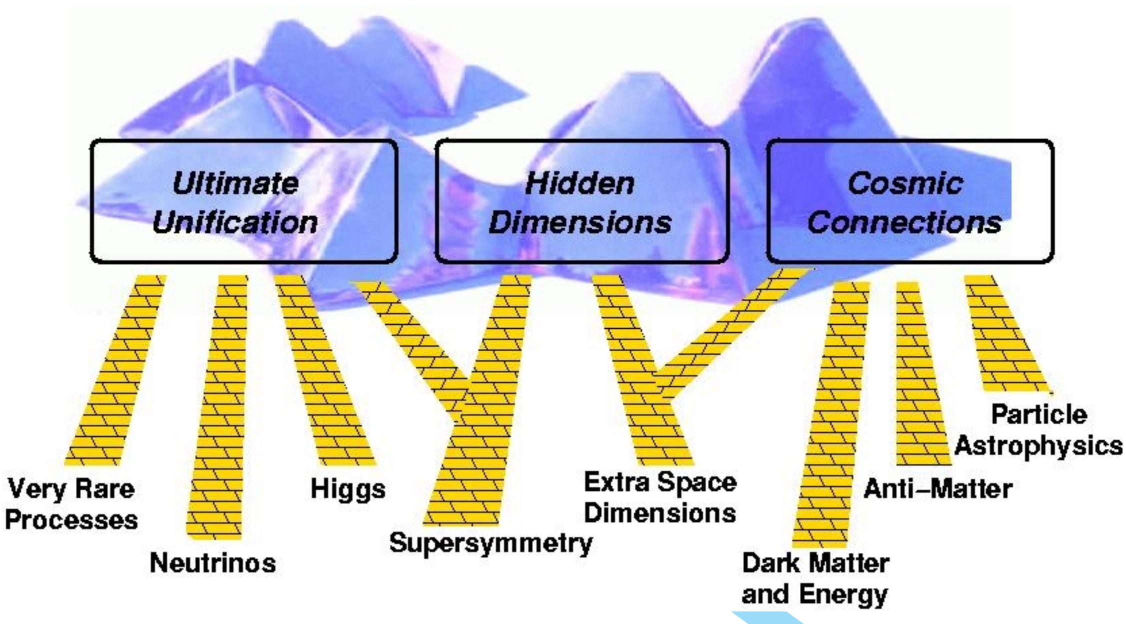


Figure 1.1. Paths to the Goals of Particle Physics.

advances suggest that a new scientific revolution is within our reach. We can only guess how these advances will shape the years to come.

The United States has an illustrious past in high-energy physics, as both a leader and an innovator, having pioneered the discovery and exploration of this great new world. A striking feature of high-energy physics research, however, has been its development into a truly global adventure, typified by large facilities built in different countries and shared by the international community. We recognize that our field has become a global enterprise, perhaps more so than other fields of basic research, so any realistic plan for the U.S. program must be formulated in an international context.

The size, complexity and sophistication of the next generation of particle accelerators make them among the most challenging and ambitious projects ever undertaken. Extensive international partnerships will be required to build and use them. These projects will be symbols of international cooperation and collaboration, and the reach of their science will span generations. The United States has the opportunity to remain a partner in this great international effort; we present a realistic plan to lead the way.

1.2 The Goals of Particle Physics: Matter, Energy, Space and Time

What are the scientific opportunities that modern particle physics research presents, and how can they be realized? The purpose of the field is to explore the frontiers of matter, energy, space, and time: from the highest energy particles to the seeming emptiness of space, from the shortest distances we can imagine to hidden dimensions of space-time.

A program aimed at reaching these goals has several interrelated areas of focus, each promising important new discoveries. During the next twenty years, we will try to understand how the disparate forces and particles of the universe merge together into a

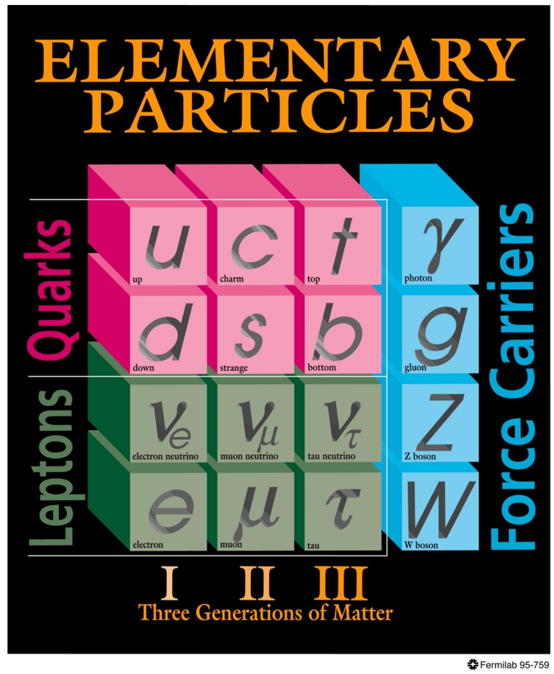


Figure 1.2. The Standard Model of Particle Physics. The six quarks and six leptons interact through the strong, weak and electromagnetic forces. The forces themselves are described by particles, labelled γ, g, *W* and *Z*.

single coherent picture, which we call *Ultimate Unification*. We will seek new dimensions of space-time, which we refer to as *Hidden Dimensions*. And we will seek the mysterious particles and forces that have created indelible imprints on our universe, a very exciting new area we call *Cosmic Connections*.

From each of these goals flows a diverse research program that will be carried out in partnership with colleagues across the globe. Although we discuss them separately, the topics have many connections, both internally and to other fields. Understanding and developing these connections is a crucial component of our long-range plan.

1.2.1 First Goal: Ultimate Unification

Unification is the search for simplicity in a universe filled with a multitude of particles and forces. We have learned that the elementary building blocks of our universe are six kinds of quarks, which make up the protons and neutrons in nuclei, and six types of leptons, like the electrons that orbit around the nuclei to make atoms. The quarks and leptons interact with one another through four forces: gravity, electromagnetism, the strong force that holds a nucleus together, and the weak force that is responsible for some types of radioactive decay. The Standard Model of particle physics describes all the forces except gravity.

The rich and complex phenomena we observe today may well have emerged from a much simpler world at high energies. Experiments of the last few decades have confirmed that new fundamental particles must exist just beyond the reach of current accelerators. New facilities are being designed and built to create these particles in the laboratory. The new

The Standard Model

The Standard Model describes our understanding of the fundamental particles in the context of three of the four fundamental forces of nature: the strong, weak and electromagnetic forces. Its building blocks are illustrated schematically in Figure 1.2.

In the Standard Model, all interactions arise from the exchange of elementary particles, called gauge bosons. The electromagnetic force results when charged particles exchange photons (γ). The strong force, which holds together protons and neutrons, comes from the exchange of gluons (g). The weak force, which explains radioactive decay, arises from the exchange of W and Z bosons.

The W and Z are massive, unlike the photon and gluon; their mass is responsible for the weakness of the weak force. Even the simplest explanation for their mass requires a new particle, the as-yet-unobserved Higgs boson. The W and Z acquire mass by coupling to the Higgs.

The matter particles in the Standard Model are called quarks and leptons. Quarks carry strong charge, and leptons do not. The quarks and charged leptons also gain mass by coupling to the Higgs.

Quarks interact through all three forces. Because of the strong force, quarks are always bound in groups called mesons (quark-antiquark pairs) or baryons (quark trios). There are six quarks, called up, down, charm, strange, top and bottom. The quark masses range from a few MeV (for the up and down quarks) to 174 GeV (for the top).

The leptons do not feel the strong force, so they always appear individually. Charged leptons interact via the weak and electromagnetic forces. There are three such leptons, the electron, the muon (μ) and the tau (τ). The charged lepton masses range from 0.5 MeV (for the electron) to 1.8 GeV (for the tau). Neutral leptons are called neutrinos. In the Standard Model, they are massless.

The Standard Model has been dramatically confirmed by precision measurements carried out at laboratories around the world. But recent experiments have revealed a crack: Neutrinos have mass, something that cannot be explained by the minimal Standard Model. Moreover, theorists have demonstrated the Standard Model itself is mathematically inconsistent – unless a Higgs particle (or something else) appears at the TeV scale. These two mysteries will guide the first steps of our journey to understand the nature of matter, energy, space and time.

particles may be manifestations of new dimensions of space-time, new quantum dimensions, or something even more radically different.

One likely candidate is the long-sought Higgs particle. Discovery of the Higgs would explain how the weak and electromagnetic forces unify into a single electroweak force. But this is just a first step. Precision measurements from the new accelerators will tell us whether the electroweak force unifies with the strong force at still higher energies. A detailed exploration of the energy frontier will begin to chart this exciting new territory. Ultimately, it will tell us if Einstein's dream of a unified theory is realized.

Unification may provide the DNA of matter, the simple principle that gives particles their complex identities. For example, at energies we now probe, quarks exhibit a complicated structure of masses and mixings. We expect that at some higher energy scale, this so-called flavor structure simplifies and quarks become more alike. To penetrate the mysteries of quarks, we must first measure their flavor properties completely and with precision. This motivates the study of mesons that contain one of the heavy quarks. These mesons are produced in abundance at hadron colliders and also at specially built electron-positron colliders.

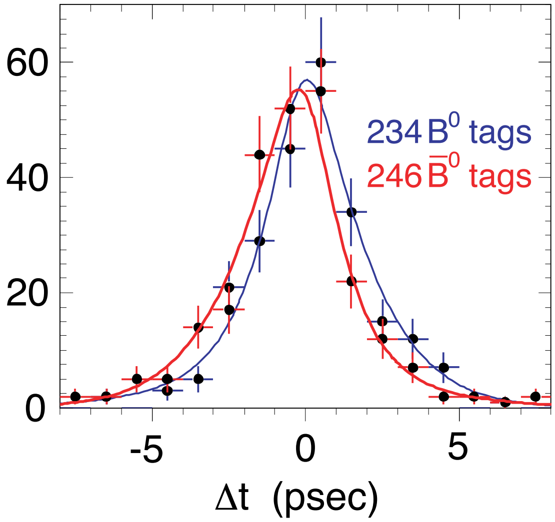
Neutrinos provide another window on unification. Many theoretical models for unification predict that neutrinos have mass, a prediction that was dramatically confirmed by recent experiments. The tiny masses of neutrinos are related to the energy scale of unification, and ultimately to the masses of quarks. In fact, the ghostly neutrinos that stream through the Earth may be secret siblings of the quarks that make us up. They are vastly different here on Earth, but identical at the high energies of unification. We need precise measurements of neutrino masses and mixings to explore these connections. These measurements can be extracted from a variety of experiments that look for signs of neutrinos changing flavor.

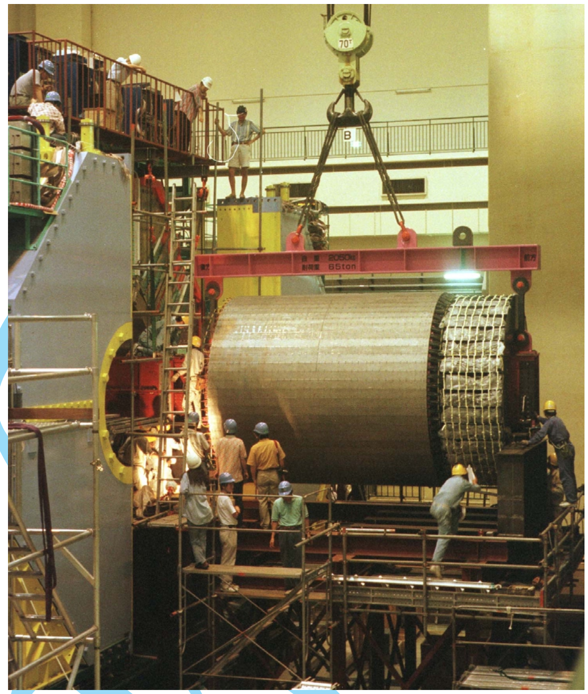
Finally, searching for very rare processes is a particularly sensitive way to probe for consequences of grand unification. One such consequence is that the proton is not completely stable! Although the predicted proton lifetime is incredibly long, sensitive techniques have been developed to detect decaying protons. Observation of proton decay would be a spectacular verification of unification at energies well beyond those accessible with accelerators.

1.2.2 Second Goal: Hidden Dimensions

In the world in which we live, we can move in three dimensions. From all data we have, there is still room for extra dimensions that cannot be observed in the everyday world. But if there were other dimensions, how would we know? One way is to kick particles with enough energy so that they can move through the extra dimensions. The programs we propose would allow us to find such dimensions, if they exist, and to determine their shapes and sizes.

It is also possible that the hidden dimensions are intrinsically quantum mechanical, as in supersymmetric theories. Particles moving into the new quantum dimension transform into new states of matter – which look like new particles, when viewed from our world. In such theories, every particle we know has a partner that moves in the new quantum





SLAC BaBar Data **KEK BELLE Detector**

Figure 1.3. Matter-Antimatter Asymmetry. BaBar at SLAC and BELLE at KEK have detected matter anti-matter asymmetry in the decays of B mesons.

dimension. We can discover these quantum dimensions by discovering the particles that populate them.

There are strong reasons to think that such dimensions will be observed in future colliders. Experiments are already gearing up to explore this possibility. Hidden dimensions of space may also be detectable in new tests of gravity, either on submillimeter scales or on cosmological scales. A variety of astrophysical phenomena are sensitive to the existence of these new dimensions as well. Even if extra dimensions are not directly accessible, there are good prospects to pin down the energies associated with possible extra dimensions and with quantum gravity. This detective work will overlap with our investigation of unification.

Extra spatial or quantum dimensions provide a way to connect powerful theoretical concepts to physical phenomena we can observe. Einstein showed that gravity is actually a consequence of curved space-time. Adding extra dimensions may be the bridge that finally unifies gravity with quantum theory. Recent conceptual breakthroughs have shown that the world of quantum gravity may be accessible with the next generation of accelerators.

To probe the Big Bang, we will need to understand quantum gravity. String theory is our theoretical working model for quantum gravity, although it is still poorly understood. In the long term, string theory may provide the ultimate unification of forces, including matter, energy, space and time. Experimentally, string theory makes a number of fascinating predictions, including properties of black holes, the existence of supersymmetry, and the existence of seven extra dimensions of space.

A direct discovery of extra dimensions of space would be an epochal event in the history of science, causing a redirection and refocusing of the entire field of particle physics and of cosmology. Within our lifetimes, science fiction may pale compared to science fact.

1.2.3 Third Goal: Cosmic Connections

The simple picture we seek must have shaped the very early history of the universe, which provides one of the fundamental connections between particle physics and Moreover, particles and forces shape the evolution and present state of the universe. We now suspect that every corner of empty space is filled with so-called dark energy, which is pushing the universe to expand at an ever-increasing rate. There are also unidentified cosmic accelerators beaming ultra-high-energy particles to Earth. We are on the brink of discovering the nature of these mysterious particles and forces through experiments deep underground, on land and in space.

Another major goal is to understand the deep connections between the physics of elementary particles and the physics that determines the structure of the universe. For example, we know that most matter in the universe is dark, unlike any conventional matter observed here on Earth. Leading candidates for this dark matter are the unknown heavy particles that will be sought at future accelerator experiments. In addition, experiments on Earth seek to detect these weakly interacting particles as they reach us from space. With a balanced approach involving high-energy colliders and particle astrophysics techniques, the identity of dark matter should be revealed soon.

1.2.4 The Road Ahead

The achievements of the last decade have been deep and impressive, and they put our goals into sharp focus. These advances come from experimental discoveries and measurements, as well as from new theoretical ideas. We have recently completed the periodic table of quarks and leptons by finding the last quark and neutrino – a very important milestone on the road to unification. These two discoveries used complementary approaches, one involving the highest energy accelerator on Earth, the other using one of the highest intensity beams ever made.

We are still missing a crucial piece in the puzzle: the Higgs particle. Precision measurements made at laboratories around the world suggest that the Higgs will soon be within reach. Discovery and study of this particle is the next crucial milestone for our field.

The Higgs particle and the new force associated with it represent revolutionary concepts in elementary particle physics. The Higgs force resists the motion of elementary particles and effectively gives them mass. Without it quarks and leptons would be massless, the weak interactions would be much stronger, and our universe would be a very different place.

Discovering the Higgs is essential to understanding the unification of the weak and electromagnetic forces. Recent theoretical breakthroughs suggest a more comprehensive unification that involves new hidden dimensions within our reach. String theory requires extra spatial dimensions to unify gravity with the other forces of nature. These

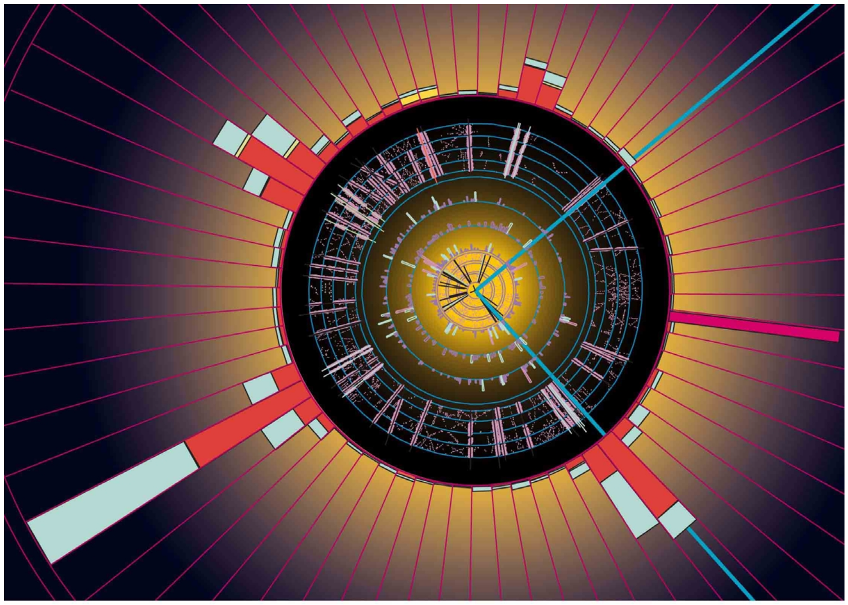


Figure 1.4. The Last Quark: A Top Quark Event from Fermilab. At present, the Fermilab Tevatron is the only accelerator able to produce the most massive quark.

dimensions might well be visible at future colliders. Supersymmetry gives rise to a completely new type of dimension – a quantum dimension of space-time. Indirect evidence from recent precision measurements suggests that supersymmetric particles might be close at hand; discovering them will be a major goal for experiments during the next ten years.

Particle physics has become increasingly interconnected with neighboring sciences. During recent years, we have worked with astrophysicists and cosmologists to link the inner space of particle physics with the outer space of the cosmos. The question of dark energy has launched promising new collaborations that will be pursued in the coming years.

The theoretical and experimental accomplishments of the past decade place us at the threshold of a new era. Taken as a whole, they show that the expedition we propose has a strong base and a clear mission.

1.3 The Field of Particle Physics

As the last section showed, particle physics is defined by the questions we ask, and not by the tools we use. Nevertheless, our primary tools are and will continue to be particle accelerators. The highest energy accelerators probe the shortest distances and provide the most direct way to answer the questions we face. Other accelerators are used to study rare phenomena or carry out precision measurements. Additional questions can be addressed using particles that come from outer space. Research in high-energy physics has increasingly important overlaps with astrophysics, cosmology and nuclear physics. A strong program in high-energy physics needs all these components.

Experiments in particle physics are carried out in international collaborations using large and sophisticated particle detectors. The collaborations operate much like independent international laboratories, attracting contributions from around the world. The large numbers of scientists highlights the fact that our endeavors are not single experiments in the classical sense. Instead, they address many distinct experimental goals.

There are approximately 4,000 particle physicists active in the U.S. today. Nearly 80% of them are affiliated with universities. The university commitment speaks to the compelling intellectual nature of our enterprise. The universities attract students into our field and into the rest of science. They allow us to leverage manpower and financial support. They give our field a strong intellectual base and make it a natural arena to integrate research and education. Universities offer us the opportunity to reach far beyond particle physics itself.

A vital component of the high-energy physics program is a strong effort directed toward future facilities. It is a formidable R&D challenge to develop a new generation of accelerators and advanced detectors. We give this program particular attention in this report. The R&D program in high-energy physics pushes the state of the art in many directions and has benefits well beyond our field.

Clearly, a broad range of partnerships is crucial for the health and success of any global, multi-disciplinary science program. In particle physics, international partnerships are becoming increasingly important. Our large collaborations already operate on a global scale. New frontier accelerator facilities are so large and complex that they too will need to be international. Developing and nurturing collaboration and cooperation between countries, and between scientists working in different countries, is essential to the future of the field.

In particle physics, partnerships with other fields have become increasingly important as the boundaries between disciplines become blurred. At an intellectual level, particle physics shares scientific interests with mathematics, nuclear physics, astrophysics, and cosmology, among others. These interconnections extend to a technological level as well, where each field helps drive the others.

Partnerships between agencies have been, and will continue to be, key ingredients in the success of many projects. DOE and NSF have worked together for many years to support the U.S. particle physics program, and they have worked with foreign agencies to carry out international projects. Both agencies should be lauded for the flexibility they have shown toward support of experiments at the interface of particle physics and other disciplines, and for developing partnerships with other agencies such as NASA. These partnerships have been particularly important in the exciting new field of particle astrophysics, which by its nature is highly interdisciplinary.

1.3.1 Our Role in Science Education

Public education is both a responsibility and privilege of our field. We currently engage in a variety of education and public outreach efforts. Our major laboratories host extensive programs for education and outreach. The Lederman Science Center at Fermilab supports K-12 education programs including *Quarks to Quasars* and the Teacher Resource Center. The Particle Data Group at Berkeley is developing *The*

Figure 1.5. QuarkNet Program at Fermilab. Quarknet connects high school students and teachers with universities and laboratories engaged in cutting-edge science.

Particle Adventure, a web-based “interactive tour of quarks, neutrinos, antimatter, extra dimensions, dark matter, accelerators and particle detectors.” QuarkNet is a nationwide effort to partner secondary school teachers and students with university researchers in front line research. Many particle physics groups also participate in the NSF Research Experience for Undergraduates program. Individual physicists at universities and laboratories reach a geographically diverse public through face-to-face contact.

Bolstered by these successes, we believe that as a field we can and should do more in this area. Our field attracts bright students to careers in science and engineering, both at the undergraduate and graduate levels. These students use their technical and scientific training in a broad array of careers, ranging from information technology, microelectronics and medical physics, to finance, national defense and public policy.

We believe we can broaden our impact in K-12 science education through additional direct partnerships with educators. We can offer assistance as states and local districts struggle to improve science education. Increased educational efforts will raise our profile in the community, draw the public into the excitement of our future discoveries, and foster pride in our society's investment in science.

To strengthen the impact of our field on science education, we urge that all current and future large particle physics experiments incorporate project-specific education and outreach programs as part of their mission. Such efforts, linked very closely to the research programs, represent key investments in the future and must be given sufficient priority. More specifically, the level of activity on education and outreach in the field should be doubled, in order to ensure a viable, effective and sustainable program. This extra effort will significantly increase our impact on education and society without adversely affecting our research program.

1.3.2 Our Connections to Science and Society

Harold Varmus, Nobel laureate and former director of NIH, has spoken eloquently on the "interdependence of the sciences" and the necessity of "balance [in support] of the sciences that is essential to progress in all spheres." The revolutionary advances in any area of science are rooted in fundamental discoveries in many other different fields. Varmus notes that "…medical advances may seem like wizardry. But pull back the curtain, and sitting at the lever is a high-energy physicist, a combinatorial chemist or an engineer." Therefore while we conveniently divide science into disciplines and sub disciplines, identifying ourselves as organic chemists or condensed matter physicists, such divisions and self identification can obscure the deeply interconnected nature of all branches of science and obscure the profound impact different disciplines of science have on one another.

The fabric of science, society and scientific achievement is tightly woven, and includes contributions from all the sciences. This was recognized by the US Commission on National Security/21st Century. In the Commission's view, "the inadequacies of our systems of research and education pose a greater threat to US national security over the next quarter century than any potential conventional war that we might imagine.… If we do not invest heavily and wisely in rebuilding these two core strengths, America will be incapable of maintaining its global position long into the 21st century."

What is particularly relevant in the Commission's report is that National Security rests on the strength of our scientific and technological base. It is not a single area or technology that must be fostered, but the entire R&D portfolio that must be maintained to ensure the health, welfare and security of the nation in years to come. R&D and technical developments in particle physics have broad applications, ranging from particle accelerators, to technical developments in electronics and medical imaging, to advanced applications of computing and data handling. Moreover, the sociology of our science, with its international and highly distributed nature, is well-matched to our increasingly global culture.

1.4 Summary

In this report, we articulate the long-term goals of high-energy physics and present a roadmap for the next twenty years. Our roadmap outlines a national program focused on achieving those goals in a worldwide context. The program envisions a variety of efforts, national and international, large and small, that will keep the United States at the frontier of the exploration of nature. In time our discoveries will seed new ideas and technologies that will impact other fields both inside and outside of physics, renewing the cycle of discovery that is the basis of all science.

2. THE PARTICLE PHYSICS ROADMAP

We recommend a twenty-year roadmap for our field to chart our steps on the frontiers of matter, energy, space and time. The map will evolve with time to reflect new scientific opportunities, as well as developments within the international community. It will drive our choice of the next major facility and allow us to craft a balanced program to maximize scientific opportunity.

We recommend a new mechanism to update the roadmap and set priorities across the program. We understand that this will require hard choices to select which projects to begin and which to phase out. Factors that must be considered include the potential scientific payoff, cost and technical feasibility, balance and diversity, and the way any proposed new initiative fits into the global structure of the field.

2.1 Introduction

Particle physics is a quest to explore the frontiers of matter, energy, space and time. We have many tools at our disposal, from forefront accelerators to satellites in space to experiments deep underground. Some tools are available in the U.S.; others are available abroad. Our experimental strategies are determined by our drive to discover new, fundamental physics.

Across the world, our field is in the midst of planning and running a bold array of experimental initiatives, ranging from large experiments and facilities with broad physics programs to small experiments designed to answer more focused questions. Some of these initiatives straddle the boundaries between particle physics, astrophysics, and nuclear physics.

We seek to maintain leadership in particle physics by optimizing our scientific program. As a first step, we present a twenty-year roadmap for U.S. particle physics. The roadmap identifies opportunities in our field and connections between individual projects and our long-range scientific goals. It illustrates our scientific breadth and our interdependence with other areas of science.

The roadmap is a logical continuation of the world-class program in particle physics that the United States has built over the last fifty years. The twenty-year time frame is set by the time required to design and operate large projects in our field. The roadmap will evolve with time to reflect new physics discoveries, new technological breakthroughs, and actual funding levels. New projects can be added when new funding opportunities arise, as through the NSF Major Research Equipment (MRE) program.

We intend for the roadmap to become an integral part of the planning process in our field. It shows decision points for projects, both large and small. It indicates the time frame for decisions on other proposed initiatives, and the opportunity costs associated with our decisions. The roadmap allows us to plan for international collaboration on major

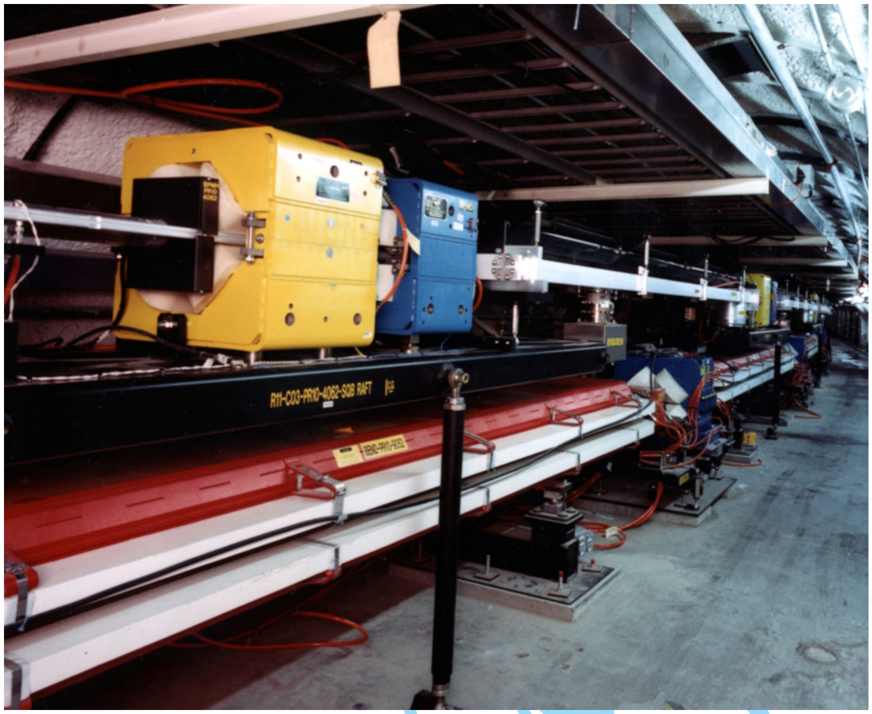


Figure 2.1. The PEP-II Accelerator at SLAC. PEP-II accelerates electrons (bottom ring) and positrons (top ring), colliding them in the BaBar detector.

facilities and experiments. It also invites other fields such as astronomy and nuclear physics to plan jointly with us. A balanced program is necessary for the vitality of our field, and can be achieved if we manage our resources well.

The roadmap lays out the opportunities as we see them now, outlines the options, and indicates when decisions need to be made. Not all projects illustrated on the roadmap can be pursued. Some will have to be sacrificed because of limited manpower and resources in the field. Many difficult choices will have to be made during the years to come. The roadmap will help focus our efforts on the best scientific opportunities.

In this chapter, we recommend creation of a Particle Physics Project Prioritization Panel (P5), charged with prioritizing the mid-scale projects in our field. We believe that prioritization is central to our plan for a diverse, aggressive program of particle physics. The P5 process will help us ensure an optimal program of scientific investigation.

2.2 The Roadmap

The complete roadmap for particle physics is presented in Appendix A. In this section we sketch its main elements. The roadmap lists the physics possibilities that we can see over the next twenty years. However, not all the avenues will be pursued, either here or abroad. The roadmap provides the basis for the difficult choices that will have to be made.

We follow the roadmap with sample scenarios for the U.S. program, presented in section 2.3. These scenarios illustrate the scope and priorities of the program. The actual program will evolve according to the P5 process, described in section 2.4. It will depend on how physics results, funding levels and technological innovations progress over this time period.

2.2.1 The Existing and Near-Term Program

The current world program in particle physics centers on collider, neutrino, and particle-astrophysics experiments, carried out at home, abroad and in space. In the U.S., the Tevatron and PEP-II are foundations of our on-going program. These recently completed facilities are pushing the energy and luminosity frontiers. At the Tevatron, the CDF and DØ experiments are pursuing a rich physics agenda that includes the search for the Higgs and supersymmetry, studies of CP violation, and the first detailed examination of the top quark. At PEP-II, the BaBar experiment is helping to explain the mysteries of quark mixing and CP violation through studies of bottom quark decays. These experiments will make important new discoveries through the end of this decade.

Studies of neutrinos, heavy quarks, and the weak and strong interactions are other key components of the U.S. program. These experiments offer an important window to physics at the electroweak scale and beyond. In addition, experiments in particle astrophysics contribute a significant and growing part of our program. They represent a new direction that is expanding the scope of our field.

2.2.2 The Energy Frontier

The worldwide particle physics community has reached consensus that the next major step for the field must be the thorough exploration of the electroweak energy scale. Discoveries in that domain will be essential for progress on our long-range goals of ultimate unification, hidden dimensions and cosmic connections.

These discoveries will begin at the CERN LHC, the first accelerator to directly access this TeV energy scale. The LHC will collide protons against protons at an energy of 14 TeV, seven times that of the Tevatron. American particle physicists are making important contributions to the accelerator and the ATLAS and CMS experiments. The LHC will revolutionize our field when it begins to operate during the second half of the decade. The United States provides one of the largest and most influential groups within this international collaboration.

Over much of its history, particle physics has relied on different types of accelerators. Discoveries at one machine point the way to discoveries at others. Such synergies maximize progress across the field. On the energy frontier, one can point to the recent productive interplay between the Fermilab Tevatron, a hadron collider, and LEP and SLC, electron-positron colliders at CERN and SLAC.

Looking to the future, we have no doubt that the synergy will continue. There is now a worldwide consensus that the LHC and a high-energy, high-luminosity, electron-positron linear collider are both essential to discover and understand the new physics at the TeV scale. This conclusion is reflected in reports from the Asian and European Committees on Future Accelerators, as well as in the recommendations of this subpanel.

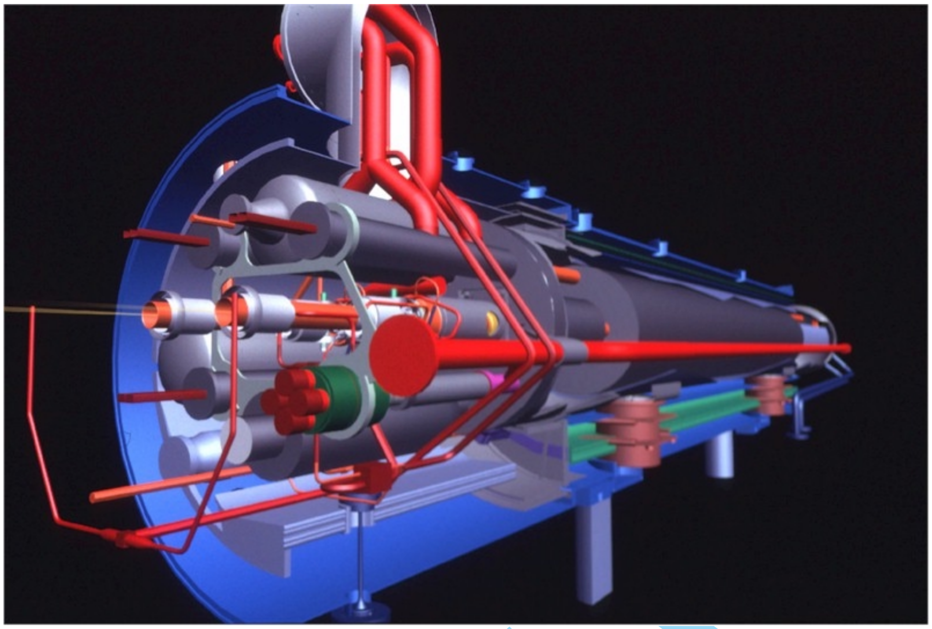


Figure 2.2. LHC Magnet. The LHC will revolutionize our field when it begins operation during the latter half of this decade.

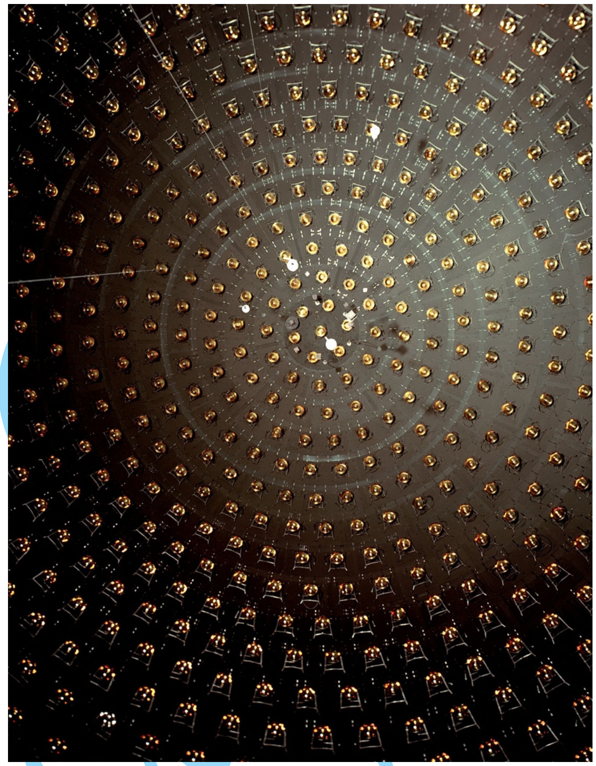
Many years of accelerator R&D have brought us to the point where it is now possible to propose construction of a linear collider. More work is necessary to choose a final design and to determine the construction cost. However, we already know that the scope, cost, and complexity of the linear collider are such that the effort must be international from the start. The world community recognizes this fact, and is starting to create an international collaboration to manage the design, construction and operation of this powerful accelerator.

Our highest priority is full participation in this exciting new facility, wherever in the world it is sited. Its science will be excellent, and its technology will benefit our field and enrich society at large. In chapter 3 we make the physics case for the linear collider, and in chapter 4 we argue that the United States should bid to host this international facility. We discuss the U.S. role as host country and outline the substantial benefits that a linear collider will bring to the U.S.

While the world particle physics community has reached widespread consensus on the linear collider, there is also broad agreement that we are not at the end of our journey. The ultimate goals of particle physics require an energy frontier beyond the LHC and the linear collider. Exciting plans are underway to reach the far-energy frontier using a very large hadron collider or a multi-TeV electron or muon collider. Vigorous accelerator R&D on a worldwide basis is necessary to realize colliders beyond the TeV scale.

2.2.3 Theoretical Physics

A strong program of theoretical research is an essential part of a healthy program in particle physics. Formal "top-down" and phenomenological "bottom-up" approaches are both necessary to find the underlying theory of nature. Theory can inspire new areas of



The Soudan Mine **The MiniBooNE Detector**

Figure 2.3. Fermilab Neutrino Experiments. Accelerator-based neutrino oscillation experiments will study neutrino oscillations with the MINOS detector, in the Soudan mine, and with MiniBooNE, on the Fermilab site.

experimental and observational work. In many cases, theoretical tools are crucial in extracting underlying explanations and interpretations from measurements. Full exploitation of our experimental physics program requires active theoretical participation. We must provide a level of support for theory that is commensurate with its vital role and allows for diversity in formal and phenomenological efforts.

2.2.4 Lepton Flavor Physics

During the past decade, we have made substantial progress in understanding the masses and mixings of neutrinos. We still have much to learn. More comprehensive studies with both neutrinos and charged leptons are an essential component of our future program.

A new generation of accelerator-based neutrino oscillation experiments is a key element of the future particle physics program. Such experiments will require the development of one or more intense neutrino sources. A number of schemes are being studied worldwide. A source could be built in the United States, or in Europe or Asia with U.S. participation.

An intense neutrino source will require a new (or upgraded) proton driver capable of delivering one or more megawatts of beam power. That driver could also be used to provide beams of muons and kaons for rare decay studies. It might also be a first step towards a future very high intensity muon storage ring/neutrino factory and eventually a muon collider. From today's vantage point, we cannot say how the science will unfold. The future neutrino program will be shaped by the results from the present generation of experiments.

There are other important future directions for neutrino physics, many of which could benefit from a very deep underground site. Worldwide, we expect greatly improved neutrino oscillation studies using solar, atmospheric and reactor sources. In addition, neutrinoless double-beta decay experiments can measure other important neutrino properties, and possibly determine whether neutrinos are their own antiparticles.

2.2.5 Quark Flavor Physics

After a decade of intensive effort, we are closing in on a detailed understanding of the mass, mixing, and CP violation in the quark sector. An excellent series of experiments is being proposed to make use of strange, charm, and bottom hadrons. While we cannot do all these experiments in the U.S., it is important that we participate in some.

The future program in B physics will focus on precision studies of CP violation, mixing and rare decays. Long-term opportunities include a very high luminosity electron-positron collider operating at the $\Upsilon(4S)$, as well as complementary hadronic B experiments. The results of present experiments will guide us whether and how to pursue these studies.

2.2.6 Very Rare Processes

Very rare processes provide additional probes of quark and lepton flavor physics. They can offer important insights into the nature of physics beyond the reach of accelerators. For example, we do not yet know if the proton is absolutely stable, and observing its decay would have a profound impact on our understanding of particle physics. Studies of highly suppressed K meson decays, and comparisons between measurements made in the K and B systems, would allow new tests of the quark flavor structure.

A worldwide collaboration has begun to develop the design for a next-generation proton decay experiment. Assuming that an affordable and credible design is reached, it is likely that a large proton decay detector will be proposed somewhere in the world, and that American physicists will want to participate in its construction and utilization.

A large underground proton decay detector would also serve as a major neutrino telescope. In addition, it might be used as a neutrino detector for future experiments using a bright neutrino source or a neutrino factory. (See section 2.2.4.)

The observation of lepton flavor mixing would have other important consequences for unified theories. A proposed experiment to detect muon-electron conversion is sensitive to a substantial range of new physics, particularly supersymmetry-based models of lepton-flavor violation.

Finally, studies of highly suppressed K meson decays, and comparisons between measurements in the K and B systems, allow new tests of the quark flavor structure, and provide a powerful probe for new physics in the quark flavor system.

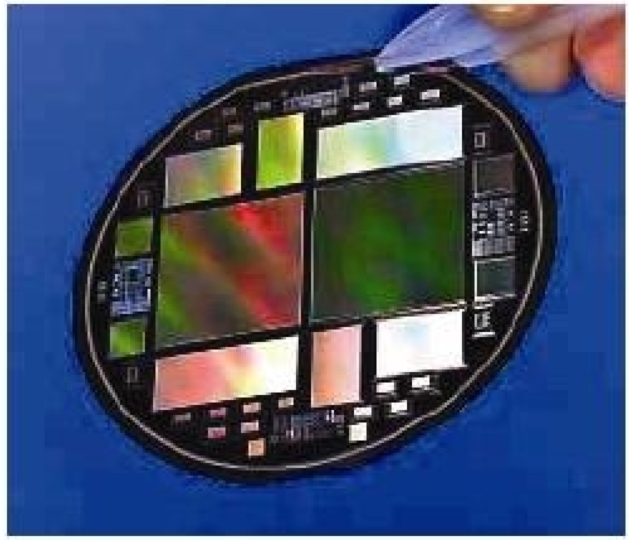


Figure 2.4. The SNAP Dark Energy Detector. SNAP requires R&D to develop a detector with one billion CCD's.

2.2.7 Cosmology and Particle Physics

A complete picture of how the universe formed and evolved requires a variety of experimental and theoretical inputs, including experiments studying dark energy and dark matter, the microwave background radiation, and the large-scale structure of the universe. These experiments will be carried out by the astronomical and particle physics communities.

Particle physicists are currently searching for particle dark matter in the galactic halo. Additional projects may be proposed in the future. Dark matter searches are complemented by the search for supersymmetry at the Tevatron, LHC and linear collider, since the lightest superparticle is a favored candidate for dark matter.

Several possible approaches to studying dark energy are under development. One uses Type Ia supernovae. Another uses measurements of the large-scale distribution of dark matter from observations of weak gravitational lensing. It is likely that several types of approaches will be necessary to fully understand the nature of dark energy.

2.2.8 High-Energy Particle-Astrophysics

Astrophysical sources are capable of accelerating particles to energies well beyond what we can produce here on Earth. Experiments that detect very high-energy particles from space are exploring the physics of extreme conditions in the universe. For example, gamma-ray bursts, among the most powerful explosions since the Big Bang, may be sources of ultrahigh-energy neutrinos and cosmic rays.

High-energy particle-astrophysics detectors also probe physics beyond the standard models of particle physics and cosmology. Gamma ray and neutrino telescopes are sensitive to supersymmetric galactic dark matter, and ultrahigh-energy cosmic rays may result from unusual particles produced in the early universe. A variety of efforts are underway in this field. New proposals will likely emerge in the future.

2.3 Scenarios for the Future

In the previous section, we outlined the major elements of the worldwide program in particle physics over the next twenty years. In this section, we present two possible scenarios for the U.S. program. The scenarios are distinguished by whether the linear collider is built in the United States (onshore) or in Europe or Asia (offshore). These scenarios illustrate the types of physics programs that can be carried out over the next twenty years.

The scenarios represent programs that contain a major U.S. commitment to the linear collider, completion of ongoing experimental programs, full exploitation of experiments and facilities under construction, including the LHC, plus a very selective evolving set of smaller initiatives chosen from the roadmap and aimed at the important goals of the field. The eventual program will depend on the physics results, the funding levels, and the technological innovations that occur over this time period. It will also depend on the choices we make along the way.

Although the cost of the linear collider is uncertain so early in the project, there is a detailed estimate for the TESLA project, proposed for the DESY laboratory in Germany, as well as a preliminary cost estimate for the NLC, a project being formulated by scientists in the United States and abroad. Continued R&D and value engineering are needed to refine the technology and fix the cost.

Our subpanel used these estimates to make a model, based on the following assumptions. We assumed a total project cost of about $5-7B for the collider, in FY 2001 dollars, if it is built in the U.S. We estimated that $1-2B of the cost could be supported through sacrifice and redirection of the present U.S. program, taking advantage of resources already available in our laboratories and universities. We also estimated that another $1.5-2.5B, up to about one-third of the cost, could be contributed from non-U.S. sources. If the linear collider is built offshore, we assumed that sacrifice and redirection of the present U.S. program would also be required.

These cost assumptions are reflected in our long-range plan. We used them to study a variety of scenarios in which a linear collider is built either onshore or offshore. We integrated the costs into a time-phased program that meets the scientific goals we outline in this report. In that way we estimated the resources required for a lean, but intellectually strong, U.S. particle physics program over the next twenty years. We found that both scenarios, to be fully realized, need some new resources beyond a constant level of effort.

Below we outline the main components of two sample scenarios:

2.3.1 Scenario with an Onshore Linear Collider

This scenario ensures the United States a leadership position in particle physics. The U.S. hosts one of the forefront scientific facilities of the 21st century, and selectively participates in other important experiments in the field. The program includes:

- An electron-positron linear collider in the United States, with the U.S. contributing about 2/3 of the total project cost;
- Participation in the LHC and its possible upgrades;
- A program of neutrino physics sited offshore with significant U.S. participation, possibly coupled with a proton decay detector;
- Significant participation in a joint agency effort to address key cosmological questions of interest to particle physics;
- A continued program of flavor physics in the U.S., tapering off by the end of this decade, but possibly continuing offshore;
- Continued participation in particle astrophysics by selective pursuit of new opportunities through the twenty year timeline;
- Continued accelerator R&D aimed at future accelerator facilities.

This scenario requires a net increase of about 30% in total funding to the field over twenty years.

2.3.2 Scenario with an Offshore Linear Collider

This scenario includes significant participation in an offshore linear collider, together with the LHC, and a vigorous and diverse domestic program. It includes:

- An electron-positron linear collider in Europe or Asia, with the U.S. contributing a significant share of the total project cost;
- Participation in the LHC and its possible upgrades;
- A program of neutrino physics sited in the U.S., with significant international participation, possibly coupled with a proton decay detector;
- A focused accelerator R&D program aimed at future accelerator facilities, such as a very large hadron collider or a multi-TeV lepton collider;
- Significant participation in a joint agency effort to address key cosmological questions of interest to particle physics;
- A continued program of flavor physics in the U.S., with possible opportunities extending through the twenty year timeline;
- Continued participation in particle astrophysics by selective pursuit of new opportunities through the twenty-year timeline.

This scenario requires a net increase of about 10% in total funding to the field over twenty years.

These two sample scenarios represent strong and diverse U.S. programs over the next twenty years. The scenarios are faithful to the scientific priorities presented in this report. In both scenarios, realization of the linear collider will require significant sacrifices in other parts of the program, and we will not be able to pursue many exciting opportunities. Our scenarios assume, in either case, very substantial contributions from the international community, not just to the linear collider but to other initiatives as well. The onshore scenario provides the United States with a flagship international laboratory for fundamental physics and ensures U.S. leadership in one of the forefront scientific activities of the 21st century. The offshore scenario contains a viable onshore program of important smaller initiatives. In both scenarios, sacrifices will need to be made.

In addition, we have analyzed scenarios at a constant level of effort. Under such scenarios, the United States can play an important but selective role in high-energy physics, but not in the leadership capacity advocated here. The choice of experiments will depend on results from current projects. In all scenarios, the U.S. program will be determined by the process set forth in the next section of this report. Guidance for the near-term future is given in section 2.5.

2.4 Setting Priorities and Making Choices

In this chapter, we have described an exciting program of experiments in the United States and abroad, together with a roadmap to guide the program in the years to come. We recognize that we cannot pursue all avenues that we identify. Important constraints affect our planning, including limited human and financial resources and the need to dovetail our program with those of other countries and other fields closely connected to our own. Proper prioritization is essential to obtain the highest possible return on tax dollars. Difficult decisions will need to be made to allocate our resources wisely.

Projects in accelerator-based particle physics vary greatly in scale. Smaller projects (less than about $50M) can usually be accommodated within laboratory operating budgets, under the purview of the laboratory director in consultation with the laboratory physics advisory committee, HEPAP, and the agencies. Very large projects (more than about $500M) must be truly international, and require the consensus of the worldwide particle physics community. Such projects are necessarily infrequent and must be decided on a case-by-case basis. Medium-scale projects (with total project costs between $50M and $500M), which require significant resources, make up a major part of the U.S. program. They must be evaluated in competition with each other, in the context of the overall constraints and goals of our field. We believe that the U.S program will greatly benefit from a new mechanism to assess and prioritize these mid-scale initiatives.

We propose the formation of the Particle Physics Project Prioritization Panel (P5), charged with carrying out this important task. In what follows we outline general guidelines for how the panel will work. The detailed plan must be worked out by HEPAP, in coordination with agencies and laboratories. We believe prioritization is central to our plan for a diverse, aggressive program of particle physics.

We envision a panel made up of scientists from particle physics, accelerator physics, and astrophysics. Panelists will be chosen from the university, laboratory and international communities. The panel will meet on a regular basis to review mid-scale projects that have a significant impact on the particle physics program. Only projects that have been successfully peer reviewed will be considered. The agenda for the panel should be set by the agencies in consultation with HEPAP.

The main purpose of the panel will be to advise HEPAP and the agencies on the prioritization of these projects. The priorities should reflect physics quality (opportunity, reach, and uniqueness) in the context of the overall roadmap, together with the balance of projects and resources in the particle physics program and other programmatic concerns. Where relevant, the panel should consider proposals in the context of the international particle physics community, and in relation to the programs and advisory mechanisms of related fields, such as nuclear physics, astrophysics, and cosmology. In addition, and where relevant, the panel should compare competing projects that have similar physics goals and reach.

We believe that the P5 process will play an important role in helping HEPAP and the agencies plan for the future in the context of the long-term roadmap. It will also provide an important link between the essential project review mechanisms already in place and the broader considerations of the overall particle physics portfolio.

2.5 Near Term Guidance

In Appendix A, we describe the large and midscale projects that are presently under consideration on our roadmap. We show in some detail how they fit into the scientific goals of the field. We indicate, where possible, the decision points in the projects.

PROJECT	GOAL	STATUS
LHC Upgrade	Energy Frontier	Referred to P5
Proton Driver	Quark/Lepton Flavor Physics	Referred to P5
BTeV	Quark Flavor Physics	Initial guidance by subpanel
CESR-c	Quark Flavor Physics	Initial guidance by subpanel
Super B Factory	Quark Flavor Physics	Referred to P5
Proton Decay	Very Rare Processes	Referred to P5
CKM	Very Rare Processes	Referred to P5
RSVP	Very Rare Processes	Initial guidance by subpanel
NUSL	Underground Science	Initial guidance by subpanel
SNAP	Cosmology and Particle Physics	Initial guidance by subpanel
IceCube	High-Energy Particle-Astrophysics	Initial guidance by subpanel

Table 2.1: Status of Mid-Sized Projects

We have recommended a new method for prioritizing the opportunities that will arise during our twenty-year plan. However, the program requires immediate guidance on some of the opportunities before us. For those projects that require such guidance, we make recommendations from the perspective of the long-range plan presented in this report. The status of mid-sized projects is indicated in Table 1.

2.5.1 The Current Program

The Tevatron and PEP-II are central pieces of our current program. The CDF and DØ experiments at the Tevatron provide the only opportunities in the world for discovery of the Higgs until the start of the LHC. The BaBar experiment at PEP-II is studying quark mixing and CP violation through bottom quark decays.

We recommend that we capitalize on previous investments by fully utilizing the facilities and experiments in our current program. We also urge that projects under construction, such as LHC, NuMI/MINOS and GLAST, be completed as scheduled.

2.5.2 The Near-Term Future

There are six projects currently under consideration for which immediate guidance is required. They are BTeV, CESR-c, ICECUBE, NUSL, RSVP and SNAP R&D. These projects are discussed in some detail in Appendix A.

Our recommendations are as follows:

- The BTeV experiment is designed to probe for new physics at the electroweak scale by searching for inconsistencies in the CKM description of bottom quark transitions. It will carry out precision studies of CP asymmetries and flavor-changing processes in the B meson system. Through its cutting edge detector technology, BTeV's physics reach exceeds that of other planned experiments in some of the important measurements of the B system.

  BTeV would normally be a candidate for P5 evaluation. With a total project cost of $250M, it has significant impact on the overall HEP budget and programmatic implications for the future of the Tevatron. Its science overlaps but does not duplicate that of LHC-b. If there were time, P5 would rank BTeV in relation to the rest of the program.

  The BTeV collaboration has been waiting over a year for a funding decision. We cannot ask BTeV to wait for the start of the P5 process, so our subpanel must offer guidance. Budget constraints and programmatic concerns made it impossible to fund BTeV as a line item in FY 2002. Our projections show that we cannot fund BTeV as a line item in the near future. Therefore, despite BTeV's attractive physics program, we regret that we cannot recommend funding BTeV as a line item at this time.

- The CLEO collaboration has proposed a program using electron-positron annihilation in the 3 to 5 GeV energy region, optimized for physics studies of charmed particles. The conversion of the CESR ring for low energy running would cost about $5M. The subpanel endorses CESR-c and recommends that it be funded.

- RSVP aims to conduct two experiments at Brookhaven to measure extremely small symmetry violations in muon conversion and kaon decay. The National Science Board has approved the $115M RSVP proposal as an MRE. The subpanel endorses the physics goals of RSVP and believes it is a timely opportunity to pursue.
- Construction of a National Underground Science Laboratory at the Homestake Mine has been proposed to NSF. A proposal for a laboratory under the San Jacinto mountain has been submitted to DOE and NSF. These proposals are motivated by a very broad science program, from microbiology to geoscience to physics. Construction of a national underground laboratory is a centerpiece of the NSAC Long Range Plan.

  We believe that experiments requiring very deep underground sites will be an important part of particle physics for at least the next twenty years, and should be supported by the high-energy physics community. Particle physics would benefit from the creation of a national underground facility.
- The SNAP experiment aims to probe dark energy by measuring the expansion of the universe as a function of time from observations of Type Ia supernovae. The SNAP team has developed an instrument concept and has requested R&D funding to develop a full instrument design, as well as to estimate of the total project cost. We endorse R&D funding to carry out these tasks.

  The project is expected to cost approximately $400M, including launch. We recommend that the full SNAP project, if approved, include significant NASA participation in the construction and launch of the instrument, in partnership with DOE and NSF. A decision on whether to build SNAP is expected in 2004-5.
- IceCube is a detector with unprecedented sensitivity to astrophysical sources of TeV and PeV neutrinos, proposed to be built in the South Pole ice. The National Science Board has approved the $240M IceCube proposal as an MRE. We endorse the scientific goals of IceCube as an example of a mutually beneficial cross-disciplinary effort between astrophysics and particle physics.

2.6 Summary

In this chapter, we presented a roadmap for the U.S. particle physics program, to make clear the connections between experimental projects and scientific goals, and to guide our decision-making and prioritization process.

The first step on our roadmap is full exploitation of the facilities and experiments in our current program, as well as those presently under construction. The next step is the thorough exploration of the TeV energy scale. This exploration will require a high-energy, high-luminosity electron-positron linear collider in addition to the CERN LHC. Our highest priority is full participation in this exciting new facility. The ultimate goals of particle physics are certain to require pushing the energy frontier beyond the LHC and linear collider. It is important that accelerator studies for these future possibilities be carried out.

Many of our crucial scientific questions require new initiatives involving small and medium scale projects. We must pursue some of these projects if we are to have a vital and balanced scientific program, but we cannot pursue them all. We described two possible scenarios for the evolution of the U.S. particle physics program over the next twenty years. Each scenario includes a very selective set of initiatives aimed at important goals of the field. The actual program will depend on the physics results, funding levels and technological innovations that occur over this time period.

In recognition of the financial and human constraints that necessarily affect our planning, we recommended that a new prioritization panel, P5, be implemented to set priorities for major initiatives across the program. The time scale for some opportunities, however, is more immediate. We gave guidance, where appropriate, in those cases.

3. THE LINEAR COLLIDER: A MAJOR NEW INITIATIVE AT THE ENERGY FRONTIER

We recommend that the highest priority of the U.S. program be a high-energy, high-luminosity, electron-positron linear collider, wherever it is built in the world. This facility is the next major step in the field and should be designed, built and operated as a fully international effort.

We also recommend that the United States take a leadership position in forming the international collaboration needed to develop a final design, build and operate this machine. The U.S. participation should be undertaken as a partnership between DOE and NSF, with the full involvement of the entire particle physics community. We urge the immediate creation of a steering group to coordinate all U.S. efforts toward a linear collider.

3.1 Introduction

In the previous chapters, we laid out a balanced twenty-year roadmap for elementary particle physics, involving both major new facilities and smaller experiments targeted at more specific goals. The centerpiece of that roadmap is the thorough exploration of the TeV energy scale because that is the crucial next step in our quest to discover ultimate unification, hidden dimensions, and cosmic connections.

This work will begin, but not end, with the CERN LHC. There is now a worldwide consensus that the LHC and a linear collider are both essential to discover and understand the new physics at the TeV scale, and that a coherent approach, exploiting the strengths of both machines, will maximize the scientific contributions of each. In this chapter we make the case for a high-energy, high-luminosity electron-positron linear collider.

In a linear collider, intense beams of electrons and positrons are accelerated to near the speed of light and then brought into collision under tightly controlled conditions. The technical challenges to build and operate a linear collider are immense, and were considered at or beyond state-of-the-art just a few years ago. However, the challenges were met through the imagination and ingenuity of scientists and engineers the world over. The success of this R&D program has brought the world high-energy physics community to the point where it is ready to move towards construction of an electron-positron linear collider.

The scientific case for the linear collider motivates a strategy of building the machine to initially operate at an energy of about 500 GeV, to explore the Higgs and related phenomena, and then increasing the energy to 800-1,000 GeV, to more fully explore the TeV energy scale. The synergy between the LHC and the linear collider argues for an early start. The linear collider should be ready to begin construction in 2005. Results from 500 GeV operations and from the LHC would influence the timescale for converting to higher energies.

We would like to see the international linear collider project provide for construction of an 800-1000 GeV machine that would begin operation at 500 GeV. Plans for a linear collider have been developed through a collaborative international effort involving major laboratories in the United States, Germany and Japan. The process of internationalization should be continued and strengthened so that a fully international project can be created, one in which all partners are assured of full ownership and participation. A number of the important principles that should guide this process are described in Chapter 4.

We strongly urge DOE and NSF to begin working with our partners around the globe to form the international collaboration that will carry the project forward. As a first step, we recommend the creation of a Linear Collider Steering Committee to coordinate U.S. efforts towards building the machine. The Committee will work with our partners, at home and abroad, to build a robust technical, political and managerial program for the linear collider.

3.2 The Case for the Linear Collider

In particle physics, an intense worldwide effort has led to the discovery of the basic building blocks of nature. We now know that all matter is made of quarks and leptons, and that the forces between them arise from the exchange of other particles known as gauge bosons. We have developed a mathematical theory – the Standard Model of particle physics – that describes the world of elementary particles with unparalleled precision.

There is no doubt that the Standard Model will remain one of the lasting achievements of the 20th century. However, the theory is not an end in itself. Moreover, the Standard Model is incomplete: it is mathematically inconsistent at the TeV scale, an energy almost ten times higher than we have achieved to date. At this energy scale, new physics must appear.

According to our present understanding, the new physics is likely to include a Higgs boson. As emphasized in chapter 1, discovery of this long-sought particle is the next major goal at the energy frontier. We are missing a crucial piece of the puzzle – one that is necessary to understand how the elementary particles get their mass. But, whether the Higgs exists or not, we know that new physics lies just over the horizon, well within our reach.

During the next decade, we will carry out experiments that will begin to probe the TeV scale, first at the Fermilab Tevatron and later at the CERN LHC. These experiments are likely to discover the Higgs, or whatever takes its place. But as in any scientific enterprise, the first signs of discovery are likely to be murky. To reach our ultimate goals, we need a clear and coherent picture of physics at the TeV scale. If we find a new particle, we need to know whether it is a Higgs or something else. Does it have spin zero and even parity, as required for a Higgs boson? Does it generate masses for the W and Z, and for the quarks and leptons? Does it generate its own mass? Does a Higgs field permeate the universe? We need to answer these questions before we can say that we have discovered a Higgs particle and that it is responsible for the origin of mass.

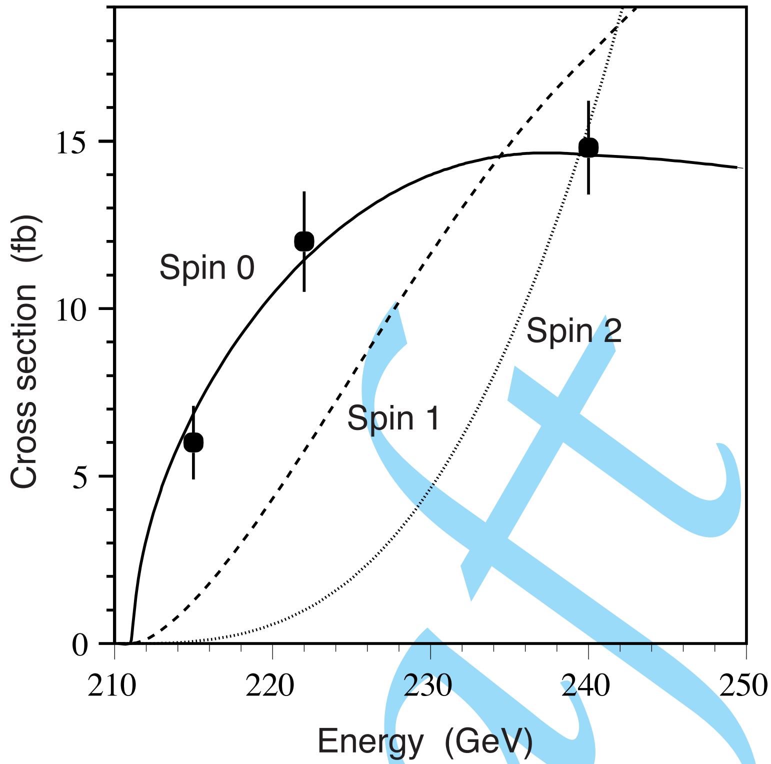


Figure 3.1. The Spin of the Higgs. Experiments at a linear collider can measure the spins of new particles. A Higgs boson has spin zero, unlike any fundamental particle discovered to date. From Dova, Garcia-Abia and Lohmann, LC-PHSM-2001-055.

During the past few years, a series of studies has convincingly demonstrated that a linear collider is necessary to answer these questions. The linear collider accelerates electrons and positrons, essentially structureless particles that interact through precisely calculable weak and electromagnetic interactions. Because of this, a linear collider can unambiguously determine the spins and quantum numbers of new particles. Cross section and branching ratio measurements are also straightforward and can be compared to expectations for underlying new physics. Electron beam polarization can be used to distinguish electroweak quantum numbers and measure important mixing angles. The point-like probes of an electron-positron collider enable precision measurements that expose crucial details of new physics. These facts underlie strong endorsements from the Asian and European Committees for Future Accelerators, from the U.S. high-energy physics community during the 2001 Snowmass workshop, and from this subpanel in this report.

International studies demonstrate that the LHC and the linear collider are both essential tools to uncover the physics of the TeV scale. Experiments at the two machines will show how the electromagnetic and weak forces unify into a single electroweak force. They will reveal the mechanism by which the Higgs gives mass to the elementary particles. If there is no Higgs, experiments at the two machines will discover what takes its place. Taken together, discoveries from the two machines will revolutionize our presently limited understanding of physics at the TeV scale.

Higgs Physics

The LHC and the linear collider can unravel the physics of the Higgs:

- After a candidate Higgs particle is discovered, it is essential to measure its spin. A Higgs particle must have spin zero – or else it is not the Higgs! The LHC can determine the spin of a Higgs particle if its decay into *ZZ* has sufficient rate, while the linear collider can measure the spin of any Higgs it can produce. Since precision data from FNAL, SLAC and CERN point to a low Higgs mass, the linear collider is likely to play a crucial role in Higgs physics. This is illustrated in Figure 3.1, which illustrates how the process $e^+e^- \to HZ$ can be used to measure the spin of a 120 GeV Higgs particle. The error bars in the figure are based on $20\ \text{fb}^{-1}$ of luminosity at each point.

- The Higgs couplings must be precisely measured – some to the few percent level – to determine whether a candidate Higgs is responsible for generating mass. The LHC will measure *ratios* of Higgs couplings to the top quark, *W* and *Z* bosons, and a combination of $\gamma\gamma/gg$ states, often under additional assumptions. The LHC and the linear collider, working together, can determine the *magnitudes* of these and other couplings very precisely, and with fewer model assumptions. Figure 3.2 shows the excellent precision with which a linear collider could measure the branching fractions of a 120 GeV Higgs, with 500 fb^{-1} integrated luminosity.

- If there are multiple Higgs particles, as supersymmetry predicts, some might escape discovery at the LHC. The linear collider can find new Higgs particles up to their kinematic limits. With the precision contributed by the linear collider, measurements of the quark and lepton couplings may reveal the presence of additional Higgs particles.

- Finally, precision measurement of the Higgs trilinear self-coupling – crucial to a full understanding the dynamics of electroweak symmetry breaking – can only be performed at a high-luminosity linear collider. The self-coupling of a 120 GeV Higgs can be measured to about 20% accuracy in a 500 GeV linear collider with an integrated luminosity of 1000 fb^{-1}. For larger Higgs masses, higher machine energies are necessary to reach this level of accuracy.

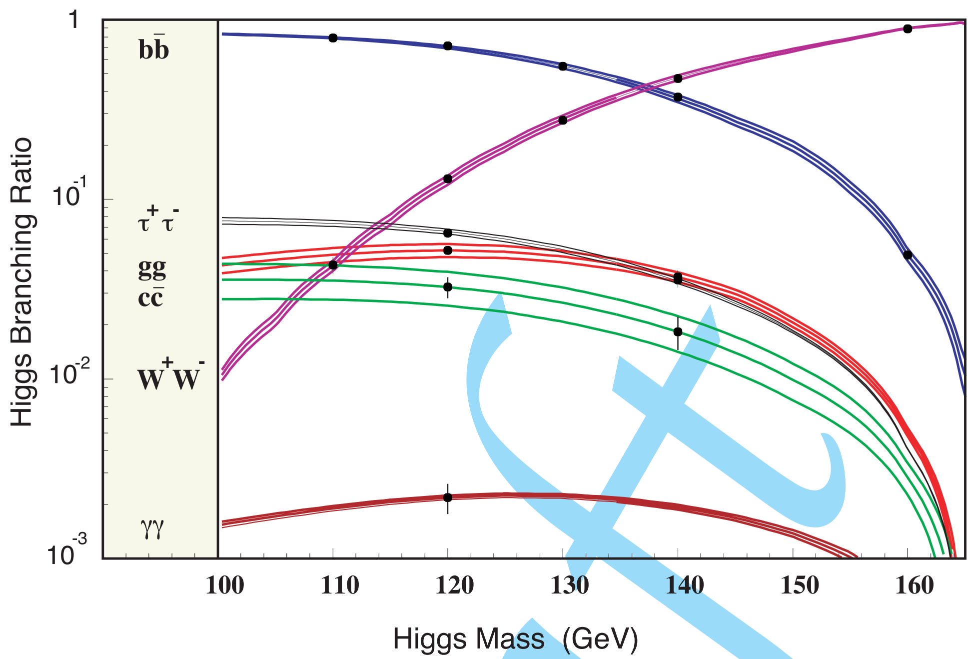


Figure 3.2. Decays of the Higgs. A high-luminosity linear collider can measure Higgs boson couplings to see whether the Higgs particle is responsible for the origin of mass. From Battaglia, hep-ph/9910271.

Our goals – ultimate unification, hidden dimensions, and cosmic connections – all point to new physics at the TeV scale. Most particle physicists expect that the Higgs will be accompanied by other new physics. Whether new particles, new forces or new dimensions, we expect the TeV scale to be fertile ground for discovery.

For example, most particle theorists believe that electroweak unification is the first step towards the ultimate unification of all forces and matter. Experiments at the LHC and the linear collider will point the way. Precise measurements of forces and particles at the linear collider may reveal that the electroweak force is unified with the strong nuclear force. That discovery would have profound consequences, including the prediction that protons are unstable and eventually decay.

Present-day experiments already hint at another new unifying principle, called supersymmetry. Supersymmetry provides a deep connection between matter and the forces of nature, through a new quantum dimension that extends our very notions of space and time. In more practical terms, supersymmetry predicts that every known particle has a supersymmetric partner, or superparticle, waiting to be discovered at the TeV scale. If supersymmetry is correct, the LHC and linear collider will be needed to discover and understand a whole new world of superparticles.

For supersymmetry to be verified, we must do more than find new particles. Precision measurements from the linear collider will be needed to test whether the superparticles

New Dimensions

At the TeV scale, theories predict that it may be possible to move into new space-time and/or quantum dimensions. The LHC and the linear collider have the potential to discover and map out these new dimensions of our universe. The linear collider would allow us to determine the quantum numbers of the particles that move in these new dimensions.

- New space-time dimensions might be found by studying the emission of gravitons into the extra dimensions, together with a photon or jets emitted into the normal dimensions. Or, they might be revealed through indirect effects from the exchange of gravitons in these dimensions. Figure 3.4 shows the cross section needed to produce extra-dimensional gravitons, in association with ordinary photons, at the linear collider. Measurements at different beam energies can be used to determine the number and size of the extra dimensions. The plot assumes 500 (1000) fb^{-1} of luminosity at 500 (800) GeV, together with beam polarization.

- Quantum dimensions are at the heart of supersymmetry. The LHC is ideal for discovering particles that couple through the strong interaction, such as the superpartners of quarks (squarks) and gluons (gluinos), as well as superparticles that appear in their decays.

- All the superparticle masses and couplings can be precisely measured at a high-energy linear collider, with few model assumptions, provided they can be produced. Some superparticles are expected to be in range of a 500 GeV machine, but exploration of the full spectrum requires at least 800-1000 GeV. Knowledge of the entire spectrum of superparticles is essential to discovering the new forces in nature that control supersymmetry breaking. These measurements require enough energy to produce the superparticles, high integrated luminosity (about 1000 fb^{-1}), and high beam polarization.

- The linear collider would allow us to establish that the superparticles have the same interactions as their Standard Model counterparts, and that their spins differ by one-half. Precision measurements of gaugino-sfermion-fermion couplings are crucial tests of supersymmetry; 1–10% deviations from the tree-level predictions open a window on very high masses.

- In many supersymmetric theories, gaugino masses unify at the same scale as the gauge couplings. The LHC and the linear collider can test this hypothesis. The LHC will measure the gluino mass; the linear collider will provide precision mass determination for the superpartners of electroweak gauge and Higgs bosons. These masses can then be extrapolated to high energies, as shown in Figure 3.3. The figure shows that the linear collider's precision is necessary to learn whether gaugino masses unify at the same scale as gauge couplings – an important clue to new physics.

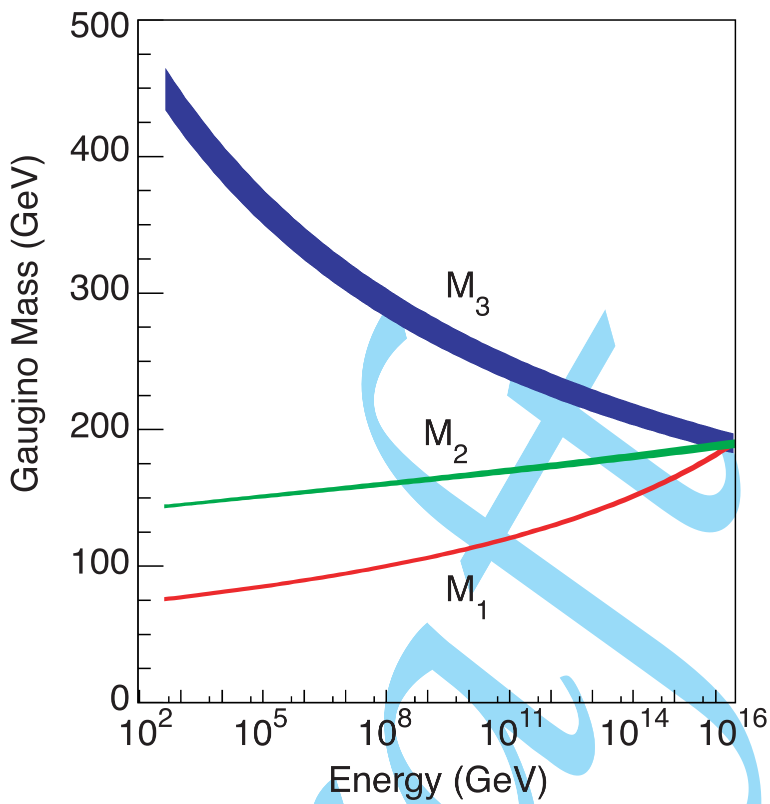


Figure 3.3. Quantum Dimensions. Discovery of gaugino mass unification requires precision measurements from the LHC (M_3) and the linear collider (M_1 and M_2). From Blair, Porod and Zerwas, hep-ph/0007107.

have the spins and couplings dictated by supersymmetry. Precision measurements will probe the mechanism of supersymmetry breaking, and shed light on the unification of the superparticle masses. The linear collider will allow us to discover the deep connections between supersymmetry and ultimate unification, moving us closer to Einstein's goal of unifying gravity with the other three forces.

The linear collider is a powerful instrument to probe the hidden dimensions of space-time. Some theoretical explanations of electroweak unification involve new spatial dimensions hidden from the everyday world. Particles moving in these dimensions give rise to observable effects at the TeV scale. The LHC can find hidden dimensions; the linear collider can map their nature, shapes and sizes. For example, if gravitons travel in a warped extra dimension, the linear collider can demonstrate that they have spin two, as expected. Even if the hidden dimensions are not directly accessible, precision measurements at the linear collider can look for their indirect effects on TeV physics. The discovery of extra dimensions would be an epochal event in the history of science.

What is the dark matter that pervades the universe? Many models of TeV physics contain new particles that could do the job. For example, the dark matter might very well be neutralinos, stable neutral superparticles predicted by supersymmetric theories.

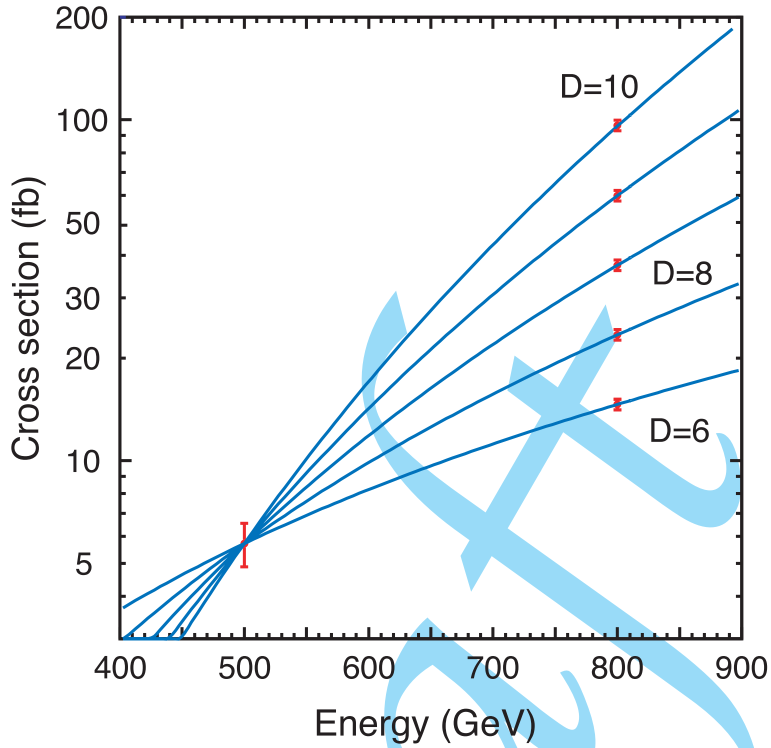


Figure 3.4. Extra Dimensions. The linear collider can measure the number (D) of space-time dimensions, using events in which particles disappear into the extra dimensions. From Wilson, LC-PHSM-2001-010.

Measurements at the linear collider will allow us to develop a predictive theory of this dark matter. These measurements would push our detailed knowledge of the early universe back to a trillionth of a second after the Big Bang.

The crucial importance of a high-luminosity linear collider, covering the energy range 500 GeV to 800-1000 GeV, has been dramatically revealed in a series of studies over the past decade. In the accompanying sidebars, we highlight some of these results in more technical terms, using as examples Higgs physics, hidden dimensions, supersymmetry and unification.

3.3 Science-Driven Requirements for the Linear Collider

A linear collider with a maximum energy near 1 TeV is well matched to our goal of exploring the TeV energy scale. However, precision data from experiments at CERN, Fermilab and SLAC suggest that the Higgs mass is below 200 GeV. Thus even today, before the start of the LHC, there is a strong argument for starting linear collider operation at about 500 GeV. This should be enough energy to detect the Higgs, study its properties, and determine whether it is responsible for generating the masses of the quarks, leptons, and gauge bosons of the Standard Model. Alternately, if a light Higgs is not found, 500 GeV should be enough energy to test alternate explanations for the origin of mass.

After a rich, multiyear program at 500 GeV, we will need to increase the collider's energy to complete our exploration of the TeV scale and take full advantage of our large investment in the machine. We anticipate equally exciting discoveries at these higher energies. Our long-range goals require a linear collider with a reach comparable to that of the CERN LHC. Because electrons are elementary and protons are built from quarks and gluons, this implies a linear collider operating at about 1 TeV with luminosity in excess of 10^{34} cm^{-2} s^{-1}. The linear collider can be raised to its full energy by increasing its acceleration gradient or by increasing its length; this capability must be built into the design and plans from the outset.

The higher energy would likely be needed to search for exotic Higgs particles and to see whether the Higgs is responsible for its own mass. Theories like supersymmetry also require new physics at energies near 1 TeV. The lightest supersymmetric particles are expected to be in range of a machine operating at about 500 GeV. But a complete understanding requires access to the heavier states. This demands a collider with 800-1,000 GeV of energy.

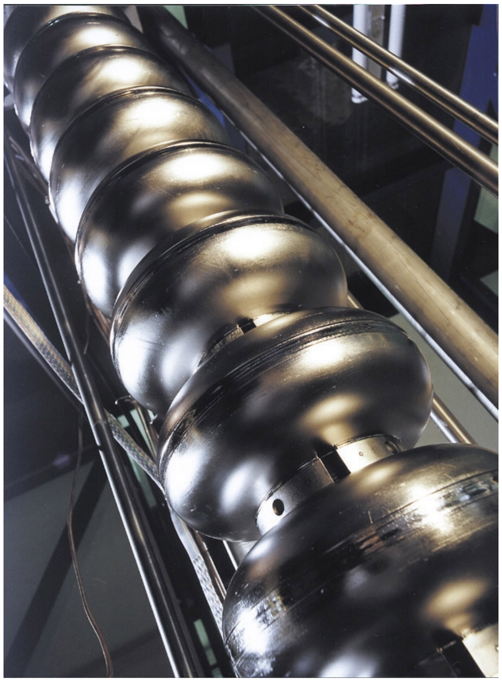
The luminosity of the linear collider should be *at least* 10^{34}cm^{-2} s^{-1}, to provide 100 fb^{-1} integrated luminosity per year of running. This luminosity corresponds to approximately 10,000 events per year for a process with a typical electron-positron cross section at 1 TeV. Such integrated luminosities are necessary to determine the nature of unification, extra dimensions, and electroweak symmetry breaking. They are necessary to observe important rare processes and to measure crucial coupling constants. These measurements are vitally important, and motivate the large investment required for this machine.

A final basic requirement of the linear collider is a polarized electron beam, which is essential for thoroughly measuring the spins and couplings of the new particles. It is also necessary for studying extra dimensions and supersymmetry. Polarization helps the linear collider open domains of physics inaccessible to the LHC.

3.4 Linear Collider Technologies

Since the late 1980's, a number of regional and international workshops have studied the physics goals and requirements for an electron-positron linear collider. Over the same time period, SLAC, KEK and DESY engaged in extensive R&D aimed at developing linear collider technologies capable of accessing the physics of the TeV energy scale.

The accelerator community recognized quite early that a number of issues had to be solved to build a TeV-scale linear collider. These problems included creating high-gradient accelerating systems at a reasonable cost, controlling nanometer scale beams, aligning components to high accuracy, and developing intense electron and positron sources with small beam emittances.



TESLA Superconducting Cavity **NLC High-Power Klystron**

Figure 3.5. Linear Collider R&D. Each linear collider design uses state-of-the-art technologies, developed by international teams of scientists and engineers.

Great progress has been made in all of these efforts. All major issues have been essentially solved. Although further development remains, the international accelerator community now firmly believes that a TeV-scale linear collider can be successfully built at a reasonable cost with the correct science-driven capabilities. Throughout this R&D period, there has been a strong level of international cooperation and communication. There has been formal collaboration between laboratories on R&D topics and even discussion of direct collaboration in the construction of a 1 TeV-scale linear collider.

Each of the three laboratories has developed a concept for a linear collider. SLAC spearheaded an approach called the NLC (Next Linear Collider). DESY proposed an approach called TESLA (TeV-Energy Superconducting Linear Accelerator), while the KEK laboratory developed a concept called the JLC (Japanese Linear Collider). All three approaches share common physics goals. Their status is outlined below.

3.4.1 TESLA

TESLA is a linear collider project proposed by the DESY laboratory in Hamburg, in partnership with collaborating institutions from nine nations. TESLA would provide electron-positron collisions at a center-of-mass energy of 500 GeV, and it would be expandable to about 800 GeV. Over 1,000 scientists from 36 countries developed the TESLA technical design report, which was released in March 2001.

TESLA would be a 33-kilometer electron-positron linear collider based on superconducting technology. It would accelerate each beam to 250 GeV, and would

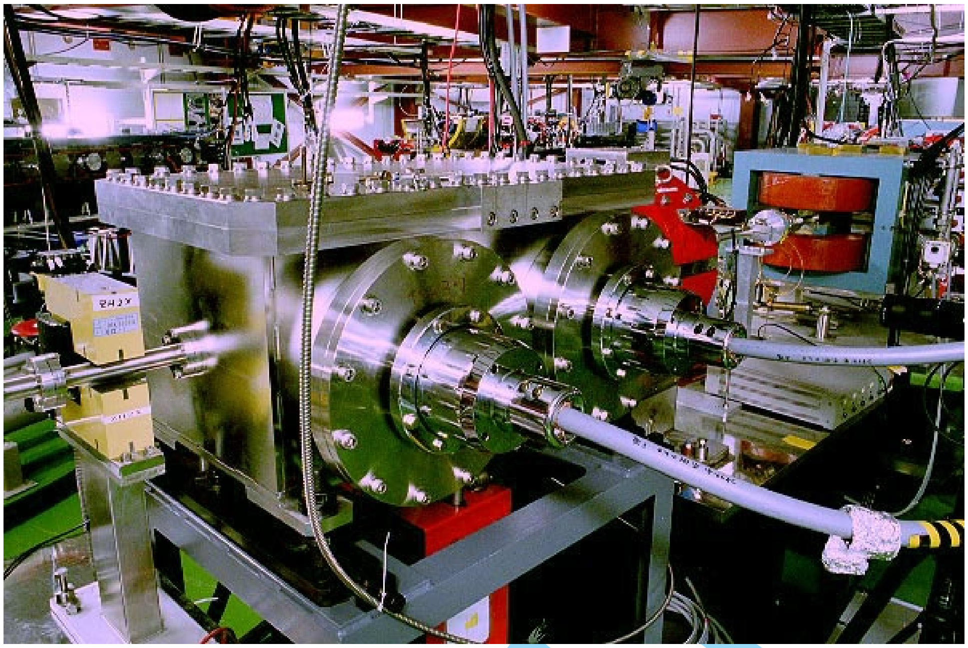


Figure 3.6. Linear Collider R&D. The illustration shows part of the Accelerator Test Facilty at KEK, in Japan.

bring the beams into collision with a luminosity of a few times $10^{34} cm^{-2} s^{-1}$. TESLA would be located in a subterranean tunnel that would extend north from the current DESY site towards the North Sea. The design calls for one detector, with the possibility of adding a second later.

In addition to its colliding beam capabilities, the TESLA proposal includes an X-ray laser facility. The facility would provide extremely short and intense laser-quality flashes to create new research opportunities for physics, chemistry, biology, materials science and medicine. The X-ray laser would be driven by an electron beam generated by an alternate electron source but accelerated using the TESLA superconducting cavities. It would provide X-rays between 1 and 0.1 nanometer wavelengths to 20 or 30 experimental stations.

To accelerate the beams, TESLA would use over 21,000 L-band (1.3 GHz) superconducting resonators, fabricated from pure niobium. R&D on these resonators has resulted in record acceleration voltages for superconducting resonators and a reduction in their production costs by about a factor of ten. These resonators would also tailor the electron bunches to the compact dimensions needed to drive the X-ray laser.

The TESLA Technical Design Report (March 2001) was submitted to the German Scientific Council. The German federal government, the city of Hamburg, and the German state of Schleswig-Holstein will make a decision on the TESLA proposal, perhaps in early 2003. The proposal calls for construction by an international collaboration and the establishment of TESLA as an international research center as early as 2011 or 2012.

3.4.2 JLC

The JLC is aimed at an initial center-of-mass energy of 250-500 GeV, with an eventual goal of reaching the TeV region. At 500 GeV, the JLC design luminosity is also a few 10^{34} cm^{-2} s^{-1}, but the starting energy and the luminosity would depend on physics developments and on the initial budget.

The JLC would be about 25 kilometers long. Electrons and positrons would be accelerated to 2 GeV by several injector-linac stages that would also improve beam quality. The beams would then be injected into the main linacs and accelerated to the maximum beam energy. These high-energy beams would be squeezed to a nanometer level by a final focus system. They would then be collided at the interaction point.

To achieve a high accelerating field, two alternative acceleration mechanisms are being pursued, one utilizing C-band (5.7 GHz), the other X-band (11.4 GHz), the latter in close collaboration with SLAC. Many of the important milestones have been achieved, including the development of prototype high power X-band and C-band klystrons. The designers anticipate that the energy of the machine would be increased incrementally through improvements of the high power RF system, including the klystrons.

KEK is a full participant in the worldwide R&D effort. A major endeavor is the development of the Accelerator Test Facility, a linac equipped with a low impedance damping ring and constructed to create an intense beam with very low emittance. Many of its essential goals have been achieved. Finally, Nagoya University and SLAC have developed photocathodes that are close to generating the required beam currents with the desired 80% polarization.

As in the TESLA design, the X- or C-band linacs could in principle produce the high quality electron beam needed to generate an intense free electron laser. Researchers are pursuing R&D efforts toward a next generation synchrotron radiation facility for materials science, nanotechnology, chemistry and the life sciences.

In 1986, the Japanese High Energy Committee first recommended the JLC as a possible major facility in Japan. It was endorsed as an international facility in the Asia-Pacific region by ACFA, the Asian Committee for Future Accelerators. The JLC is currently on the agenda of an advisory committee for Mombu-kagakusho (Ministry of Education, Science and Culture).

In Japan, the JLC is recognized as the next major facility in high-energy physics, but a formal proposal has yet to be submitted. In 1997, the High Energy Committee again endorsed JLC as the next principal project for high-energy physics in Japan. It urged that every effort be made to start construction early in this decade. In response, KEK has officially set up a JLC project office and committee, and submission of a formal proposal is expected soon.

3.4.3 NLC

The American effort on a TeV-scale linear collider has been led by SLAC, in collaboration with KEK and Fermilab. The Next Linear Collider (NLC) is based on experience gained with the Stanford Linear Collider (SLC), the first linear collider ever built. The NLC is optimized to deliver electron-positron collisions at a center-of-mass energy of 1 TeV, although operations could begin at lower energy. The design luminosity is a few times 10^{34} cm^{-2} s^{-1}.

The NLC would be roughly 30 kilometers in length. The room-temperature linacs are each about 12 kilometers long, with a six kilometer central region that brings the beams into collision. The linacs would have several extraction points at intermediate lengths, so collisions could take place across a broad range of energies. The final focus would deliver the beams to one of the two interaction regions.

The NLC is based on X-band RF technology, four times the frequency of the SLAC linac, to attain higher gradients of roughly 50 million volts per meter. The accelerating structures are disk-loaded cylindrical structures, approximately one meter long. Microwave RF power is generated by high power klystrons. It is transported through an RF pulse compression system to the accelerating structures. There are no explicit plans to include an X-ray free electron laser similar to the Linac Coherent Light Source at SLAC or the TESLA X-ray FEL as part of the NLC facility, but such an instrument could be added.

The NLC design was first presented to the high-energy physics community at the 1996 Snowmass meeting. In 1999, the NLC project was reviewed by a DOE Lehman Committee, which concluded that the project was ready to start a Conceptual Design Report (CDR). Since 1999, the NLC program has been directed toward optimizing performance, reducing costs, and increasing the reliability of components and subsystems. The next step for the NLC would be to develop a CDR with a baseline design and detailed cost estimate for a construction project.

3.5 The Linear Collider R&D Program

Over the last decade, there has been enormous progress toward a linear collider. There are now at least *two* technologies that could be used. Much of the initial R&D effort went into developing the RF systems required to accelerate the beams to the desired energies. In the United States and Japan, efforts were focused on developing high power klystrons and the accelerator structures that are needed to accelerate low emittance beams. In Germany, the focus was on reducing the cost and increasing the gradient of superconducting RF cavities. Some of the most important accomplishments are noted below:

3.5.1 Accomplishments of the R&D Program

The Stanford Linear Collider (SLC) operated from 1989 through 1998 and demonstrated the feasibility of the linear collider concept. At the SLC, numerous techniques were pioneered to preserve the quality of the very small beams from the injectors to the collision point. Spot sizes at the collision point of 1.7 μm by 700 nm were generated and routinely maintained.

The TESLA Test Facility (TTF) in Germany, operating since 1997, demonstrated the basic RF components for the 500 GeV TESLA linear collider, including the modulator, klystron, and accelerator cavities. The TTF exceeded the design gradient of 15 MV/m and operated with gradients as high as 22 MV/m, close to the TESLA-500 goal of 23 MV/m.

The NLC Test Accelerator (NLCTA) at SLAC, operating since 1997, demonstrated the basic components for a 500 GeV X-band linear collider including the conventional modulators, X-band klystrons, RF pulse compression, and accelerator structures. The NLCTA operated reliably at 40 MV/m with the original accelerator structure design. More recently, the NLCTA was used to test new structure designs that have operated at gradients as high as 80 MV/m.

The high power RF klystrons required to generate the RF power for the linear accelerators were demonstrated for X-band, C-band and TESLA designs. Thompson produced two of the multi-beam 10 MW long-pulse klystrons needed for the TESLA design. Toshiba, Marconi and CPI have produced some of the X-band and C-band prototype klystrons for the JLC and NLC designs.

Specialized damping and detuning techniques were developed to reduce the higher-order modes that can drive the beam breakup instability. These techniques have been verified in the ASSET facility at SLAC and the TTF at DESY. Additional improvements in these damping techniques are expected to further reduce these higher order modes to the point where they are completely negligible.

The beam loading compensation, which is necessary to operate with the long trains of bunches and attain the high luminosities, was demonstrated for the normal and superconducting designs in the NLCTA and the TTF.

The Final Focus Test Beam (FFTB) at SLAC operated from 1994 through 1997. The FFTB focused 50 GeV beams to spot sizes of 2,000 by 69 nm. It demonstrated greater demagnification than would be needed for a future linear collider. The FFTB project also developed stripline and RF cavity beam position monitors with better than 1 μm and 25 nsec pulse-to-pulse resolutions, respectively. In addition, remote translation stages were developed to move the magnets with step sizes of 300 nm, similar to those needed in the final focus system of a linear collider.

An important challenge for the linear collider designs is to control the extremely small beams (about 200 nanometers by a few nanometers at the collision point). Alignment of 100 microns or better is required, and even small misalignments of accelerator components would spoil the performance of the machine. Advanced feedback and alignment techniques, modeled on those developed for the SLC and the Final Focus Test Beam project, will control the beams.

At the Accelerator Test Facility at KEK in Japan, a prototype linear collider damping ring began operation in 1997 and has attained its design normalized single bunch emittances.

Over the last decade, ground motion measurements at numerous sites around the world found stability much better than required to collide nanometer-sized beams. Active stabilization demonstrations at SLAC and DESY reduced the residual vibration in the relevant frequency range by an order of magnitude.

3.5.2 The Future R&D Program

Further R&D is still needed, mostly in the areas of the RF systems, luminosity performance, and systems engineering, to confirm the ultimate energy and luminosity reach of the machines.

For the X-band systems of the American and Japanese designs, further investigation is required to find optimal accelerating structures that will reliably reach the full design gradient. Good progress is being made and recent tests with short structures are encouraging. These tests should conclude by the beginning of 2003, for both short and full-length structures. By the end of 2003, the NLC collaboration aims to complete a full test of the RF system suitable for 1 TeV operation, including the moderator, klystrons, RF pulse compression system, and high gradient structures.

For TESLA, the remaining R&D will be mainly devoted to proving that results on accelerating field gradients are applicable to the fully integrated system and to increasing the gradient from 23 MV/m to 35 MV/m, necessary for the 800 GeV upgrade. In addition, the collaboration is investigating a potential cost reduction by powering a pair of nine-cell cavities using one coupler. This would save on the length of the machine and halve the number of RF couplers. This program should have conclusive results by 2003.

For the C-band RF system, R&D is focused high-power testing, and on developing more efficient components, including klystrons, modulators, and pulse compression system. Routes to high collision energies and luminosities are actively being pursued.

At KEK and elsewhere, studies are also continuing to better understand the beam dynamics in the damping rings. These studies are needed for the NLC/JLC damping rings as well as the less conventional TESLA damping ring.

Finally, further studies on aspects of control, stabilization, and diagnostics are also underway.

3.5.3 The Technology Choice

The International Committee for Future Accelerators (ICFA) is carrying out a technical assessment of the two competing technologies (room temperature and superconducting). A report from ICFA's study should be forthcoming within a year. However, it appears that either technology could be used to construct a linear collider, and that the actual technology choice will depend on many factors.

The international collaboration that will build the linear collider must decide on the optimum technology for a given site and proposal. That decision must be based on sufficient R&D so that all relevant issues have been addressed in enough detail to support the decision. For the case of a U.S.-hosted machine, we recommend developing a process for making this decision as early as possible, to focus the development work on the technology to be employed.

It should be noted that the R&D being carried out on both approaches will have significant payoff beyond supporting the technology choice. Many developments are likely to be utilized by the scientific and technological communities at large. In particular, R&D on superconducting RF technology has and will continue to have a significant impact on other accelerator systems, even outside high-energy physics. For example, the Spallation Neutron Source will use a high-gradient superconducting RF linac which is expected to be less expensive and more efficient than alternate approaches. Similarly, the R&D on normal conducting systems will be essential for multi-TeV two-beam accelerator systems, as well as a variety of other accelerator applications, including medical and industrial accelerators where compact size is desirable. If history is a guide, these developments will eventually be used by industry, generating significant economic return on the R&D investment made for the linear collider.

We emphasize the importance of making an early technology choice for a linear collider. This will require a focused and intensified R&D program, which must be given very high priority within the U.S. program. We discuss this and other organizational issues further in the next chapter.

3.6 Summary

There is now a widespread consensus in the worldwide high-energy physics community that our next large project should be a TeV-scale linear collider. The linear collider must be designed to be capable of reaching an energy of 800-1,000 GeV with high luminosity, above $10^{34}cm^{-2}\,s^{-1}$. The strong scientific case and the advanced level of R&D strongly support starting construction on the linear collider in 2005.

We have recommended that the highest priority of the U.S. program be a high-energy, high-luminosity, electron-positron linear collider, wherever it is built in the world. To optimize the design for performance and cost in a timely manner, the United States and its partners must vigorously pursue an intensified R&D program. We also recommend that a steering committee be formed in the U.S. to coordinate all activities and to work with our international partners on choosing the best technology for the linear collider project.

4. HOSTING THE LINEAR COLLIDER IN THE UNITED STATES

We recommend that the United States prepare to bid for the linear collider in a facility that is international from the inception, with a broad mandate in fundamental physics research and accelerator development. We believe that the intellectual, educational, and societal benefits make this a wise investment of our nation's resources.

We envision financing the linear collider through a combination of international partnership, use of existing resources, and incremental project support. If it is built in the U.S., the linear collider should be sited to take full advantage of the resources and infrastructure available at SLAC and Fermilab.

4.1 Introduction

The linear collider promises to be one of the greatest scientific projects of our time. It will be at the frontier of basic science, of advanced technological development, of international cooperation, and of educational innovation. It will attract many of the top scientists in the world to participate in the scientific and technical opportunities it offers.

We believe that the possibility of becoming the host country for the linear collider is a rare and timely opportunity, and one that should be undertaken within the U.S. science program. By hosting the project, the United States would be the center of scientific and technical activity for this great international project and important field of science. The linear collider would help us maintain a leadership role in the exciting quest to unravel the mysteries of matter, energy, space and time.

In this chapter, we present the case to host the machine. We analyze the technical and organizational resources that we can bring to the task. Of equal importance, we discuss how this new project offers the possibility to create a truly international framework for initiating and implementing this project, and more broadly, an international laboratory for physics research in the United States As a starting point, we recommend the formation of a U.S. steering committee to oversee all these activities, from coordinating the technical R&D to helping create the international partnership necessary to build the linear collider.

4.2 The Case for Hosting the Linear Collider

We believe the time is right for the United States to host the linear collider. A healthy worldwide physics program requires a distribution of major facilities around the globe. At present, the LHC is being constructed in Europe, and the JHF, a major high intensity proton facility, is underway in Japan. A U.S. decision to bid to host the linear collider would complete the triangle. It would send an important signal of American leadership and responsibility in this increasingly international field.

Past investments in accelerator facilities have enormously enriched our society. History shows that accelerator facilities provide important platforms for major advances in physics and technology. But they do even more. They excite the imaginations of our children and grandchildren and the public at large. A linear collider in the United States would help attract a new generation of students to the physical sciences. Some would stay in science and advance basic and applied research. Others would contribute their analytical and technical skills to society by becoming leaders in business, government, teaching, and industry. The linear collider would attract some of the brightest scientists from around the world to the U.S. American society would benefit from their creativity and intelligence.

We believe that an international linear collider facility in the United States should have a broad mandate in fundamental physics research, accelerator development, and outreach. The opportunity to develop a truly international project would enable the U.S. to take the lead in forging a new approach to planning, collaboration and management in science on a global scale. The linear collider would also be an important opportunity to further develop new technology for distributed computing and data sharing, as well as for monitoring and operating complex detectors and accelerators from afar. These technical developments would build on the invention of the World Wide Web, and on our present work on the LHC.

Locating such a facility in the United States would allow a greater portion of our economic investment to be recaptured through jobs and technological benefits. A linear collider would push technical requirements in various industrial areas, such as electronics, computing, micromechanics and construction. This economic return is a key reason why other regions have chosen to host large projects.

The economic benefits of previous accelerators are well documented by studies done at CERN. These studies indicate that for every Swiss franc spent by CERN in high technology, three Swiss francs were generated through increased economic activity and cost savings in European high-tech industries. Note that this analysis does not include the effect of such major spin-offs as the World Wide Web, whose concept and protocol were invented at CERN.

Many nationally prominent figures have called for an initiative to substantially increase funding in the physical sciences. The recent report of the U.S. Commission on National Security/21st Century concluded that the nation has been living off the economic and security benefits generated by the last three generations' investment in science and education – and that these systems are in serious crisis. After its first recommendation to create a National Homeland Security Agency, the Commission's second recommendation was to double the federal research and development budget by 2010. An initiative to substantially increase funding for research in the physical sciences is consistent with this recommendation, and is necessary to enhance the nation's long-term scientific and technological competitiveness. As a flagship facility for 21st century science, the linear collider could be a centerpiece of a national effort to boost the physical sciences.

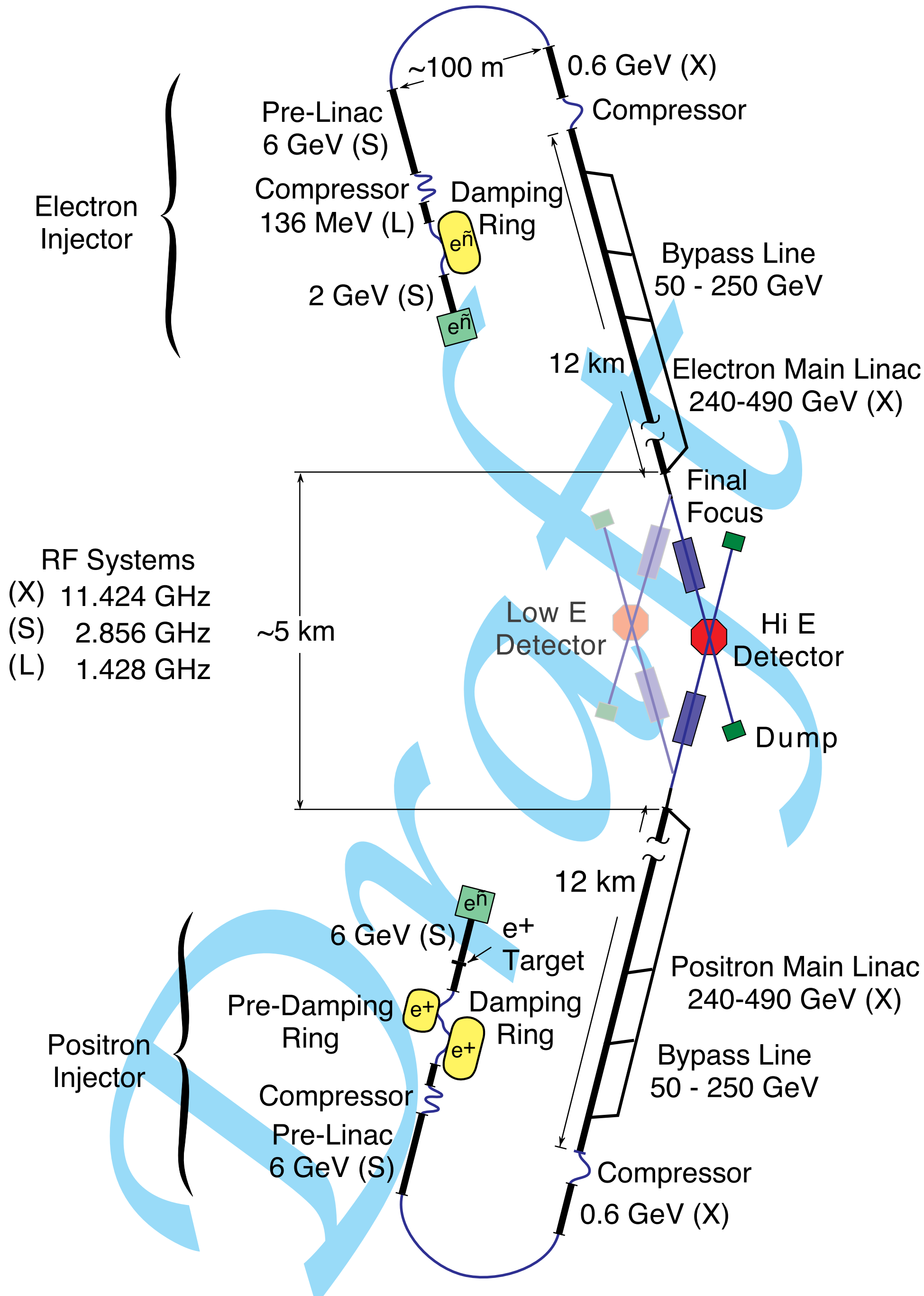


Figure 4.1. The NLC. The schematic drawing illustrates the elements of a linear collider. In one model of international organization, different countries could supply different parts of the project. From this point of view, TESLA and JLC are similar to the NLC.

4.3 Constructing the Linear Collider

As described in Chapter 2, the roadmap for our field has many exciting opportunities over the next twenty years. We believe that our scientific goals are best achieved by balancing investments across these opportunities. Our top priority, however, is the linear collider. The project is of a size, complexity and scope that it can only be realized by an international collaboration, wherever it is built in the world.

If the linear collider is sited in the United States, we envision financing it through a combination of investments from non-U.S. collaborators, the use of existing infrastructure and human resources within the U.S. program, and increased support to the U.S. particle physics program. This report will consider each of these contributions in turn.

International investment is essential for a project of this scale. A number of issues are independent of the site. First and foremost, all partners must feel ownership, so full internationalization must begin at the start of the project and cover all its aspects and stages. This means that initial steps toward internationalization should begin immediately, independent of the final location of the facility. For the linear collider, endorsements from the international high-energy physics community have already set the stage for global participation.

A significant fraction of the linear collider must be financed from the existing U.S. high-energy physics program. This can be accomplished through sound management and site selection. For example, accelerator physicists, engineers and technicians already engaged in linear collider R&D will work on the new facility. In addition, we expect a large segment of the U.S. particle physics community to be attracted by the exciting science and technology opportunities at the linear collider.

We believe that a bold new initiative like the linear collider justifies new funding from the U.S. government. The linear collider is an important investment for this country. It would bring one of the greatest scientific projects of our time to the United States, together with its associated intellectual, educational, technological, and economic benefits. We envision that the host country, in this case the U.S., would contribute about two-thirds of the cost of the project. This would require incremental funding beyond the resources available through redirection of our present program.

At existing laboratories, we foresee a natural realignment of accelerator physicists, technicians, engineers, and particle physicists as the linear collider project ramps up and other activities fulfill their scientific objectives. Universities and national accelerator laboratories would devote their efforts to providing major subsystems of the collider. A significant portion of the staff in the existing high-energy physics laboratories, including those engaged in procurement, human resources, project management and safety, would also be devoted to the new project.

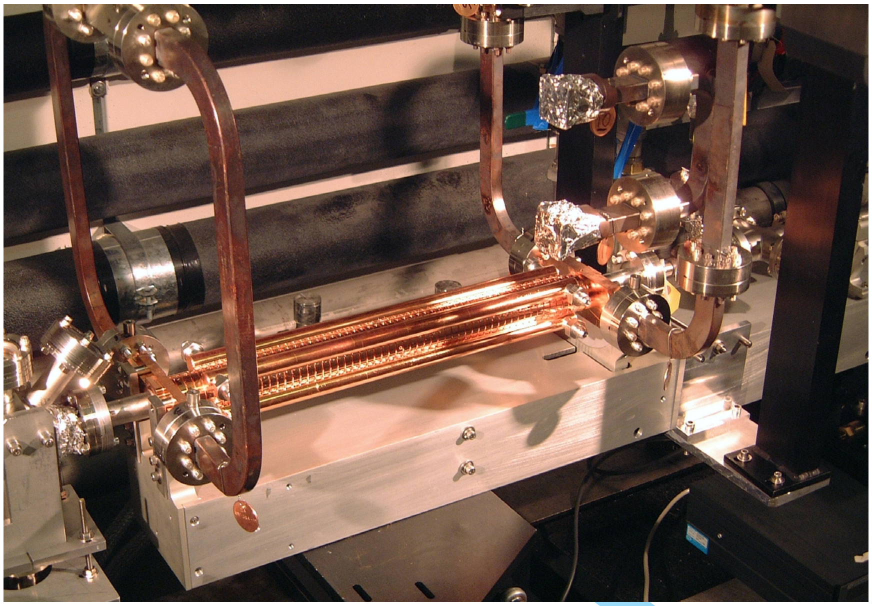


Figure 4.2. Linear Collider R&D. The photo shows a test accelerating structure for the NLC.

If a linear collider is built in the U.S, the site should be at or near an existing high-energy physics laboratory, to take full advantage of existing resources. The project would greatly benefit from existing laboratory infrastructure, including lab and office space, as well as the support services outlined above. Most important, however, are the experienced personnel – physicists, engineers, and technicians – who would join the new project, providing expertise not otherwise available.

Drawn by the exciting physics possibilities, we expect that many in the experimental physics community would join their colleagues from abroad in the conception and development of the first experiments at the new facility. Indeed, following recent examples both in the United States and Europe, we expect that approximately half of the scientists and in-kind contributions to such experiments would come from outside the U.S.

4.3.1 Technical Resources and In-Kind Contributions

The linear collider will be built from technical components produced by a broad collaboration. Since many contributions will be in-kind, the financial burden will rest on laboratories distributed around the world. This cooperative model will foster vigorous and dynamic programs at all the laboratories participating in the linear collider.

The three phases of construction – R&D, construction and installation, and commissioning – all require different skills. These skills are available at laboratories and universities in the United States and abroad. In one possible model for constructing a linear collider in the U.S., the United States would assume responsibility for the conventional facilities for the project, as well as for some of the technical components.

The international partners would contribute the remaining technical components, primarily in an in-kind fashion.

The practice of in-kind contributions has a long history of success in the construction of large detectors. For example, the CDF detector at Fermilab was built with in-kind contributions from eleven countries, at a total cost of several hundred million dollars. More recently, BaBar at SLAC and PHENIX at Brookhaven were built with subsystems originating from many countries in the world.

In-kind contributions have also become common in accelerator projects. The Final Focus Test Beam (FFTB) at SLAC, with a total project cost of approximately $25M, was constructed with about $10M of in-kind technical components contributed by international collaborators. The U.S. contribution of $531M to the CERN LHC is mostly in-kind. The LHC contribution is managed by a partnership of five national laboratories in collaboration with many American universities. Finally, the $1.4B Spallation Neutron Source at Oak Ridge is being constructed using contributions and the technical expertise from six national laboratories.

4.3.2 The Accelerator Physics Base in the United States

Because of the extensive R&D for the linear collider, as well as technical developments for other accelerators, the worldwide community has the technological base to build the linear collider. In particular, the United States is well positioned to be a major contributor to either the room-temperature or the superconducting technology. Both technologies have been developed in great detail (as described in Chapter 3) with important contributions from the U.S. national R&D program.

Accelerator physics in the United States is based on several strong components. First, the U.S. has long played a leading role in international collaborations, starting with organization of the first international accelerator physics conferences in the early sixties. American scientists have played a major role in accelerator design and construction from the very beginning of the field. Second, the United States has maintained a diverse base for the development of accelerator technology, with different DOE and NSF laboratories contributing to a variety of basic technologies. These programs have contributed to the development of the technologies that are foreseen today as candidates for the next generation linear collider: a normal conducting linac, based on 11.4 GHz RF technology, or a superconducting linac based on 1.3 GHz RF technology. Finally, previous construction and operation of complex accelerators, particularly the Tevatron collider and the SLC, provide essential expertise for building and operating a future linear collider.

The normal conducting option for the linear collider is being developed by a collaboration involving SLAC, FNAL, LBNL, LLNL, KEK in Japan, CERN in Europe, and BINP in Russia. Over the last several decades, SLAC has developed many of the key technologies. It has also gained the accelerator physics experience relevant for the construction and operation of large-scale linear accelerators. Much of this experience was obtained by operating SLAC's two-mile linear accelerator, and its extension, the SLC. The techniques to generate, accelerate, focus, and collide low emittance electron/positron beams were refined during the development and operation of the SLC. These techniques represent the essential technological base for all linear colliders.

Superconducting linac technology was pioneered by the development of superconducting RF cavities at Cornell. The technology has twice been transferred to major construction projects in the United States, the Continuous Electron Beam Accelerator Facility at Thomas Jefferson Laboratory and the Spallation Neutron Source at Oak Ridge. Fermilab and Brookhaven also have significant expertise in cryogenics and pulsed power technology. Moreover, Cornell and Fermilab were partners during much of the TESLA Test Facility construction; they have been members of the TESLA collaboration from the very beginning.

As a result of all these efforts, we believe that the world community has the technical base to build a linear collider. The early foundations for an international organization to support, construct and operate the facility have been laid. In the United States, there are approximately 300 accelerator physicists at national laboratories and universities supported by DOE and NSF. Approximately 150 are already working on some aspect of electron-positron colliders. We expect an additional 100 people to join the project as funding increases. This group will represent the core of accelerator physics in the U.S. for more than a decade. The U.S. contingent will be supplemented by a significant number of accelerator physicists from the project's international partners.

4.3.3 The Technology Choice

The time is approaching to choose a technology for the linear collider. The choice will follow on a process that began in the mid-1980's and continued through a series of conferences on linear collider physics, detectors, and accelerator developments. Initial comparisons of the technologies were made at these conferences. In 1996, an international technical review committee, chaired by Greg Loew from SLAC, issued a preliminary report. This document contained a comprehensive description of the various R&D programs then underway. The International Committee for Future Accelerators (ICFA) has requested an update, aimed for FY 2003, with a preliminary report in the summer of 2002.

On the basis of the Loew Committee assessment, and with the full involvement of our international partners, we urge that a decision on the technology be made as early as possible, preferably in FY 2003/2004, which would allow the beginning of the linear collider construction project in FY 2005. We envision an eight-year construction period to achieve 500 GeV in the center of mass. Following a multiyear research program, as described in chapter 3, we expect the energy would be increased, and that the completed facility would run at 800-1,000 GeV.

4.3.4 Organizational Issues

A number of issues need to be resolved if we are to start construction of a linear collider in the United States by 2005. These include reaching final agreement on the technical design for the machine, working toward the definition of an optimized experimental program, and conducting negotiations in the political sphere to arrange an international collaboration to build the facility.

The R&D program is already well underway. It is divided among the international participants working in existing, effective collaborations. The detector working groups need to include a larger community, and the appropriate level of funding for detector R&D must be defined, so the R&D can be completed.

The formation of an international organization under scientific leadership is necessary to complete the linear collider design and to initiate the collaborations for its physics use. As a first step, we recommend that a U.S. Linear Collider Steering Committee, with strong centralized leadership, be formed as soon as possible. This group should bring together the laboratory and university efforts toward the linear collider. We also recommend that DOE and NSF quickly establish a joint-agency partnership for the linear collider accelerator and its detectors.

The linear collider facility will require an organization providing international governmental oversight, with responsibility to participating governments. We recommend that the Linear Collider Steering Committee, along with DOE and NSF, take the lead in defining and organizing the appropriate inter-governmental management structure. In parallel, DOE and NSF should together seek the necessary governmental endorsements to allow the Linear Collider Steering Committee to work with our international partners to form a wholly international organization.

DOE and NSF have jointly managed U.S. participation in the LHC through a Joint Oversight Group. They have also participated actively on the board of funding agencies that examines and monitors LHC resources. These two LHC organizations, one local that monitors U.S. spending and progress, and the other international, provide models that could be built on to establish a comprehensive international linear collider project. Defining the project will require significant discussion among the representatives of the responsible political bodies of the participating countries.

4.5 Summary

The time is right for the U.S. to host the linear collider. By hosting the project, the United States would be the center of the scientific and technical activity for one of the greatest scientific enterprises of our time.

The intellectual and economic benefits from hosting this international facility would make it a flagship for our program in the physical sciences. It offers the possibility to create a truly international laboratory for physics research in the United States

As a starting point, we recommend the formation of a steering committee to oversee all linear collider activities in the U.S., including work towards defining and organizing the appropriate inter-governmental structure to manage the facility.

5. INVESTING FOR THE FUTURE

We recommend that vigorous long-term R&D aimed toward future high-energy accelerators be carried out at high priority within our program. It is also important to continue our development of particle detectors and information technology. These investments are valuable for their broader benefits and crucial to the long-range future of our field.

5.1 Introduction

The long-term success of particle physics research depends critically on human and technological resources. A vigorous university program and R&D throughout the field are vital ingredients in developing the new ideas and tools that make particle physics such an exciting field of discovery.

University-based high-energy physicists contribute much intellectual capability to the field. They train students who represent the future, and generate many of the new ideas that underlie the major advances and discoveries in the field. A long-range plan for particle physics must maintain the successful partnership between university researchers and the large laboratories and facilities. A healthy partnership is necessary to pursue the scientific goals discussed in this report. In particular, it is important to ensure that university-based researchers remain fully integrated into our increasingly global field.

A vigorous program of technological research and development at our universities and laboratories is essential to develop the advanced equipment we need. These tools include the accelerators and detectors that have led to so many discoveries in the past, and with appropriate R&D, will do so again in the future. Advanced computing is another essential element for our science, allowing us to examine incredibly large volumes of data and facilitating the work of our global collaborations.

In this chapter, we discuss investments for the future of the field. These include university-based research, as well as three key areas of technology development: (1) accelerators that provide ever-higher energies and intensities of particles; (2) particle detectors that make visible the reactions we study; and (3) software and computer tools that enable us to mine data. The extreme performance we require has given rise to new techniques for particle acceleration, advanced computation and the detection of particles and radiation. These advances have found broad application in other fields of science, as well as technology, health, information technology, and defense.

5.2 University-Based Research

The high-energy physics program in the United States is built around a strong university-based community. Our major national laboratories, Fermilab and SLAC, were created in the 1960's to centralize the major facilities used by university researchers. Universities Research Association, a large university consortium, operates Fermilab, while Stanford University runs SLAC. Both laboratories have large university-based communities fully involved in their research programs. A healthy balance between universities and national laboratories is key to the success of the program we outline in this report.

University faculty, graduate students and postdoctoral researchers make up more than 80% of the scientists working in elementary particle physics. University-based research is a particularly cost-effective component of the overall program since universities pay faculty salaries during the academic year. They also provide a considerable fraction of graduate student support. University groups play a critical role in renewing our field. They are instrumental in opening new areas of research, such as the exciting connection between cosmology and particle physics.

The theoretical effort in high-energy physics, primarily based at universities, is an important part of our program. High-energy physics thrives on a continual interchange between theory and experiment. Theorists develop new ideas about the basic particles and their interactions, as well as space, time, and the fate of the universe. Theorists also help interpret the data produced by experiments. Building on data and new theoretical ideas, theorists help to identify the experimental avenues that have the greatest promise for important future discoveries. Experimentalists validate or disprove theoretical ideas, and more often than not, find surprises that fundamentally change our way of thinking.

University scientists provide training for our undergraduate and graduate students. The intrinsic excitement of high-energy physics makes it a wonderful vehicle for drawing young people into science, and for demonstrating the importance of fundamental research. Graduate education in theoretical and experimental particle physics provides effective training for a variety of technical and scientific careers. The experiences of attacking complex problems in depth, and of communicating and defending the results in a 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 prized in many settings.

Experimentalists obtain specialized experience in electronics, advanced software techniques, and development of state-of-the-art detectors. Particle physics experiments also offer 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. All these abilities have a wide range of applications in the modern global economy.

The health of university-based research is a crucial element of our long-range plan. Budget problems over the past decade have hit university groups particularly hard, since practically all of their expenditures support people. It is important that a high priority be given to restoring the strength of university-based research, as recommended by the 1998 HEPAP Subpanel.

5.3 Accelerator R&D

Advances in our understanding of particle physics depend critically on our ability to develop more powerful particle accelerators. In the past, accelerators with higher and higher energies revealed exciting new phenomena. We have every reason to expect the next steps to be at least as exciting. Higher energies must be complemented by higher intensities, which make it possible to study rare processes with great precision.

We give such high priority to accelerator R&D because it is absolutely critical to the future of our field. Accelerator R&D is the essential tool to make future facilities both feasible and affordable. As particle physics becomes increasingly international, it is imperative that the United States participates broadly in the global R&D program.

The relationship of the U.S. accelerator R&D effort to other international programs is one of collaboration and mutual support. Good communication and frequent exchange of personnel between accelerator centers in the United States and abroad have resulted in a common pool of knowledge and techniques. This has prepared the way for undertaking a truly international accelerator project.

The accelerator R&D that we perform has also had important impact elsewhere in science and technology. Two examples are synchrotron radiation sources that are central to research in materials science and biological systems, and high intensity pulsed neutron sources that play an important role in understanding the chemistry and physics of materials.

In the following discussion, we describe accelerator R&D roughly corresponding to the time horizon of the work and the stage of technical development: long-range or advanced accelerator R&D; mid-range or focused advanced accelerator R&D; and short-range accelerator R&D.

5.3.1 Advanced Accelerator R&D

Advanced accelerator R&D is the breeding ground for future particle acceleration techniques. The motivations for supporting this work include curiosity-driven pursuits of new accelerator science, the discovery and development of new concepts or techniques for high-energy accelerators, and the training of graduate students. Advanced accelerator R&D is an effective way to attract scientists into the field.

The DOE high-energy physics program supports a formal program of accelerator R&D that is largely university-based and proposal-driven. NSF also supports this type of R&D, including work at Cornell University that pioneered superconducting RF cavities. These programs are important for particle physics and for other fields that use accelerator technology.

The subpanel urges DOE and NSF to conduct a broad-based review of advanced accelerator R&D because of its importance to the long-term progress of our field. We suggest that the following questions be considered:

- Are the most important R&D activities being adequately pursued?
- Are the mechanisms for identifying and supporting relevant R&D topics effective?
- What resources are needed for the R&D to succeed?
- What is the appropriate distribution of advanced and generic accelerator R&D between the universities and the national laboratories? Should there be increased

collaboration in these areas? If so, what mechanisms might foster an increase in collaboration?

5.3.2 Focused Accelerator R&D

The focused R&D efforts currently underway are aimed at an electron-positron linear collider, a very high-energy hadron collider, and a muon collider/neutrino source. The program is currently dominated by work done at DOE laboratories, except for the muon collider/neutrino source collaboration, which is supported by DOE and NSF. The muon collider/neutrino source collaboration has been successful in engaging university groups, a strategy we strongly endorse.

The linear collider is the highest priority in this report. The associated R&D is discussed in some detail in chapter 3, and will not be repeated here. Increased R&D will be required to support the design and construction of a linear collider, whether it is built onshore or offshore.

Beyond the linear collider, a Very Large Hadron Collider (VLHC) is a long-range objective for our field. The 1998 HEPAP Subpanel recommended "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."

A national VLHC collaboration was organized in response to this recommendation. The collaboration has achieved significant R&D results, particularly in magnet development. Recent design studies have explored a staged approach, starting with low field magnets in Stage I, and then going to high field magnets and an energy of 100 to 200 TeV in Stage II. Alternate designs have also been considered.

Detailed specifications for the VLHC must wait for physics discoveries at the LHC. However, since a VLHC is so central to the long-term goals of our field, we strongly support R&D toward such a machine and recommend that it be continued at about the current level of effort. We also suggest that the research take a long-term perspective toward developing new technologies and techniques relevant to such a machine.

High-field magnet research is particularly important. This work is essential for upgrading the LHC, and has considerable potential for applications in high-energy physics and other fields, including industry. Experience with high-field magnets is needed to find the optimum design for new hadron or muon colliders. To assemble the necessary intellectual and financial resources, efforts should be made to form an international collaboration as early as possible. Critical accelerator physics issues, such as the influence of ground motion and a study of transverse instabilities, should also be studied at a modest level.

The 1998 Subpanel also recommended that "an expanded R&D program be carried out on a multi-TeV 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 was established to carry out the R&D program.

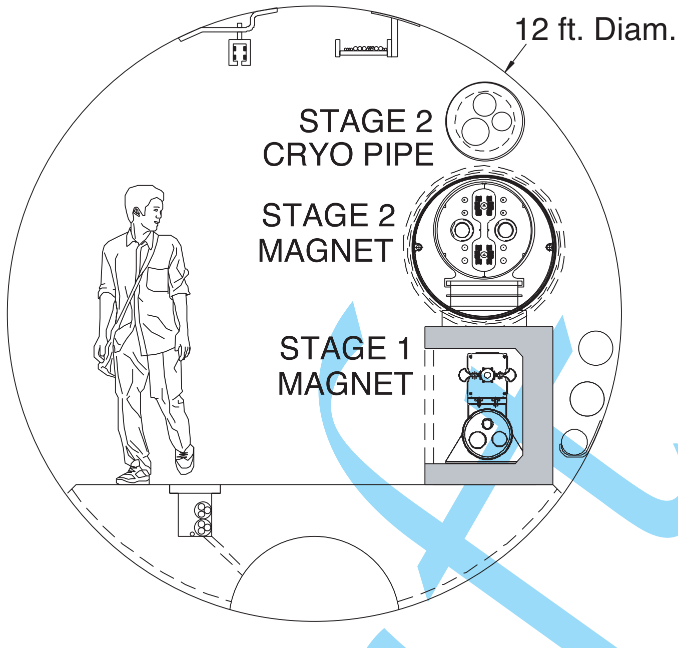


Figure 5.1. The VLHC. A Very Large Hadron Collider is likely to be a long-term option for the field. The accelerator might be built in two stages.

The Collaboration identified a neutrino source as the primary goal, partially because of exciting new developments in neutrino physics.

We support the decision to concentrate on the development of intense neutrino sources, and recommend continued R&D near the present level of $8M per year. This level of effort is well below what is required to make an aggressive attack on all of the technological problems on the path to a neutrino factory. Therefore we strongly support further development of concepts and detailed simulations, activities that require great intellectual effort but minimal additional costs. We also encourage strong international collaboration to make the best use of investments in this field, recognizing the importance of the present international collaboration on the essential muon cooling experiment.

Other concepts for future multi-TeV electron-positron colliders are being studied. The two-beam concept, or CLIC, is the subject of a major focused R&D program at CERN. This development may benefit from work done on a linear collider.

In general, the focused R&D program is closely aligned with our need to develop new capabilities to address our science. However, it is important that the program retain its long-term flexibility to investigate new concepts as they develop. The program should be periodically reviewed to identify promising directions and to help coordinate international efforts.

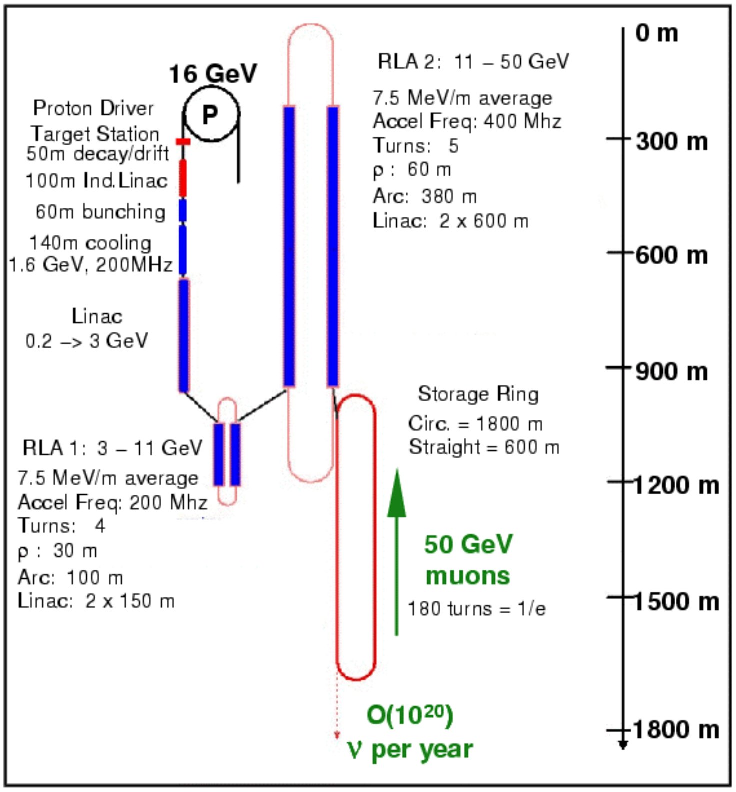


Figure 5.2. A Neutrino Factory. A muon-based neutrino factory is another long-term option for the field. Such a facility might lead to a high-energy muon collider.

5.3.3 Short-Term Accelerator R&D

We finally note that specific short-term R&D projects are necessary to develop new technological approaches for specific applications. Such R&D involves adapting new technologies for accelerator applications, straightforward extensions of existing techniques, or system integration of new combinations of technologies. Such efforts are usually intended to improve a funded or operating facility, and the risk of failure is relatively low. Typical examples of such work are the development of new fast kicker magnets, more capable feedback systems, and new final focus optical elements.

Short term R&D is usually supported within the ongoing program. These activities are crucial to improving, reconfiguring and fully exploiting existing facilities.

5.4 Detector R&D

While particle physics has been paced largely by the construction of new accelerators of higher energies and intensities, many physics discoveries have also required new detector

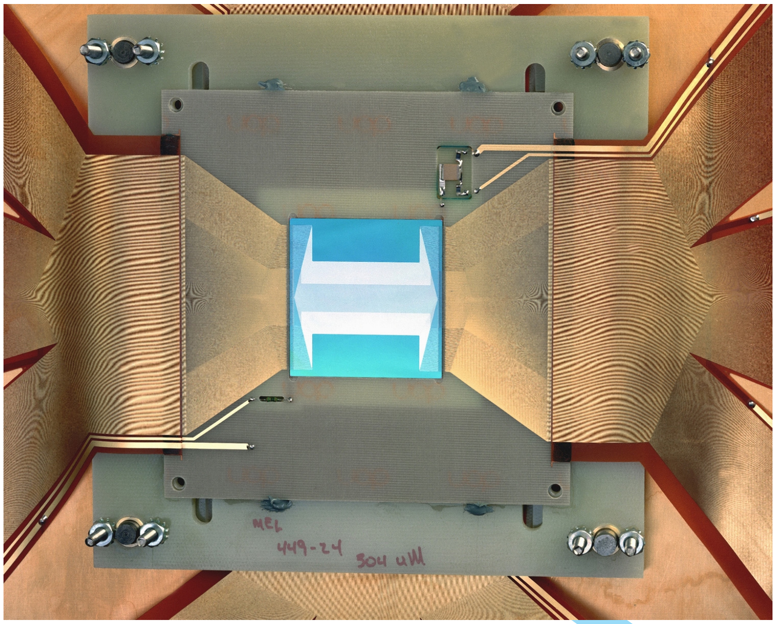


Figure 5.3. Detector R&D. Particle physics detectors challenge the state-of-the-art in electronics and other technologies.

techniques. At hadron colliders, for example, higher energies necessitated better detectors to handle the increased data flow.

The science and technology of particle detectors has a long and illustrious history, originating in the study of particles from radioactive sources and cosmic rays. Detector development has exploited a wide variety of physical techniques. The science of detectors has drawn upon and enriched large parts of physics and chemistry, as well as allied domains of technology.

The first steps in detector innovation are often small, and are well suited to universities and student participation. The national laboratories also play important roles in detector development, supporting a wide variety of advanced technologies that are often essential for the development of new techniques.

As international collaborations on large experiments have become the norm, detector development has also become international. We expect this trend to continue. Strong international collaborations bring a critical mass of intellectual power, as well as access to advanced facilities.

While the effort devoted to accelerator science is of a larger scale, research on detectors is equally vital for particle physics experiments. Both provide major sources of innovation for applications in other fields of science, technology and the life sciences. In fact, the large return on R&D in radiation and tracking detectors is strong justification for the entire particle physics program.

Highly radiation-resistant electronics and other components are important examples of national needs being filled by the high-energy physics detector R&D program. Experimental demands are increasing at the same time as industrial capabilities are diminishing because of changing defense requirements. Our field may soon be the main repository of expertise in this field, especially for large scale and complex systems.

5.4.1 New Challenges for Detector R&D

In the last few decades, a large worldwide investment in electronic and optical technologies has led to many remarkable advances. In some of these areas, our field has been able to stay close to the leading edge, as in detector electronics that use industrial CMOS integrated circuits.

On the other hand, there are very important areas where the gap between the leading edge of technology and the state of the art in detector development is increasing. For example, technologies involved with connecting semiconductor elements have important applications to the large area, fine-grained structures we require. We depend on such innovations, but need the ability to translate imaginative ideas into practice.

The last few years have illuminated some of the difficulties in making satisfactory collaborations with industry. The problems are most serious when we wish to use new kinds of technology in rapidly advancing areas of optics and semiconductor devices. Our field does not have the resources to access such advanced technology. Significant resources are used to develop detectors during the construction of experiments. But at the development stage, we lack the resources to collaborate with technologists.

5.4.2 Detector Development for Specific Applications

In addition to work carried out at universities and laboratories within the ongoing program, each new colliding beam machine has a formal structure to develop detectors for experiments at that facility. Such programs encourage work in universities, and teaching and outreach is often associated with these programs. The results of this research have been useful, even in cases where the technique has not been taken up in the immediate accelerator application. The time is right to begin an international program to develop detectors for the linear collider.

Detector development in universities has been supported very effectively by the Major Research Initiative grants (MRI) of NSF. These grants have enabled universities to acquire instrumentation that allows them to participate in leading edge research. This program has been a major factor in allowing universities to contribute to the leading edge of detector development.

5.4.3 Research into New Concepts

In response to the 1998 Subpanel, DOE initiated a program to fund research into new detector concepts, which should considerably strengthen this field. The viability of future accelerators like the VLHC and the muon collider depends on development of improved detectors for very high rates and backgrounds. Some developments will arise as improvements of current detection methods, but others will require new detector concepts.

There has been relatively limited research on detector concepts that use advanced technology: advanced semiconductor fabrication, nanotechnology, optical technology, low temperature cryogenics, etc. We need collaborations between particle physicists and other scientists active in these areas. Universities and laboratories are ideal environments for these collaborations. Detector R&D is expensive, so projects must strike a balance between innovation, the likelihood of commensurate return on the investment, and the short-term importance of the application.

National laboratories must maintain directly funded detector R&D programs that allow them to keep abreast of new developments in electronics, connections, optics and detectors. It is also essential that universities be able to carry out cutting edge detector R&D. The new DOE program for detectors has substantially strengthened this capability. The NSF MRI program will continue to be vital for universities. The infrastructure coming with linear collider R&D will help maintain these essential capabilities as well.

5.4.4 The Future of Detector R&D

We support the practice of setting up R&D programs for detectors at new accelerator facilities. This program should naturally extend to facilities with international organization, such as the linear collider, and to non-accelerator experiments as well. In addition, small-scale detector development, which is within the scope of the ongoing program, should be continued and actively encouraged. Finally, the funding of advanced detector development should be increased, if possible, with the goal of allowing universities to keep abreast of the most modern technology. The same goal should be met at national laboratories.

5.5 Information Technology in High-Energy Physics

Information technology (IT) has been integrated into high-energy physics research perhaps more pervasively than any other scientific discipline. This integration has been driven by the demands of accelerator-based experiments, where rapid increases in data volume (Gigabytes to Terabytes) and data complexity (thousands to millions of channels) have confronted us with the need to filter, collect, store and analyze this data. As a result, we devote significant resources to data acquisition, processing, storage and networking infrastructures, as well as to human capital in the form of software and computing teams. The rising costs of these and other computing technologies, as well as the creation of large laboratory-based computing infrastructures, reflect their critical importance in making new scientific discoveries in our field.

We are not alone in our adoption of information technology, and like many other fields, we have benefited enormously from the IT advances of the past two decades. However, the extreme computational, data handling and analysis needs of our experiments have forced us to invest significant resources in IT research and development, and adapt cutting edge technologies to our purposes, either internally or in partnership with industry. These projects complement and extend similar efforts in detector and accelerator research that were described earlier in this chapter.

Some examples that show the breadth of these developments include: (1) the development of data acquisition systems for modern collider experiments that contain thousands of PC equivalents of computing power and are capable of manipulating many Terabytes/second; (2) the implementation of massive offline computational resources, which permit simultaneous access to massive data archives by hundreds of physicists; (3) the creation of large packages for pattern recognition, accurate simulation and sensitive statistical analyses, which together permit the maximum amount of scientific information to be extracted from data; (4) the design and simulation of complex accelerator and detector components, whose behavior can be modeled with high confidence; and (5) the use of networked computational resources in support of lattice gauge calculations, which, through the implementation of powerful new algorithms, promise to directly calculate fundamental particle properties.

5.5.1 Current Activities in Information Technology

The role of information technologies in our field continues to expand as unit costs fall, as more powerful systems become available and as new capabilities are identified. For example, software engineering methods and powerful database technologies from industry have been widely adopted to develop more robust data acquisition, data analysis, and simulation systems. Unit prices for CPU and storage have recently reached the point where significant computing resources can be marshaled by small institutions. Hardware resources present less of a constraint than in previous years.

The exploitation of advanced information technologies is becoming more sophisticated in computing systems for experiments, where new software engineering methods reduce errors, and in accelerator design and detector development, where advanced CAD and simulation techniques allow complex components to be designed and tested before committing resources to construction. Similar techniques are being developed and extended to new areas.

5.5.2 Future Activities in Information Technology

The computing and software systems being designed for the LHC and other experiments face a series of unprecedented challenges associated with communication and collaboration at a distance, long-term robust operation, globally distributed computational and data resources, and software development and physics analysis by global collaborations. New capabilities will have to be provided for the demanding new environments expected in future experiments: for example, intelligent trigger and data acquisition systems having sufficient power to filter and collect information at the highest luminosities, and analysis software capable of extracting small or new "discovery" signals from overwhelming backgrounds.

More generally, future computing and software systems must provide rapid access by global collaborations to massive distributed computing and data archives, must operate across networks of varying capabilities, and must possess sufficient robustness and flexibility to support international collaborative research over a period of decades. They must also effectively manage limited computing, data handling and network resources. Clearly, the creation of such capable information technology systems requires careful design using modern engineering tools and close collaboration with computer professionals and industry.

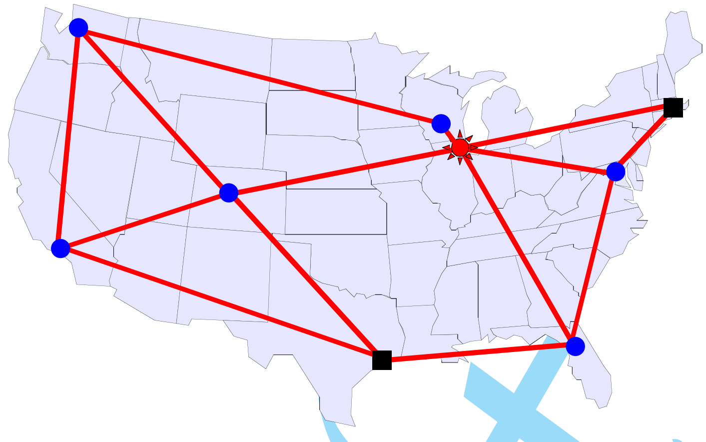


Figure 5.5. Data Grid. A data grid links computational and storage resources using a high speed network.

The requirements for LHC research demonstrate some specific computational challenges that future information technology systems will have to address over the next two decades. Online data filtering systems will select and store 100 interactions out of the 1 billion that occur every second, a number that will grow with luminosity. LHC detectors will have approximately ten times more electronic channels than today's detectors and will have to cope with an extremely complex environment where every interesting collision must be disentangled from the debris of 10-20 background collisions. LHC core software will contain millions of lines of code designed by worldwide teams of physicists and computer professionals. Software and computing systems will have to arbitrate among hundreds of jobs requesting access to geographically distributed resources that contain hundreds of Teraflops of processing power and hundreds of Petabytes of data.

Information technologies compose an increasing fraction of the budget in construction and maintenance of experiments, primarily because of significant personnel costs to develop and maintain these complex systems. An increased effort to rely more on shared tools and expertise can slow the rise in these expenditures. Moreover, investments in information technology can reduce travel expenses, improve the efficiency of facility operations and significantly improve physics productivity.

5.5.3 Collaborative Research: Networks and Data Grids

Information technology systems of the future have the potential to address much more than quantitative increases in computational and data handling performance. Recent dramatic increases in network capacities have opened new possibilities for collaborative research, catapulting networks to a position of strategic importance for global collaborations such as the proposed Global Accelerator Network (GAN).

Collaborative research includes standard activities such as e-mail, web browsing and remote login, but also encompasses large-scale data transfers between laboratories, regional centers and institutions, videoconferencing (including the widely used VRVS system developed by high-energy physicists in the United States and Europe), remote operation of facilities, and Grid computing, explained below. These activities will acquire tremendous importance when operating international high-energy physics facilities.

The recent development of Data Grids offers a comprehensive framework for supporting collaborative research. Data Grids are geographically separated computation resources, configured for shared use with large data movement between sites. Such grids preserve local autonomy while providing an immense, shared computing resource that can be accessed anywhere in the world.

Data Grids enhance collaboration and communication in several critical areas, all of which contribute to physics productivity. First, they enable intellectual resources to be fully engaged in scientific research regardless of location, a welcome development for many universities and countries that cannot afford the expense of sending people to national or international laboratories. Grid-based computing environments also enhance the training and participation of students in forefront research and bring home the excitement of this research to benefactors and the public. Finally, the seamless integration of university and laboratory computing systems into a single resource will further strengthen university partnerships with national and international laboratories.

The inherent advantages of coherently operating geographically disparate resources has caught the attention of many scientific disciplines as well as industry, where the Grid is seen as a strategic framework for business operations and commerce. As a result, research groups and industry in the United States, Europe and Asia are undertaking a broad array of Grid research and technology development efforts. Particle physicists in these regions have taken a leading role in defining a unifying architectural framework and in deploying a common multi-continent Grid laboratory, including a multi-Gigabit/second link between the United States and Europe, in partnership with other disciplines. The scale of this laboratory, which has a large focus on LHC computing, is expected to greatly advance progress in Data Grid technologies. Support for Grid projects has rapidly increased with time, with funding levels expected to exceed $100M over the next three years.

5.5.4 Connections Outside our Field

In developing and deploying advanced information technologies for our field, high-energy physicists work closely with industry and other disciplines, particularly computer science. In networking, American university and laboratory physicists are strongly involved in the activities of NSF (Internet2) and DOE (ESNet) supported networks, international networking committees, and the funding of a U.S.-CERN international link.

Data intensive research, an area where particle physics has recognized expertise and where it continues to carry out pioneering work, is benefiting other scientific and engineering disciplines whose research requires managing and accessing massive data archives. Particle physicists in the United States have joined with computer scientists, astronomers, and gravity wave experimenters to develop and build large-scale Data Grids; these scientists, in collaboration with European and Asian partners, are playing a leading role in the worldwide development of the Data Grid infrastructure and facilities that will be used by many scientific disciplines.

These collaborative activities clearly benefit other disciplines, but they also have a broad societal impact through improved products and through the training of students and postdocs.

5.5.5 The Future of Information Technology in High-Energy Physics

The particle physics community, in coordination with U.S. funding agencies and international partners, must aggressively invest in and develop information tools and technologies to enhance the productivity of future international collaborations and facilities. This activity should be structured similar to an international collaboration and explicitly include partnerships with computer science, other application disciplines and industry. Procedures should be implemented to help experiments exploit information technologies more productively and efficiently.

Particle physicists must be closely involved in the development of wide area networks, particularly the international networks that are crucial for tomorrow's global collaborations. This involvement should be made in collaboration with computer scientists and industry, and should include working groups within national and international networking committees, pursuit of new advanced networking initiatives within DOE and NSF, and development of joint networking initiatives with other disciplines and international partners.

5.6 Summary

The long-term future of high-energy physics depends critically on developing the human and technical resources necessary to attack the challenging scientific problems in our field. In this chapter we have described the strong integration of university-based scientists into our program, and we emphasize the importance of maintaining a healthy university program in the face of budget constraints and other needs. The university program must remain a central part of our field.

To do our science, we need high-energy particle accelerators, some of the most sophisticated and ambitious scientific instruments we are able to make. Historically, particle physics has progressed by investing substantial effort and resources in developing these machines. The resulting technologies have propelled our field, and have found broad application in other areas of science and society as well. Accelerator challenges will be at least as great in the future, so we emphasize the importance of a vigorous and healthy accelerator R&D program.

Finally, to use these sophisticated facilities, we need particle detectors and computer systems that challenge the state of the art. For that reason, we must also invest significant resources to develop detector techniques and computing applications that satisfy our requirements.

APPENDIX A: Roadmap for Particle Physics

In this Appendix, we present our roadmap for particle physics, outlining potential projects and opportunities for the U.S. program. The roadmap reflects our present understanding, and will be regularly revised as part of the P5 process. Dates and costs will change as more information becomes available. All costs in this report include estimates of contingency but not escalation.

A.1 Energy Frontier

The Tevatron collider is presently the world's highest energy accelerator. The CDF and DØ experiments are embarked on Run II, pursuing a rich physics agenda that includes the search for the Higgs and supersymmetry, studies of CP violation, and the first detailed examination of the top quark.

CDF and DØ will phase out when the LHC begins operation during the second half of this decade. U.S. particle physicists are making essential contributions to the LHC

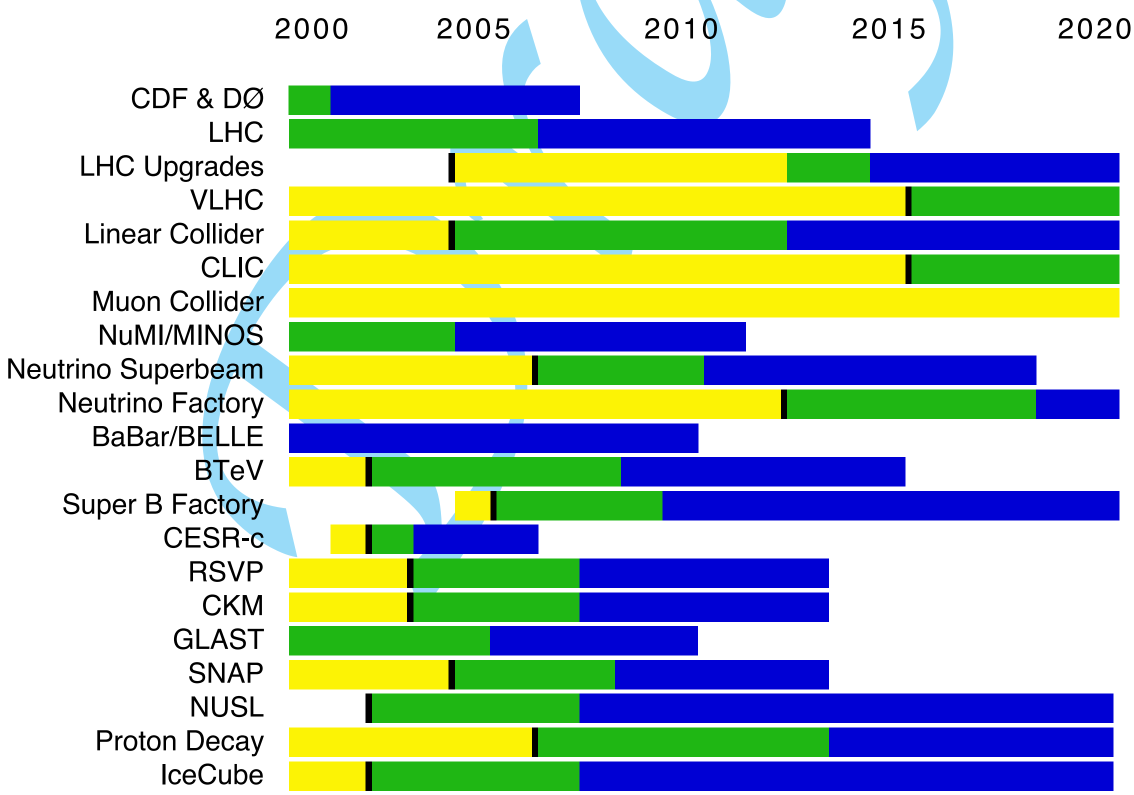


Figure A.1: Timelines for Selected Roadmap Projects. Approximate decision points are marked in black. R&D is marked in yellow, construction in green, and operation in blue. All timelines will be updated as part of the P5 process.

accelerator and the ATLAS and CMS experiments. The LHC will revolutionize our field when it begins operation. U.S. participation in the LHC is essential to reach the scientific goals described in Chapter 1.

Exploration of the energy frontier will require a high-energy, high-luminosity electron-positron linear collider. The LHC and the linear collider are both needed to discover and understand the new physics at the TeV scale. Full participation in the design, construction and operation of the linear collider is our highest priority.

A.1.1 LHC Luminosity Upgrade

The LHC accelerator will reach its design luminosity four or five years after it begins operation. Beyond that, a luminosity increase of an order of magnitude is thought to be feasible. Physics studies indicate a 20% greater mass reach with the enhanced luminosity.

LHC detector upgrades will be necessary with or without a luminosity upgrade. They will be designed with increased luminosity in mind. The most significant challenges will be in tracking, triggering and data acquisition, as well as calorimetry and muon detection at large rapidities. The actual upgrades will depend on detector performance and on the potential for additional discoveries.

For upgrades to begin in 2011, detector and accelerator R&D needs to start in the middle of this decade. The U.S. contribution to the upgrade itself is estimated to be about $100M. We believe that it is important that the U.S. participate in the upgrade of the LHC and its experiments. LHC physics is central to our long-range goals, and upgrades are a cost-effective way to leverage our large investment in this facility.

A.1.2 Electron-Positron Linear Collider

The electron-positron linear collider is at the center of our roadmap. It is described in Chapters 3 and 4.

A.2 Lepton Flavor Physics

We expect that important new experiments will be proposed in this area during the next two decades. Some will use accelerators, either existing or upgraded; others will not. Some might benefit from facilities in this country; others can be carried out abroad.

Results from solar and atmospheric neutrinos motivate a worldwide program of accelerator-based long-baseline neutrino experiments. In the United States, the MINOS experiment is being built to measure neutrino oscillations between Fermilab and the Soudan mine in Minnesota. Construction will be complete in the middle of the decade, and the experiment is scheduled to take data for five years. Other experiments include KamLAND and MiniBooNE, and its extension, BooNE.

A.2.1 Accelerator-Based Neutrino Oscillation Experiments

The worldwide neutrino program will be shaped by results from the present generation of experiments. The possibility of studying CP violation in the neutrino sector motivates the development of intense neutrino sources, based on “superbeam” facilities, and of neutrino factories, based on muon storage rings. Several possibilities are under discussion, either as new facilities or as substantial upgrades to existing accelerators.

An intense neutrino source will require a new (or upgraded) proton driver capable of delivering one or more megawatts of beam power. The driver could also provide beams of muons and kaons for rare decay studies. It could also be a first step toward a future very high intensity neutrino factory and possibly a muon collider.

Several proton driver projects are under consideration. In Japan, the JHF has been approved with a 50 GeV proton beam. Its 800 kW beam power in Phase I will ultimately be raised to 3 MW. In Europe, a superconducting linac is being considered that would use existing cavities to achieve 4 MW at 2.2 GeV. At Fermilab and Brookhaven, upgrades to existing facilities are being evaluated. These upgrades would deliver 1 to 4 MW beams at energies between 8 and 120 GeV. Finally, at Rutherford, a modest upgrade to a rapidly cycling synchrotron is being discussed.

The far detector will be an important component of any long-baseline experiment. This detector could be a very large, underground water Cherenkov detector that could also search for proton decay (see A.4.1).

The JHF is likely to be the first step in an international program of superbeam facilities. Future steps will be proposed as the physics capabilities and technical means come into sharper focus. Proposals for U.S. involvement in on-shore or off-shore superbeam projects will be evaluated by P5 and/or HEPAP, as appropriate, and may become a major part of the future U.S. particle physics program. A superbeam facility in the United States would cost approximately $500M.

Results from superbeam facilities would inform a decision on whether or not to pursue a neutrino factory based on a muon storage ring, perhaps near the end of this decade. Such a facility would cost significantly more than $1B.

A.2.2 Non-Accelerator Neutrino Experiments

Advances in the neutrino sector may come, as they have in the past, from experiments using natural sources. The Super-Kamiokande experiment has confirmed atmospheric neutrino deficits beyond doubt. Measurements of solar neutrino oscillations from SNO and Super-Kamiokande, and future results from reactor experiments, like KamLAND, will point the way to a new generation of solar neutrino detectors.

The new detectors promise to measure the real-time flux of neutrinos produced by p-p reactions in the Sun. Other detectors will search for neutrinoless double beta decay reactions that can measure other important neutrino properties, and possibly determine whether neutrinos are their own antiparticles. Both types of experiments need low-background environments, so they could be important components of the research program at an underground laboratory.

A.2.3 MECO

The MECO experiment is described below in the section on Very Rare Processes.

A.3 Quark Flavor Physics

Bottom and charm physics are an important part of our ongoing program, with dedicated experiments being carried out at the SLAC and KEK B-Factories, and a small-scale charm effort proposed at Cornell. Furthermore, the BTeV collaboration has submitted a proposal for a major hadronic B experiment at Fermilab. Bottom-quark physics will also be studied by LHC-b at the CERN LHC, and possibly by experiments at the linear collider.

Depending on the results of present experiments, heavy flavor physics could be further explored by a very high luminosity electron-positron collider operating at the Υ(4S). Such a machine could be built as a major upgrade to existing SLAC or KEK facilities.

A.3.1 BTeV

The BTeV experiment is designed to probe for new physics at the electroweak scale by searching for inconsistencies in the CKM description of bottom quark transitions. It would carry out precision studies of CP asymmetries and flavor-changing processes in the B meson system. Through its cutting edge detector technology, BTeV's physics reach exceeds that of other planned experiments in some of the important measurements of the B system.

The Fermilab Program Advisory Committee evaluated BTeV last year and found that it "has the potential to be a central part of an excellent Fermilab physics program in the era of the LHC. With excitement about the science and enthusiasm for the elegant and challenging detector," the Committee unanimously recommended Stage I approval at the laboratory.

BTeV would normally be a candidate for P5 evaluation. With a total project cost of $250M, it has significant impact on the overall HEP budget and programmatic implications for the future of the Tevatron. Its science overlaps but does not duplicate that of LHC-b. If there were time, P5 would rank BTeV in relation to the rest of the program.

The BTeV collaboration has been waiting over a year for a funding decision. We cannot ask BTeV to wait for the start of the P5 process, so our subpanel must offer guidance. Budget constraints and programmatic concerns made it impossible to fund BTeV as a line item in FY 2002. Our projections show that we cannot fund BTeV as a line item in the near future. Therefore, despite the PAC's strong recommendation and BTeV's attractive physics program, we regret that we cannot recommend funding BTeV as a line item at this time.

A.3.2 CESR-c

The CLEO collaboration has proposed a program using electron-positron annihilation in the 3 to 5 GeV energy region, optimized for physics studies of charmed particles. These studies would use the CESR storage ring, modified for running at lower energies, and the upgraded CLEO detector. The storage ring would offer significantly higher luminosity and the CLEO detector would provide much better performance than has been available to previous experiments in this energy region.

The improved measurements of charmed particle properties and decays are matched to theoretical progress in calculating charm decay parameters using lattice QCD. The conversion of the storage ring for low energy running would cost about $5M, and could be completed in a year, so that physics studies could begin sometime in 2003. The physics program would then require three years of running. The subpanel endorses CESR-c and recommends that it be funded.

A.3.3 Super B Factory

Feasibility studies have started on the possibility of major upgrades to the SLAC and KEK B Factories. A major upgrade of one of these facilities could provide an instantaneous luminosity sufficient to deliver a data sample up to 50 ab^{-1} in size, or about 100 times the data sample expected from the current program.

The major physics goals for the increased luminosity are order of magnitude improvements in measurements of CKM matrix elements and improved sensitivities to rare decay modes. Branching ratios would be measured in as many ways as is feasible. Detectors would need to be upgraded to handle the large data rate while preserving the ability to make sensitive measurements. At SLAC, it is currently estimated that upgrading the accelerator and detectors would cost on the order of $500M. A decision on a major luminosity upgrade to one of the existing B-factories should be made after the physics case is more fully worked out and we know where the linear collider will be built. We anticipate that this decision can be made after 2005.

A.4 Very Rare Processes

Very rare processes provide additional probes of quark and lepton flavor physics (see A.2 and A.3), and offer important insights into the nature of physics at the TeV scale and beyond. For example, the discovery of proton decay or neutron-anti-neutron oscillations would point toward the ultimate unification of forces. The observation of lepton flavor mixing would have other important consequences for unified theories. Studies of highly suppressed K meson decays, and comparisons between measurements made in the K and B systems, would allow new tests of quark flavor structure.

A.4.1 Proton Decay

If protons decay, their lifetimes are long, so proton decay experiments require massive detectors. A worldwide collaboration has begun to develop the design for a next-generation proton decay experiment. Such a detector should be at least an order of magnitude larger than Super Kamiokande. A next-generation experiment would extend the search for proton decay into the regime favored by unified theories.

Current thinking favors the use of a large water Cherenkov detector, as in the UNO approach. The detector would be situated underground to reduce cosmic-ray backgrounds. A large water Cherenkov detector could simultaneously serve as the long-baseline target for an accelerator neutrino beam. It would also expand our ability to observe neutrinos from supernovae.

Present estimates suggest a price of about $650M for such a detector. Given its strong science program, and assuming that an affordable design can be reached, we believe it likely that a large proton decay detector will be proposed somewhere in the world, and that U.S. physicists will participate in its construction and utilization. The R&D effort should be completed over the next several years. A decision might be made near the middle of the decade, perhaps in conjunction with a decision on a neutrino superbeam facility.

A.4.2 CKM

CKM is a flagship experiment for a future fixed target program at the Fermilab Main Injector. The experiment intends to constrain the Standard Model quark mixing parameters by measuring the branching ratio for $K^+ \rightarrow \pi^+ \nu \bar{\nu}$ with about 100 signal events and 10% background. Comparison with other experiments would probe flavor-changing physics beyond the Standard Model. The experimental challenge will be to achieve the photon veto necessary to eliminate background events involving neutral pions.

CKM has received Stage I approval at Fermilab. The $100M experiment will need P5 approval in 12 to 18 months. The P5 evaluation will balance the funding request, the physics case, and programmatic issues.

A.4.3 RSVP

RSVP aims to conduct two experiments at Brookhaven to measure small but dramatic symmetry violations in muon conversion and kaon decay.

The first of these experiments, MECO, seeks to measure muon-electron conversion in the presence of a nucleus. The MECO collaboration proposes to search for this process to a level 10,000 times more sensitive than any previous experiment. A measurement at this sensitivity would probe a substantial range of new physics, particularly supersymmetry-based models of lepton-flavor violation. The main challenge for MECO will be to reduce backgrounds to achieve the most sensitive measurement allowable.

The second experiment, KOPIO, plans to measure a highly suppressed flavor-changing K meson decay, predicted to occur in the Standard Model with a branching fraction of 3×10^{-11}. The experiment aims to measure the branching fraction to approximately 20%, leading to a 10% determination of the CP violating parameter. Comparison of results from the K and B systems would provide a powerful probe for new sources of CP violation.

The RSVP experiments will run at Brookhaven to take advantage of the AGS pulsed beam structure. This structure should eliminate many sources of background, the largest challenge for such experiments. The National Science Board has approved the $115M RSVP proposal as an MRE. The subpanel endorses the physics goals of RSVP and believes it is a timely opportunity to pursue.

A.4.4 National Underground Science Laboratory

There has been considerable interest in developing a deep underground laboratory to carry out a diverse program of scientific research, much of it directly related to particle physics. A number of locations have been considered, including Homestake (South Dakota), San Jacinto (California), Soudan (Minnesota), and WIPP (New Mexico). Of these sites, only Homestake and San Jacinto are deep enough to provide the very low background required for a variety of experiments.

Worldwide, the program of experiments of interest to particle physicists that require underground locations is broad and often technically challenging. Experiments include: searches for neutrinoless double beta decay; searches for weakly interacting dark matter; measurements of solar, atmospheric, reactor and supernova neutrinos; searches for proton decay; and studies of neutrino properties using beams from distant accelerators.

Construction of a National Underground Science Laboratory at the Homestake Mine has been proposed to NSF. A proposal for a laboratory under the San Jacinto mountain has been submitted to DOE and NSF. These proposals are motivated by a very broad science program, from microbiology to geoscience to physics. Construction of a national underground laboratory is a centerpiece of the NSAC Long Range Plan.

We believe that experiments requiring very deep underground sites will be an important part of particle physics for at least the next twenty years, and should be supported by the high-energy physics community. Particle physics would benefit from the creation of a national underground facility.

A.5 Cosmology and Particle Physics

There is currently a vigorous program of cosmological investigation supported by NASA, DOE, NSF, and private sources. Modern cosmology is closely connected with particle physics. For example, cosmological measurements of dark energy and particle dark matter have direct implications for particle physics. We expect that this exciting field will continue to expand, and endorse a strong multi-agency approach to address its multi-faceted scientific goals. Here we discuss possible future efforts in this area that may be supported by the U.S. particle physics program.

A.5.1 Dark Energy

Dark energy can be probed by a number of techniques. Among the most powerful are measurements of the expansion rate of the universe from observations of Type Ia supernovae, and measurements of the large scale distribution of dark matter from observations of weak gravitational lensing. Telescopes in space and on the ground can exploit these techniques.

Several possible approaches are under development. SNAP proposes to use a 2m satellite telescope to detect many more supernovae, and measure their properties with significantly better accuracy, than present observations. LSST proposes to use an 8 m ground-based telescope optimized for weak-lensing studies. These two approaches have differing strengths and differing potential systematic limitations.

Several approaches will probably be necessary to fully understand the nature of dark energy. So far, only SNAP has requested funding from the particle physics program. The SNAP team has developed an instrument concept and has requested R&D funding to develop a full instrument design, as well as to estimate the total project cost. We endorse R&D funding to carry out these tasks.

The project is expected to cost approximately $400M, including launch. We recommend that the full SNAP project, if approved, include significant NASA participation in the construction and launch of the instrument, in partnership with DOE and NSF. A decision on whether to build SNAP is expected in 2004-5.

A.5.2 Dark Matter

A variety of experiments are underway to learn the origin of dark matter. At the present time, several smaller-scale projects are partially funded by the U.S. particle physics program. There has been discussion within the dark matter community of possible medium-scale efforts in the future, but as yet, there are no concrete proposals. A next-generation dark matter experiment would require a low-background environment; it would be well-suited to a deep underground laboratory.

A.5.3 Connections Between Particle Physics and Cosmology

The quest to understand the origins of dark energy and dark matter are important components of a broader program of cosmological measurements, including studies of the cosmic microwave background radiation and the large-scale structure of the universe.

Particle physicists are involved in this broad program through a variety of experimental efforts. We expect that this effort will continue to grow during the next decade.

A.6 High-Energy Particle-Astrophysics

A variety of observations have dramatically changed our understanding of very high-energy particles from space during the last decade. Gamma-ray telescopes, both in space and on the ground, have detected many more astrophysical sources of GeV and TeV radiation than had been previously suspected. Ultrahigh-energy cosmic ray experiments have detected particles at energies beyond the expected cutoff. The existence of these particles suggest either powerful cosmic accelerators whose mechanism we do not understand, or possible new physics beyond the Standard Model. We also suspect that the central regions of powerful astrophysical sources produce very high-energy neutrinos that we can detect on Earth.

A number of experimental projects are supported in part, or in whole, by the U.S. particle physics program. Larger efforts include GLAST and the proposed VERITAS (gamma rays), HiRes and Pierre Auger (ultra high-energy cosmic rays), and AMANDA and IceCube (neutrinos). Most of these projects have a substantial involvement of the international community. GLAST is an example of a successful partnership between DOE and NASA. We expect that such experiments will continue and that new proposals will emerge. Here we list projects that are on the immediate or near-term horizon.

A.6.1 Ice Cube

A number of experimental efforts are underway worldwide to develop a large detector for very high-energy neutrinos. IceCube is a proposed detector to be built in the South Pole ice, following on the successful construction and operation of the AMANDA detector. The effective area of IceCube is a factor of thirty times larger than any previous neutrino telescope built to date. Its size gives it unprecedented sensitivity to astrophysical sources of TeV and PeV neutrinos, including gamma-ray bursts and active galactic nuclei. IceCube is also sensitive to processes of interest to particle physics, including supersymmetric WIMP annihilation in the Earth or Sun.

The National Science Board has approved the $240M IceCube proposal as an MRE. We endorse the scientific goals of IceCube as an example of a mutually beneficial cross disciplinary effort between astrophysics and particle physics.

A.6.2 Highest-Energy Cosmic Rays

The Pierre Auger Observatory consists of a large array of charged particle detectors and several wide-angle atmospheric fluorescence detectors. The goal of Pierre Auger is to probe the origins of the highest-energy cosmic rays through measurements of their energy spectra, anisotropies, and compositions. The southern hemisphere observatory is currently under construction in Argentina by an international collaboration from more than thirty countries. A decision will be taken during the next few years on whether to go forward with the northern observatory. The U.S. contribution to the northern hemisphere Auger observatory will be about $25M.

Planning efforts are underway to develop the next generation of ultrahigh-energy cosmic ray instruments. These efforts, which include the Telescope Array, EUSO, and OWL, may request partial funding from the U.S. particle physics program.

A.7 The Far Frontier

The ultimate goals of the scientific roadmap in our field will demand accelerators with an energy reach well beyond the LHC and linear collider. Future experiments will determine the energy and luminosity thresholds for the new accelerators. But even today, we can say with considerable certainty that we will eventually need even more powerful accelerators to examine the new symmetries, possible extra dimensions, and the new interactions that will lead to ultimate unification.

In the future, the global particle physics community will likely require a multi-TeV hadron-hadron collider and a multi-TeV lepton-lepton collider, working in concert. Hadron colliders generally provide the raw energy to uncover new physics. The point-like probes of lepton colliders generally provide precision measurements to expose crucial details of the new physics.

A.7.1 Muon Collider

The lepton-lepton collider has traditionally been our most powerful tool for precision measurements of new states. Extrapolating from our current understanding of accelerator physics, we expect this to remain true for the time period covered by our roadmap.

Radiation losses represent a major technical challenge for very high-energy lepton-lepton colliders. One potential solution is to accelerate muons, whose higher mass reduces these losses, enabling higher energies to be reached with circular machines. The small radiative losses lead to a very small beam energy spread, which allows very precise measurements of the masses and widths of new states.

The difficulty is that muons are unstable particles. Achieving muon production, beam cooling, and acceleration, all within the lifetime of the muon, is a daunting technical challenge, but one that is thought to be possible after appropriate accelerator R&D.

Muon collider R&D is closely related to R&D for a muon-based neutrino factory. The Muon Collider Collaboration has decided to focus its R&D in the latter direction. We support this decision, and recommend continued R&D near the present level of effort. We also encourage strong international collaboration to make the best use of investments in this field. Approximately $8M is spent per year on this research.

The timescale of a muon-based neutrino factory is discussed in A.2.1. The timescale for realizing a muon collider is beyond the end of this report.

A.7.2 Multi-TeV Electron-Positron Collider

R&D on a multi-TeV electron-positron linear collider is being vigorously pursued at CERN by an international collaboration from Europe, Russia, Japan, and, to a limited extent, the United States The Compact Linear Collider (CLIC) study is exploring the technical feasibility of beam acceleration by traveling wave structures at room temperature and very high frequency (30GHz), powered by a drive beam, the so-called two beam acceleration scheme. In this approach, RF power for the main linac is extracted from a secondary, low-energy, high-intensity electron beam, running parallel to the main linac. The target accelerating gradient is 150 MV/m. The ultimate goal is a 3-5 TeV linear collider with high luminosity, 10^{34} –10^{35}cm^{-2}s^{-1}.

A test facility is being constructed at CERN to demonstrate technical feasibility, in particular the key concept of the novel power source. A decision on a CLIC project is not anticipated until after the LHC and linear collider are operational.

A.7.3 Very Large Hadron Collider

The Very Large Hadron Collider (VLHC) is the term for a proton-proton collider with an energy beyond the CERN LHC. Early plans envisioned a center-of-mass energy of order 100 TeV, as compared to the 14 TeV of the LHC.

Recent VLHC design studies have explored technologies and accelerator physics issues for such a machine. They have considered a staged approach, in which the first stage would employ relatively inexpensive low field magnets to achieve proton-proton collisions with a center-of-mass energy of about 40-50 TeV, and a maximum design luminosity similar to that of the LHC (10^{34} cm^{-2}s^{-1}). The second stage would use much higher field magnets in the same tunnel. Using the first ring as an injector, the high field accelerator would aim for energies of 100-200 TeV, with a maximum luminosity of 2×10^{34} cm^{-2}s^{-1}. Alternative VLHC designs have also been discussed, with smaller circumferences and intermediate-field magnets.

The history of elementary particle physics illustrates the importance of higher energies; we believe it very likely that a VLHC will be necessary. However, it is difficult to propose specific machine requirements until the physics discoveries of the LHC and linear collider are known.

We strongly support R&D on a VLHC, and recommend that it be continued at about the current level of effort. We also suggest that the research take a long-term perspective toward developing new technologies and techniques relevant to such a machine, especially at high luminosities. High-field magnet research is a particularly important area for R&D. To assemble the necessary intellectual and financial resources, an international collaboration should be formed as early as possible.

APPENDIX B: Charge to the Subpanel

Professor Frederick Gilman
Carnegie Mellon University
5000 Forbes Avenue
Pittsburgh, PA 15213

Dear Professor Gilman:

This letter is to request that the High Energy Physics Advisory Panel (HEPAP) establish a subpanel to review the central scientific issues that define the intellectual frontier of particle physics research and, based on that review, to develop a long-range plan for the U.S. High Energy Physics (HEP) program. The plan should include careful consideration of the international character of HEP research and the present and future role of U.S. physicists in international HEP research collaborations.

The U.S. High Energy Physics program supported by the Department of Energy (DOE) and the National Science Foundation (NSF) addresses some of the most profound intellectual questions in science--questions whose answers have altered our basic understanding of matter, space and time, and of the forces which govern the genesis and very structure of the universe. It is no wonder that the excitement of this field captures the imagination and interest of some of the brightest young people worldwide.

The U.S. community has played a leadership role in many of the most important discoveries in HEP. This has occurred because of the world-class facilities developed, constructed, and operated in this country, and the experiments at these facilities that have produced answers to a broad range of fundamental questions. However, there are still many outstanding theoretical questions that can only be addressed by advanced research facilities.

With the completion of the Large Hadron Collider (LHC) in the middle of this decade, the United States will no longer have a facility operating at the energy frontier, where critical discoveries are likely to be made. Meanwhile, international studies exploring the technical feasibility and potential performance of near-future and next-generation facilities are in progress. In addition, new proposals for innovative non-accelerator experiments offer many exciting scientific opportunities. Therefore, it is timely for the U.S. program to examine its long-term research directions and needs in terms of maintaining its traditional role among the world leaders in HEP research.

Thus, we are charging the subpanel to undertake a long-range planning exercise that will produce a national roadmap for HEP for the next twenty years. The subpanel should describe the discovery potential and intellectual impact of the program and recommend the next steps to be taken as part of an overall strategy to maintain the United States in a leadership role in HEP. In considering the many scientific opportunities facing the field and some potentially large associated costs, the plan will have to address some difficult questions, weigh options, and set priorities. In particular, the subpanel should weigh the scientific promise and programmatic importance of both accelerator and non-accelerator based efforts in relation to their expected costs. To be most helpful, the plan should indicate what funding levels the roadmap would require (including possible construction of new facilities), and what the impacts and priorities should be if the funding available provides constant level of effort (FY 2001 President's Budget Request) into the outyears (FY 2002-2022).

As part of the charge, the subpanel, in developing its plan, should address the following central issues:

1. MAJOR INTELLECTUAL CHALLENGES & SCIENTIFIC APPROACHES:
What are the central questions that define the intellectual frontier of HEP? The reach of the subpanel's considerations should include the accelerator-based particle physics program, related activities in astrophysics and cosmology, theory, and the proper balance of these elements. Describe these questions in relation to the tools, existing and new, required to effectively explore them.

2. STRATEGY REGARDING THE ENERGY FRONTIER:
The leading discovery tool in HEP in the 20th century, and as far into the future as one can see, is the energy frontier accelerator/storage ring. In the context of the worldwide scientific effort in particle physics, formulate a plan that optimizes the U.S. investment of public funds in sustaining a leadership role at the high-energy frontier, including a recommendation on the next facility that will be an integral part of the U.S. program.

3. MEETING TECHNOLOGY CHALLENGES:
Identify technology developments essential for new instruments and facilities required to address the central questions noted above, and how these developments are captured in R&D plans. Explain the connection and importance of these R&D activities to the U.S. HEP program over the 20-year span of the plan developed by the subpanel.

4. BROAD IMPACTS AND INTELLECTUAL RENEWAL OF HEP:
Summarize the wide-ranging impacts of the field on society; and recommend ways in which the excitement and the broad, long-term benefits of HEP can be maintained and conveyed to students at all levels, to society at large, and to government.

There have been several high quality strategic HEP planning efforts in the past few years, and we expect the subpanel to take advantage of the wisdom and information contained therein. Those excellent reports notwithstanding, there is a need for the community to go further in the present exercise. Specifically, the long-range plan must contain a broad vision of the future of HEP in terms of resources needed; and further, it must enjoy the widespread support of the U.S. HEP community. This clearly will require extensive consultation with leaders of the field, and with the community, through such mechanisms as the Snowmass Workshop being planned for July 2001, and other town meetings and proactive interactions. Although we want the community to enunciate its vision of the future in the way that seems most appropriate, the subpanel's plan must also be responsive to the specific charges given above.

The long-range plan should have a concise executive summary that is accessible to government officials, the press, and scientists in other fields. In addition, a briefing book consisting of presentation material should be produced to facilitate communication of the long-range vision to diverse audiences. It would be most useful in the budget planning cycle to have a draft of the report and the briefing book by October 1, 2001, with the final forms of the publications by January 1, 2002.

We believe that the following decades will see revolutionary advances in our basic knowledge of matter, space and time, advances that will profoundly impact fundamental science and our understanding of the universe, and which will also become an integral part of our culture at many levels. If this quest is to be successful, it will require a unified and vibrant HEP community.

We wish you well in this important exercise.

Sincerely,

Mildred S. Dresselhaus
Director, Office of Science
U.S. Department of Energy

Robert A. Eisenstein
Assistant Director forMathematical and Physical Science
National Science Foundation

cc: J. Dehmer, NSF
M. Goldberg, NSF
J. O'Fallon, SC-22
S. Peter Rosen, SC-20

APPENDIX C: Subpanel Membership

Jonathan Bagger - Co-Chair
Johns Hopkins University

Paul Avery
University of Florida

Janet Conrad
Columbia University

Persis Drell
Cornell University

Glennys Farrar
New York University

Larry Gladney
University of Pennsylvania

Don Hartill
Cornell University

Norbert Holtkamp
Oak Ridge National Laboratory

George Kalmus
Rutherford Appleton Laboratory, UK

Rocky Kolb
Fermi National Accelerator Laboratory

Joseph Lykken
Fermi National Accelerator Laboratory

William Marciano
Brookhaven National Laboratory

Barry Barish - Co-Chair
California Institute of Technology

Jay Marx
Lawrence Berkeley National Laboratory

Kevin McFarland
University of Rochester

Hitoshi Murayama
University of California at Berkeley

Yorikiyo Nagashima
Osaka Univiversity, JAPAN

Rene Ong
University of California at Los Angeles

Tor Raubenheimer
Stanford Linear Accelerator Center

Abraham Seiden
University of California at Santa Cruz

Melvyn Shochet
University of Chicago

William Willis
Columbia University

Fred Gilman (Ex-Officio)
Carnegie Mellon University

Glen Crawford (Executive Secretary)
Department of Energy

John Marriner
Fermi National Accelerator Laboratory

APPENDIX D: Letters to the Community

FIRST LETTER

January 18, 2001

Dear Colleague –

We are writing to ask your help. We are the co-chairs of a new panel that has been commissioned by the Department of Energy and the National Science Foundation. The panel has been asked to chart a twenty-year future for the U.S. program in high-energy physics. Its full charge can be found at

http://hepserve.fnal.gov:8080/doe-hep/lrp_panel/charge.html

We would like your suggestions for members of this panel. We are seeking scientists who have a broad vision for the future of our field. We are looking for physicists with a thorough understanding of the issues we face, as well as a deep appreciation of the connections between physics, astronomy, and the other sciences. Our goal is to form a panel that is broadly representative of our community.

Please send your ideas and suggestions to us, at panel@pha.jhu.edu. We will give them careful consideration. As the process unfolds, we will be in touch again to seek your comments on the issues at hand.

Thank you –

Jonathan Bagger

Barry Barish

SECOND LETTER

April 4, 2001

Dear Colleague –

We are delighted that our last letter generated many thoughtful suggestions for the membership of our panel on future planning in US high-energy physics. The panel is now complete and the roster is posted on our web site, at

http://hepserve.fnal.gov:8080/doe-hep/lrp_panel/index.html

Last week the panel held its first meeting in Washington; the agenda and presentations are available on the web site. (The presentations from future meetings and other reference material will be available at the same location.)

We believe that it is crucial for us to include the community in our deliberations. As a first step, we have scheduled a series of town meetings in conjunction with our visits to Brookhaven, SLAC and Fermilab. The dates and contact people are listed below. We hope to hear from many of you during these meetings.

In addition, we plan to solicit your thoughts in writing as the issues before us become more clear. We also look forward to seeing many of you at Snowmass, where we will hold another town meeting.

We appreciate your help in this important planning process.

Jonathan Bagger

Barry Barish

THIRD LETTER

June 20, 2001

Dear Colleague –

The HEPAP subpanel on long-range planning has completed the first phase of its information gathering process. During the past four months, the panel has heard a series of presentations in Gaithersburg and at Brookhaven, Fermilab and SLAC. The slides are available on the panel's web site, at

http://hepserve.fnal.gov:8080/doe-hep/lrp_panel/index.html.

The next phase will begin with the Snowmass Workshop in July. As part of that process, the panel would like to invite written comments from the community. To be most helpful, letters should address the issues raised by our charge. In particular, we are grappling with the following questions:

- What is the scope of particle physics?
- What are the most important scientific questions facing the field?
- What tools and approaches are required to address these questions?
- Does the science require a major new facility?
- If so, should the US bid to host it? What are the advantages and disadvantages? How might the disadvantages be mitigated?
- What are the essential elements of a successful international partnership? How should it be implemented?
- What is the role of astroparticle physics and cosmology in the field?
- What is the relation between particle physics and other fields of science and technology?
- What are the important issues facing university groups? What is their role in the future evolution of the field?
- What are the most pressing R&D goals for accelerators and detectors?
- What does particle physics offer to society?
- What are the contributions of our field to other areas of science and technology?
- How do accelerator and detector R&D benefit society?
- What should the particle physics community do differently?

We welcome your thoughts on these questions and the issues before us, as well as any other comments you might have. Please send your comments to panel@pha.jhu.edu. Unless you request otherwise, your letter will be posted on our web site.

Thank you for your help in this important endeavor.

Jon Bagger

Barry Barish

APPENDIX E: Communications from the Community

The subpanel heard presentations from the community during Town Hall meetings at Brookhaven, SLAC and Fermilab, as well as during public and private sessions during the Snowmass Workshop on the future of particle physics. The subpanel is grateful for the presentations, as well as for the thoughtful letters it received from the following members of the community.

A. Abashian
T. Adams
M. Albrow
W. Barletta
U. Baur
I. Bigi
H. Blumenfeld
T. Bolton
G. Brooijmans
D. Bugg
P. N. Burrows
J. Butler
W. Carithers
A. Chao
D. Christian
D. Cinabro
D. Cline
H. Davoudiasl
M. Derrek
R. Diebold
K. Dienes
M. Dima
M. Dine
M.V. Diwan
R. Erbacher
T. Fields
D. Finley
R. Frey
S. Geer
M. Gill
M. Goodman
P. Grannis
G. Gratta
M. Gundersen
R. Gustafson
H. Haber
G. Hanson
E. Hawker
L. Jones
G. Kane
D. Kaplan
R. Kephart
S. Klein
M. Kruse
Y. Kuno
K. Lane
P. Langacker
D. Larson
J. Learned
P. Limon
K.U. Lu
F. Mamedov
P. McIntyre
T. Meyer
J. Norem
J. O'Boyle
R. Palmer
R. S. Panvini
V. Papadimitriou
J. Pati
M. Paulini
M. Peskin
J. Pullin
V. Radeka
R. Raja
P. Ramond
D. Reeder
N. Roe
J. Rosner
Y. Semertzidis
R. Shrock
N. Solomey
S. Stone
M. Strassler
R. Sugar
B. Svoboda
F. Tangherlini
J. Thaler
A. Tollestrup
W. Tung
M. Turner
R. Vidal
C. Wagner
J. M. Williams
G. Wilson
R. Wilson
S. Wojcicki
J. Womersley
M. Woods
A. Yagil

APPENDIX F: Meeting Agendas

DOE/NSF HEPAP Subpanel on

Long-Range Planning for US HEP

Holiday Inn, Gaithersburgh MD

March 28-29, 2001

Wednesday, March 28

8:30am	Welcome (J. Bagger / B. Barish)
8:40	Introductory Remarks – NSF (R. Eisenstein)
9:00	Introductory Remarks – DOE (P. Rosen (by phone))
9:20	Overview of NSF Programs in HEP + Related Fields (J. Dehmer)
10:30	Break
10:45	NASA Perspectives on HEP (A. Bunner)
11:15	Overview of DOE HEP Program Physics Research; University Program; Facilities Operations; Technology R&D (J. O'Fallon, P.K. Williams, J. Ritchie, D Sutter)
12:25pm	Lunch
1:30	Remarks by the HEPAP Chair (F. Gilman)
1:45	NRC Study on Physics of the Universe (M. Turner)
2:15	Panel on Underground Physics (J. Bahcall)
2:45	The White Paper on Planning for US HEP (F. Gilman)
3:15	Break
3:30	Perspectives on the Future of HEP (M. Tigner)
4:00	Executive Session

6:00	Adjourn

Thursday, March 29

8:30am	Congressional Perspectives on HEP (H. Watson)
9:00	OMB Perspectives on HEP (M. Holland / D. Radzanowski)
9:30	Executive Session
12:30pm	Lunch
1:30	Executive Session
4:00	Adjourn

DOE/NSF Panel HEPAP Subpanel on

Long-Range Planning for US HEP

Brookhaven National Lab

April 19-20, 2001

Thursday, April 19

8:30am	Executive Session
8:45	Welcome (P. Paul)
9:00	Muon Collider and Neutrino Factory
	Overview (A. Sessler)
	Physics at a Neutrino Factory (D. Harris)
	Feasibility Studies (R. Palmer)
10:30	Break
10:45	Targetry Experiment and Plans (K. McDonald)
	MUCOOL Component R&D, Test Facilities and
	University Participation (D. Kaplan)
	Acceleration (H. Padamsee)
	R&D Plans (M. Zisman)
	Wrap-up (A. Sessler)
12:15pm	Working Lunch/Executive Session
1:30	Connections Between HEP and NP (W. Zajc)
2:00	NSAC Long-Range Plan (J. Symons)
2:30	BNL Perspectives on the Future of HENP (T. Kirk)
3:00	Break
3:15	Executive Session

4:30	Town Meeting

Friday, April 20

8:30	ATLAS Physics Program (I. Hinchliffe)
9:10	US ATLAS Research Program (H. Gordon)
9:30	US CMS Long-term Plans (D. Green)
10:30	Break
10:45	RSVP: The MECO Experiment (W. Molzon)
11:15	RSVP: The KOPIO Experiment (L. Littenberg)
11:45	Perspectives on the Future of HENP (N. Samios)
12:15pm	Perspectives on the Future of HENP (J. Sandweiss)
12:45	Working Lunch/Executive Session
1:30	Executive Session, continued
5:00	Adjourn

DOE/NSF Panel HEPAP Subpanel on

Long-Range Planning for US HEP

Stanford Linear Accelerator Center

May 23-24, 2001

Wednesday, May 23

8:30am	Executive Session
8:45	SLAC'S Perspective on the Future of HEP (J. Dorfan)
9:15	Linear Collider Physics
	The Case for a 500 GeV LC (P. Grannis)
	Physics Beyond 500 GeV at a LC (J. Hewett)
10:30	Break
10:45	The NLC Project
	The NLC (D. Burke)
	Linear Collider: From R&D to Construction (S. Holmes)
12:15pm	Working Lunch/Executive Session
1:15	SLAC Program Overview (S. Williams)
2:00	B Factory
	PEP-II: The Next 10 Years (J. Seeman)
	BaBar: The Next 10 Years (S. Smith)
2:45	GLAST (P. Michelson)
3:10	Advanced Accelerator R&D (R. Ruth)
3:45	Executive Session
4:30	Town Meeting

Thursday, May 24

8:00am	Executive Session
8:45	Cornell Program (J. Alexander, P. Lepage, M. Tigner)
9:55	LBNL Program (J. Siegrist)
10:25	SNAP (S. Perlmutter)
10:55	Break
11:10	CDMS (B. Sadoulet)
11:30	Perspectives on the Future of HEP (S. Drell)
12:00pm	Perspectives on the Future of HEP (B. Richter)
12:30	Perspectives on the Future of HEP (G. Trilling)
1:00	Working Lunch/Executive Session
2:00	Executive Session, continued
5:00	Adjourn

DOE/NSF Panel HEPAP Subpanel on

Long-Range Planning for US HEP

Fermi National Accelerator Lab

June 11-12, 2001

Monday, June 11

8:30am	Executive Session
8:45	Welcome and Overview of Fermilab Tevatron Upgrades (M. Witherell)
9:15	Physics & Detector Upgrades (J. Womersley)
9:45	Accelerator Upgrades (D. McGinnis)
10:15	Break
10:45	Future Prospects for Neutrino and Fixed Target Physics (M. Shaevitz)
11:25	BTeV (J. Butler / S. Stone)
12:05pm	CKM + KAMI (R. Tschirhart)
12:25	Lunch/Executive Session
1:55	VLHC Introduction (P. Limon)
2:05	Physics Possibilities (U. Baur)
2:40	Accelerator Physics (M. Syphers)
3:00	Magnets & Accelerator Systems (G.W. Foster)
3:20	Geology & Civil Construction (P. Garbincius)
3:40	Summary (P. Limon)
3:55	Executive Session
4:45	Town Meeting
7:15	Adjourn

Tuesday, June 12

8:30am	Accelerator R&D at Fermilab (S. Holmes)
9:00	Future of Fermilab and US HEP (M. Witherell)
9:45	Auger (J. Cronin / P. Mantsch)
10:05	Break
10:30	SDSS (S. Dodelson)
10:50	Ice Cube (F. Halzen)
11:10	Perspectives on the Future of HENP (L. Lederman)
11:40	Perspectives on the Future of HENP (J. Peoples)
12:10pm	Lunch/Executive Session
5:00	Adjourn

DOE/NSF HEPAP Subpanel on
Long-Range Planning for US HEP
Latham Hotel, Washington D.C.
August 16-18, 2001

Thursday, August 16
NCSA Alliance Center
901 Stuart Street Suite 800
Arlington, Virginia

8:30am	Executive Session
8:45	DPF Overview (S. Dawson)
9:15	DPB Overview (R. Davidson)
9:45	Electroweak Symmetry Breaking (A. Turcot)
10:15	Break
10:30	Flavor Physics (B. Kayser)
11:00	Scales Beyond 1 TeV (J. Hewett)
11:30	Astro/Cosmo/Particle Experiments (T. McKay)
12:00pm	Particle Physics and Technology (W. Smith)
12:30	Adjourn

The Latham Hotel
3000 M Street NW
Washington D.C.

1:00	Working Lunch (with afternoon speakers)
1:45	Perspectives from State Department (N. Neuriter - State Dept.)
2:15	ITER (M. Roberts - DOE)
2:45	NASA Perspectives (A. Bunner -NASA)
3:15	ALMA (R. Dickman -NSF)
3:45	Gemini Telescope (W. VanCitters - NSF)
4:15	LHC (T. Toohig - DOE)
4:45	TESLA (H. Krech - DESY)
5:30	Global Science Forum (S. Michalowski - OECD)
6:15	Adjourn

Friday, August 17

8:30am	Executive Sessions (All Day)

Saturday, August 18

8:30am	Executive Sessions (All Day)