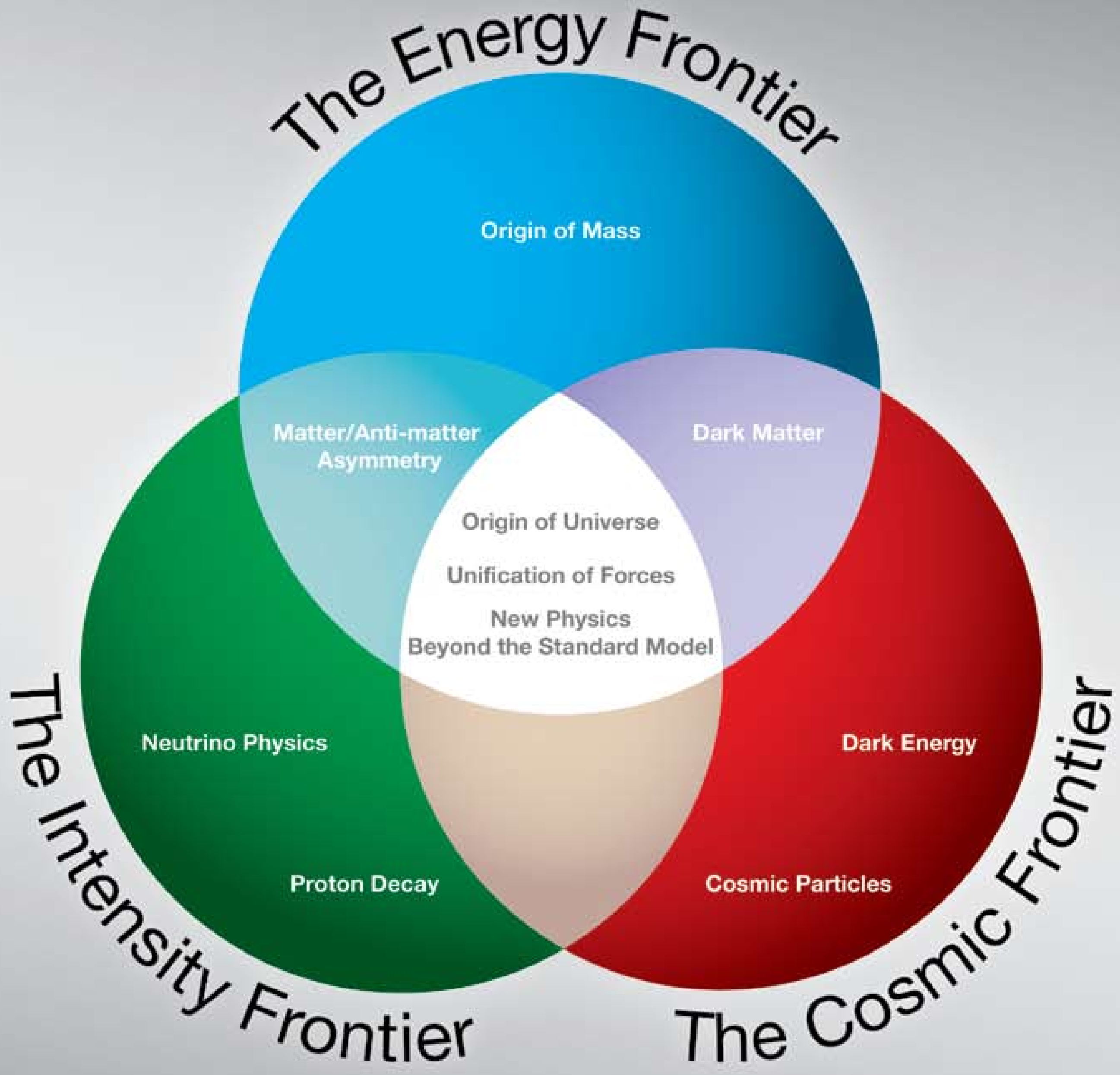
US Particle Physics: Scientific Opportunities

A Strategic Plan for the Next Ten Years

Report of the Particle Physics Project Prioritization Panel

29 May 2008



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About the P5 Subpanel

In November 2007, at the request of the Office of High Energy Physics of the Department of Energy and the National Science Foundation, the High Energy Physics Advisory Panel reconstituted the Particle Physics Project Prioritization Panel for the purpose of developing a plan for US particle physics for the coming decade under a variety of budget assumptions. Appendix A of this report gives the charge to the P5 panel; Appendix B lists its membership. To carry out this charge the panel organized three information-gathering meetings, at Fermilab in January, at Stanford Linear Accelerator Center in February, and at Brookhaven National Laboratory in March of 2008. Appendix C gives the agendas for these meetings. Besides talks by experts in the field, each of the three meetings included a Town Meeting, an open session where members of the community could voice their advice, suggestions and concerns to the panel. The panel also invited letters from the worldwide particle physics community, to offer their points of view for consideration. The panel held an additional meeting in early April to put together the first draft of this report.

The strategic plan and recommendations contained in this report, if adopted by HEPAP, are advisory input to the Department of Energy and the National Science Foundation. The actual design and implementation of any plan in these agencies is the responsibility of program management.

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Executive Summary

1 EXECUTIVE SUMMARY

Particle physics is a central component of the physical sciences, focused on the fundamental nature of matter and energy, and of space and time. Discoveries in this field, often called high-energy physics, will change our basic understanding of nature. The Standard Model of particle physics provides a remarkably accurate description of elementary particles and their interactions. However, experiment and observation strongly point to a deeper and more fundamental theory that breakthroughs in the coming decade will begin to reveal.

To address the central questions in particle physics, researchers use a range of tools and techniques at three interrelated frontiers:

- The Energy Frontier, using high-energy colliders to discover new particles and directly probe the architecture of the fundamental forces.

- The Intensity Frontier, using intense particle beams to uncover properties of neutrinos and observe rare processes that will tell us about new physics beyond the Standard Model.

- The Cosmic Frontier, using underground experiments and telescopes, both ground and space based, to reveal the natures of dark matter and dark energy and using high-energy particles from space to probe new phenomena.

As described in the box on pages 9-11, these three frontiers form an interlocking framework that addresses fundamental questions about the laws of nature and the cosmos. These three approaches ask different questions and use different techniques, but they ultimately aim at the same transformational science.

The changing context

Recent reports, including the National Research Council's "Revealing the Hidden Nature of Space and Time" (the EPP2010 report) and earlier P5 reports, have discussed the outlook for the field of particle physics in the United States. The scientific priorities have not changed since those reports appeared, but the context for the scientific opportunities they describe has altered.

Particle physics in the United States is in transition. Two of the three high-energy physics colliders in the US have now permanently ceased operation. The third, Fermilab's Tevatron, will turn off in the next few years. The energy frontier, defined for decades by Fermilab's Tevatron, will move to Europe when CERN's Large Hadron Collider begins operating. American high-energy physicists have played a leadership role in developing and building the LHC program, and they constitute a significant fraction of the LHC collaborations—the largest group from any single nation. About half of all US experimental particle physicists participate in LHC experiments.

As this transition occurs, serious fiscal challenges change the landscape for US particle physics. The large cost estimate for the International Linear Collider, a centerpiece of previous reports, has delayed plans for a possible construction start and has led the particle physics community to take a fresh look at the scientific opportunities in the decade ahead. The severe funding reduction in the Omnibus Bill of December 2007 stopped work on several projects and had damaging impacts on the entire field. The present P5 panel has developed a strategic plan that takes these new realities into account.

Overall recommendation

Particle physics explores the fundamental constituents of matter and energy and the forces that govern their interactions. Great scientific opportunities point to significant discoveries in particle physics in the decade ahead.

Research in particle physics has inspired generations of young people to engage with science, benefiting all branches of the physical sciences and strengthening the scientific workforce. To quote from the EPP2010 report:

> *"A strong role in particle physics is necessary if the United States is to sustain its leadership in science and technology over the long term."*

The present P5 panel therefore makes the following overall recommendation:

> **The panel recommends that the US maintain a leadership role in world-wide particle physics. The panel recommends a strong, integrated research program at the three frontiers of the field: the Energy Frontier, the Intensity Frontier and the Cosmic Frontier.**

The Energy Frontier

Experiments at energy-frontier accelerators will make major discoveries about particles and their interactions. They will address key questions about the physical nature of the universe: the origin of particle masses, the existence of new symmetries of nature, the existence of extra dimensions of space, and the nature of dark matter. Currently, the Tevatron at Fermilab is the highest-energy collider operating in the world.

> **The panel recommends continuing support for the Tevatron Collider program for the next one to two years, to exploit its potential for discoveries.**

In the near future, the Large Hadron Collider at CERN in Geneva, Switzerland will achieve much higher collision energies than those of any previous accelerator, to explore the energy range we call the Terascale. The LHC represents the culmination of more than two decades of international effort and investment, with major US involvement. Experiments at the LHC are poised to make exciting discoveries that will change our fundamental understanding of nature. Significant US participation in the full exploitation of the LHC has the highest priority in the US high-energy physics program.

> **The panel recommends support for the US LHC program, including US involvement in the planned detector and accelerator upgrades.**

The international particle physics community has reached consensus that a full understanding of the physics of the Terascale will require a lepton collider as well as the LHC. The panel reiterates the importance of such a collider. In the next few years, results from the LHC will establish its required energy. If the optimum initial energy proves to be at or below approximately 500 GeV, then the International Linear Collider is the most mature and ready-to-build option with a construction start possible in the next decade. A requirement for initial energy much higher than 500

GeV will mean considering other collider technologies. The cost and scale of a lepton collider mean that it would be an international project, with the cost shared by many nations. International negotiations will determine the siting; the host will be assured of scientific leadership at the energy frontier. Whatever the technology of a future lepton collider, and wherever it is located, the US should plan to play a major role.

For the next few years, the US should continue to participate in the international R&D program for the ILC to position the US for an important role should the ILC be the choice of the international community. The US should also participate in coordinated R&D for the alternative accelerator technologies that a lepton collider of higher energy would require.

> **The panel recommends for the near future a broad accelerator and detector R&D program for lepton colliders that includes continued R&D on ILC at roughly the proposed FY2009 level in support of the international effort. This will allow a significant role for the US in the ILC wherever it is built. The panel also recommends R&D for alternative accelerator technologies, to permit an informed choice when the lepton collider energy is established.**

The Intensity Frontier

Recent striking discoveries make the study of the properties of neutrinos a vitally important area of research. Measurements of the properties of neutrinos are fundamental to understanding physics beyond the Standard Model and have profound consequences for the evolution of the universe. The latest developments in accelerator and detector technology make possible promising new scientific opportunities in neutrino science as well as in experiments to measure rare processes. The US can build on the unique capabilities and infrastructure at Fermilab, together with DUSEL, the Deep Underground Science and Engineering Laboratory proposed for the Homestake Mine in South Dakota, to develop a world-leading program of neutrino science. Such a program will require a multi-megawatt-powered neutrino source at Fermilab.

> **The panel recommends a world-class neutrino program as a core component of the US program, with the long-term vision of a large detector in the proposed DUSEL and a high-intensity neutrino source at Fermilab.**

> **The panel recommends an R&D program in the immediate future to design a multi-megawatt proton source at Fermilab and a neutrino beamline to DUSEL and recommends carrying out R&D on the technologies for a large multi-purpose neutrino and proton decay detector.**

Construction of these facilities could start within the 10-year period considered by this report.

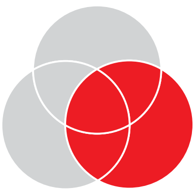
A neutrino program with a multi-megawatt proton source would be a stepping stone toward a future neutrino source, such as a neutrino factory based on a muon storage ring, if the science eventually requires a more powerful neutrino source. This in turn could position the US program to develop a muon collider as a long-term means to return to the energy frontier in the US.

The proposed DUSEL is key to the vision for the neutrino program. It is also central to nonaccelerator experiments searching for dark matter, proton decay and neutrinoless double beta decay. DOE and NSF should define clearly the stewardship responsibilities for such a program.

The panel endorses the importance of a deep underground laboratory to particle physics and urges NSF to make this facility a reality as rapidly as possible. Furthermore the panel recommends that DOE and NSF work together to realize the experimental particle physics program at DUSEL.

Scientific opportunities through the measurement of rare processes include experiments to search for muon-to-electron conversion and rare-kaon and *B*-meson decay. Such incisive experiments, complementary to experiments at the LHC, would probe the Terascale and possibly much higher energies.

The panel recommends funding for measurements of rare processes to an extent depending on the funding levels available, as discussed in more detail in Sections 3.2.2 and 7.2.3.



The Cosmic Frontier

Although 95 percent of the universe appears to consist of dark matter and dark energy, we know little about either of them. The quest to elucidate the nature of dark matter and dark energy is at the heart of particle physics—the study of the basic constituents of nature, their properties and interactions.

The US is presently a leader in the exploration of the Cosmic Frontier. Compelling opportunities exist for dark matter search experiments, and for both ground-based and space-based dark energy investigations. In addition, two other cosmic frontier areas offer important scientific opportunities: the study of high-energy particles from space and the cosmic microwave background.

The panel recommends support for the study of dark matter and dark energy as an integral part of the US particle physics program.

The panel recommends that DOE support the space-based Joint Dark Energy Mission, in collaboration with NASA, at an appropriate level negotiated with NASA.

The panel recommends DOE support for the ground-based Large Synoptic Survey Telescope program in coordination with NSF at a level that depends on the overall program budget.

The panel further recommends joint NSF and DOE support for direct dark matter search experiments.

The panel recommends limited R&D funding for other particle astrophysics projects and recommends establishing a Particle Astrophysics Science Advisory Group.

Enabling technologies

The US must continue to make advances in accelerator and detector R&D to maintain leadership at the Intensity and Cosmic Frontiers of particle physics; to allow for a return to the Energy Frontier in the US; and to develop applications for the benefit of society.

The panel recommends a broad strategic program in accelerator R&D, including work on ILC technologies, superconducting rf, high-gradient normal-conducting accelerators, neutrino factories and muon colliders, plasma and laser acceleration, and other enabling technologies, along with support of basic accelerator science.

The panel recommends support for a program of detector R&D on technologies strategically chosen to enable future experiments to advance the field, as an essential part of the program.

Benefits to society

The drive to understand the world around us is a basic part of our humanity. Research in fundamental science provides the ideas and discoveries that form the long-term foundation for science and technology as a whole, which in turn drive the global economy and our very way of life. Each generation of particle accelerators and detectors builds on the previous one, raising the potential for discovery and pushing the level of technology ever higher. From the earliest days of high energy physics in the 1930s to the latest 21st-century initiatives, the bold and innovative ideas and technologies of particle physics have entered the mainstream of society to transform the way we live. Section 2 addresses these benefits in more detail.

Unique to particle physics is the scale of the science: the size and complexity not only of accelerators and detectors but also of scientific collaborations. For example, superconducting magnets existed before Fermilab's Tevatron accelerator, but the scale of the accelerator made the production of such magnets an industrial process, which led to cost-effective technology for magnetic resonance imaging. The World Wide Web was invented to solve the problem of communicating in international collaborations of many hundreds of physicists. The scale on which particle physicists work results in innovations that broadly benefit society.

Particle physics has a profound influence on the workforce. The majority of students trained in particle physics find their way to diverse sectors of the national economy such as national defense, information technology, medical instrumentation, electronics, communications, transportation, biophysics and finance—wherever the workforce requires highly developed analytical and technical skills, the ability to work in large teams on complex projects, and the ability to think creatively to solve unique problems.

The international context

The scientific opportunities provided by particle physics bring together scientists from every corner of the globe to work together on experiments and projects all over the world. Both the technical scale and the costs of today's largest accelerators and experiments put them beyond the reach of any single nation's ability to build or operate. Particle physics projects now take shape as international endeavors from their inception. As the costs and scale of particle physics facilities grow, international collaboration becomes increasingly important to the vitality of the field. Global cooperation, a hallmark of particle physics research, will be even more important in the future.

The Large Hadron Collider accelerator and detector system, for example, drew from innovation and expertise in Europe, the Americas and Asia to deliver the cutting-edge technology required for this next-generation collider program. The proposed LHC upgrades will likewise have continuing and very significant contributions from these regions. The successful programs at the KEK and SLAC *B* factories and at the Tevatron provide additional examples of the benefits of international collaboration. These scientific collaborations take on new significance as beacons for free and open exchange among men and women of science of all nations. They offer an inspiring model for cooperation from a field long known for its leadership in international collaboration.

As particle physics moves into the future, the balance of the physical location of the major facilities among the regions of the world will be key to maintaining the vitality of the field in each region and as a whole. In developing a strategic plan for US particle physics, the P5 panel kept the international context very much in mind.

The funding scenarios

The funding agencies asked the panel to develop plans in the context of several DOE funding scenarios:

A. Constant level of effort at the FY2008 funding level
B. Constant level of effort at the FY2007 funding level
C. Doubling of budget over ten years starting in FY2007
D. Additional funding above the previous level, associated with specific activities needed to mount a leadership program

The FY2007 DOE funding level was $752M; the FY2008 level was $688M. Constant level of effort here means that the budget increases with inflation in then-year dollars. The panel also received guidance on NSF budget assumptions. Interagency collaboration on particle physics experiments has become increasingly important. The plan presented in this report depends on such collaborative funding among DOE, NSF and NASA.

The panel evaluated the scientific opportunities for particle physics in the next 10 years under the various budget scenarios.

Scenario B: Constant level of effort at the FY2007 level

The scenario of constant level of effort at the FY2007 level, Scenario B, would support major advances at all three interrelated frontiers of particle physics. At the Energy Frontier, the Fermilab Tevatron would run in 2009, but the planned run in 2010 to complete the program could not take place due to budgetary constraints. The LHC experiments would be well under way. These experiments will likely make significant discoveries that could change our fundamental understanding of nature. R&D would go forward on future lepton colliders. At the Intensity Frontier, the MINOS, Double Chooz, Daya Bay and NOvA experiments would yield a greatly improved—if not complete—understanding of the fundamental properties of neutrinos. Precision measurements, limited to a muon-to-electron conversion experiment, would be carried out and the US would participate in one offshore next-generation *B* Factory. On the Cosmic Frontier, greatly improved measurements shedding light on the nature of dark energy would come from the DES, JDEM and LSST projects. The next generation of dark matter search experiments would reach orders-of-magnitude greater sensitivity to—perhaps even discover—particles that can explain dark matter.

Under Scenario B, the US would play a leadership role at all three frontiers. Investments in accelerators and detectors at the LHC would enable US scientists to play a leading role in the second generation of studies at the Energy Frontier. Investments in facility capabilities at the Intensity Frontier at Fermilab and DUSEL would allow the US to be a world leader in neutrino physics in the following decade. Funding of the cutting edge experiments studying dark matter and dark energy would insure continued US leadership at the Cosmic Frontier. Investments in a broad strategic accelerator R&D program would enable the US to remain at the forefront of accelerator developments and technologies focused on the needs of the US program at the Energy and Intensity Frontiers.

Scenario A: Constant level of effort at the FY2008 level

Budget Scenario A would significantly reduce the scientific opportunities at each of the three frontiers compared to Scenario B over the next 10 years. It would severely limit scientific opportunities at the Intensity Frontier during the next decade. Scenario A would require canceling planned experiments and delaying construction of new facilities. It would slow progress in understanding dark energy at the Cosmic Frontier and R&D toward future accelerator facilities at the Energy Frontier. It would cut the number of scientists, as well as graduate students and postdoctoral fellows. Scenario A would unduly delay projects, extending them over a longer period.

Scenario A would most profoundly limit studies at the Intensity Frontier, with a negative impact on both neutrino physics and high-sensitivity measurements. It would require cancellation of the NOvA neutrino experiment that is ready for construction. The MINERvA experiment could not run beyond FY2010 due to lack of funds to operate the Fermilab accelerator complex. Consequently, a first look at the neutrino mass hierarchy would be unlikely during the next decade, and experimenters could not measure neutrino cross sections, including those important to future long-baseline neutrino oscillation experiments. The US could not contribute significantly to the next-generation overseas *B* factories that will carry out unprecedented studies of matter-antimatter asymmetry and searches for new processes in the quark sector. Furthermore, this budget scenario would delay the construction of a high-intensity proton source at Fermilab by at least three to five years. This delay would in turn severely compromise the program of neutrino physics and of high-sensitivity searches for rare decays at the Intensity Frontier in the subsequent decade.

For dark-energy studies at the Cosmic Frontier, Budget Scenario A would delay DOE funding for the ground-based LSST telescope.

This budget scenario could not support the investment in new facilities for advanced accelerator R&D, important for future accelerators both at the energy frontier and for other sciences. As discussed above, it would also delay the construction of a high-intensity proton source, postponing the establishment of a foundation for energy frontier studies at a possible future muon collider.

Scenario A would require an additional reduction of approximately 10 percent beyond the FY2008 cuts in the number of scientists over the 10-year period. It would lead to a significant drop in the number of graduate students and postdoctoral fellows. Scenario A's drought in R&D coupled with delays in facility construction imposed during this decade would limit scientific opportunities in the subsequent decade.

Overall, while this funding level could deliver significant science, there would be outstanding scientific opportunities that could not be pursued. It would sharply diminish the US capability in particle physics from its present leadership role.

Scenario C: The doubling budget

Budget Scenario C would support a world-class program of scientific discovery at all three frontiers in the decade ahead. It would provide strong support for the development of future research capabilities and of the scientific work force. Programs could move forward at a more efficient pace, with reduced costs, more timely physics results and increased scientific impact.

At the Energy Frontier, this budget scenario would extend the discovery potential of the Fermilab Tevatron Collider by supporting operation in FY2010. Budget scenario C would provide robust funding for exploitation of the LHC physics potential. It would increase operations funding for US groups working in Europe on the LHC and provide the needed personnel support at both universities and national laboratories for LHC detector and machine upgrades.

Progress toward a future lepton collider is a very high priority of the field worldwide. Should results from the LHC show that the ILC is the lepton collider of choice, funding in this scenario would support R&D and enable the start of construction of an ILC abroad. If LHC results point to another lepton collider technology, its R&D would advance. Increased funding for muon collider R&D would lead to an earlier feasibility determination for a neutrino factory and perhaps a muon collider.

Scenario C would significantly advance the exploration of physics at the Intensity Frontier. Construction of a new high-intensity proton source at Fermilab, which would support both neutrino physics and precision searches for rare decays, would be complete. Scenario C would enable an earlier construction start than would Scenario B and would shorten the construction time. It would also advance the design and construction of a beamline to DUSEL and would reduce the overall cost and risk of both these projects. Efforts to develop the technology for large-scale liquid argon or water Cerenkov detectors for neutrino physics and proton decay would benefit greatly from increased funding, leading to an earlier construction start, shorter construction period and reduced risk for a large underground detector at DUSEL. Scenario C would enable the high-sensitivity neutrino experiment to operate during the decade, providing great sensitivity to matter-antimatter asymmetry in neutrinos. Scenario C would also enable new rare *K*-decay experiments highly sensitive to new physics.

At the Cosmic Frontier, Scenario C would advance the exploration of dark energy by enabling the timely completion of the two most sensitive detectors of dark energy, the JDEM space mission and the ground-based LSST telescope. Scenario C enables strategic, large-scale investments in exciting projects at the boundary between particle physics and astrophysics, the study of high-energy particles from space. Without these investments, the US will likely lose leadership in this rapidly developing area.

Budget scenario C would provide needed additional funds to advance accelerator R&D and technology goals. These goals go well beyond preparation for possible participation in ILC. Accelerator goals for the field include advancing the development of key enabling technologies such as superconducting rf technology, high-field magnet technology, high-gradient warm rf accelerating structures, rf power sources, and advanced accelerator R&D, all of which could greatly benefit from increased funding.

Increased funding in Scenario C would allow a robust detector R&D program in the U.S. to prepare for future experiments at both the energy and intensity frontiers.

Budget Scenario C provides desperately needed resources to rebuild university and laboratory infrastructure that has eroded during lean funding years and would allow retention and hiring of needed laboratory and university technical staff. This budget scenario would provide additional support for university groups, further addressing the pressing needs enunciated in several recent reports, among them the National Academy's "Rising Above the Gathering Storm."

Scenario D: Additional funding

The following scientific opportunities would justify additional funding above the level of the funding scenarios discussed above.

A lepton collider will be essential for the in-depth understanding of new physics discovered at the LHC: the source of the masses of the elementary particles, new laws of nature, additional dimensions of space, the creation of dark matter in the laboratory, or something not yet imagined. Major participation by the US in constructing such a facility would require additional funding beyond that available in the previous funding scenarios.

The study of dark energy is central to the field of particle physics. DOE is currently engaged with NASA in negotiations concerning the space-based Joint Dark Energy Mission. If the scale of JDEM requires significantly more funding than is currently being discussed, an increase in the budget beyond the previous funding scenarios would be justified.

The Three Frontiers of Particle Physics

What are the most basic building blocks of the universe? What are the forces that enable these elementary constituents to form all that we see around us? What unknown properties of these particles and forces drive the evolution of the universe from the Big Bang to its present state, with its complex structures that support life—including us? These are the questions that particle physics seeks to answer.

Particle physics has been very successful in creating a major synthesis, the Standard Model. At successive generations of particle accelerators in the US, Europe and Asia, physicists have used high-energy collisions to discover many new particles. By studying these particles they have uncovered both new principles of nature and many unsuspected features of the universe, resulting in a detailed and comprehensive picture of the workings of the universe.

Recently, however, revolutionary discoveries have shown that this Standard Model, while it represents a good approximation at the energies of existing accelerators, is incomplete. They strongly suggest that new physics discoveries beyond the Standard Model await us at the ultrahigh energies of the Terascale. The Large Hadron Collider will soon provide a first look at this uncharted territory of ultrahigh energy; a future lepton collider will elucidate the new phenomena with great precision.

A striking development in neutrino physics is the discovery that the three kinds of neutrinos, which in the Standard Model are massless and cannot change from one type to another, do in fact have tiny masses and can morph from one kind to another. This discovery has profound implications not only for the Standard Model but also for understanding the development of the early universe.

The accelerating expansion of the universe, yet another remarkable discovery, implies the existence of a mysterious entity, a dark energy that makes up almost three quarters of the energy-matter content of the universe, driving it apart at an ever-increasing rate. Dark Energy has interesting properties that could change our understanding of gravity.

Astrophysical observations have also revealed that about a quarter of the universe consists of an unknown form of matter called dark matter. No Standard Model particle can account for this strange ingredient of our universe. In the next decade, the combination of LHC results and dedicated dark-matter-search experiments promise to shed light on dark matter’s true character.

All these discoveries make the field of particle physics richer and more exciting than at any time in history. New accelerator and detector technologies bring within reach discoveries that may transform our understanding of the physical nature of the universe.

A set of interrelated questions, articulated in several previous reports, defines the path ahead:

1. How do particles acquire mass? Does the Higgs boson exist, or are new laws of physics required? Are there extra dimensions of space?

2. What is the nature of new particles and new principles beyond the Standard Model?

3. What is the dark matter that makes up about one quarter of the contents of the universe?

4. What is the nature of the dark energy that makes up almost three quarters of the universe?

5. Do all the forces of nature become one at high energies? How does gravity fit in? Is there a quantum theory of gravity?

6. Why is the universe as we know it made of matter, with no antimatter present? What is the origin of this matter-antimatter asymmetry?

7. What are the masses and properties of neutrinos and what role did they play in the evolution of the universe? How are they connected to matter-antimatter asymmetry?

8. Is the building block of the stuff we are made of, the proton, unstable?

9. How did the universe form?

Physicists address these questions using a range of tools and techniques at three frontiers that together form an interlocking framework of scientific opportunity.

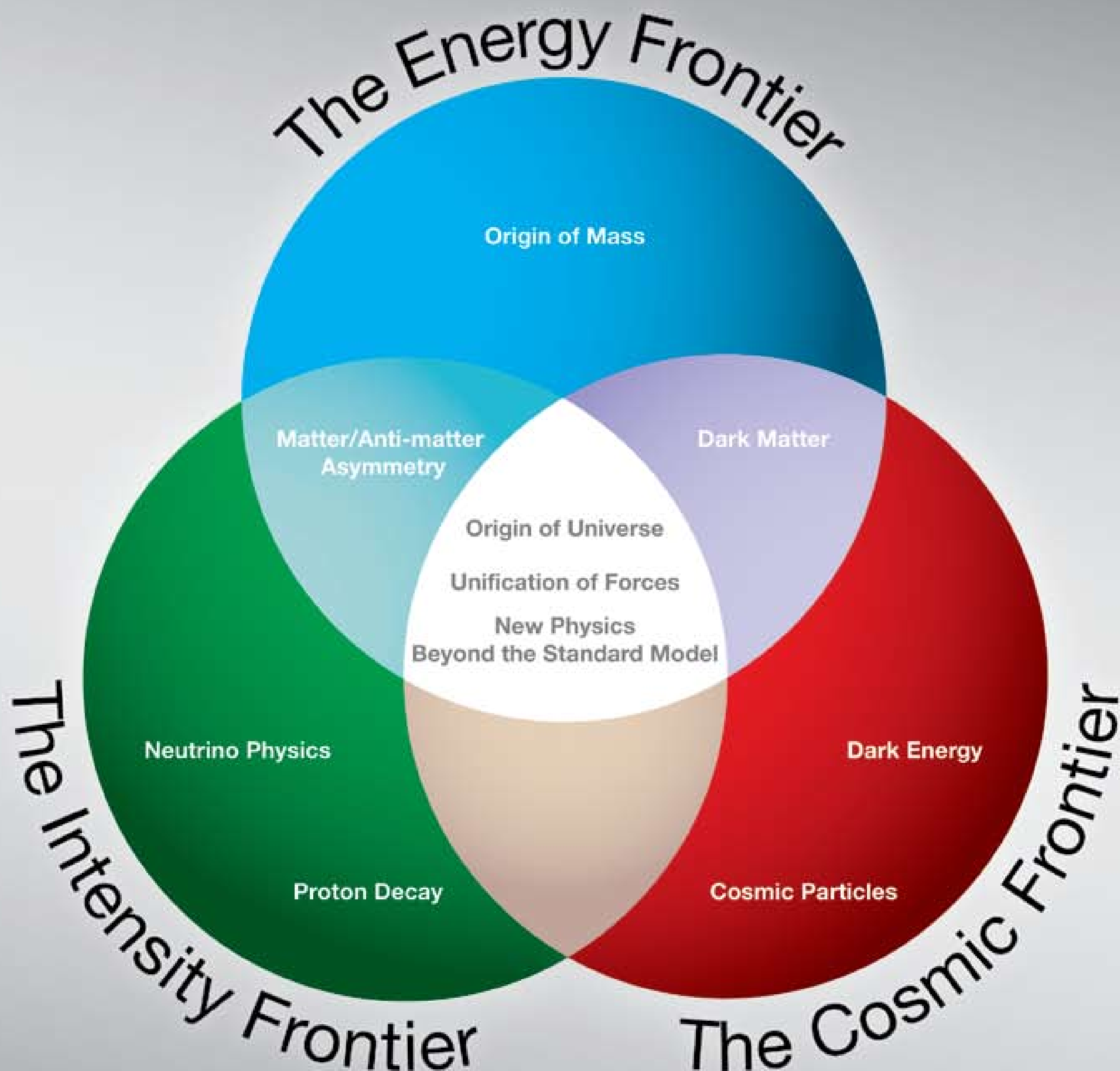
The Energy Frontier
Experiments at energy-frontier accelerators will make major discoveries leading to an ultimate understanding of particles and their interactions. Outstanding questions that present and future colliders will address include the origin of elementary particle masses, the possible existence of new symmetries of nature, the existence of extra dimensions of space, and the nature of dark matter. Experiments at the energy frontier, at the LHC and at a future lepton collider, will allow physicists to directly produce and study the particles that are the messengers of these new phenomena in the laboratory for the first time.

The Intensity Frontier
Measurements of the mass and other properties of neutrinos are fundamental to understanding physics beyond the Standard Model and have profound consequences for the understanding of the evolution of the universe. The US program can build on the unique capabilities and infrastructure at Fermilab, together with the proposed deep underground laboratory at Homestake, to develop a world-leading program of neutrino science. Such a program, not possible at the large collider facilities, will require a multi-megawatt-powered proton source at Fermilab. Incisive experiments using muons, kaons or *B* mesons to measure rare processes can probe the Terascale and beyond.

The Cosmic Frontier
Ninety-five percent of the contents of the universe appears to consist of dark matter and dark energy, yet we know very little about them. To discover the nature of dark matter and dark energy will require a combination of experiments at particle accelerators with both ground- and space-based observations of astrophysical objects in the distant cosmos.

The three frontiers of research in particle physics form an interlocking framework that addresses fundamental questions about the laws of nature and the cosmos.

These three approaches ask different questions and use different techniques, but they are ultimately aimed at the same transformational science. Discoveries on one frontier will have much greater impact taken together with discoveries on the other frontiers. For example, the discovery of new particles at the energy frontier, combined with discoveries from the intensity frontier about neutrinos and rare processes, may explain the dominance of matter over antimatter. Synthesizing discoveries from all three frontiers creates the opportunity to understand the most intimate workings and origins of the physical universe.



Three frontiers of research in particle physics form an interlocking framework that addresses fundamental questions about the laws of nature and the cosmos.

Chapter 2
Particle Physics in the National and International Context

2 PARTICLE PHYSICS IN THE NATIONAL AND INTERNATIONAL CONTEXT

2.1 LONG-TERM VALUE OF RESEARCH IN FUNDAMENTAL SCIENCES

The drive to understand the world around us is a basic part of our humanity. Research in fundamental science provides the ideas and discoveries that form the long-term foundation for science and technology as a whole, which in turn drive the global economy and our very way of life.

In 2005, a panel of nationally recognized experts from across the spectrum of science and society, chaired by Norman Augustine, retired chairman and chief executive officer Lockheed Martin Corporation, produced “Rising Above the Gathering Storm: Energizing and Employing America for a Brighter Economic Future.*”* To quote from the report:

> *“The growth of economies throughout the world has been driven largely by the pursuit of scientific understanding, the application of engineering solutions, and the continual technological innovation. Today, much of everyday life in the United States and other industrialized nations, as evidenced in transportation, communication, agriculture, education, health, defense, and jobs, is the product of investments in research and in the education of scientists and engineers. One need only think about how different our daily lives would be without the technological innovations of the last century or so.”*

The “Gathering Storm” report makes the following recommendation:

> *“Sustain and strengthen the nation’s traditional commitment to long-term basic research that has the potential to be transformational to maintain the flow of new ideas that fuel the economy, provide security, and enhance the quality of life”*

The “Gathering Storm” report was influential in forging a bipartisan accord in Washington to strive toward global leadership in science for the US by doubling the funding for research in the physical sciences over the next decade, among other actions.

Particle physics is a central component of the physical sciences, focused on the fundamental nature of matter and energy, and of space and time. Discoveries in particle physics will change our basic understanding of nature. Particle physics has inspired generations of young people to get involved with science, benefiting all branches of the physical sciences and strengthening the scientific workforce.

To quote from another National Academies report, “Charting the Course for Elementary Particle Physics,” the work of a panel including leaders from both science and industry and chaired by economist Harold Shapiro:

> *“A strong role in particle physics is necessary if the United States is to sustain its leadership in science and technology over the long term.”*

That report continues:

> *“The committee affirms the intrinsic value of elementary particle physics as part of the broader scientific and technological enterprise and identifies it as a key priority within the physical sciences.”*

Besides its long-term scientific importance, particle physics generates technological innovations with profound benefits for the sciences and society as a whole.

2.2 BENEFITS TO SOCIETY

It’s a simple idea. Take the smallest possible particles. Give them the highest possible energy. Smash them together. Watch what happens. From this simple idea have come the science and technology of particle physics, a deep understanding of the physical universe and countless benefits to society.

Each generation of particle accelerators and detectors builds on the previous one, raising the potential for discovery and pushing the level of technology ever higher. In 1930, Ernest O. Lawrence, the father of particle accelerators, built the first cyclotron at Berkeley, California. He could hold it in his hand. Larger and more powerful accelerators soon followed. After a day’s work, Lawrence often operated the Berkeley cyclotrons through the night to produce medical isotopes for research and treatment. In 1938, Lawrence’s mother became the first cancer patient to be treated successfully with particles from cyclotrons. Now doctors use particle beams for the diagnosis and healing of millions of patients. From the earliest days of high energy physics in the 1930s to the latest 21st-century initiatives, the bold and innovative ideas and technologies of particle physics have entered the mainstream of society to transform the way we live.

Some applications of particle physics—the superconducting wire and cable at the heart of magnetic resonance imaging magnets, the World Wide Web—are so familiar they are almost clichés. But particle physics has myriad lesser-known impacts. Few outside the community of experts who study the behavior of fluids in motion have probably heard of the particle detector technology that revolutionized the study of fluid turbulence in fuel flow.

What is unique to particle physics is the scale of the science: the size and complexity not only of accelerators and detectors but also of scientific collaborations. For example, superconducting magnets existed before Fermilab’s Tevatron, but the scale of the accelerator made the production of such magnets an industrial process, which led to cost-effective technology for magnetic resonance imaging. The World Wide Web was invented to solve the problem of communicating in an international collaboration of many hundreds of physicists. The scale on which particle physicists work results in innovations that broadly benefit society.

Selected examples from medicine, homeland security, industry, computing, science, and workforce development illustrate a long and growing list of beneficial practical applications with origins in particle physics.

Medicine: cancer therapy
The technologies of particle physics have yielded dramatic advances in cancer treatment. Today, every major medical center in the nation uses accelerators producing X-rays, protons, neutrons or heavy ions for the diagnosis and treatment of disease. Particle accelerators play an integral role in the advance of cancer therapy. Medical linacs for cancer therapy were pioneered simultaneously at Stanford and in the UK in the 1950s using techniques that had been developed for high energy physics research. This R&D spawned a new industry and has saved millions of lives.

Today it is estimated that there are over 7,000 operating medical linacs around the world that have treated over 30,000,000 patients.

Fermilab physicists and engineers built the nation's first proton accelerator for cancer therapy and shipped it to the Loma Linda University Medical Center, where it has treated some 7,000 patients. Relative to X-rays, proton therapy offers important therapeutic benefits, especially for pediatric patients. The Neutron Therapy Facility at Fermilab has the highest energy and the deepest penetration of any fast neutron beam in the United States. Neutrons are effective against large tumors. More than 3,500 patients have received treatment at the Neutron Therapy Facility.

Medicine: diagnostic instrumentation
Particle physics experiments use an array of experimental techniques for detecting particles; they find a wide range of practical applications. Particle detectors first developed for particle physics are now ubiquitous in medical imaging. Positron emission tomography, the technology of PET scans, came directly from detectors initially designed for particle physics experiments sensing individual photons of light. Silicon tracking detectors, composed of minute sensing elements sensitive to the passage of single particles, are now used in neuroscience experiments to investigate the workings of the retina for development of retinal prosthetics for artificial vision.

Homeland security: monitoring nuclear nonproliferation
In nuclear reactors, the amount of plutonium builds up as the uranium fuel is used, and the number and characteristics of antineutrinos emitted by plutonium differ significantly from those of antineutrinos emitted by uranium. This makes it possible for a specially doped liquid scintillator detector monitoring the antineutrino flux from a nuclear reactor core to analyze the content of the reactor and verify that no tampering has occurred with the reactor fuel. Lawrence Livermore National Laboratory has built and is testing a one-ton version of this type of detector, originally developed by high energy physicists to study the characteristics of neutrinos and antineutrinos, as a demonstration of a new monitoring technology for nuclear nonproliferation.

Industry: power transmission
Cables made of superconducting material can carry far more electricity than conventional cables with minimal power losses. Underground copper transmission lines or power cables are near their capacity in many densely populated areas, and superconducting cables offer an opportunity to meet continued need. Further superconducting technology advances in particle physics will help promote this nascent industry.

Industry: biomedicine and drug development
Biomedical scientists use particle physics technologies to decipher the structure of proteins, information that is key to understanding biological processes and healing disease. To determine a protein's structure, researchers direct the beam of light from an accelerator called a synchrotron through a protein crystal. The crystal scatters the beam onto a detector. From the scattering pattern, computers calculate the position of every atom in the protein molecule and create a 3-D image of the molecule. A clearer understanding of protein structure allows for the development of more

effective drugs. Abbott Labs' research at Argonne National Laboratory's Advanced Photon Source was critical in developing Kaletra®, one of the world's most-prescribed drugs to fight AIDS. Next-generation light sources will offer still more precise studies of protein structure without the need for crystallization.

Industry: understanding turbulence

Turbulence is a challenge to all areas of fluid mechanics and engineering. Although it remains poorly understood and poorly modeled, it is a dominant factor determining the performance of virtually all fluid systems from long distance oil pipelines to fuel injection systems to models for global weather prediction. Improvements to our knowledge will have payoffs in reducing energy losses in fuel transport, improving efficiency of engines and deepening our understanding of global climate behavior. Technology developed for particle physics and applied to problems of turbulence has extended our understanding of this difficult phenomenon by more than tenfold. Silicon strip detectors and low-noise amplifiers developed for particle physics are used to detect light scattered from microscopic tracer particles in a turbulent fluid. This technique has permitted detailed studies of turbulence on microscopic scales and at Reynolds numbers more than an order of magnitude beyond any previous experimental reach.

Computing: the World Wide Web

CERN scientist Tim Berners-Lee developed the World Wide Web to give particle physicists a tool to communicate quickly and effectively with globally dispersed colleagues at universities and laboratories. The Stanford Linear Accelerator Center had the first Web site in the United States, Fermilab had the second. Today there are more than 150 million registered Web sites. Few other technological advances in history have more profoundly affected the global economy and societal interactions than the Web. Revenues from the World Wide Web exceeded one trillion dollars in 2001 with exponential growth continuing.

Computing: the Grid

Particle physics experiments generate unprecedented amounts of data that require new and advanced computing technology to analyze. To quickly process this data, more than two decades ago particle physicists pioneered the construction of low-cost computing farms, a group of servers housed in one location. Today, particle physics experiments push the capability of the Grid, the newest computing tool that allows physicists to manage and process their enormous amounts of data across the globe by combining the strength of hundreds of thousands of individual computers. Industries such as medicine and finance are examples of other fields that also generate large amounts of data and benefit from advanced computing technology.

Sciences: synchrotron light sources

Particle physicists originally built electron accelerators to explore the fundamental nature of matter. At first, they looked on the phenomenon of synchrotron radiation as a troublesome problem that sapped electrons' acceleration energy. However, they soon saw the potential to use this nuisance energy loss as a new and uniquely powerful tool to study biological molecules and other materials. In the 1970s, the Stanford Linear Accelerator Center built the first large-scale light source user facility. Now, at facilities around the world, researchers use the ultra-powerful X-ray beams of dedicated synchrotron light sources to create the brightest lights on earth. These luminous sources provide tools for protein structure analysis, pharmaceutical research and drug development, real-time visualization of chemical reactions and biochemical processes, materials science, semiconductor circuit lithography, and historical research and the restoration of works of art.

Sciences: spallation neutron sources
Using accelerator technologies, spallation neutron sources produce powerful neutron beams by bombarding a mercury target with energetic protons from a large accelerator complex. The protons excite the mercury nuclei in a reaction process called spallation, releasing neutrons that are formed into beams and guided to neutron instruments. Using these sophisticated sources, scientists and engineers explore the most intimate structural details of a vast array of novel materials.

Sciences: analytic tools
Particle physicists have developed theoretical and experimental analytic tools and techniques that find applications in other scientific fields and in commerce. Renormalization group theory first developed to rigorously describe particle interactions has found applications in solid state physics and superconductivity. Nuclear physics uses chiral lagrangians, and string theory has contributed to the mathematics of topology. Experimental particle physicists have also made contributions through the development of tools for extracting weak signals from enormous backgrounds and for handling very large data sets. Scientists trained in particle physics have used neural networks in neuroscience to investigate the workings of the retina and in meteorology to measure raindrop sizes with optical sensors.

Workforce development: training scientists
Particle physics has a profound influence on the workforce. Basic science is a magnet that attracts inquisitive and capable students. In particle physics, roughly one sixth of those completing Ph.D.s ultimately pursue careers in basic high-energy physics research. The rest find their way to diverse sectors of the national economy such as industry, national defense, information technology, medical instrumentation, electronics, communications, biophysics and finance—wherever the workforce requires highly developed analytical and technical skills, the ability to work in large teams on complex projects, and the ability to think creatively to solve unique problems.

A growing list
The science and technology of particle physics have transformational applications for many other areas of benefit to the nation's well-being.

- Food sterilization
- Medical isotope production
- Simulation of cancer treatments
- Reliability testing of nuclear weapons
- Scanning of shipping containers
- Proposed combination of PET and MRI imaging
- Improved sound quality in archival recordings
- Parallel computing
- Ion implantation for strengthening materials
- Curing of epoxies and plastics
- Data mining and simulation
- Nuclear waste transmutation
- Remote operation of complex facilities
- International relations

At this time there exist few quantitative analyses of the economic benefits of particle physics applications. A systematic professional study would have value for assessing and predicting the impact of particle physics technology applications on the nation's economy.

2.3 THE INTERNATIONAL CONTEXT

The scientific opportunities provided by particle physics bring together hundreds of scientists from every corner of the globe to work together on experiments and projects all over the world. Both the technical scale and the costs of today's large accelerators put them beyond the reach of any single nation's ability to build or operate. Particle physics projects now take shape as international endeavors from their inception. These scientific collaborations take on new significance as beacons for free and open exchange among men and women of science of all nations. They offer an inspiring model for cooperation from a field long known for its leadership in international collaboration.

Collider experiments have had strong international collaboration from the outset. Experiments at CERN, Fermilab and SLAC combined the strengths of US, European and Asian groups to achieve the groundbreaking discoveries that define particle physics today. Accelerator design and construction is now a joint effort as well. American accelerator physicists and engineers helped the Europeans build the Large Hadron Collider at CERN and collaborated with the Chinese to build the Beijing Electron-Positron Collider. The GLAST project involves a seven-nation collaboration of France, Germany, Italy, Japan, Spain, Sweden and the US.

Japan is currently constructing a 50-GeV proton synchrotron at the Japan Proton Accelerator Research Complex. The JPARC synchrotron will produce an intense neutrino beam aimed at the large Super-Kamiokande detector to study neutrino oscillations and matter-antimatter asymmetry. This experiment has significant US participation, as did its predecessors. US physicists are also working on two overseas reactor neutrino experiments, Daya Bay in China and Double Chooz in France.

The KEK *B*-Factory and the Belle detector continue to operate, and plans are under way to significantly increase the collider's beam intensity to improve sensitivity to physics beyond the Standard Model. Modest US participation continues in this collaboration. At lower energies, the new BEPC-II collider in China is about to start operation. A number of US groups are working on its experimental program.

Cosmic Frontier experiments have also involved international collaboration, but on a smaller scale due to the hitherto modest size of the experiments. Here too, however, the magnitude of future experiments makes international collaboration essential.

Planning for the future of the field is also international. Both HEPAP and P5 have members from Europe and Asia, essential for understanding the current and future programs in those regions at all three scientific frontiers in particle physics.

The transformation occurring in the international scene has presented challenges to this panel. Free access for physicists of all nations to the world's accelerators rests on the assumption that each region takes its share of responsibility by building and operating such facilities. In recent decades, each region hosted major collider experiments and a variety of smaller experiments. But now, with the end of both the Cornell and SLAC collider programs and with the Fermilab Tevatron collider about to complete its program in the next few years, the map of the field is changing rapidly. Most of the accelerator-based experiments in the near term will occur overseas. The panel has given careful consideration to how the changing international context will affect the ability of the US to pursue most effectively the extraordinary scientific opportunities that lie ahead and to remain a world leader in the field of particle physics.

Chapter 3
The Frontiers of our Science: Beyond the Standard Model

3 THE FRONTIERS OF OUR SCIENCE: BEYOND THE STANDARD MODEL

Researchers address the major questions in particle physics with a variety of tools and techniques broadly classified into three frontiers. Each of these frontiers presents opportunities for major discoveries during the next decade. Accelerators at the energy frontier may produce new particles and gain direct access to new phenomena of the Terascale. Experiments at the intensity frontier could observe particle transformations never seen before, echoes of powerful processes that shaped the evolution of the universe. Observations at the cosmic frontier may pierce the darkness that veils the identity of the dominant forms of matter and energy in our universe. A synthesis of discoveries from all three frontiers could address the most fundamental questions about nature and the cosmos.

3.1 THE ENERGY FRONTIER: EXPLORING THE TERASCALE

Making particles collide at the highest possible energies is an essential pathway to discoveries in particle physics. These collisions convert energy into new particles, including particles that were prominent in the early universe but are no longer present on Earth. These particles are messengers carrying news about the fundamental nature of the universe and its origins. Studying their properties under controlled laboratory conditions gives particle physicists direct access to their secrets.

Advancing the energy frontier requires higher energy accelerators with a higher rate of particle collisions. Higher energies enable the production of heavier particles, and higher collision rates ensure a large enough sample of these particles for their properties to be understood. The energy available to create new particles depends crucially on whether the particles being accelerated are protons or electrons. Electrons are elementary—in collisions with their antiparticles all of their energy can be converted into new forms of matter. Protons are more complex objects made of quarks and gluons; as a result typically only about 10 percent of their energy is converted into new particles. On the other hand, protons are easier than leptons to accelerate to the highest possible energies.

The highest energy particle collider operating today is the Tevatron at Fermilab, where protons collide with antiprotons at a total energy of 2 TeV. The Large Hadron Collider at CERN will smash protons on protons with a total energy of 14 TeV. The LHC's huge increase in both energy and collision rate should give LHC experiments direct access to the new phenomena of the Terascale. Planned upgrades of the LHC will boost the collision rate by another large factor.

A future lepton collider, accelerating electrons or possibly muons, could access Terascale physics while operating at a lower collision energy than the LHC, with a much cleaner collision environment. Such a collider would provide a unique opportunity to penetrate the inner workings of the new phenomena awaiting us at the Terascale frontier.

3.1.1 THE TEVATRON COLLIDER EXPERIMENTS

Until the Large Hadron Collider turns on and collects a significant volume of data, the Tevatron collider at Fermilab will remain the clearest window onto new physics, including Higgs bosons, supersymmetric particles, and signals for extra dimensions. The first LHC collisions are expected in 2008, but it is unlikely that the LHC results will supersede the Tevatron during the first year or two of LHC running.

The Tevatron has performed outstandingly in the last five years, and its experiments, CDF and DZero, continue to accumulate data rapidly. As of February 2008 each experiment had collected about 3 fb^{-1} (inverse femtobarns) of data, providing evidence for never-before-observed processes, such as the production of single top quarks, and B_s to anti-B_s oscillations.

By the end of FY2010 (FY2009), the accelerator could deliver up to 8.8 (6.7) fb^{-1}, yielding data sets of about 7.4 (5.7) fb^{-1} for most CDF and DZero analyses, since both experiments have a data collection efficiency of about 85 percent. Running the Tevatron in FY2009 will double the amount of data collected and significantly increase the physics reach of the two experiments. This increase in integrated luminosity gives the CDF and DZero collaborations the potential to discover new physics through direct searches. The many distinct physics channels being analyzed—about a hundred per experiment—further enhance the chances for discovery. Besides the increase in integrated luminosity, the experiments continue to make improvements in the trigger, the jet energy resolution and electron identification.

The search for the Higgs boson is a major goal of particle physics. The dominant production processes for a Standard Model Higgs boson at the Tevatron are gluon fusion and associated *WH* and *ZH* production. At the Tevatron, the best way to observe the Higgs boson is when it is produced along with a *W* or *Z* boson. In the low mass region, the Higgs boson must be reconstructed as a broad resonance in the *b*-anti *b* invariant di-jet mass spectrum above a large background from other processes. Excellent *b*-tagging performance and di-jet mass resolution are essential to achieve the necessary signal-to-background ratio. The Tevatron experiments have greater sensitivity for Higgs boson masses around 160 GeV where the gluon fusion and the *WH/ZH* production modes can be exploited using decays of the Higgs boson to *W* pairs.

With approximately 8 fb^{-1}, the CDF and DZero experiments would have sensitivity to Higgs masses up to 185 GeV at the 95 percent confidence level. For masses between 155 and 175 GeV, they could achieve evidence for the Higgs at the 3 sigma level. The Higgs mass range probed by the Tevatron experiments is of particular interest, since it is the range preferred by precision electroweak measurements within the Standard Model, in particular the top-quark and *W*-boson masses, which the Tevatron experiments are measuring with ever-improving precision.

The Standard Model Higgs scenario is the simplest, with only one Higgs boson, but it is not the only possibility. The Minimal Supersymmetric Model, called MSSM, has two Higgs doublets, which yield five physical Higgs bosons, three neutral and one charged pair. The production rate for heavy MSSM Higgs bosons at the Tevatron depends on the boson mass m_A and on an additional parameter called $\tan\beta$. The Higgs production rate is highest at large $\tan\beta$. The Tevatron Higgs-boson searches cover a significant fraction of the ($\tan\beta$, m_A) parameter space. For $\tan\beta = 40$, the m_A region with masses below 225GeV can be probed at the 95 percent confidence limit for integrated luminosities of about 7 fb^{-1} for each experiment.

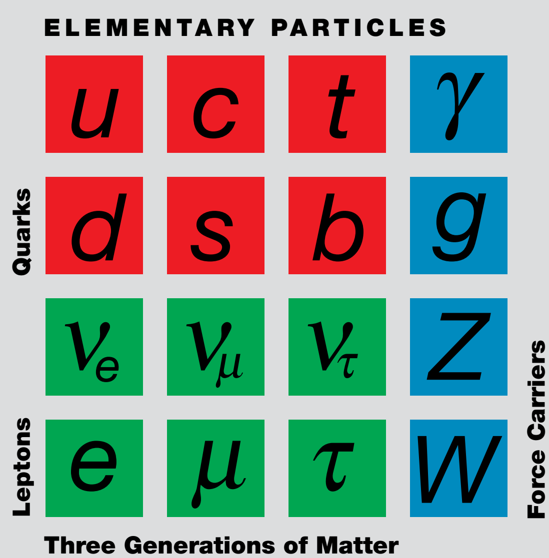
The Tevatron also provides important measurements of the *B* system including the remarkable measurement of B_s to anti-B_s oscillations with a frequency of ~$18x10^{12}$

The Standard Model

The Standard Model is the best theory that physicists currently have to describe the building blocks of the universe. It is one of the biggest achievements in twentieth-century science. It says that everything around us is made of particles called quarks and leptons with four kinds of forces that influence them. The Figure below summarizes the constituent particles, the six quarks and the six leptons, and the carriers of the four types of forces that act between these particles.

The most familiar forces are electromagnetism and gravity; the other two are less well known. The strong force binds atomic nucleons together, making them stable. Without it, there would be no atoms other than hydrogen: no carbon, no oxygen, no life. The weak force causes the nuclear reactions that have let the sun shine for billions of years. As a result, trillions of neutrinos come from the sun and go through our bodies every second; we don't feel them because the weak force is so weak.

Despite its incredible success, the Standard Model has serious deficiencies. For example, if forces and matter particles are all there are, the Standard Model says that all particles must travel at the speed of light—but that is not what we observe. To slow them down, theorists have proposed a mysterious, universe-filling, not-yet-seen energy field called the Higgs field. Like an invisible quantum liquid, it fills the vacuum of space, slowing motion and giving mass to matter. Also, physicists now understand that 96 percent of the universe is not made of matter as we know it, and thus it does not fit into the Standard Model. How to extend the Standard Model to account for these mysteries is an open question to be answered by current and future experiments.



per second. The branching ratio of $B_s \to \mu^+\mu^-$ is expected to be extremely small in the Standard Model. Extensions to the Standard Model could increase this branching ratio by orders of magnitude making this decay mode very sensitive to new physics. Since this channel is almost background free, the gain in sensitivity will increase significantly with more data. The combination of all the available experimental information on B_s mixing, including the very recent tagged analyses of $B_s \to J/\psi\phi$ by the CDF and DZero collaborations, may already provide hints of a possible deviation of the CP-violating phase of the B_s mixing amplitude with respect to Standard Model predictions. Doubling the data will improve the measurement of the B_s mixing phase and could provide information about new physics contributions.

The *B* physics program of the Tevatron benefits from the production of all species of *B* hadrons, which leads to systematic studies of phenomena such as B_s mixing, the *B* lifetime difference and rare *B*-meson decays. The combination of the B_s and B_d results from the Tevatron and the *B* factories has improved and can still improve our understanding of how different types of quarks transform into one another. Moreover any discovered inconsistency could indicate the presence of new physics.

As described above, the Tevatron Collider has a very rich program. It is actively searching for new physics in multiple promising channels. These include channels with either leptons or jets and missing energy. Large missing energy signatures are a characteristic of models with neutral stable particles, which may be candidates for dark matter. These particles are present, for instance, in many low-energy supersymmetric extensions of the Standard Model. These searches have so far found no convincing evidence of physics beyond the Standard Model, although experimenters have observed a small but interesting excess of events in certain channels.

Another interesting signature accessible at the Tevatron is the resonance production of a pair of top quarks. Such signatures are characteristic of many models of new physics in which the third generation quarks appear more strongly coupled to new, heavy gauge bosons than are quarks from the first two generations.

The experiments also anticipate further precision measurements of the top quark and *W* boson masses and of the production rate for single top quarks as well as a variety of QCD, *B* physics and electroweak measurements.

Recommendation

> **The panel recommends continuing support for the Tevatron collider program for the next one to two years, to exploit its potential for discoveries.**

3.1.2 LARGE HADRON COLLIDER AND BEYOND

Overview

When the Large Hadron Collider begins operating, it will open a new era in particle physics with discoveries anticipated that have the potential of revolutionizing our understanding of the physics of the universe. This proton collider will take a major step in accelerator performance, with an energy seven times higher than the Tevatron's. Two general-purpose LHC experiments, ATLAS and CMS, will probe the most basic interactions of nature. The smaller dedicated experiments LHCb and ALICE will help elucidate the origin of matter-antimatter asymmetry and of the quark-gluon plasma, respectively.

Intense LHC commissioning activities have included two successful beam injection tests into the LHC ring and, more recently, first power tests of machine sectors up to almost the nominal current. These tests indicate that the accelerator will perform to design specification. The LHC is presently being cooled down to the operation temperature. CERN plans first beam injections in summer 2008, to be followed by first beam-beam collisions. The LHC will run at a center-of-mass energy of 14 TeV. The intensity, or luminosity, during the first few years of running will be a factor of six higher than the Tevatron's world record. The ultimate luminosity will be another factor of five higher. A further increase by a factor of ten, the so-called Super LHC, is planned, and will require detector and accelerator upgrades.

The installation of the four experiments in the underground caverns is essentially complete, and vigorous commissioning campaigns with cosmic rays have been underway for some time. In parallel, scientists are carrying out so-called "data challenges," massive simulations and world-wide distributions of event samples similar to those expected at the LHC, in order to finalize the software tools and to stress-test the LHC's Grid-based computing infrastructure all over the globe.

Physics program overview

The physics program planned for the LHC is broad, diverse and exciting. When the collider turns on at a low startup luminosity, measurements of the production rates and properties of QCD jets, photons, *W* and *Z* bosons, and quarks with heavy flavor will probe the Standard Model in a new kinematic regime. The large number of top quarks produced at the LHC will allow precise measurements of the top mass and top's couplings, and will permit sensitive searches for nonstandard top decays. Searches for rare or forbidden *B* decays will constrain possible extensions to the Standard Model. Studies of these processes will begin shortly after accelerator turn-on and will improve in precision with increasing data.

The most tantalizing possibilities for discovery at the LHC stem from the exploration of physics at the Terascale. There is by now strong evidence that the Standard Model is not a complete theory of the elementary particles and their interactions. Recent observations that dark matter is not made of known particles and that neutrinos have nonzero masses provide strong evidence for the existence of new physics. The Standard Model cannot give satisfactory answers to many key questions: Why is the universe made of matter, with very little antimatter? What is the relationship between gravity and the forces acting on elementary particles? At the Terascale, physicists expect to discover new particles and interactions to address these Standard Model problems.

The most urgent question concerns the nature of electroweak symmetry breaking, the as-yet-unverified mechanism that generates particle masses. The LHC experiments will conclusively test the Standard Model answer to this problem, the Higgs mechanism, by looking for the Higgs boson over the fully-allowed mass range. With about 1 fb^{-1} (one inverse femtobarn) of well-understood data, likely available by 2009, the absence of a signal would exclude the existence of a Higgs boson. If the Higgs boson exists, on the other hand, 0.5-5 fb^{-1} are needed for discovery, depending on its mass. LHC experiments would observe the Higgs in a number of decay modes. The increased collision intensity of SLHC will test whether the new particle is indeed the Higgs boson. Alternative models call for several Higgs bosons (or even no Higgs boson). If the experiments find more than one, the non-Standard-Model origin of the Higgs will be clear. If there is no Higgs boson, other new phenomena should appear. In this case, detailed measurements would require the higher collision intensities of the SLHC.

Physicists have developed several candidate theories for physics beyond the Standard Model over the past decades. They often predict a rich array of new particles and interactions. ATLAS and CMS, with their direct discovery reach for new particles extending up to masses of 5-6 TeV, will tell us which, if any, scenario is correct. They should also be able to perform precise measurements of newly discovered particles and phenomena, providing insight into the underlying new theory.

Supersymmetry, often referred to as SUSY, is among the most promising scenarios for physics beyond the Standard Model. An intriguing SUSY feature is that the lightest supersymmetric particle, the neutralino, is the best candidate today for dark matter. If Terascale supersymmetry exists, then the LHC will produce copious quantities of neutralinos. In such a scenario, the LHC would become a dark-matter factory.

Within the wide range of existing SUSY models, the predicted masses of the SUSY particles and their dominant decay modes depend on a number of phenomenological parameters. Analyses of Monte Carlo samples by ATLAS and CMS have demonstrated both experiments' ability to observe SUSY, if the mass of the SUSY particles is less than a few TeV, and to perform precise measurement of SUSY parameters. Such measurements could have important cosmological implications, providing insights into the physics of the early universe.

Theories aimed at unifying gravity with the other forces that affect elementary particles generally require more than three spatial dimensions. In these theories, the additional spatial dimensions are too small to observe at the energies of past accelerators. In some such theories, the LHC has sufficient energy to allow experiments to observe the effects of these extra dimensions. The signatures vary with the details of the model, but often include the production of events with large apparent missing transverse energy and momentum, in conjunction with high-momentum jets of particles or with leptons of high transverse momentum. If these theories are correct, the LHC would have profound effects on our understanding of the nature of space-time. It would provide the first experimental evidence for a unified theory of gravity and quantum mechanics.

US involvement in LHC

The tremendous excitement of the LHC program has attracted many US physicists. The recent HEPAP subpanel on the university grant program estimates that half of US particle physicists will conduct research at the LHC. These scientists represent a significant fraction of the LHC experimental community, roughly 20 percent of ATLAS and 35 percent of CMS. They play strong technical and scientific leadership roles on both experiments. US physicists will make major contributions to the LHC physics program and to the great discoveries that result. The LHC experiments will also offer a matchless opportunity for the training of US graduate students. Together, the two experiments currently support the research of several hundred US students.

US high-energy physicists have played critical roles in the construction of the LHC accelerator and detectors, developing the cutting-edge technologies required for such a challenging project. They will play active roles in the commissioning and operation of the LHC. Accelerator physicists from Fermilab, Brookhaven and LBNL are members of the US LHC Accelerator Construction Project. Among the components designed and constructed by US collaborators are inner triplet focusing quadrupole magnets, beam separation dipoles, interaction region distribution boxes, and interaction region absorbers. The US also played an important role in testing and characterizing the accelerator's superconducting cable.

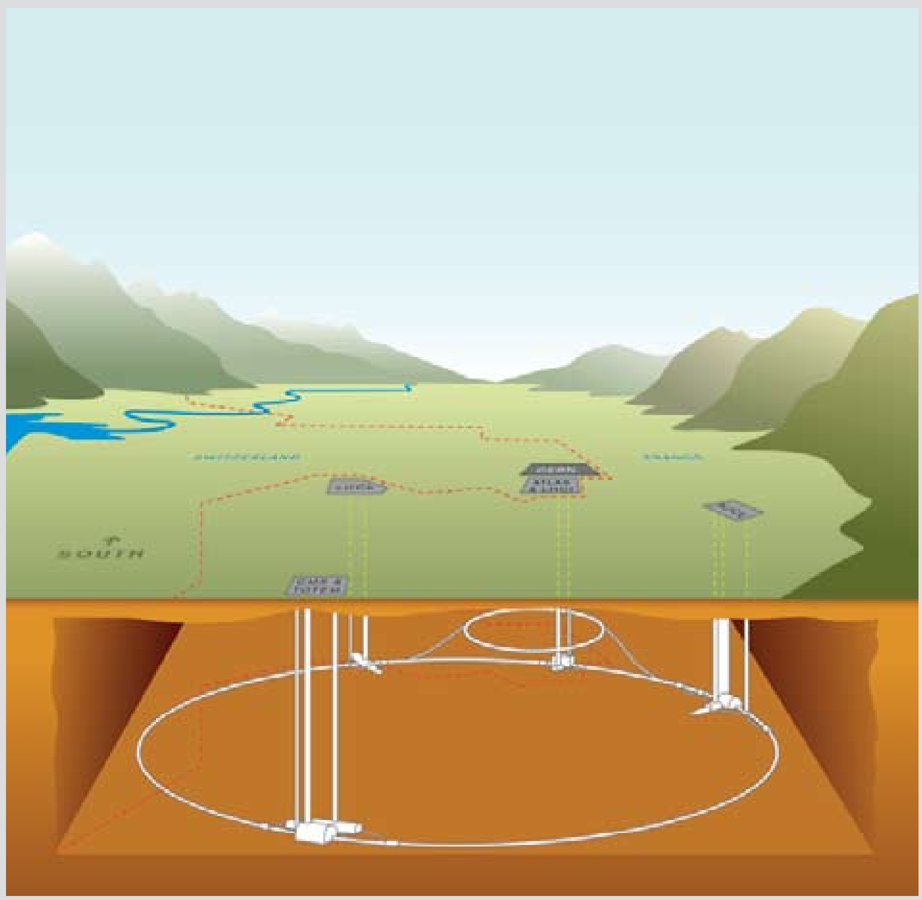
The US LHC Accelerator Research Program comprises accelerator physicists from Fermilab, Brookhaven, LBNL and SLAC. The LARP mission is to provide US expertise to enable optimal performance of the LHC and to use, develop and preserve the unique US resources and capabilities in accelerator science and technology. LARP scientists will be involved in LHC commissioning and performance optimization and will coordinate the US R&D program for LHC accelerator improvements.

Full scientific participation in experiments abroad brings its own challenges. To maintain their scientific and technical leaderships roles, US groups working on LHC experiments must maintain a sizeable presence at CERN. For example, US CMS

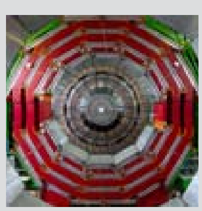
Hadron Colliders

Hadron colliders accelerate beams of protons (or protons and antiprotons) in opposite directions around giant circular rings at nearly the speed of light, producing millions of high-energy collisions per second. The colliding protons contain quarks and gluons, each carrying a fraction of the proton's total energy. Since a typical collision involves one quark or gluon from each proton, the resulting collision energy is lower than the protons' total energy. These collisions convert energy into new particles whose discovery illuminates the fundamental nature of the physical universe.

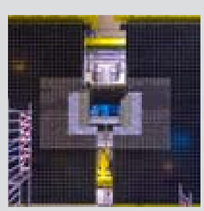
The highest-energy hadron collider operating today is the Tevatron at Fermilab, where protons collide with antiprotons with a total energy of 2 TeV. The Large Hadron Collider, the world's next-generation hadron collider, with a total energy of 14 TeV, will provide direct access to the very high energies of the Terascale and the discoveries that it harbors. Arguably the most ambitious scientific endeavor ever undertaken, the LHC is nearing completion in a 27-kilometer tunnel at CERN, the European particle physics laboratory in Geneva, Switzerland. For most of their split-second journey around the accelerator ring, the LHC's hair-thin beams of protons will travel in separate vacuum pipes; but at four points they will collide and produce the interactions of interest. At these collision points are massive experiments—huge both in size and in worldwide participation—known by their acronyms: ALICE, ATLAS, CMS, and LHCb.



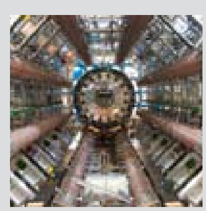
The Large Hadron Collider at CERN



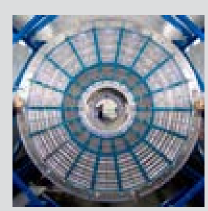
CMS



LHCb



ATLAS



ALICE

management and operating plans require stationing about 120 US physicists at CERN. Numbers for ATLAS are comparable. These requirements have significant cost implications, especially at a time when the dollar-to-euro exchange rate is at an historical low. Because the majority of the physicists living at CERN come from university groups, the university research program will largely bear these costs. Second, for US groups to participate fully in the LHC physics program, physicists based in the US must be able to contribute without impediment to the scientific collaborations and discussions. The use of high-quality collaborative tools such as videoconferencing can achieve this goal. Third, in order to perform complex analyses that require sophisticated data-mining and analysis techniques, US physicists must have excellent access to computing resources.

The 2007 University Grants Program subpanel report extensively discusses the issues summarized here. That report made the following recommendations, which P5 endorses:

- The agencies should continue their efforts to ensure that the vision for LHC computing is realized. This includes working across and within agencies to ensure sufficient network and computing capacity.
- The agencies should support efforts to ensure that both US sites and key sites abroad are equipped with remote videoconferencing systems that are reliable, robust and readily available.
- The agencies should support the increased travel and subsistence costs of university researchers participating in the LHC and other overseas experiments.

Significant US participation in the full exploitation of the LHC has the highest priority in the US high-energy physics program.

Evolution of the LHC

The LHC will be the premier facility at the energy frontier for more than a decade. It is appropriate to consider the long-term program of operation and of accelerator and detector improvements that are a natural part of any successful collider program. The Tevatron, for example, had several upgrades to increase energy and luminosity. Upgrades to the detectors accompanied each collider upgrade, to allow operations in the new and more challenging accelerator environment and to fully exploit the physics opportunities. The LHC anticipates similar upgrades.

CERN plans an LHC upgrade program, Super-LHC or SLHC, to increase the discovery potential of the machine and to allow more precise measurements of the phenomena observed in early running. The SLHC will extend the reach of the initial LHC by 30 percent in terms of particle masses. Precise measurements of properties of new particles discovered at the LHC will also benefit from the tenfold increase in luminosity. As an example, ratios of Higgs boson couplings to fermions and bosons can be measured to an accuracy of up to 10 percent, providing crucial additional insight into the electroweak symmetry breaking mechanism. Furthermore, several rare decays of the Higgs boson (e.g., $H \rightarrow \mu\mu$) and of supersymmetric particles, not possible at the LHC, become accessible with the huge data sample provided by SLHC.

Phase I of SLHC has a goal of providing reliable operation at twice the nominal LHC design luminosity. Plans call for completion of construction in 2013. Phase II is more ambitious, with a goal of achieving an order of magnitude higher luminosity than that of the initial LHC design. Details of the Phase II upgrade will depend on the performance of the LHC and on the physics observed in the initial running. CERN will take a decision on the scope of these more ambitious upgrades around 2011, and the upgrade itself would occur later in the decade. Upgrades to the ATLAS and CMS detectors will accompany each phase.

To fully exploit their intellectual and technical contributions to the LHC and continue exploring the new physics it will uncover, US scientists have expressed a strong desire to participate in the SLHC construction project on both detectors and accelerator. In fact, US physicists are already deeply involved in R&D and planning for these upgrades. While the details of participation are still under discussion, the scale would be commensurate with the roles US scientists are playing in the current project. The complexity and technical challenge of SLHC upgrades require significant R&D efforts, calling for new concepts and developments that will keep US scientists on the cutting edge of technology. Because of the technical complexity of the project, R&D is underway for both Phase I and II and will need increased support.

Recommendation

> **The panel recommends support for the US LHC program, including US involvement in the planned detector and accelerator upgrades, under any of the funding scenarios considered by the panel.**

3.1.3 FUTURE LEPTON COLLIDERS

The physics of lepton colliders

The international particle physics community has reached consensus that a full understanding of the physics of the Terascale will require a lepton collider in addition to the Large Hadron Collider.

Basic features differentiate hadron and lepton colliders. Because protons and antiprotons (hadrons) are complex objects made of quarks and gluons, collisions between these hadrons take myriad different forms depending on which of the constituents collide. A single quark-quark or quark-gluon collision can produce a signal of new physics. The other quarks and gluons are merely spectators, forming a background to this signal. In hadron collisions, each individual quark or gluon carries only a fraction of the total energy of the proton, typically around 10 percent, so the useful energy for producing new physics is typically one tenth of the total collision energy. Furthermore, the energy and the detailed quantum numbers of the quark-quark or quark-antiquark initial state are unknown.

Lepton colliders, on the other hand, create collisions of pointlike, fundamental particles with no internal components. The full energy of each collision goes into producing new physics, with the energy and quantum numbers of the initial state well known. In addition, researchers can control the polarization, or spin, of the colliding electrons and positrons.

The complexity of hadron collisions provides a richness in the variety and energies of the initial states that has allowed hadron colliders to discover new physics. The complementary lepton colliders with their clean and well-defined initial states have discovered new physics on their own, and have also helped to define the nature of the new physics discovered at any accelerator.

A lepton collider complements a hadron collider. It can probe a similar energy scale with one tenth of the energy of a hadron collider. The last generation of lepton colliders, LEP and the SLC, with energies of around 100-200 GeV, complemented the 2-TeV Tevatron. All contributed important discoveries to our present understanding of the Standard Model. A lepton collider of around a TeV will probably be needed to complement the discoveries of the 14-TeV LHC. This rule of thumb is only approximate; results from the LHC will yield a better estimate of the needed energy of the next lepton collider.

The Higgs boson and supersymmetric particles as they would be measured at an ILC illustrate the complementarity between hadron and lepton colliders. Knowledge of the initial state of the interaction, including the energy and spin directions of the interacting leptons, would inform ILC physics measurements. The interactions are simple processes with comparable cross sections for competing channels. For example, the rate of producing a *Z* boson and a 120 GeV Higgs boson at a 500 GeV collider ($e^+e^- \rightarrow ZH$) is comparable to the cross section of diquark production of any flavor. With an inclusive trigger and highly polarized beams (80 percent for electrons, as well as positron polarization), events are clean and revealing.

Study of the Higgs boson through this process means events can be tagged by reconstruction of the *Z*, without reference to the decay of the Higgs itself. Even invisible decays of the Higgs are accessible in this way. A precise measurement of the decay modes of the Higgs is possible, and deviations from the Standard Model can be discovered with great sensitivity, without model assumptions. If a Standard Model-like Higgs boson is responsible for electroweak symmetry breaking, the ILC would precisely measure its mass and width (to 0.03 percent for a 120 GeV Higgs boson). The ILC would measure the decay rates of the Higgs into many channels with a few percent uncertainty, and determine its quantum properties. Discovery of non-Standard Model properties would be possible with precision well beyond that achieved first at the LHC. In addition, should nature choose alternative scenarios for electroweak symmetry breaking, the ILC would have sensitivity to strong coupling up to several TeV, exceeding the reach of the LHC in some scenarios.

A lepton collider would provide powerful access to supersymmetric particles. The capability to collect data at a series of energies around the threshold of a new physical process is unique to a lepton collider, making it an extremely powerful tool for measuring particle masses with high precision and unambiguously determining particle spins. Selecting particular running energies can boost cross sections and minimize backgrounds for particular studies. The polarized beam capabilities of an ILC would make it particularly useful for these measurements. Electron polarization would allow experiments to distinguish electroweak quantum numbers and measure important mixing angles. For example, slepton production cross sections are expected to be electron-polarization dependent. Polarization would be a powerful tool in suppressing backgrounds.

The International Linear Collider

In the last decade, the Tevatron at Fermilab, LEP at CERN, and the SLC at SLAC have provided indirect evidence that the mass of the Higgs boson should be in the 100- to 200-GeV range. This evidence has led to the expectation that a linear e^+e^- collider with a 500 GeV center-of-mass energy, upgradable to 1 TeV, could be the appropriate next lepton collider. An international collaboration explored paths to realize such a collider. The International Committee for Future Accelerators, or ICFA, created the International Linear Collider Steering Committee to coordinate the effort. An ILCSC subpanel recommended superconducting rf as the technology of choice. The ILCSC commissioned the Global Design Effort to lead an international team of scientists to develop a design for a linear collider. The GDE released its Reference Design Report in early 2007, with a first cost estimate. This estimate indicated a higher cost for the ILC than US funding agencies had expected—a higher cost than several previous reports on the future of US particle physics had assumed.

The scientific value of the ILC has not diminished. However, its high cost and the consequent delay in a possible construction start require a new strategy.

Results from the LHC will confirm or contradict this choice of energy. If the optimum initial energy for a lepton collider proves to be at or below approximately 500 GeV, then the ILC as designed by the Global Design Effort is the most mature and ready-to-build option, with a construction start possible in the next decade. The cost and scale of the ILC mean that it would have to be an international project, with the

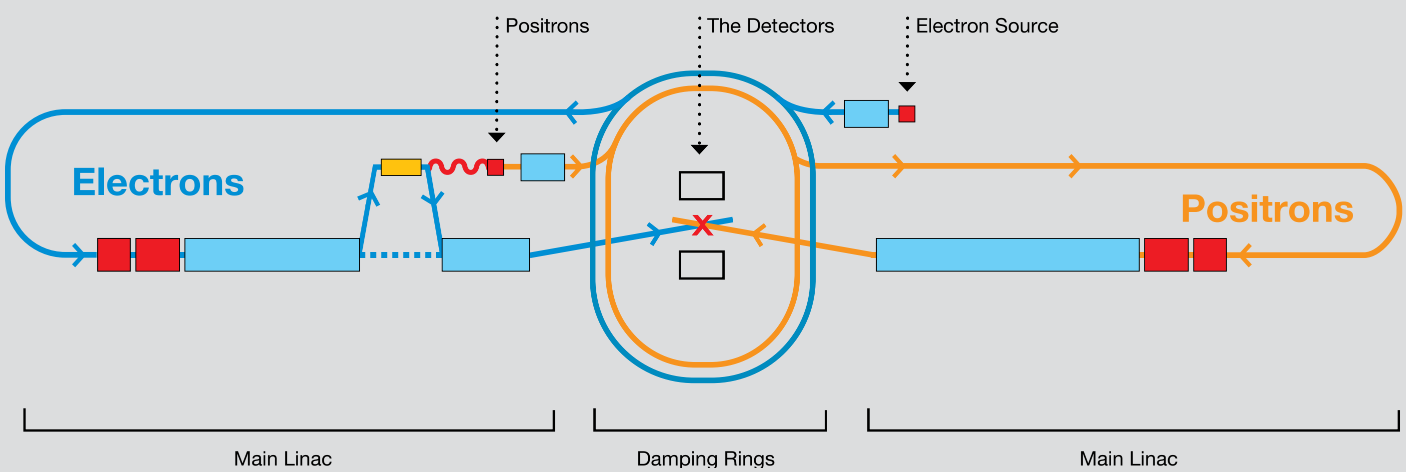
Lepton Colliders

Lepton colliders to date have accelerated electrons and positrons and brought them into collision; they are called e^+e^- colliders, or electron colliders for short. In the future it may be possible to produce and accelerate muons and bring them into collision to produce $\mu^+\mu^-$ collisions in a muon collider. The early electron colliders accelerated and stored electrons and positrons in circular machines, just like protons in a hadron collider. Charged particles such as electrons and protons radiate energy, called synchrotron radiation, when accelerated in a circular path. Synchrotron radiation is much stronger for lighter particles than for heavy ones and grows rapidly with the energy of the particle. It is not a severe problem for protons, but the energy loss of electrons in a circular accelerator becomes prohibitively large as the energy of the electrons exceeds 100 GeV. This led to the pioneering development at SLAC of e^+e^- colliders that accelerate the electrons and positrons in a linear path so that the radiation energy loss is greatly reduced.

Linear colliders are the technology of choice for TeV class electron colliders. The design of the International Linear Collider envisions two linear accelerators, one for electrons and one for positrons, aimed at each other so that the particles, accelerated to 250 GeV each, collide to produce a total energy of 500 GeV. Each accelerator would be about 12 km long. These collisions would take place in detectors located at the point between the two accelerators where the collisions occur. In the ILC design, superconducting radio frequency cavities accelerate the electrons, an efficient technology for reducing the collider's power consumption.

Because muons are much heavier then electrons, they experience less energy loss to radiation, so muon colliders are envisioned to be circular machines. Circular accelerators are more efficient then linear colliders, since the vast majority of the particles that do not interact at the collision point continue to circulate and can be repeatedly recirculated to the collision point. In a linear collider, the beams have only one chance to interact. However, the muon's extremely short lifetime creates challenges for muon collider design.

An example of a linear collider, the SLAC Linear Collider, has operated successfully with an energy of 100 GeV, so that the design of linear e^+e^- colliders is at a relatively advanced stage. Muon colliders, on the other hand, still need considerable R&D just to establish proof of principle. Muon colliders are therefore further off in time then electron colliders.



cost shared by many nations. International negotiations will determine the siting; the host will be assured of scientific leadership at the Energy Frontier.

A requirement for energy higher than the ILC's would mean considering other collider technologies, such as the CLIC technology under study at CERN, high-gradient warm cavities, or the technology of a muon collider.

Lepton collider R&D
The US should continue to participate in the international R&D program for the ILC for the next few years to preserve the option of playing an important role in case the ILC proves to be the choice of the international community. In view of the new realities in both the US and in the UK, the GDE has formulated a minimal plan for the international R&D program with the goal of producing a proposal for the ILC (but without a detailed engineering design) by 2012. By then, results from the LHC should be available to inform the choice of energy for a lepton collider. In this plan, the US role in ILC R&D would focus solely on activities where the US accelerator community has unique expertise to support the international effort. The FY2009 budget request reflects this level of R&D, which should continue for the next four years under any of the funding scenarios considered by the panel.

If LHC results dictate a higher energy, a room-temperature linear collider concept could have more potential. In any case, future experiments will eventually need higher energies. Thus CERN has continued to develop the Compact Linear Collider based on a two-beam accelerator concept for the rf power generation. CERN plans to develop a conceptual design for CLIC by early in the next decade. The R&D program on CLIC has made excellent progress, recently modifying parameters to benefit from the X-band rf R&D programs in the US and Japan that operated through the early 2000s. R&D has continued in all three regions of the world on the next generation of accelerator structures for gradients in excess of 100 megavolts per meter. Plans call for demonstration of two-beam acceleration in a CLIC module by 2010. Another option for the rf power source is rf klystrons of an improved design, which may have a relatively fast demonstration path. At present the R&D funding on normal-conducting linear collider technologies is minimal in the US. Progress would require an expanded R&D program in room-temperature cavity structures and associated power delivery.

Recent advances in plasma and laser acceleration technologies hold promise for extremely compact accelerators that could reach much higher energy in a cost-effective manner. Gradients as high as 50 gigavolts per meter have been achieved in plasma accelerators—roughly 1000 times higher than in rf accelerators. It will take significant further R&D and new test facilities to demonstrate the feasibility of these approaches for a high beam intensity multi-TeV linear collider and to further develop the technologies.

Finally, a muon collider may be an effective means to reach multi-TeV energies. A muon collider would be free of the beam effects that can limit an e^+e^- collider at very high energies and would have the potential for highly efficient conversion of site power to useful collision energy. Using muons instead of electrons also has the advantage that recirculating linacs could use the accelerating structures multiple times to provide energy to both particle beams simultaneously. The challenge for a muon collider is to produce, collect, cool and accelerate enough muons to provide the luminosity required to study new phenomena in detail. Recent studies using a jet of mercury in a strong magnetic field have demonstrated that such a target is capable of surviving a four-megawatt proton beam. This first step toward providing muons is very encouraging. The next step is the demonstration of cooling using a combination of ionization energy loss and dispersion in a low-energy, low-frequency, acceleration system. Support for R&D for this program has been very limited. Demonstrating its feasibility or understanding its limitations will require a higher level of support.

Whatever the technology of a future lepton collider, and wherever it may be located, the US should plan to play a major role, and this requires an active program of R&D. In the lower budget scenarios, the US should maintain a broad R&D program so that it can contribute to the technology choice and engage in the construction negotiations. In the higher budget scenarios, the US should position itself through this R&D program to be a potential leader and host. In all funding scenarios, funding for construction of a future lepton collider, either on- or off-shore, would require a temporary increase in funding.

R&D for lepton collider detectors

The physics questions that lepton colliders will address require capabilities beyond the performance of current detectors. The lepton collider environment offers the potential for complete and precise event reconstruction for practically every interaction, a powerful tool for discovery. To reconstruct the anticipated final states and to identify and measure them with the requisite precision requires measuring essentially all charged leptons, quarks and gluons, photons, and W and Z bosons extremely well. This requires excellent particle identification using vertex detectors, excellent tracking and momentum resolution, as well as jet energy resolutions only possible with new approaches for jet energy measurements with so called particle flow algorithms.

The global structure responsible for the linear collider physics program and its detectors is the World Wide Study. The WWS has three chairs, one from each of the three regions of the world participating in the ILC. The corresponding regional entity is the American Linear Collider Physics Group, which coordinates physics and detector efforts in the US.

Achieving these advances in detector performance will require a well-orchestrated worldwide detector R&D program. Over the last few years, Europe has realized such a program. It has been reviewed and funded and is addressing some key R&D areas including sensor R&D for vertex detectors and new hadron calorimeters. In the US, a coherent and funded program is only partially in place. An existing university-based ILC detector R&D program has received funding at a modest but increasing level for several years; in FY2007 it reached $2.1M/year. This program is based on proposals from groups or individuals at universities and multiprogram laboratories engaged in high-energy physics. Most of the detector advances from this directed program are generic, and will be applicable to higher lepton-collider energies. The future of this critical program for FY2008 and beyond is uncertain. Besides the university-based program, a lab-based detector R&D program informally couples to the university program. It is fair to say that US efforts on linear collider detector R&D are lagging behind efforts in Europe in both funding and manpower. The funding shortfall over the past few years has made it difficult to define a coherent US program. A leading role for the US in a linear collider will require addressing this issue and putting in place a well defined and sufficiently funded US linear collider detector program.

Recommendation

> **The panel recommends for the near future a broad accelerator R&D program for lepton colliders that includes continued R&D on ILC at roughly the proposed FY2009 level in support of the international effort. This will allow a significant role for the US in the ILC wherever it is built. The panel also recommends R&D for alternative accelerator technologies, to permit an informed choice when the lepton collider energy is established.**
>
> **The panel also recommends an R&D program for detector technologies to support a major US role in preparing for physics at a lepton collider.**

3.2 THE INTENSITY FRONTIER: NEUTRINO PHYSICS AND PRECISION MEASUREMENTS

At the Intensity Frontier, precision measurements of the properties of leptons and quarks can lead the way to resolving some of the universe's deepest mysteries.

3.2.1 NEUTRINO PHYSICS

Neutrino physics has had a long and distinguished history, with US scientists at the forefront of many discoveries. We outline an ambitious vision that builds on that strong scientific tradition to capture the unique scientific opportunities of neutrino science.

Results of recent experiments have revolutionized and brought renewed excitement to this field. They have shown that neutrinos have nonzero masses, mix with one another, and oscillate among the neutrino flavor states. Cosmology tells us that the neutrino masses are very small, less than one millionth of the electron's mass. Oscillation studies find tiny nonzero neutrino mass differences between generations, but large values of two of the three mixing angles, $\theta_{23} \sim 45$ degrees and $\theta_{12} \sim 32$ degrees. Currently we only have an upper limit of about 10 degrees on the third angle, θ_{13}.

Collectively, these advances in neutrino physics have opened the first crack in the Standard Model of particle physics. They have significantly changed our view of neutrinos and the special role they play in elementary particle physics, astrophysics and cosmology.

In the coming years, neutrino physics presents exciting opportunities: the measurement of the mixing angle between the heaviest and lightest neutrinos, determination of the hierarchy of neutrino masses, the search for matter-antimatter asymmetry (CP violation) in neutrino mixing, and lepton number violation. These opportunities are fundamental to the science of particle physics and have profound consequences for the understanding of the evolution of the universe.

For the mixing angle measurements, the US has strong participation in reactor experiments in France and China, with first results expected by 2012 and final results by 2014. The US leads the world in lepton number violation measurement through several strong double beta decay experiments; one will begin operation this year. The NOvA experiment, currently the only approved experiment with a chance of measuring the mass hierarchy, is scheduled to begin taking data in 2013 with final results by 2020.

Full exploration of the mass hierarchy and discovery and study of CP violation present an extraordinary scientific opportunity for the US program. A high-power neutrino beam from Fermilab to the proposed Deep Underground Science and Engineering Laboratory, DUSEL, represents an excellent chance for the US to establish clear leadership in these two measurements that would begin to unlock the secret of the matter dominance in the universe and physics at very high energies. Coupled with an already strong program in double beta decay and reactor experiments, the Fermilab-DUSEL long baseline experiment would firmly establish the US as the leader in neutrino science.

Questions for the future

As the first chapter in the study of neutrino oscillations comes to an end, a new chapter begins. The great progress in neutrino physics over the last few decades raises new questions and provides opportunities for major discoveries. Among the compelling issues today:

1) What is the value of θ_{13}, the mixing angle between first- and third-generation neutrinos for which, so far, experiments have only established limits? Determining

the size of θ_{13} has critical importance not only because it is a fundamental parameter, but because its value will determine the tactics to best address many other questions in neutrino physics.

2) Do neutrino oscillations violate CP? If so, how can neutrino CP violation drive a matter-antimatter asymmetry among leptons in the early universe (leptogenesis)? What is the value of the CP violating phase, which is so far completely unknown? Is CP violation among neutrinos related to CP violation in the quark sector?

3) What are the relative masses of the three known neutrinos? Are they “normal,” analogous to the quark sector, ($m_3>m_2>m_1$) or do they have a so-called “inverted” hierarchy ($m_2>m_1>m_3$)? Oscillation studies currently allow either ordering. The ordering has important consequences for interpreting the results of neutrinoless double beta decay experiments and for understanding the origin and pattern of masses in a more fundamental way, restricting possible theoretical models.

4) Is θ_{23} maximal (45 degrees)? if so, why? Will the pattern of neutrino mixing provide insights regarding unification of the fundamental forces? Will it indicate new symmetries or new selection rules?

5) Are neutrinos their own antiparticles? Do they give rise to lepton number violation, or leptogenesis, in the early universe? Do they have observable laboratory consequences such as the sought-after neutrinoless double beta decay in nuclei?

6) What can we learn from observation of the intense flux of neutrinos from a supernova within our galaxy? Can we observe the neutrino remnants of all supernovae that have occurred since the beginning of time?

7) What can neutrinos reveal about other astrophysical phenomena? Will we find localized cosmic sources of very-high-energy neutrinos?

8) What can neutrinos tell us about new physics beyond the Standard Model, dark energy, extra dimensions? Do sterile neutrinos exist?

The small neutrino masses find a natural explanation in the so-called see-saw mechanism. The see-saw mechanism requires the existence of very heavy additional sterile neutrinos at a high mass scale possibly associated with grand unification of all interactions. This suggests that neutrinos may be their own antiparticles. If so, they can give rise to lepton number nonconservation, an otherwise exact symmetry. The measured values of oscillation parameters indicate that it may be possible to observe CP violation through the different behavior of neutrinos and antineutrinos in long-baseline neutrino oscillations, an exciting goal for future experiments.

Both CP violation and lepton number nonconservation are required for leptogenesis, a mechanism that can lead to an asymmetry between leptons and antileptons in the very early universe. Leptogenesis, in turn, can drive baryogenesis (the creation of an excess of quarks relative to antiquarks) at a later stage in the universe’s evolution. Together, leptogenesis and baryogenesis could explain the observed matter-antimatter asymmetry of our present day universe. Neutrino physics thereby may hold the key to understanding our very existence today.

Neutrinos also play other roles in astrophysics and cosmology. For example, they are crucial for understanding the dynamics of supernova explosions that give rise to the heavy elements necessary for planet formation and the conditions for life.

Scientific opportunities in neutrino physics

A coordinated research program is necessary to address the many questions of neutrino physics. Some of the experiments in this program require accelerator-based neutrino beams, some use reactors, and others, such as double beta decay experiments, are high-sensitivity studies of nuclear decays.

The accelerator-based measurements are extremely challenging and have ambiguities in the interpretation of results. The proposed U.S. and Japanese programs take complementary approaches that together would greatly enhance the understanding of the underlying science. One particular advantage of the envisioned US program is the long baseline available from Fermilab to the Homestake site.

Accelerator-based neutrino experiments

The ongoing program at Fermilab

The MiniBooNE, MINOS and SciBooNE experiments are currently running. The MINOS experiment, driven by the Main Injector and NuMI beamline, uses a 735 km baseline with a detector located in Minnesota's Soudan Mine. MINOS has produced the best measurement of $|\Delta m_{32}|^2$ by measuring the disappearance of muon neutrinos through mixing en route from Fermilab to Soudan. Further running of the MINOS experiment will improve our knowledge of this important parameter. MINOS also has some sensitivity to the appearance of electron neutrinos that are created through mixing en route from Fermilab. MINOS can probe to beneath the current reactor limit, down to about $\sin^2 2\theta_{13}$ of 0.07 based on an exposure of about 10^{21} protons on the NuMI target over the next several years. The MINERvA experiment has started construction and will measure key neutrino production cross-sections from the NuMI target. The proposed MicroBooNE experiment, a step in the development of liquid argon detectors, is under consideration by the Fermilab Physics Advisory Committee.

Long Baseline Neutrino Oscillation Experiments

Experiments can use traditional accelerator-produced muon neutrino and antineutrino beams to study oscillations via disappearance or the appearance of electron neutrinos or tau neutrinos. Because of their higher energies, accelerator neutrino beams require much longer oscillation distances than do reactors.

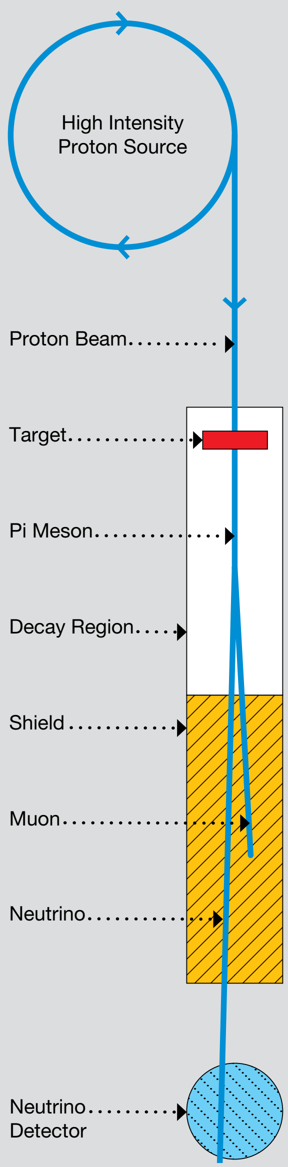
The centerpiece of the long-term US neutrino program will be a high-intensity proton source at Fermilab providing an intense neutrino beam towards a large detector in the proposed DUSEL 1300 km away. This combination of an intense neutrino beam and a very large detector at a very long distance has broad physics discovery potential. More specifically, this long baseline will enhance matter effects, which will improve the sensitivity to both mass ordering and CP violation. The detector and the beam design should be optimized to study these two key questions.

In addition, measuring neutrino (and antineutrino) disappearance and electron neutrino appearance, would, in principle, make possible the measurement of all the parameters of three-generation mixing in one experiment; some with high precision. This program would also be sensitive to new physics effects beyond the Standard Model such as sterile neutrinos and extra dimensions.

Experimenters could also use the neutrino detector to search for proton decay and to study atmospheric neutrino oscillations. They could detect a supernova in our galaxy, estimated to occur about every 40 years, and observe relic neutrinos left over from early supernovae that have occurred since the beginning of time. Collectively, those capabilities would make the very large detector DUSEL's flagship program.

This potential DUSEL detector would be a natural successor to the multipurpose water Cerenkov Super-*K* detector currently operating in Japan. To extend various proton decay partial lifetime searches by at least an order of magnitude over current

High-Intensity Particle Beams



Because neutrinos interact only weakly with the particles of ordinary matter, sensitive neutrino experiments require an intense neutrino source (many trillions of neutrinos) and a large neutrino detector (large mass for neutrinos to interact with). Scientists have long studied neutrino properties with neutrinos produced in the sun during the fusion process and with neutrinos produced in the atmosphere by cosmic rays. Experiments today also use neutrinos produced by nuclear reactors or particle accelerators. Reactors produce only one species of neutrino, electron-type anti-neutrinos. Reactor neutrinos have low energy and are distributed uniformly in all directions. Particle beams from accelerators, on the other hand, can produce either muon neutrinos or muon anti-neutrinos in high-energy beams directed at neutrino detectors.

Accelerators produce beams of neutrinos in a sequence of steps. A proton beam interacts with nuclei in a target to produce a variety of secondary particles via the strong nuclear interaction. Most of the secondary particles are charged pions. Because these charged pions have significant momentum in the direction of the proton beam that hits the target, they can be focused into a secondary pion beam. The pions are unstable and decay, via the weak nuclear interaction, into pairs of particles, each pair consisting of a muon and a neutrino. The neutrinos from pion decay form a beam of neutrinos aligned with the pion beam. A beam absorber, a large mass of material in which all particles other than neutrinos lose their energy, absorbs the pions that do not decay, along with all the other particles traveling along the pion beam. Only neutrinos, because they interact so weakly with matter, traverse the beam absorber and emerge as a neutrino beam.

The primary proton beam line establishes the direction of the neutrino beam that emerges. For that reason, directing a neutrino beam from Fermilab to a future large neutrino detector in the Deep Underground Science and Engineering Laboratory, for example, would require a new proton beam line pointed in the right direction. The intensity, or beam power, of the primary proton beam determines the intensity of the resulting neutrino beam. Intense neutrino beams require intense proton beams, like the one that would be produced by a future high-intensity proton source at Fermilab.

Experiments that study neutrino properties by observing the oscillation of neutrinos from one neutrino type to another require a long baseline, that is they need a large distance between the neutrino source and the neutrino detector in order to give the neutrinos time to oscillate. Moreover, because neutrinos and anti-neutrinos interact differently with matter, a long baseline provides the opportunity to search for subtle differences between the properties of neutrinos and their anti-neutrino partners. A long baseline also affords the possibility to determine which neutrino types have the highest and lowest neutrino mass, the so-called neutrino mass hierarchy.

The muons produced in pion decays can be used in a variety of experiments such as studying the muon's anomalous magnetic moment or its possible conversion into an electron. Decays of muons in the circulating beam of a muon storage ring could produce a very pure, intense neutrino beam, the concept behind a possible future neutrino factory. Circulating beams of oppositely charged muons are the basis for a possible future muon collider.

Proton beams can also produce other types of secondary particle beams, including charged and neutral kaons for precision and high-sensitivity studies.

limits requires a water Cerenkov detector of about 300-500kton fiducial mass. Alternatively, a higher-resolution detector with better acceptance and lower backgrounds such as a 75-125 kton liquid argon time projection chamber could be used.

Determining the CP-violating phase to about 15 degrees with such a detector and baseline will require an intense neutrino superbeam from Fermilab. A proton beam with 2 megawatts of power or greater could generate such a beam. The currently planned upgrade of the existing Fermilab accelerator complex can produce a 700 kW beam. The eventual 2MW (or higher) facility would require the proposed 8 GeV linac addition to the accelerator complex or a new more intense synchrotron booster.

The detailed design of the long-baseline program will depend on the value of θ_{13}. The purely statistical requirements for measuring CP violation are rather insensitive to the actual value of that quantity from its current limit of 0.15 down to about 0.003. In practice, however, backgrounds will limit sensitivity. The ultimate limit will be determined by the specific value of $\sin^2 2\theta_{13}$, which will determine the signal to background ratio for the oscillations and provide guidance for choosing the best detector technology. The design of these measurements, the optimum detector technology and the timeline can be best assessed after the reactor and T2K results on $\sin^2 2\theta_{13}$ start to appear, probably around 2012. If these experiments yield only an upper limit on θ_{13} presumably around $\sin^2 2\theta_{13}$ of 0.02, the first goal of the long-baseline program would be to improve the sensitivity to this quantity by another order of magnitude.

Planning for this ambitious program should proceed so as to make physics results available at intermediate stages. The first phase, already yielding important results could consist of a 700 kW neutrino beam and a 100 kt water Cerenkov or 25 kt liquid argon detector. Even detectors of this size would represent a significant increase over what has been constructed to date (22.5 kt fiducial mass of Super-K and 600 ton liquid argon ICARUS). Vigorous R&D on both technologies should proceed as rapidly as possible.

The road to the long-range vision
In Japan, JPARC plans sensitive electron neutrino appearance experiments employing the existing Super-K detector and a new 50-GeV proton synchrotron together with a new neutrino beam line (baseline 295km), both currently under construction. An experiment at Fermilab would use the proposed NOvA detector (baseline 810km) along with the existing NuMI neutrino beam line. Both would employ off-axis neutrino beams to narrow the neutrino energy spectrum and improve signal-to-background ratio.

JPARC anticipates completing a neutrino beam in 2009 and beginning their new T2K experiment with a timeline that aims to be competitive with the reactor experiments for determining $\sin^2 2\theta_{13}$, but in the electron neutrino appearance mode. There is a possibility of subsequently adding a much larger (500-1000 kton) water Cerenkov detector and upgrading the JPARC beam power, both major projects, with the aim of attaining CP violation sensitivity. This initiative is still in the early planning and R&D phase.

In Europe, CERN is considering a staged approach based on a multi-megawatt proton source that would be constructed as part of the LHC upgrade potentially followed by a beta beam or a neutrino factory.

NOvA is the only approved experiment that can address the vital question of the neutrino mass hierarchy. Strategically, it offers continuity of important measurements and discovery in the time between the current US accelerator-based neutrino program and an ambitious future neutrino program based on a multi-megawatt beam at Fermilab and a large detector at DUSEL. The NOvA project exploits the existing neutrino beamline at Fermilab and includes the planned neutrino beam upgrade to 700 KW that is an essential step towards a future program aimed at DUSEL. Due to slower than expected funding, NOvA now anticipates starting the search for electron neutrino appearance with a partial detector in 2013 and a full detector in 2014. Funding constraints have required reducing the NOvA detector mass from 20 to 15 kt. Current NOvA plans call for a run of six years, three years of neutrinos and three years of antineutrinos. The experiment addresses several important questions in neutrino physics.

By comparing the rates and spectra of the $\nu_\mu \rightarrow \nu_e$ oscillation for neutrinos and antineutrinos, NOvA has the capability of determining the neutrino mass hierarchy. The reach depends on the values of the CP phase delta and the as-yet-unmeasured parameter θ_{13}. For relatively large values of $\sin^2 2\theta_{13}$, e.g. 0.1, this determination should be possible at the two sigma level for about 50 percent of the possible values of the phase delta. The reach decreases as the value of $\sin^2 2\theta_{13}$ decreases and vanishes around $\sin^2 2\theta_{13} = 0.03$.

NOvA data taking with a full detector will start in 2014, about three to five years after the start of the reactor and T2K experiments, making it unlikely that NOvA would be the first experiment to measure θ_{13} if it is relatively large, e.g. $\sin^2 2\theta_{13} > 0.025$. For smaller values of θ_{13} combining NOvA's results with the reactor measurements will play an important role in determining its value and helping to decide whether the more ambitious long-baseline to DUSEL program can determine the mass hierarchy.

Improved knowledge of $\sin^2 2\theta_{23}$ is important in interpreting the observed $\nu_\mu \rightarrow \nu_e$ rate measurements in long-baseline experiments. Both NOvA and T2K claim to have potential for significant improvement in measuring its value. In addition, NOvA could search for sterile neutrinos by looking for a deficit of neutral current events. Finally, NOvA would collect about 900 neutrino events in the event of a supernova in the middle of our galaxy.

Nuclear reactor experiments

Nuclear reactors provide a prolific source of anti-electron neutrinos. The Kamland detector in Japan has recently used reactor-produced neutrinos with great success to precisely determine Δm_{21}^2 and to confirm solar neutrino oscillation parameters via anti-electron neutrino disappearance over long distances. KamLand's findings confirm that the goal of measuring CP violation in neutrino oscillations is a realistic possibility.

The earlier Chooz and Palo Verde reactor experiments provide currently the best limit on θ_{13}, $\sin^2 2\theta_{13} < 0.15$. Several new multi-detector reactor experiments under construction, Double Chooz in France, Daya Bay in China and Reno in South Korea aim to significantly improve the sensitivity to that important parameter. They expect to have first results from for the two-detector phase beginning in about 2010. By 2012, the most ambitious, Daya Bay, may start to approach $\sin^2 2\theta_{13}$ 0.01-0.02 sensitivity. Ultimately, by 2014, Daya Bay hopes to reach 0.008 at the 90 percent confidence level. Those experiments offer the fastest and most direct way to determine $\sin^2 2\theta_{13}$. US physicists have leadership roles in both Double Chooz and Daya Bay.

Neutrinoless Double Beta Decay

Neutrinoless double beta decay is a process in which a nuclear decay occurs with the emission of two electrons and no antineutrinos. It violates lepton number by two units. If it occurs, it is extremely rare and therefore requires a very large double-beta-decay source. Searching for such a reaction is by far the most sensitive known way to probe lepton number nonconservation. If observed, it would indicate that neutrinos are their own antiparticles and may provide a direct measurement of their masses. It would also strongly support the leptogenesis explanation of the observed matter-antimatter asymmetry in the universe. Such a finding would have immense scientific value. An experiment claims an observation of neutrinoless double beta decay in ^{76}Ge with a lifetime of 2.2×10^{25} yr, but that interpretation is highly controversial.

Currently, neutrinoless double beta decay experiments are in progress or are planned worldwide. Within the US, they involve both the nuclear physics and high energy physics communities and rely on funding from their respective programs in DOE and NSF. The EXO experiment represents a major HEP initiative. It is preparing for a Phase I run at the WIPP facility in New Mexico employing 200kg of isotopically purified liquid xenon. The Phase I sensitivity should be able to confirm or refute the claimed observation. A much larger Phase II (8000kg) experiment, if feasible, could be constructed at DUSEL. This experiment could probe the lifetime range suggested by the oscillation results.

Recommendations

The panel recommends a world-class neutrino program as a core component of the US program, with the long-term vision of a large detector in the proposed DUSEL laboratory and a high-intensity neutrino source at Fermilab.

The panel further recommends that in any funding scenario considered by the panel Fermilab proceed with the upgrade of the present proton source by about a factor of two, to 700 kilowatts to allow for an extended physics reach using the current beamline and a timely start for the neutrino program in the Homestake mine.

The panel recommends support for a vigorous R&D program on liquid argon detectors and water Cerenkov detectors in any funding scenario considered by the panel. The panel recommends designing the detector in a fashion that allows an evolving capability to measure neutrino oscillations and to search for proton decays and supernovae neutrinos.

In all but the lowest funding scenario, the panel recommends a rapid NOvA construction start. However, the lowest funding scenario would further delay the experiment's construction start, and the costs of NOvA construction and operation would displace other programs of higher priority. The panel therefore recommends that Fermilab not proceed with the NOvA experiment under the lowest funding scenario.

The reactor experiments, Double Chooz and Daya Bay, are designed to carry out measurements of the mixing angle θ_{13}, an important physics parameter. The panel recommends support for these experiments under any of the funding scenarios considered by the panel.

Nonaccelerator experiments searching for neutrinoless double beta decay have the potential to make discoveries of major importance about the fundamental nature of neutrinos. The panel recommends support for these experiments, in coordination with other agencies, under any funding scenario considered by the panel.

3.2.2 PRECISION MEASUREMENTS OF QUARKS AND LEPTONS

The flavor of new physics

"Flavor" is the congenital trait of fundamental particles that distinguishes quark and lepton siblings from one another. The 1937 discovery of the muon, the heavier sibling of the electron, initiated a program of research in flavor physics that culminated in formation of the Standard Model. The flavor structure of the Standard Model in turn is closely linked to the new physics of the Terascale that experiments will explore at the Large Hadron Collider.

Flavor phenomena shape the Standard Model as much as any other ingredient. The existence of flavor for quarks and leptons gives the Standard Model its family and generation structure. Assembling quarks in electroweak doublets to suppress flavor-changing neutral current decays of kaons led to the prediction of the charm quark, and other rare kaon decays led to the first observation of a matter-antimatter asymmetry called CP-violation. These and many other observed flavor phenomena led to the CKM model that defines how different types of quarks transform into one another, which in turn predicted the existence of a third generation of quarks, top and bottom, later discovered at Fermilab. The observation of the mixing of neutral *B* mesons correctly anticipated the large value of the top quark mass. The dramatic discovery of neutrino masses provided the first incontrovertible evidence that the Standard Model is incomplete, and may provide a window to grand unification. It is not surprising that several of the great questions of particle physics have flavor at their core, and it is reasonable to expect that flavor physics will continue playing a crucial role in the progress of the field.

Experiments at the Large Hadron Collider will explore new physics that is directly associated with flavor physics. Without a Higgs boson or some other mechanism of electroweak symmetry breaking, quark flavor effects would not even exist. All flavor phenomena in the Standard Model are encoded by a handful of seemingly arbitrary parameters. Beyond the Standard Model, flavor phenomena are even more strongly entangled with the dynamics of symmetry breaking. Flavor-changing processes may be mediated by new particles such as a charged Higgs boson, or by supersymmetric partners. New flavors may appear, either in the form of new generations, or as exotic partners of standard quarks. New sources of CP violation can arise from couplings of non-minimal Higgs sectors, or of superpartners. With all of these new sources of flavor effects, the natural suppression of most-flavor violating phenomena in the Standard Model disappears. Physicists expect large flavor-violating effects from new Terascale phenomena, larger in most models than what is observed today in precision study of flavor physics phenomena. This tension between the strong argument for Terascale physics and the absence to date of any corresponding effect in precision flavor physics studies gives rise to the so-called “flavor problem.” The flavor problem is generic to almost all ideas for physics beyond the Standard Model and is a fundamental constraint in all attempts to understand mass and its origin.

Most new physics manifestations that we can envision will provide new sources of flavor phenomena, accentuating our need to address questions related to flavor. Experiments at the energy frontier and flavor experiments at the intensity frontier will both provide tools to explore the laws of nature. A deeper understanding of the open questions of flavor physics will require discoveries and measurements from complementary facilities.

The future of quark flavor experiments

The study of the quark flavor sector is highly advanced. Just as precision measurements of electroweak observables agree very well with theoretical predictions, the flavor structure prescribed by the Standard Model appears to succeed at describing the observables. But what is the origin of this success? Are the CKM parameters just arbitrary inputs, their values randomly selected by nature, or are they the result of some underlying mechanism? We ask similar questions about the gauge structure of the Standard Model. Are the relative strengths of the gauge forces independent parameters, or are they related by some deeper connection? Here we believe we may have an answer, namely the idea of grand unification, which justifies the existence of several gauge couplings, and determines their values, as a result of the dynamics of symmetry breaking. Are there equivalent scenarios for the understanding of masses and flavor mixing? Could they emerge from some underlying mechanism? Is there a direct connection between the masses and mixings in the quark and lepton sectors? Neutrino masses and the matter antimatter asymmetry of the universe require new flavor phenomena besides those predicted by the Standard

Model. Do these phenomena give related signatures involving quarks? To address these questions requires improving the accuracy of both the measurements of and the theoretical predictions for quark flavor phenomena.

Future B, D, and tau physics

The heavy flavor physics community will continue the vigorous exploration of quark flavor phenomena, including the search for deviations from the Standard Model, at the LHC, the BEPC-II collider in China, and at possible future Super *B*-factories. Several crucial tests, such as the measurement of the rare $B^0_{d,s} \to \mu^+\mu^-$ decays, or of CP violation in the B_s system, are within the reach of the ongoing Tevatron collider experiments or forthcoming LHC experiments.

A rich program of flavor phenomena in mixing and decays of *B* and *D* mesons and tau leptons can be studied to a high level of precision at next generation e^+e^- "super flavor factories" collecting an integrated luminosity of 50-75 ab^{-1}, more than 50 times the luminosity collected to date by the current *B* factories. Many sensitive measurements of physics beyond the Standard Model, unique to a super flavor factory, can be performed with sensitivity to new physics in excess of the TeV-scale. Flavor- and CP-violating couplings of new particles that may be discovered at the LHC can be measured in most scenarios, even in unfavorable cases with minimal flavor violation. The Super KEKB initiative is central to the recently released Japanese particle physics roadmap for the next decade. The Super *B* proposal for a new laboratory in Tor Vergata, Italy, has attracted broad international interest.

The physics reach of a super flavor factory is well motivated and grounded in the very rich suite of measurements produced by the current generation of *B* factories. Two offshore super flavor factory initiatives are now being developed with US involvement. A modest level of R&D should continue toward a goal of supporting an informed consideration of any significant US investment in a super flavor factory. The maturity of the field of *B* physics supports a strategy of significant US investment in a single next-generation overseas facility.

Future kaon physics

The ultra-rare kaon decays $K^0_L \to \pi^0 \nu\bar{\nu}$ and $K^\pm \to \pi^\pm \nu\bar{\nu}$ are particularly sensitive to effects of new physics. The Standard Model branching fractions for these decay modes are extremely small; moreover, they are predicted with small theoretical uncertainty of a few percent of the Standard Model rate. Consequently, these decays are sensitive to new physics effects with rates that are a fraction of the Standard Model rates, whereas new physics models typically predict rates of 10 percent to 40 times the Standard Model rates. These decays provide sensitivity even to minimal flavor-violating scenarios.

The next generation of high-sensitivity kaon experiments is now proceeding in Japan with a new high-intensity proton source; and in Europe with an evolution of the successful kaon program driven by the CERN SPS. The proposed NA62 experiment at CERN would measure the $K^\pm \to \pi^\pm \nu\bar{\nu}$ mode with a sensitivity of 100 standard model events. Measurement of the mode $K^0_L \to \pi^0 \nu\bar{\nu}$ is being pursued in a staged manner now at KEK and will go forward later at JPARC with an initial sensitivity goal of a few Standard Model events followed by an eventual measurement phase with a sensitivity of 10-100 events. The US is supporting these initiatives through the transfer of techniques and experimental equipment to these offshore facilities and a modest level of US collaboration should be encouraged.

The potential of very-high-power kaon beams produced by a future high-intensity proton source at Fermilab has motivated consideration of experiments capable of observing 1000 Standard Model events, which would match the theoretical uncertainty of a few percent of the Standard Model rates. Recent studies suggest that this ultimate sensitivity is plausible and could be approached in a staged manner with useful sensitivity at each stage. Such experiments would be a major component of a future high-sensitivity physics program at Fermilab, and their implementation should be pursued. Progress toward proposals for these experiments is tied to the time scale and capability of the future high-intensity proton source at Fermilab.

Charged lepton flavor experiments

The discovery of neutrino masses and lepton mixing established that individual lepton-flavor numbers—electron number, muon number and tau number—are not conserved; however, all lepton-number violating effects to date have been observed in the neutral lepton sector, through the phenomenon of neutrino oscillations. Charged-lepton flavor-violation, on the other hand, has been the subject of intense experimental searches since the discovery of the muon, but, to this date, these searches have never uncovered evidence of charged-lepton flavor-violation. Indeed, these searches have had great historical importance in the evolution of the Standard Model and in constraining new physics extensions. For example, nonobservation of the decay $\mu \rightarrow e\gamma$ helped establish the muon as a distinct elementary particle rather than an excited electron and later motivated the introduction of the muon neutrino. Searches for rare tau decays, such as $\tau \rightarrow \mu\gamma$, are now starting to have a role of similar importance.

The Standard Model predicts very small rates for CLFV processes; whereas many models for new physics, for example supersymmetry, predict measurable rates. CLFV processes can be sensitive to new physics at and well above the TeV scale. They are also affected by the mechanism of neutrino mass generation, although expectations depend dramatically on the mechanism responsible. In any case, searches for CLFV are bound to play a key role in uncovering the origin of neutrino masses.

A muon-to-electron conversion experiment at Fermilab could provide an advance in experimental sensitivity of four orders of magnitude. The experiment could go forward in the next decade with a modest evolution of the Fermilab accelerator complex. Such an experiment could be the first step in a world-leading muon-decay program eventually driven by a next-generation high-intensity proton source. Development of a muon-to-electron conversion experiment should be strongly encouraged in all budget scenarios considered by the panel.

Flavor-conserving muon properties are also likely to reveal important information concerning physics beyond the Standard Model. The long muon lifetime and relatively large mass allow measurement of some of these properties with great precision. The muon anomalous magnetic moment $(g_\mu-2)/2 = a_\mu$ is particularly significant in this regard.

A next-generation (g-2) experiment could be mounted at Brookhaven or Fermilab or offshore at JPARC. There is an excellent physics case for this classic precision measurement. Nonetheless, the estimated cost to the US particle physics program is substantial and would compromise the timely development of higher-priority precision physics experiments such as muon-to-electron conversion. US participation in an experiment at JPARC would cost less and the US in-kind contribution of the existing precision storage ring, which is central to the experiment, would be substantial. A modest level of R&D support should be made available for the (g-2) collaboration to determine the optimal path toward a next generation experiment.

Precision studies using other techniques

Precision tests of the Standard Model have been carried out using atoms, for example by measuring their weak interaction coupling. Searching for an electric dipole moment in atoms provides great sensitivity to some models of new physics because EDMs are highly suppressed in the Standard Model. Ideas are now being developed to search for EDMs of charged objects such as the muon, proton or deuteron.

Precision neutrino scattering experiments
Measurements of neutrino scattering have played an important role in development of the Standard Model. The first measurements of the weak neutral current came from neutrino scattering measurements on both electrons and nuclei, and neutrinos have probed the quark structure of the nucleon and provided measurements of the strong coupling constant across a broad range of momentum transfers.

The previous section discussed oscillation measurements at many hundreds of kilometers from the neutrino source. Here we discuss the questions that neutrino scattering measurements at short distances can address. A clear need for new scattering measurements comes from the reliance of neutrino oscillation experiments on precise knowledge of neutrino scattering cross sections. The 2004 APS "Multidivisional Neutrino Study Report," which set a roadmap for neutrino physics, predicated its recommendations on a set of assumptions about current and future programs including "determination of the neutrino reaction and production cross sections required for a precise understanding of neutrino-oscillation physics."

Neutrino scattering measurements from low to high energy
The SciBooNE experiment is now collecting data. The MINERvA experiment is under construction and is scheduled to start collecting data in 2010. These small-scale experiments will perform measurements interesting in their own right and important to the success of the large-scale long-baseline neutrino oscillation experiments. Under Scenario A, MINERvA would not collect data beyond FY2010.

There is an excellent physics case for a next-generation high-energy neutrino scattering experiment, such as NuSOnG or HiResMv. The Tevatron could be reconfigured to drive this high-energy fixed-target experiment following the completion of the ongoing Tevatron collider run, but the operating costs would be substantial. Under none of the budget scenarios considered by the panel would the sum of the capital costs of the experiment and operational costs of the Tevatron be justified. However, new physics such as observation of a *Z*' or heavy neutrino at the LHC would warrant revisiting this conclusion.

Recommendation

> **The panel recommends pursuing the muon-to-electron conversion experiment, subject to approval by the Fermilab PAC, under all budget scenarios considered by the panel. The intermediate budget scenario, scenario B, would allow pursuing significant participation in one overseas next-generation *B* factory. The more favorable funding scenario, scenario C, would allow for pursuing a program in rare *K* decay experiments.**

3.2.3 A HIGH-INTENSITY PROTON SOURCE AT FERMILAB

Fermilab's management has proposed a long-range vision for the laboratory based on constructing a new high-intensity proton source. The new proton source would combine modifications of the existing Main Injector and Recycler rings with a new accelerator replacing the existing 8 GeV Booster and its injection linac.

Fermilab's physics vision for this proton source has three main elements:

1. *A neutrino beam for long baseline neutrino oscillation experiments.* A new 2-megawatt proton source with proton energies between 50 and 120 GeV would produce intense neutrino beams, directed toward a large detector located in a distant underground laboratory.

2. *Kaon-and muon-based precision experiments exploiting 8 GeV protons from Fermilab's Recycler, running simultaneously with the neutrino program.* These could include a world-leading muon-to-electron conversion experiment and world-leading rare kaon decay experiments.

3. *A path toward a muon source for a possible future neutrino factory, and, potentially, a muon collider at the Energy Frontier.* This path requires that the new 8 GeV proton source have significant upgrade potential.

These physics objectives define the mission need. The laboratory proposes an aggressive R&D program whose goal is the complete technical design of a facility capable of meeting these objectives. The R&D program would include design and development of technical components, as well as a fully developed baseline scope, cost estimate and schedule. This program would be carried out by a multi-institutional collaboration, including other DOE national laboratories and international collaborators.

A new high-intensity proton source would provide 120-GeV beams that exceed the capabilities of upgrades to the Fermilab complex that do not involve replacing the Booster. Additionally, a high-intensity proton source would provide capability to operate precision experiments powered by 8-GeV beams without diminishing the intensity of neutrino beams. A new high-intensity proton source would replace the aging 8-GeV Booster, which has reliability concerns.

Fermilab is currently pursuing a design based on an 8 GeV superconducting H^- linac. The laboratory is investigating the use of ILC cavities, klystrons and modified ILC cryomodules for the 1.2 GeV-8 GeV section of this linac. Any linac design based on superconducting rf could benefit from elements of the current US-based ILC or generic superconducting rf R&D efforts. This alignment, however, should not impose design constraints on the proposed proton source. Rather, the proton source design should be optimized for the three major physics objectives, which all benefit from the highest possible beam power. Some years ago Fermilab also investigated the possibility of a new synchrotron-based proton source. The current understanding is that a synchrotron might offer cost savings but might not be able to meet all of physics requirements outlined above and might not offer the same potential for upgrade to even higher beam power.

A high-intensity proton source at Fermilab, combined with a large detector in the Homestake mine, will lead to a neutrino program that complements and in many ways surpasses the program in other regions. In Japan, the new JPARC proton source will provide neutrino beams to the Super-Kamiokande detector. This new proton source is expected to achieve 100 kW of 30 GeV proton power by next year, with further upgrades to 600 kW within a few years. JPARC is planning upgrades to 1.7 MW at 50 GeV by the middle of the next decade. Thus, the power of the proposed high-intensity proton source at Fermilab has beam power comparable to or higher than in Japan. Moreover, the Fermilab-Homestake baseline is longer than the T2K baseline, between JPARC and the Super-K detector, enhancing matter effects that distinguish between mass hierarchies and amplify CP-violation effects. In Europe, CERN is considering a staged approach based on a multi-megawatt proton source that would be constructed as part of the LHC upgrade potentially followed by a beta beam or a neutrino factory.

Construction of a high-intensity proton source at Fermilab could begin within the period considered by this report.

Recommendations

The panel recommends proceeding now with an R&D program to design a multi-megawatt proton source at Fermilab and a neutrino beamline to DUSEL and recommends carrying out R&D on the technology for a large multi-purpose neutrino and proton decay detector.

The panel further recommends that in any funding scenario considered by the panel Fermilab proceed with the upgrade of the present proton source by about a factor of two, to 700 kilowatts to allow for an extended physics reach using the current beamline and a timely start for the neutrino program in the Homestake mine.

3.3 THE COSMIC FRONTIER

Particle astrophysics uses the cosmos as a laboratory to probe the fundamental laws of physics in ways that complement accelerator-based experiments. These efforts have provided strong evidence for physics beyond the Standard Model in the form of nonzero neutrino masses, nonbaryonic dark matter, dark energy and primordial inflation, the last likely tied to unification physics near the fundamental Planck scale. Together, these elements provide a consistent accounting for the constituents of the universe. The interactions of these constituents profoundly shape the structure of the universe over cosmic time. The deep and beautiful connection between inner space and outer space enables us to uncover new particle physics through astrophysical observations.

A combination of accelerator-based and non-accelerator (particle astrophysics) experiments must address fundamental physics questions raised by the new paradigm that these observations have revealed:

- What is dark matter?
- What physical phenomenon is causing the expansion of the universe to speed up?
- Was an epoch of primordial inflation responsible for the origin of large-scale structure? Did it leave observable imprints that can shed light on the unification of the fundamental particles and forces at energies that far exceed those accessible to terrestrial accelerators?
- What is the origin and nature of the highest-energy cosmic rays?
- What are the neutrino masses and what is their impact on cosmic evolution?

The EPP2010 report, "Charting the Course for Elementary Particle Physics," noted that addressing these questions will form an important component of the overall particle physics program and recommended an expanded program in particle astrophysics as a share of the US HEP budget. With relatively modest levels of investment to date, the US has developed a position of world leadership in this field. The panel places high priority on maintaining and developing that leadership position, which will require increased investment going forward.

In some cases, particle astrophysics experiments use techniques that have traditionally been the province of astronomy, adapting them to focus on questions of fundamental physics. This overlap complicates the evaluation of projects, since there are particle astrophysics experiments that can probe fundamental physics while also producing data of broader interest in astronomy. At the same time, the cross-fertilization of research techniques and researchers in particle astrophysics has tremendously enriched both high-energy physics and astronomy. The litmus test questions for evaluating and prioritizing particle astrophysics experiments in the context of the US particle physics program should be the same as for the other frontier areas of high-energy physics: Is the science compelling and world-class? Will it address questions of fundamental physics? Does it complement the other areas of the particle physics program? Is there a natural leadership role for the US?

As a relatively young field, particle astrophysics has yielded important advances with modest scale experiments. As the field matures, the scale of experiments is growing apace, and strategic planning will be essential for achieving a balanced program. Since particle astrophysics involves efforts at DOE, NSF and NASA, such planning should involve all three agencies where appropriate. As in other areas of particle physics, we recommend that this planning allow for flexibility by maintaining diversity of efforts among large and small-scale projects. We note that the Astronomy and Astrophysics Advisory Committee provides assessments and recommendations regarding the coordination of programs among the three agencies. The recommendations on particle astrophysics in this report are quite consistent with those in the recent March 2008 annual report of the AAAC.

The rest of this section describes particular areas of research on the Cosmic Frontier: direct detection of dark matter; dark energy and cosmic acceleration; high-energy particles from space; and cosmic microwave background radiation.

3.3.1 DIRECT DETECTION OF DARK MATTER

The problem of dark matter lies squarely at the confluence of particle physics and astrophysics and touches on profound issues in both fields. Dark matter's role in the evolution of the universe and the development of structure are crucial and far-reaching, ensuring its central position in the study of cosmology. At the same time, the cosmological constraints on the cosmic baryon density imply that most of the dark matter is nonbaryonic; dark matter therefore requires physics beyond the Standard Model, making its elucidation an important goal for particle physics. The search for dark matter involves three independent and complementary approaches: indirect searches, which seek evidence for dark matter annihilations occurring near the center of our galaxy and in other astrophysical bodies; direct searches, which seek evidence for dark matter particles from the halo of our galaxy passing through terrestrial detectors; and accelerator-based searches, which seek to produce dark matter and probe its properties in collider experiments.

We focus here on direct searches for dark matter. This is a rapidly developing field, with a tremendous diversity of approaches and many novel ideas that have only recently appeared on the stage. This P5 report comes less than a year after the extensive review of this field by the Dark Matter Scientific Assessment Group. Although significant progress has occurred even in this short time period, the conclusions and recommendations of DMSAG remain timely and valid. In this report we reconfirm their recommendations and provide a short update on the progress since the DMSAG report.

Early evidence for dark matter included the large measured velocity dispersions of galaxy clusters and flat rotation curves of galaxies. More recently, measurements of gravitational lensing, hot gas in galaxy clusters, the cosmic microwave background, and large-scale structure have indicated that about 25 percent of the energy density of the universe takes the form of nonrelativistic matter. Since the cosmic microwave background and big bang nucleosynthesis indicate that only 4 percent of the density consists of baryonic matter, the remaining approximately 20 percent must be non-baryonic dark matter. Models of structure formation indicate that the bulk of the dark matter has likely been nonrelativistic over most of cosmic history; it is therefore known as cold dark matter.

While the cosmic density of dark matter in the universe is becoming known to high precision, the identity of the CDM particle remains a complete mystery. No particle contained within the laws of physics as we know them has the right properties to be CDM. However, CDM particles do emerge naturally in a variety of well-motivated theories of physics beyond the Standard Model. The two most compelling candidates for dark matter are axions and weakly interacting massive particles, or WIMPs. These particles are well motivated not only because they could resolve the dark matter puzzle, but also because they are associated with solutions of longstanding problems of the Standard Model.

Axions are elementary particles that arise naturally in theories that explain why observers have not detected large CP- violating effects predicted by the Standard Model. Axions are expected to be both lighter, 1-10 μeV, and more weakly interacting than neutrinos, posing a great experimental challenge to their detection. Nevertheless, experiments such as ADMX have recently reached the extraordinary sensitivity required to detect axions if they constitute all or much of the dark matter. Axions passing through a microwave cavity in a strong magnetic field would convert to photons, which could then be detected.

WIMPs are particles that interact through the weak interactions of the Standard Model and are expected to have masses near the electroweak scale of about 100 GeV-1 TeV. The neutralino, the lightest stable supersymmetric particle in a wide class of SUSY models, is a prime dark-matter candidate. Direct detection experiments are designed to find evidence for WIMPs from the halo of the galaxy through their collisions with ordinary matter. The local density of dark matter is estimated from the dynamics of the Milky Way to be about 0.3 GeV/cm^3, which translates to about one particle per coffee-cup volume for a 100-GeV dark matter candidate; this is about five orders of magnitude higher than the cosmic density of dark matter. Although the local dark matter density is appreciable, its interaction cross-section with ordinary matter is constrained to be quite small, and the nuclear recoil energy per event is only a few tens of keV. Therefore, these experiments must be sensitive enough to observe the extremely small energy deposited in very rare interactions, while at the same time discriminating against other particle interactions that might mimic WIMP collisions.

To maximize the WIMP signal, high-mass target nuclei are generally preferred, because, in the case of spin-independent WIMP-nucleus interactions, the rate is proportional to the square of the nuclear mass. In addition, the recoil energy of the nucleus is maximized when the mass of the nucleus is equal to the WIMP mass.

Proposed WIMP detectors use a variety of techniques to discriminate signal from background. The three most common experimental techniques are ionization, scintillation and phonon emission. The most sensitive detectors generally use a combination of two of these techniques. By comparing information from two channels, experimenters can discriminate nuclear recoils caused by WIMPs and neutrons from other backgrounds due to electron recoils caused by photons and electrons. Eliminating the neutron-induced events requires further discrimination and reduced radioactive backgrounds.

The range of WIMP interaction strengths is model-dependent. In supersymmetric models, for example, spin-independent WIMP-proton scattering cross-sections typically range from 10^{-42} to 10^{-46} cm^2. By comparison, the present CDMS sensitivity, currently the world's best spin-independent limit for WIMP masses above 40 GeV, is of the order of 4.5×10^{-44} cm^2 for a WIMP mass of 60 GeV, corresponding to an interaction rate of about 0.01 event/kg/day. Therefore, to test the broader range of supersymmetric predictions requires the capability to isolate and identify about 10 nuclear recoil events per ton of detector per year, a major experimental challenge at the low energies where a WIMP signal is expected.

The identification of dark matter will most likely not be immediately unambiguous, but will rather unfold gradually. An experimental program must take into account this possibility. As an example, a possible scenario for dark matter identification is the following:

- In phase one, an experiment must identify a clear nuclear recoil signal and show that it cannot be reasonably be attributed to neutron background, radon chain disintegration products or some other background.

- In phase two, detection with at least one other target nucleus must confirm the interaction cross-section and WIMP mass, if possible, of the reported signal. At this phase, the LHC as well could produce dark matter candidates, playing an essential role in connecting an astrophysical observation with particle physics.

- In phase three, if the cross-section is large enough to make this possible, a large-statistics experiment sensitive to annual or daily modulation would confirm the galactic nature of the signal, once the directionality of the signal has been determined.

US groups are currently world leaders in direct dark matter searches, and the experimental program is in a state of rapid transition. CDMS, a cryogenic solid-state WIMP detector, recently achieved unprecedented detection limits, as mentioned above. XENON10, a cryogenic noble-liquid detector, recently set an upper limit on the spin-independent WIMP-nucleon cross-section of 5.5×10^{-44} cm^2 for a WIMP mass of 100 GeV. COUPP, a super-heated liquid detector, has achieved stable operation of a 2 kg CF_3I bubble chamber and published its first spin-dependent limit based on 52 kg-days of operation. These experiments are all planning upgrades to more massive detectors. In addition, the LUX collaboration is constructing a 360 kg liquid xenon detector for installation in the Homestake mine as part of a laboratory early implementation plan. A Princeton group has identified an underground source of argon that is depleted in ^{39}Ar by over a factor of 20, which may make it economically feasible to consider liquid argon detectors of ton-scale. A Boston University-Brandeis-MIT collaboration has been developing a low-pressure CF4 time projection chamber and has observed directional discrimination of fluorine atom recoils. This is encouraging for eventual development of a WIMP detector with directional sensitivity, which could aid in WIMP identification. Other noble liquid WIMP detectors such as WARP and ZEPLIN-II-both European-led but with US participation—are also quickly progressing.

The development of the proposed DUSEL laboratory has encouraged the dark-matter detection collaborations to start planning for larger-scale detectors that could be ready when DUSEL becomes operational. Development of cryogenic semiconductors, of noble liquids, including xenon, argon, and neon, of superheated liquid detectors, and of both low- and high-gas-pressure detectors is being pursued. Although at the present no single best detector technology has been identified, as detector masses grow to very large scales some consolidation of effort will be necessary. The DMSAG report stressed the need for a program review in 2009 to focus the limited available funding into the most promising programs. We support that view.

Our recommendation on dark matter detection reflects our enthusiasm for the potential of this growing field, the strength of the efforts already underway in the US, and the dynamic expansion of capability driven by emerging technologies.

Recommendation

> **The panel recommends that NSF and DOE jointly support direct dark matter detection experiments under any of the funding scenarios considered by the panel. The choice of which of these experiments to support in the longer term should be made after completion of ongoing experiments and the R&D on the next generation of detectors.**

3.3.2 PROBING DARK ENERGY AND COSMIC ACCELERATION

In 1998, two independent teams studying distant supernovae made the surprising discovery that the expansion rate of the universe is accelerating. Subsequent observations of the cosmic microwave background radiation anisotropy and of the large-scale distribution of galaxies, as well as observations of clusters and more recent supernova measurements, have independently confirmed and amplified this remarkable finding. According to Einstein's theory of general relativity, if the universe is filled with ordinary matter, gravity should be slowing down its expansion. Since the expansion is instead speeding up, we are faced with two possibilities, either of which would have profound implications for our understanding of the cosmos and of the fundamental laws of physics. Either three quarters of the energy density of the universe is in a new form called dark energy, or general relativity breaks down on cosmological scales and must be replaced with a new theory of gravity.

Cosmologists have suggested a variety of theoretical ideas to explain dark energy. It could be the energy of the vacuum, equivalent to Einstein's cosmological constant. Although sometimes considered the simplest model for dark energy, conventional particle physics theory predicts that the vacuum energy density should be at least 50 orders of magnitude larger than the value that would account for the present acceleration. Alternatively, dark energy could signal the existence of a new, ultra-light particle with a mass of order 10^{-33} GeV, an idea sometimes dubbed quintessence. Or cosmic acceleration could instead be signaling the need to revise Einstein's 90-year old theory of gravitation, or something else entirely new and unexpected.

While the nature of dark energy is unknown, a well-defined set of first questions has emerged: Is dark energy the cosmological constant? Is it energy or gravity? Do its properties evolve over time? The major goals of dark energy experiments will be to address these fundamental questions. A convenient way of describing the physical properties of dark energy is through its equation of state, the ratio of its pressure to its energy density. Future dark energy experiments will determine this ratio, and whether and how it evolves with cosmic time, with much greater precision than present experiments have achieved. These experiments will probe dark energy by studying its impact on both the history of the cosmic expansion rate and the growth rate of large-scale structure. In general relativity, the expansion rate, along with the properties of dark matter, determine the rate at which structure forms; departures from Einstein gravity may thus be tested by comparing the expansion rate history with the history of structure growth.

As emphasized by the 2006 Dark Energy Task Force Report, four techniques appear to hold great promise as probes of dark energy: type Ia supernovae, weak gravitational lensing of distant galaxies, baryon acoustic oscillations in the large-scale clustering of galaxies, and the abundance and clustering evolution of galaxy clusters. While this list does not exhaust the potentially powerful probes of dark energy, given our current knowledge it constitutes a robust set of techniques that together have the best opportunity to achieve the aims of probing the nature of dark energy. Supernovae and baryon acoustic oscillations provide measures of distance and expansion rate vs. redshift. Weak lensing and cluster surveys combine distance measures with the growth of structure. These techniques are therefore complementary in their power to probe dark energy and in the systematic errors to which they are susceptible.

The field has proposed a diverse and ambitious set of projects to probe dark energy using different combinations of the above techniques. The DETF report described a staged program of dark-energy experiments: on-going projects are designated Stage II; near-future, intermediate-scale projects are Stage III; and larger-scale, longer-term projects are Stage IV. More advanced stages are in general expected to deliver tighter dark-energy constraints, which the DETF quantified in terms of the equation of state, i.e. the pressure-to-energy-density ratio, and its evolution. The report noted the importance of pursuing multiple dark-energy probes at each stage and emphasized that control of systematic errors will be critical to the success of the program. Stage IV projects will be required to answer the critical questions about dark energy stated above.

The 2006 P5 report recognized the intellectual excitement and growing importance of dark-energy research. For the near term, it recommended at high priority a construction start for the Dark Energy Survey, a Stage III project that will build a new wide-field camera for the 4-meter telescope at Cerro Tololo InterAmerican Observatory in Chile and use it to carry out a multiband imaging survey covering 5000 square degrees. DES will employ all four dark-energy probes. We strongly reiterate this recommendation and note that a successful joint DOE-NSF project review in January 2008 recommended the project for CD-2/3a approval. It should move expeditiously into the construction phase. Proposed stage III projects that would probe dark energy in a manner complementary to DES, using spectroscopic surveys of galaxies and quasars to measure baryon acoustic oscillations, include the SDSS-III Baryon Oscillation Spectroscopic Survey (BOSS) in New Mexico and the Hobby-Eberly Telescope Dark Energy Experiment in Texas.

For the longer term, the 2006 P5 report strongly recommended support for R&D for two proposed Stage IV dark-energy projects, the space-based Joint Dark Energy Mission, JDEM, which would be a joint NASA-DOE project, and the ground-based Large Synoptic Survey Telescope, LSST, which would be jointly supported by NSF and DOE. For JDEM, three mission concepts based on deployment of a 2-meter class telescope have been developed using R&D support from NASA: Supernova/Acceleration Probe, SNAP, which would combine wide-field optical plus near-infrared imaging and spectroscopy to probe dark energy primarily through supernovae and weak lensing; the Dark Energy Space Telescope, DESTINY, which would use a near-infrared prism spectrograph to study supernovae; and the Advanced Dark Energy Physics Telescope, ADEPT, a spectroscopic mission with the primary goal of constraining dark energy via baryon acoustic oscillations as well as supernovae. The DOE Office of High Energy Physics has supported the SNAP R&D program.

Since the last P5 report, the National Research Council Beyond Einstein Program Assessment Committee report, *NASA's Einstein Program: An Architecture for Implementation*, recommended JDEM as the first launch priority for NASA's Beyond Einstein program and recommended that NASA and DOE OHEP move forward on soliciting proposals for a medium-scale JDEM mission. BEPAC concluded that JDEM life-cycle costs would be higher than previous estimates, and they considered a $400M DOE contribution in making their recommendation. In recent months the two agencies have worked toward a memorandum of understanding. The panel was informed that the agencies were negotiating a DOE contribution of approximately $200M, the figure used in constructing budget scenarios. An Announcement of Opportunity for JDEM is expected in 2008. The panel endorses this interagency development and recommends support for JDEM at an appropriate level by the agencies, in any of the budget scenarios considered by the panel.

The LSST is a proposed 8-meter telescope with a 10 sq. deg. field of view that would carry out a multiband optical survey over most of the southern sky. Its combination of telescope aperture and field of view would give it unprecedented survey power. It would be sited at Cerro Pachon in Chile and is designed for a 10-year survey program. The LSST project recently underwent a positive review at NSF; construction of the telescope would come primarily from Major Research Equipment and Facilities Construction funding at NSF. DOE OHEP researchers have taken the lead in design and construction of the wide-field CCD imager. The panel believes that LSST is an important component of the long-term dark energy program that complements JDEM and recommends support for the project subject to negotiations with NSF.

Clearly interagency cooperation will be critical to the success of these dark energy experiments. Moreover, all of these projects involve international partners, and the need for international cooperation will grow with the scale of the experiments. We note that the European Space Agency is considering a space-based dark energy project, currently called Euclid, that would merge a weak lensing imaging survey with a spectroscopic baryon acoustic oscillation survey, as part of its Cosmic Visions program.

Recommendations

The panel recommends support for a staged program of dark energy experiments as an integral part of the US particle physics program.

The panel recommends that DOE support JDEM at an appropriate level negotiated with NASA, under all budget scenarios considered by the panel.

The panel recommends DOE support for the ground-based LSST program, in coordination with NSF, in all funding scenarios considered by the panel, at a level that depends on the overall program budget.

3.3.3 HIGH-ENERGY PARTICLES FROM SPACE

A very vibrant field of science lies at the boundary between particle physics and astrophysics. This science would not be possible without the cooperative efforts of particle physicists and astrophysicists, and the questions addressed often fall on both sides of the boundary. Historically, this partnership has paid off: with very modest investments, great new capabilities have been achieved, opening up large and unique discovery spaces of great importance to both fields. These capabilities are usually complementary to those of other efforts, such as accelerator-based experiments, and they fill in the physical picture by providing unique information, even in cases of upper limits. For example, some of the best limits on candidate theories with higher dimensions came first from cosmic gamma-ray observations. Measurements of high-energy particles from space also provide an important discovery path for detecting dark matter.

Because the investments are relatively small, these projects are not individually very visible in P5 roadmaps. Nevertheless, these projects bring important diversity and unique discovery potential to particle physics, so relatively small investments are a high priority, even in the leanest budget scenarios. Because these projects lie at the boundary between disciplines, very large investments at this time are unfortunately only possible in the higher budget scenarios. Multi-agency, international cooperation is particularly important to support this science. This field is developing rapidly, and the panel therefore recommends under all budget scenarios considered by the panel that the agencies establish a Particle Astrophysics Science Advisory Group to provide strategic evaluation of new opportunities as they arise.

The study of gamma rays, ultra-high energy cosmic rays, and neutrinos from space makes use of astrophysical accelerators and can probe fundamental physics in ways that complement terrestrial accelerator experiments. We briefly summarize the status and near-term prospects in these three areas.

Cosmic gamma-ray experiments in space and on the ground. The successful startup of VERITAS on the ground and the imminent launch of GLAST into orbit will soon provide a completely new picture of the gamma-ray universe. Cosmic particle accelerator systems release enormous amounts of energy on remarkably short timescales, giving us clues about physics in regions of strong gravity, high matter density and enormous magnetic fields. Gamma-ray emission may also provide unique information about the character and distribution of dark matter through indirect detection of dark matter annihilation products, connecting with anticipated discoveries at both the LHC and underground direct dark-matter detectors. These general facilities, with their broad discovery potential in both particle physics and astrophysics, should now be operated to exploit their full potential. Looking toward the future, proposals call for R&D toward much larger-scale, ground-based gamma-ray facilities, such as AGIS, along with much smaller proposals such as HAWC. A large-scale facility would only be possible in the higher funding scenarios. The United States pioneered the field of ground-based gamma-ray astrophysics, but there are now very competitive efforts in other parts of the world. In the lowest budget scenario, the US would necessarily forfeit leadership in this field.

Highest-energy cosmic ray experiments. Because interactions with the cosmic microwave background limit the path length, theories predict that the detectable flux of the highest-energy cosmic rays (above 10^{18} eV) originate from relatively nearby galaxies. Intergalactic and galactic magnetic fields randomize the apparent directions of lower-energy charged particles. Therefore, observations of the highest-energy cosmic rays offer the opportunity to search for correlations between the measured direction and hypothetical source locations. Since the previous P5 report, the Auger experiment, located in Argentina with significant US participation and leadership, has detected such correlations for the first time, opening up the field of particle astronomy. Furthermore, violations of the path-length limitations or anomalies in the air shower development initiated by these fantastically energetic particles, if observed, could be a signal of new physics. With the recent success of Auger, the collaboration plans to propose a facility with complementary capabilities in the northern hemisphere, with a preferred site in southeastern Colorado.

High-energy neutrino astrophysics experiments, including IceCube. IceCube construction at the South Pole is approximately halfway to completion, and the installed modules are already operating. IceCube follows on the successful construction and operation of the AMANDA array. Observers have as yet detected no cosmic neutrinos, but IceCube promises to open the possibility of viewing the universe in neutrinos. Besides providing a unique diagnostic of the same types of cosmic accelerators seen in gamma rays (in particular, are the charged particle beams hadronic or leptonic?), IceCube will allow unique searches for physics beyond the Standard Model. We look forward to the completion of IceCube, and the continuing active data analysis.

Recommendation

> **The panel recommends limited R&D funding for these particle astrophysics projects under all budget scenarios considered by the panel, but support for any possible large construction projects should be considered only under funding scenarios C and D. The panel recommends that the funding agencies establish a Particle Astrophysics Science Advisory Group to advise DOE and NSF on the relative merits of the various proposals anticipated in this area.**

Cosmic microwave background radiation

Cosmic microwave background research has made impressive contributions to particle physics, astrophysics and cosmology. The first measurements provided unambiguous confirmation for the Big Bang, while the first detection of CMB anisotropy provided important evidence for an epoch of primordial inflation as the origin of the seed fluctuations that grew into large-scale structure. More recently, precision CMB anisotropy measurements from the ground and from space have provided a wealth of information about the geometry, matter density, energy density and history of the universe. Now on the horizon is the potential to use the polarization of the CMB anisotropy as a probe of unification-scale physics, through detection of polarization *B* modes, as described in the 2006 CMB Task Force report presented to HEPAP.

CMB research is another example of relatively small investments with big payoffs for particle physics and of fruitful cooperation between agencies: DOE supported Smoot's work on COBE, resulting in the recent shared Nobel Prize. In recent years, DOE has received requests for only very small amounts of support for CMB research, with the bulk of the funding coming from NSF and NASA, but DOE-funded technical contributions have been key. Both NSF and DOE should remain open regarding future investment in this area if the correct opportunity arises. In any case, small levels of continued support for detector development are certainly warranted.

Chapter 4
The Deep Underground Science and Engineering Laboratory—DUSEL

4 THE DEEP UNDERGROUND SCIENCE AND ENGINEERING LABORATORY—DUSEL

The Deep Underground Science and Engineering Laboratory would offer a major new facility for US particle physics. Located in the Homestake mine in Lead, South Dakota, DUSEL would be an underground laboratory housing a wide spectrum of experiments. When the first parts of the laboratory begin operation around 2013, DUSEL would be a key element in the US particle physics program. A large detector for long-baseline neutrino physics would be part of the initial suite of experiments, as would detectors for dark matter and double beta decay experiments.

In July 2007, NSF announced the selection of the Homestake Mine as the site for DUSEL, to be operated by the South Dakota Science and Technology Authority. A team led by the University of California at Berkeley is developing a proposal for the NSF Major Research Equipment and Facilities Construction program. A National Science Board review is expected in 2010. If DUSEL goes ahead, MREFC funding will start in 2012 and extend until 2018. Construction could start in 2012 with the first deep underground space at the 4850-foot level open in 2015 and construction completed in 2017.

Between now and 2009, DUSEL will develop an initial suite of experiments. The initial suite comprises experiments from physics, biology, geology and engineering, dominated by physics. Initial experiments will be partly funded from the MREFC, with approximately half of the total $500M MREFC request for construction of the lab and half for the initial suite of experiments.

The planned DUSEL facility consists of three major parts: the surface facilities in Lead, the 300-foot-level underground campus and two lower campuses.
The 300-foot level allows horizontal access and will house research and development laboratories, as well as providing an underground experience for visitors.

The main laboratories for particle physics will be at 4850 and 7800 feet. The 4850-foot level, consisting of up to four large caverns, would be an appropriate level for a large neutrino and proton-decay detector. The 7800-foot level, which will be the deepest large underground laboratory area in the world, will have three smaller laboratories for dark matter and double beta decay detectors. Before DUSEL opens, the South Dakota Science and Technology Authority will operate the Sanford Underground Science and Engineering Laboratory. The Sanford Laboratory is funded by state and private resources and will provide limited access to the 4850-foot level.

Physics Opportunities

DUSEL will host experiments in particle and nuclear physics, geology, geomicrobiology and mining engineering. The particle physics experiments dominate in both cost and size.

Dark Matter

Weakly interacting massive particles provide one of the most interesting dark matter candidates. Current theoretical prejudice, based on supersymmetry, indicates the single nucleon cross section for WIMPs with masses in the few hundred GeV range lies in the cross-section range $\sigma=10^{-46}$–10^{-47}cm^2. Detecting a process with such a small cross section requires detectors with several-ton target masses, a factor of a hundred larger than current 10 kg detectors. With these very large target masses, detectors may require location at the 7800-foot level to reduce neutron background.

DUSEL's multiple campuses allow for a smooth deployment of dark matter experiments. New ideas and detectors may be easily tested at the most accessible 300-foot level. As an experiment improves in sensitivity and requires lower backgrounds, it may relocate to the deeper levels. The initial suite of experiments may contain several dark matter experiments of different technologies at the 4850-foot level.

Double beta decay

Double beta decay experiments are potentially less sensitive to neutron backgrounds than dark matter experiments and could be deployed at either the 4850 or 7800 foot levels. Current double beta decay experiments lie in the 100 kg target mass range, and planned experiments will need from one to ten tons of target mass to reach the sensitivity required to fully cover the region of interest. The initial suite of experiments may contain two double beta decay experiments of different technologies at the 4850 foot level. These experiments could probe the entire mass region indicated by neutrino oscillation results. Their deployment could occur in 2015.

A large neutrino and proton-decay detector

A neutrino beam from Fermilab to DUSEL would provide an extraordinary opportunity for accessing CP violation and the neutrino mass hierarchy. DUSEL planning anticipates several large 55 m caverns at the 4850-foot level, each capable of housing a 100-150 kt water Cerenkov detector or a liquid argon time projection chamber. The initial suite planning provides for a single 150 kt water Cerenkov detector. Further study of proton decay requires a target mass of at least 500 kt, which could be accomplished by installing additional detectors in the other caverns.

After liquid argon has proved to be a viable technology for a large detector, a liquid argon time projection chamber could be installed in one of the caverns. A liquid argon detector has higher sensitivity to the $p \rightarrow \nu K^+$ mode of proton decay. The high granularity of a liquid argon detector gives better sensitivity per unit mass for neutrino detection: conservatively a 30 kt liquid argon TPC would give comparable sensitivity to a 100 kt water Cerenkov detector.

The DUSEL physics program is of central importance to particle physics. Experiments at DUSEL would address many issues, including neutrino physics and searches for proton decay, dark matter, and neutrinoless double beta decay. The two agencies, DOE and NSF, should clearly define the stewardship responsibilities for the components of such a program.

Recommendation

> **The panel endorses the importance of a deep underground laboratory to particle physics, and it urges NSF to make this facility a reality as rapidly as possible. Furthermore, the panel recommends that DOE and NSF work together to realize the experimental particle physics program at DUSEL.**

Chapter 5
Accelerator and Detector R&D

5 ACCELERATOR AND DETECTOR R&D

5.1 ADVANCED ACCELERATOR R&D

Innovations in accelerator science and technology have made possible remarkable discoveries in particle physics over the past 60 years. Today, accelerators are critical not only to particle physics but to other programs in the Office of Science and to the national scientific enterprise. Beyond their purely scientific applications, they offer significant benefits for the nation's health, economy and security.

To improve the efficiency, quality, energy and intensity of accelerated particle beams requires a broad-based ongoing R&D program in accelerator science. The innovations developed in such a program will continue to provide benefits within and beyond the field of particle physics. Accelerator science is an essential element in educating and training the new generation of physicists who will maintain US leadership in particle physics, basic energy research, medical physics and more.

The EPP2010 report from the National Research Council highlighted the importance of accelerators and accelerator R&D as a critical element of a world-competitive US particle physics program. In 2006, a HEPAP subpanel chaired by Jay Marx studied the nation's needs for advanced accelerator R&D. The subpanel's report provides a comprehensive assessment of the needs and status of the international effort in this key area for the future of accelerators and of particle physics research. The Marx report addresses issues of relevance to national goals, stewardship, scope, quality, resources, management and training of students needed to remaintain competitive.

A summary of findings and recommendations from the Marx subpanel:

- Within the DOE Office of High Energy Physics, the breadth of accelerator R&D programs appeared to be generally appropriate for meeting national goals to advance aspects of accelerator science and technology that have a strong potential to advance the capabilities of particle physics research.

- The Office of High Energy Physics has had historical stewardship of accelerator science and technology, which has resulted in substantial benefits to science and the nation. The subpanel recommended that OHEP explicitly recognize this stewardship. A parallel situation exists in NSF with respect to two user facilities, Cornell and Michigan State University, and the subpanel recommended implementation of the proposed Accelerator Physics and Physics Instrumentation initiative.

- A graduate fellowship program could provide for the education and training of the next generation of accelerator scientists and engineers. With a limited number of educational opportunities at universities to meet anticipated future needs, the proposed fellowship program would raise the scientific stature of accelerator science more broadly and increase the number of students entering this important area.

- The subpanel identified critical enabling technologies for accelerator R&D for existing, planned or envisioned facilities. It appeared that the key issues were being addressed, the overall quality of the R&D was high, and the program was generally well balanced given the level of available support. Sustaining that level of quality requires relatively stable funding, modernization of infrastructure, and a continuous inflow of well-trained new researchers. Early industrial involvement in design and optimization for large-scale mass production is critical and should be supported at an appropriate level.

- The Marx subpanel addressed the longer-term accelerator R&D programs supported by OHEP and NSF. The mission of the DOE Office of Science requires a program of long-term R&D consisting of exploratory research focused on developing new and innovative concepts in accelerator physics and technology, on new materials to advance these technologies, and on the fundamental physics, mathematics and understanding of accelerator science. The NSF also supports accelerator research through its particle physics program.

- These programs support a balanced portfolio of curiosity-driven and strategic research in cutting-edge aspects of accelerator science and technology. The subpanel found that these unique programs provide very high scientific value. The success of long-term R&D requires support even in times of budget stress. The subpanel recommended that this level of support should be in the range of five percent of the overall OHEP budget. To maintain vitality, the subpanel recommended a periodic review of the program by a committee of accelerator scientists and experimentalists in particle physics who recognize the priority of longer-term accelerator needs.

- Accelerator science is a field with strong international collaboration, with evolution toward an eventual global strategy that would consider complementary capabilities and seek to optimize the use of resources in the interests of science and the participating nations. Such a strategy would, of necessity, require a broadly accepted view of what R&D should be considered in a worldwide context and what is best left to regional entities.

- The Marx subpanel also addressed the development of a strategic vision for the future needs of accelerator-based particle physics. A major challenge for the accelerator science community is to identify and develop new concepts for future energy frontier accelerators that will provide the exploration tools needed for particle physics at a reasonable cost to society. The future of accelerator-based particle physics depends on the development of new ideas and new accelerator directions to address the demands of beam energy and luminosity and the management of beam power, energy recovery, accelerator power, size and cost.

The US is fortunate to have a strong, world-class accelerator program. The P5 panel believes that the Marx subpanel's recommendations remain valid. Strengthening the health and vitality of accelerator science and technology is a challenge that the scientific community, the funding agencies, the universities, the national laboratories and industry must meet in order to sustain the contributions of this field at the present high level.

Opportunities in advanced accelerator R&D

The future of particle physics depends on advanced accelerator R&D not only for the next generation of accelerators but also for the development of concepts and technologies for future generations of accelerators after that. The cost and time scale of future accelerators to advance this field now approach the limits that the field can support. Accelerator science must develop new more cost effective approaches for continued advances in accelerator-based particle physics.

This report provides many examples of the need for accelerator R&D. The luminosity upgrade of the LHC, for example, will need R&D on superconducting quadrupole magnets to provide tighter focusing of the interacting proton beams. Advances in magnet technology could lead to a higher-energy hadron collider. A muon collider will require development of many technologies. R&D on superconducting rf technologies has broad applicability not only for the ILC but also for a high-intensity proton source and for fields of science beyond particle physics. Advances in normal-conducting rf linacs could provide another path to a lepton collider, as well as providing compact synchrotron light sources. R&D on high-power targets is critical for neutrino sources, muon colliders, neutron sources and positron sources. R&D on muon cooling is critical for both the neutrino factory and muon collider, and the first demonstration experiments are just beginning.

A summary of the applications of accelerator R&D is shown in table below (ref M. Tigner).

	Possible Applications									
R&D Subjects	**n factory**	**Muon collider**	**e^+e^- linear collider**	**Neutron sources**	**Storage ring light source**	**Linac light source nc**	**Linac light source sc**	**Medical, Fusion, Industrial**	**High Intensity Proton Source**	**VLHC + SLHC**
e-cloud	x	x	x	x	x			x	x	x
Ion effects			x		x	x	x	x		
Coherent Synch Rad			x		x	x	x			
Space charge effects	x	x	x	x	x	x	x	x	x	x
Short bunch wakes			x			x	x			
Simulation/computation	x	x	x	x	x	x	x	x	x	x
Theory	x	x	x	x	x	x	x	x	x	x
Instrumentation	x	x	x	x	x	x	x	x	x	x
High gradient nc (1)		x	x			x				
High gradient sc (2)	x	x	x	x			x		x	x
Stochastic cooling (3)		x								x
Ionization cooling	x	x								
HOM damping			x	x	x		x		x	
Ultra low emittance		x	x		x	x	x			
Ultra bright sources (4)			x			x	x			
High power targets	x	x	x	x					x	
Magnets sc (5)	x	x	x						x	x
Fixed Field Alt Grad	x	x						x		
Laser and plasma acc			x			x		x		
Test Facilities	x	x	x			x	x			

1. Cavities and amplifiers—improved capabilities and efficiency
2. cavities, processes and materials
3. microwave and optical
4. photosources—ncrf, scrf and dc
5. magnet structures, processes and materials

While the future of particle physics requires a broad accelerator R&D program, a focused R&D program on paths to a future lepton collider is critical at this time. The ILC's superconducting design is the most advanced lepton collider design, the only one that could begin construction in the next decade. Completion of the ILC design requires support for R&D on high-gradient superconducting cavities. Besides ILC, other lepton collider options with the potential for greater energy reach and reduced cost need to be developed. In particular, R&D should proceed on normal-conducting linear collider options including the two-beam accelerator concept for the CLIC design, as well as on klystron-based rf power sources that may provide a more conservative approach to a normal-conducting linear collider. Additional R&D is also needed on longer-term concepts including the muon collider and laser- and plasma-based linear colliders. Each has potential for greater energy reach and significant cost savings, but all still require feasibility demonstrations.

Finally, the field must address the critical lack of accelerator test facilities that R&D requires. Historically, much accelerator R&D took place using operating high-energy physics facilities. However, as the operating facilities have turned off in the US, the opportunities for experimental demonstrations have correspondingly decreased. Some dedicated facilities have been proposed; and, while such facilities tend to be expensive to operate, they are essential for progress.

In summary, advances in accelerator R&D are critical for the United States to maintain leadership not only at the energy frontier of particle physics, but also in the wider context of applications of particle accelerators and detectors reaching across science and society.

Recommendations

> **The panel recommends a broad strategic program in accelerator R&D, including work on ILC technologies, superconducting rf, high-gradient normal-conducting accelerators, neutrino factories and muon colliders, plasma and laser acceleration, and other enabling technologies, along with support of basic accelerator science.**
>
> **The panel recommends creation of a HEPAP subpanel to develop a strategic plan for accelerator R&D. This panel should be followed by an advisory group to monitor the progress and effectiveness of this program.**

5.2 ADVANCED DETECTOR R&D

Generic detector R&D over many years has made possible the large-scale detectors that scientists are currently commissioning at the Large Hadron Collider to discover the physics of the Terascale. Detector development has played a critical role in the leadership of US physicists in these high-priority, energy-frontier experiments. Beyond particle physics, applications of detector development have provided transforming benefits for medicine, industry and national security.

In the near term the proposed upgrade of the LHC luminosity presents challenges. The radiation hardness of the present detector systems, especially the inner tracking systems, must improve in order to survive the intense radiation environment of higher-luminosity operations. The track density will increase because of overlapping events; radiation-hard pixel devices must replace silicon-strip detectors. Increased luminosity will require upgrades to triggering strategies. Most of the needed R&D is part of the mandate of the detector upgrade described in the LHC section. However, as in the past, important ideas leading to significant improvements in detector capability come from generic detector R&D carried out in universities.

Lepton colliders present a different set of challenges from the LHC and require an ongoing generic detector R&D program. For US physicists to participate as leaders in planning this scientific program, they need support to develop the detectors of the future. Again, because many of the creative ideas have come from the university community, continued progress requires a balanced approach between support of the university-based programs and those at the laboratories. Some developments, in the areas of solid state detectors and calorimeter development, for example, will require the significant infrastructure resources of the laboratories. In tight budget times, support for this infrastructure is always at risk.

Detector R&D also has the potential to improve sensitivity to rare processes such as double beta decay, neutrino interactions, dark matter interactions, and proton decay, where large detector volumes are necessary to achieve sufficient signal rate. As GLAST, JDEM, and LSST will demonstrate, detector R&D enables new measurements at the Cosmic Frontier.

Finally, only the laboratories can provide access to test beams to study the detector technologies developed at universities and other laboratories. Again, in tight budget times, these test-beam facilities are at risk and need support.

The enhanced performance of an upgraded LHC and eventually a lepton collider, as well as other scientific opportunities, pose great challenges and promise exciting opportunities for the detector community. Ongoing support for generic detector R&D will be essential to the scientific success of these new facilities. A program of detector R&D on technologies strategically chosen to enable future experiments to advance the field is also an essential part of the program.

Recommendation

> **The panel recommends support for a program of detector R&D on technologies strategically chosen to enable future experiments to advance the field, as an essential part of the program.**

Chapter 6
Laboratories and Universities

6 LABORATORIES AND UNIVERSITIES

6.1 FERMILAB

Fermilab has been a world-leading center for high-energy physics for over 35 years. From the day it commenced operation, it has operated the highest-energy accelerator in the world. Fifteen years later, with the commissioning of the Tevatron collider, the laboratory produced the highest-energy particle collisions, a distinction it has held to this day. The long list of major scientific findings from Fermilab experiments includes the early studies of quark scattering using hadron, muon, and neutrino beams; the discovery of the bottom quark; precision studies of matter-antimatter asymmetry; precision tests of the Standard Model; the observation of the tau neutrino; the most sensitive searches for physics beyond the Standard Model; and of course the discovery of the top quark. In addition to this legacy of US accelerator-driven physics, the laboratory research portfolio today includes a leadership role in the US program at CERN's Large Hadron Collider and a rich program at the Cosmic Frontier.

With the energy frontier moving to the Large Hadron Collider in the coming months and the completion of the Tevatron collider program in the next few years, Fermilab's science program will change in a major way. A near-term focus will be its neutrino experiments. These experiments will take steps toward understanding the basic properties of neutrinos: the ordering of their masses, how they morph from one type of neutrino to another, and whether they violate matter-antimatter symmetry. Neutrinos have a crucial role in addressing one of the central questions of our existence: Why wasn't all of the matter in the early universe annihilated by antimatter, leaving a universe filled with light but without the matter needed to form galaxies, stars, planets and us?

Answers to these questions will require future experiments with greatly increased sensitivity. They will require a very high intensity neutrino beam from Fermilab aimed toward the proposed DUSEL underground laboratory in South Dakota. A new high-power proton source would produce the neutrino beam. Fermilab will be carrying out an R&D program for this intense proton source over the next few years. A very large detector in a DUSEL cavern would have superb sensitivity to neutrino matter-antimatter asymmetry as well as to proton decay and supernova neutrinos. Because of the long distance between Fermilab and DUSEL, this Intensity Frontier program would complement experiments in Japan. Together they could untangle the ambiguities inherent in neutrino measurements.

A high-intensity proton source would also create the opportunity for other important measurements. High-sensitivity searches for muon-to-electron conversion and very rare *K* decays probe physics beyond the Standard Model. Their results could help physicists interpret the new physics from the LHC experiments.

On a longer timescale, the proton source would provide other unique opportunities. If the third neutrino mixing angle is very small, the study of matter-antimatter asymmetry would require a neutrino factory, a facility that provides a very high-intensity neutrino beam with no contamination with other flavors of neutrinos. The new Fermilab proton source could create an intense beam of muons, which in turn would produce the neutrino beam.

Key to the success of a neutrino factory is muon cooling, a technology that makes the beam nearly monodirectional and monochromatic and thus able to be captured in a storage ring. Muon cooling is also an essential technology for a muon collider, a promising means of building a multi-TeV lepton collider. A robust accelerator-driven research program at Fermilab would position the US to host an onshore lepton collider or to participate actively in an offshore collider, regardless of the technology.

Accelerator R&D has been a central activity throughout Fermilab's history. From the design of a low-cost mass-produced dipole magnet for the original 400-GeV accelerator to innovation in superconducting magnets for the Tevatron, to high-intensity targeting for both antiproton and neutrino production, to techniques for cooling high-intensity antiproton beams, Fermilab's R&D has enabled the major improvements to the lab's accelerator complex. Fermilab also continues to carry out important R&D for future accelerators. In the past this included high-field magnet design for very high-energy hadron colliders. At present, R&D is going forward on superconducting rf, important to both a high-intensity proton source and to the ILC. The accelerator expertise at Fermilab will be important broadly for accelerator science and crucial for a US return to the Energy Frontier in the future.

6.2 NATIONAL LABORATORIES

As described in Section 6.1, most of the physical infrastructure for the national particle physics program will be located at Fermilab. However, many of the nation's accelerator and detector core competencies reside in other national laboratories that support the broad science portfolio of the DOE Office of Science and the NSF Mathematics and Physical Sciences Directorate. Examples include superconducting cavity technology at Cornell, Jefferson Lab, and Argonne; electron linacs and rf systems at SLAC; superconducting magnets at Brookhaven, Fermilab, and Lawrence Berkeley Laboratory; and pulsed power systems at Los Alamos National Laboratory and Lawrence Livermore National Laboratory. Many experimental accelerator R&D facilities necessary for advances in accelerator physics have been orphaned or closed with the closing of the operating high-energy physics accelerators. These core competencies must be maintained for the long-term health of US science.

Support for these core competencies primarily comes from DOE and NSF, but the funding comes from several different departments within these agencies. At present, there is no explicit coordination among the different departments to ensure continuity of these national resources. Improved coordination among the departments and agencies is critical for the long-term health of US particle physics as well as for the overall national scientific mission.

6.3 UNIVERSITIES

The excellence of the US particle physics program rests the strong partnership between the national laboratories and the universities, combining human resources and facilities to advance science. Current examples include collaboration on LHC and ILC programs as well as collaborative work toward developing the US program in astrophysics, cosmology and neutrinos. This partnership forms the backbone of the success of the US high energy physics program with an interdependence that thrives with the vitality of the individual partners.

The university role in the partnership with the laboratories is essential to the success of the joint scientific mission. University physicists provide scientific leadership; most particle physics experiments and many innovative techniques are initially conceived at universities. Today, university R&D efforts drive the future of the field. University researchers are at the forefront of many recent initiatives in non-accelerator physics, including searches for dark matter and neutrinoless double beta decay. University scientists are leaders in experiments for the detection of high energy cosmic rays, photons and neutrinos. Universities' combined mission of research and education rewards creativity and innovation and promotes a diverse program of science and scientific techniques. With the center of gravity of accelerator-based high energy physics research moving abroad, university physicists play an increasingly important leadership role.

Universities provide frequent opportunities for cross-disciplinary interactions with colleagues in other fields: condensed matter physics, electrical engineering, mathematics, computer science, astronomy, biology and sociology. Collaboration with experts in these fields leads to new directions for both theory and experiment. Theorists in elementary particle physics, for example, have often drawn on work in condensed matter theory, and vice versa. The development of the highly sensitive detectors critical for modern research relies strongly on work in low-temperature physics, materials science and condensed matter physics. Joint seminars, informal discussions and other encounters engender innovative ideas. In addition, universities bring resources to the national program through their financial support of personnel, equipment and infrastructure.

Universities provide the principal training ground for physics students and postdocs, whose training produces scientists with talents that advance the field and, more broadly, drive the innovation economy. Universities are the portal inviting entrance to groups underrepresented in the sciences. Investing in strong experimental and theoretical programs at universities increases our nation's technological strength, global competitiveness and scientific diversity.

Universities will lead US analysis of LHC data; carry out theoretical calculations necessary to interpret it; lead the design of the Super LHC upgrade and future lepton collider detectors; and contribute to accelerator technology. They will conceive and design new experiments in astrophysics, cosmology, flavor physics and neutrino research. Whatever the eventual site of the next collider, strong US university leadership at this frontier facility is vital to continued US leadership in worldwide particle physics.

The nation's universities are home to 80 percent of US researchers in particle physics. They bring dynamism, leadership, innovation and creativity. They attract excellent graduate and undergraduate students eager to pursue new ideas and tackle challenging problems with novel or ambitious approaches. They attract superb faculty members, hired and promoted on the basis of their drive, entrepreneurialism and accomplishment. They are diverse in terms of their facilities, their areas of strength, their geographical regions and the aspirations of their students.

However, budgetary declines have eroded the vital role of university particle physics groups over the decades. To realize their potential, university groups require an appropriate environment and resources. A faculty member with a new idea for a detector technology must have the flexibility to hire a graduate student, or redirect a postdoc to the task, to bring the technology to fruition. The erosion of technical infrastructure at the universities over the past two decades has restricted realization of the full potential of these talented and creative components of the field. Finally, US participation in the LHC and other overseas experiments creates additional burdens and expanded responsibilities, high travel and subsistence costs, and investments in remote conferencing, data networking and computing.

The panel recognizes the importance of the university program to the national particle physics effort. Stewardship of this program will have enormous benefits for the field, for the laboratories and for our society as a whole. Students, drawn by the exciting research at universities, will go on to become the scientists of tomorrow.

Without proper attention directed to the health of universities during the current period of transition in the field, the entire national particle physics research program will suffer. The P5 panel endorses the recommendation of the HEPAP subpanel on the Universities Grants Program, the UGPS, for a redirection of a modest portion of the HEP budget to the support of the research program in the universities—on the order of one percent of the overall budget for particle physics.

The UGPS examined the priorities for the use of funds redirected to the university grants program, and recommended support for three high-priority areas. The first is support for university participation in the LHC experiments and their upgrades. This priority includes sustaining participating university groups, supporting theory students and postdocs engaged in phenomenological calculations of LHC physics, and investing in university infrastructure in support of the LHC experiments and their upgrades. A second priority is to support R&D on the ILC. This includes sustaining university groups working on ILC R&D and investing in university infrastructure in support of the ILC detectors and accelerator. Finally, a high priority is to support experiments in astrophysics, cosmology and neutrinos, together with a variety of smaller-scale experiments. This includes sustaining university groups working on these programs, investing in university infrastructure to support R&D and construction of these experiments, and supporting theory students and postdocs engaged in phenomenological calculations for these programs.

The UGPS recommended that the agencies establish an advisory structure to provide feedback on the health of the university program, and creating a clear path for submission and feedback for small and mid-size experiments. The recently announced reorganization in OES/HEP makes these steps more important, in order for generic trends in the university programs not be lost because of administrative focus on the subdisciplines and research modalities.

The years ahead may be the most exciting ever in particle physics. Unprecedented opportunities exist at the Terascale and in astrophysics, cosmology and neutrino research. Renewed investment in the universities will enable the US to realize the current promise and foster future US leadership in science and technology.

Recommendation

The panel recommends preserving the funding for the university program even under the lowest funding scenario, and increasing it by close to 10 percent, as recommended by the HEPAP subpanel on the university program, at the more favorable funding scenarios.

Chapter 7

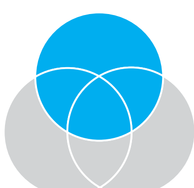
Summary of Recommendations

7 SUMMARY OF RECOMMENDATIONS

Overall recommendation

> **The panel recommends that the US maintain a leadership role in world-wide particle physics.**
>
> **The panel recommends a strong, integrated research program for US particle physics at three frontiers: the Energy Frontier, using both hadron colliders and lepton colliders to discover and illuminate the physics of the Terascale; the Intensity Frontier, comprising neutrino physics and high-sensitivity experiments on rare processes that will tell us about new physics beyond the Standard Model; and the Cosmic Frontier, probing the nature of dark matter and dark energy and other topics in particle astrophysics.**

7.1 THE ENERGY FRONTIER



Accelerators and experiments at the Energy Frontier are expected to make major discoveries leading to an ultimate understanding of the theory of particles and their interactions. They will address key questions about the physical nature of the universe: the origin of particle masses, the existence of new symmetries of nature, extra dimensions of space, and the nature of dark matter.

7.1.1 THE TEVATRON COLLIDER

The Tevatron at Fermilab is currently the highest-energy collider in the world.

> **The panel recommends continuing support for the Tevatron Collider program for the next one to two years, to exploit its potential for discoveries.**

7.1.2 THE LARGE HADRON COLLIDER

In the near future, the Large Hadron Collider at CERN in Geneva, Switzerland will achieve the highest collision energies. The LHC is an international project with significant US investment and major US involvement: Americans constitute the largest group of LHC scientists from any single nation. Significant US participation in the full exploitation of the LHC has the highest priority in the US particle physics program.

> **The panel recommends support for the US LHC program, including US involvement in the planned detector and accelerator upgrades, under any of the funding scenarios considered by the panel.**

7.1.3 LEPTON COLLIDERS

The international particle physics community has reached consensus on the importance of a lepton collider for a full understanding of the physics of the Terascale. The panel reiterates the importance of such a Terascale lepton collider.

In the next few years, results from the LHC will set the required energy for a lepton collider. If the optimum initial energy proves to be at or below approximately 500 GeV, then the ILC as designed by the Global Design Effort is the most mature and ready-to-build option, with a construction start possible in the next decade. A requirement for energy higher much than the ILC's will mean considering other collider technologies. The cost and scale of a lepton collider mean that it would be an international project, with the cost shared by many nations. International negotiations will determine the siting; the host will be assured of scientific leadership at the energy frontier.

Whatever the technology of the future lepton collider, and wherever it may be located, the US should plan to play a major role. For the next few years, the US should continue to participate in the international R&D program for the ILC. This R&D will position the US for an important role for the US should the ILC be the choice of the international community.

> **The panel recommends for the near future a broad accelerator R&D program for lepton colliders that includes continued R&D on ILC at roughly the proposed FY2009 level in support of the international effort. This will allow a significant role for the US in the ILC wherever it is built. The panel also recommends R&D for alternative accelerator technologies, to permit an informed choice when the lepton collider energy is established.**

> **The panel also recommends an R&D program for detector technologies to support a major US role in preparing for physics at a lepton collider.**

7.2 THE INTENSITY FRONTIER

At the Intensity Frontier, precision measurements of the properties of leptons and quarks can lead the way to resolving some of the universe's deepest mysteries.

7.2.1 THE ACCELERATOR-BASED NEUTRINO PROGRAM

Measurements of the mass and other properties of neutrinos are fundamental to understanding physics beyond the Standard Model and have profound consequences for understanding the evolution of the universe. The US can build on the unique capabilities and infrastructure at Fermilab, together with the proposed DUSEL, the Deep Underground Science and Engineering Laboratory proposed for the Homestake Mine, to develop a world-leading program in neutrino science. Such a program will require a multi-megawatt proton source at Fermilab.

> **The panel recommends a world-class neutrino program as a core component of the US program, with the long-term vision of a large detector in the proposed DUSEL laboratory and a high-intensity neutrino source at Fermilab.**

A neutrino program with a multi-megawatt proton source would be a stepping stone toward a future neutrino source, such as a neutrino factory based on a muon storage ring, if the science eventually requires a more powerful neutrino source. This in turn could position the US program to develop a muon collider as a long-term means to return to the energy frontier in the US.

The panel recommends proceeding now with an R&D program to design a multi-megawatt proton source at Fermilab and a neutrino beamline to DUSEL and recommends carrying out R&D on the technology for a large multi-purpose neutrino and proton decay detector.

Construction of these facilities could start within the period considered by this report.

The panel further recommends that in any funding scenario considered by the panel Fermilab proceed with the upgrade of the present proton source by about a factor of two, to 700 kilowatts to allow for an extended physics reach using the current beamline and a timely start for the neutrino program in the Homestake mine.

When they become available by about 2012, the results of θ_{13} measurements and the results of accelerator and detector R&D efforts should be used to optimize the design of the long-baseline neutrino physics program. At that point construction of the beamline and the first stage of a detector should proceed as rapidly as possible. If the decision is made to proceed with the multi-megawatt proton source, construction should start as soon as possible after the completion of the R&D program under all but the lowest funding scenarios. The lowest funding scenario would delay the construction start of a multi-megawatt proton source.

The panel recommends support for R&D on the technology for a large detector at DUSEL. The nature of such a large detector is not yet clear. The two contending technologies are water Cerenkov and liquid argon. Large-scale water Cerenkov detectors are a mature technology, although at a smaller scale than is envisioned for DUSEL.

The panel recommends support for a vigorous R&D program on liquid argon detectors and water Cerenkov detectors in any funding scenario considered by the panel. The panel recommends designing the detector in a fashion that allows an evolving capability to measure neutrino oscillations and to search for proton decays and supernovae neutrinos.

The panel realizes that such an ambitious neutrino program must proceed in stages. The NOvA experiment has received approval by previous committees, has undergone detailed design and multiple reviews, and is ready for construction.

In all but the lowest funding scenario, the panel recommends a rapid NOvA construction start. However, the lowest funding scenario would further delay the experiment's construction start, and the costs of NOvA construction and operation would displace other programs of higher priority. The panel therefore recommends that Fermilab not proceed with the NOvA experiment under the lowest funding scenario.

7.2.2 NONACCELERATOR NEUTRINO EXPERIMENTS

The reactor experiments, Double Chooz and Daya Bay, are designed to carry out measurements of the mixing angle θ_{13}, an important physics parameter. The panel recommends support for these experiments under any of the funding scenarios considered by the panel.

Nonaccelerator experiments searching for neutrinoless double beta decay have the potential to make discoveries of major importance about the fundamental nature of neutrinos. The panel recommends support for these experiments, in coordination with other agencies, under any funding scenario considered by the panel.

7.2.3 HIGH-SENSITIVITY MEASUREMENTS

The latest developments in accelerator and detector technology make possible promising new scientific opportunities through measurement of rare processes. Incisive experiments, complementary to experiments at the LHC, would probe the Terascale and possibly much higher energies. Among them are measurements that can be performed at Fermilab—muon-to-electron conversion and rare *K* decays—as well as participation in overseas *B* factories.

> **The panel recommends pursuing the muon-to-electron conversion experiment, subject to approval by the Fermilab PAC, under all budget scenarios considered by the panel. The intermediate budget scenario, scenario B, would allow pursuing significant participation in one overseas next-generation *B* factory. The more favorable funding scenario, scenario C, would allow for pursuing a program in rare *K* decay experiments.**

If the US particle physics program participates in an overseas g-2 experiment, the US could make a considerable in-kind contribution in the form of the muon storage ring now at Brookhaven. The additional expense would be relatively small, allowing pursuit of this opportunity as part of the university program.

7.2.4 THE DUSEL FACILITY

The physics program of the Deep Underground Science and Engineering Laboratory is of central importance to particle physics. Experiments at DUSEL would address many issues, including neutrino physics, proton decay, dark matter, and neutrinoless double beta decay. DOE and NSF should define clearly the stewardship responsibilities for such an experimental program.

> **The panel endorses the importance of a deep underground laboratory to particle physics and urges NSF to make this facility a reality as rapidly as possible. Furthermore the panel recommends that DOE and NSF work together to realize the experimental particle physics program at DUSEL.**

7.3 THE COSMIC FRONTIER

Although ninety five percent of the universe appears to consist of dark matter and dark energy, we know little about them. The quest to elucidate the nature of dark matter and dark energy is at the heart of particle physics—the study of the basic constituents of nature, their properties and interactions. The US is presently a leader in the exploration of the Cosmic Frontier. The field has identified compelling opportunities for dark matter search experiments, and for both ground-based and space-based dark energy investigations.

> **The panel recommends support for the study of dark matter and dark energy as an integral part of the US particle physics program.**

7.3.1 DARK-MATTER SEARCH EXPERIMENTS

The observation of dark matter particles from the galaxy would be an epochal discovery. In consonance with experiments at the LHC, dark matter detection experiments could help unravel the identity and properties of dark matter.

The panel recommends that NSF and DOE jointly support direct dark matter detection experiments under any of the funding scenarios considered by the panel. The choice of which of these experiments to support in the longer term should be made after completion of the ongoing experiments and the R&D on the next generation of detectors.

7.3.2 EXPERIMENTS TO STUDY THE NATURE OF DARK ENERGY

The cause of the accelerated expansion of the universe is a mystery. It could signal the existence of a new form of energy, dark energy, or a breakdown of Einstein general relativity. For the near term, the panel reiterates the recommendation by the previous P5 panel for a construction start of the Dark Energy Survey. The panel recommends consideration of other selected ground-based experiments. For the longer term, the space-based Joint Dark Energy Mission and the ground-based Large Synoptic Survey Telescope offer major, complementary advances in probing the nature of dark energy.

The panel recommends that DOE support JDEM, at an appropriate level negotiated with NASA, under all budget scenarios considered by the panel.

The panel recommends DOE support for the ground-based LSST program, in coordination with NSF, in all funding scenarios considered by the panel, at a level that depends on the overall program budget.

7.3.3 HIGH ENERGY PARTICLES FROM SPACE

The study of high-energy particles from space—ultra-high energy cosmic rays, gamma rays, and neutrinos—is a vibrant, rapidly developing area of science at the boundary between particle physics and astrophysics. These projects bring important diversity to particle physics, so relatively small investments are a high priority, even in the leanest budget scenarios. Due to extreme budget pressures, very large investments at this time are only possible in the higher budget scenarios. Multiagency, international cooperation is particularly important for the support of this exciting science.

The panel recommends limited R&D funding for these particle astrophysics projects under all budget scenarios considered by the panel, but support for any possible large construction projects should be considered only under funding scenarios C and D.

The panel recommends that the funding agencies establish a Particle Astrophysics Science Advisory Group to advise DOE and NSF on the relative merits of the various proposals anticipated in this area.

7.4 ADVANCED ACCELERATOR AND DETECTOR R&D

Advances in accelerator and detector R&D are critical for the United States to maintain leadership at the Energy, Intensity and Cosmic Frontiers of particle physics; to allow the possibility of hosting a future energy-frontier accelerator in the United States; and to develop applications for the benefit of society.

The panel recommends a broad strategic program in accelerator R&D, including work on ILC technologies, superconducting rf, high-gradient normal-conducting accelerators, neutrino factories and muon colliders, plasma and laser acceleration, and other enabling technologies, along with support of basic accelerator science.

The panel recommends creation of a HEPAP subpanel to develop a strategic plan for accelerator R&D. This panel should be followed by an advisory group to monitor the progress and effectiveness of this program.

The panel recommends support for a program of detector R&D on technologies strategically chosen to enable future experiments to advance the field, as an essential part of the program.

7.5 THE UNIVERSITY PROGRAM

The US particle physics program is built on a strong partnership between the national laboratories and universities that combines human resources and facilities to advance the science, with a high level of interdependence. The universities bring fresh ideas to the field, provide a crucial component in designing and realizing experiments and their subsequent data analysis, and train graduate students. Graduate studies train scientists for the field of particle physics as well as for a variety of professions that are key to future American competitiveness.

The panel recommends preserving the funding for the university program even under the lowest funding scenario, and increasing it by close to 10 percent, as recommended by the HEPAP subpanel on the University program, at the more favorable funding scenarios.

Chapter 8
Scientific Opportunities in Various Budget Scenarios

8 SCIENTIFIC OPPORTUNITIES IN VARIOUS BUDGET SCENARIOS

The panel considered the scientific opportunities for the US research program in particle physics for the next decade in the context of several different DOE funding scenarios:

A. Constant level of effort at the FY2008 funding level
B. Constant level of effort at the FY2007 funding level
C. Doubling of budget over ten years starting in FY2007
D. Additional funding above the previous level, associated with specific activities needed to mount a leadership program.

The FY2007 DOE funding level was $752M; the FY2008 level was $688M. The panel received guidance on the status of NSF's planning for DUSEL, including construction, operations and maintenance of the infrastructure; and the initial suite of experiments. The current plan submits the DUSEL proposal including the initial suite of experiments to the National Science Board in 2010. If the proposal is successful, first construction funds could be available sometime in FY2012. The P5 plan described here assumes approval of the DUSEL MREFC proposal around a 2010 time frame and sufficient growth of the NSF base budget to support DUSEL operations.

The panel began with a consideration of scenario B, then went on to consider the other scenarios. Here we present an overview of the programs' highlights based on the detailed recommendations presented in Section 7, Summary of Recommendations.

8.1 BUDGET SCENARIO B: THE FY 2007 CONSTANT-EFFORT SCENARIO

In this funding scenario the panel recommends a program that would allow major advances at all three frontiers of elementary particle physics over the next 10 years.

8.1.1 THE ENERGY FRONTIER

Experimental and theoretical results of the past several decades strongly point to discoveries—the origin of particle mass, new laws of nature, extra dimensions of space, and the nature of dark matter—at collision energies not far above those of current accelerators. Experiments at the energy frontier will be able to directly produce and study the particle messengers of these phenomena.

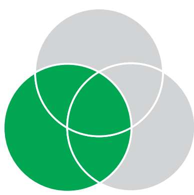
The Fermilab Tevatron is the current energy-frontier accelerator. Its experiments, CDF and DZero, would continue data-taking through FY2009.

Starting in late 2008, the experiments at the Large Hadron Collider will begin collecting data at a much higher energy and interaction rate than that of the Tevatron. The general-purpose LHC experiments ATLAS and CMS will continue operating throughout the next decade and beyond, with greatly increased sensitivity to the new phenomena.

During the next decade, upgrades to the LHC accelerator complex, SLHC, will increase the interaction rate by as much as an order of magnitude, and upgrades to the LHC detectors will allow them to exploit this increase. The US would participate in both the detector and accelerator upgrades.

A deep understanding of the nature of the new physics will require a lepton collider to complement the LHC. For energies at or below 500 GeV, the International Linear Collider is the most mature option. A much higher energy would require different technologies. Scenario B would allow a program of vigorous accelerator R&D for lepton collider technologies.

8.1.2 THE INTENSITY FRONTIER



Budget scenario B would permit a strong program in neutrino physics and a limited program in high-sensitivity measurements. In this budget scenario, significant progress in neutrino physics at accelerators and in nonaccelerator experiments would not only advance the understanding of physics beyond the Standard Model but would also shed light on the evolution of the universe.

At Fermilab, the completion of the MINOS experiment would provide the most precise measurement of the mass difference between two of the neutrino species and provide an early look at the mixing angle θ_{13}. The NOvA experiment would be constructed and take data, obtaining a first look at the ordering of the masses of the neutrinos. Construction of the MINERvA experiment would permit complete measurements of important neutrino-interaction cross sections.

At reactors, the construction and operation of the Double Chooz and Daya Bay neutrino oscillation experiments would be complete. They will measure or further constrain the mixing parameter $\sin^2 2\theta_{13}$ by an order of magnitude better than current experiments.

Neutrinoless double beta decay experiments deep underground would shed light on the fundamental nature of neutrinos.

Scenario B would permit construction of the initial experiments and R&D for next-generation detectors at DUSEL and construction of at least one very large detector. These experiments could reach a level of sensitivity that might permit detection of the signal for matter-antimatter asymmetry in neutrinos.

In this 10-year period, the DUSEL facility could be completed. Construction of the first stage of the long-baseline neutrino oscillation experiment might be near completion, including the neutrino beamline from Fermilab to DUSEL and the first stage of a large detector. The upgrade of the Fermilab proton source by about a factor of two, to 700 kilowatts, would be complete. This would lead to the most precise measurement of the mixing angle θ_{13}, measurement of the neutrino mass ordering, and perhaps a first look at CP violation in the lepton sector, depending on the basic neutrino parameters.

The R&D on accelerator aspects of the multi-megawatt proton source at Fermilab would be complete, and construction of the source itself would be well under way. This increased intensity would allow the long-baseline neutrino experiment to carry out the world's most sensitive search for CP violation.

In Budget Scenario B, the high-sensitivity program would be limited to two components: a muon-to-electron conversion experiment at Fermilab and significant participation in one overseas next-generation *B* Factory. The discovery of charged lepton number nonconservation would clearly signal new physics beyond the Standard Model. The proposed muon-to-electron conversion experiment would increase sensitivity by four orders of magnitude over current experiments even in advance of a new high-intensity proton source. A next-generation *B* factory with its broad program of measurements would be sensitive to rare processes that complement measurements at the LHC in elucidating the physics of the Terascale.

8.1.3 THE COSMIC FRONTIER

Budget Scenario B would permit great progress at the Cosmic Frontier over the coming decade.

Complementary ground-based and space-based experiments would provide a major advance in understanding how dark energy contributes to the evolution of the universe. Large advances in capabilities would greatly reduce the statistical and systematic errors on the measurements of cosmological parameters, putting dark energy models to direct test, as well as probing for possible violations of general relativity. On the ground, the Dark Energy Survey would be complete, and the Large Synoptic Survey Telescope could be nearing completion. Both projects would be jointly funded by NSF and DOE. The space-based Joint Dark Energy Mission would be constructed and launched in partnership with NASA.

Order-of-magnitude improvements in sensitivity at a variety of complementary facilities could provide the first positive, direct detection of the dark matter that surrounds us. At the same time, R&D would be complete on a group of new technologies permitting the construction start of a very-large-scale underground detector of the chosen technology, improving sensitivity by an order of magnitude. These results connect directly with results from the LHC and from operating gamma-ray and other observatories.

The study of high-energy particles from space uses the universe as a particle physics laboratory. The current facilities would operate to exploit their full potential. A few small new experiments could be considered for construction. A limited level of targeted R&D would enable choices for future investments in this rapidly developing field.

8.2 BUDGET SCENARIO A: THE FY2008 CONSTANT-EFFORT SCENARIO

Under the scenario of constant level of effort at the FY2008 level, Scenario A, the US program would be much more limited. In this case, the US could not afford leadership at all three frontiers. At the Energy Frontier, the LHC program could be supported, including the investments in the second-generation program at the LHC, so that US scientists could participate in the exploitation of this scientific opportunity. A limited level of R&D on a future lepton collider could go forward. At the Intensity Frontier, the MINOS, Double Chooz and Daya Bay experiments would be complete, producing important advances in the understanding of the properties of neutrinos, but the NOvA experiment at Fermilab could not take place. The MINERvA experiment could not run beyond FY2010, due to the high cost of operating the Fermilab accelerator complex. A possible double beta decay experiment would have to be delayed. Scenario A would permit investments for upgrading the Fermilab proton source to 700kW, and for carrying out R&D on a multi-megawatt proton source for high intensity neutrino beams. The construction of the multi-megawatt proton source, the neutrino beamline to DUSEL, and the large detector at DUSEL, however, would be delayed. Scenario A would thus delay the development of a world-leadership neutrino program by as much as five years. There would be no facility operation or neutrino program at Fermilab for the five to six years required to develop these accelerator and detector capabilities. At the Cosmic Frontier, Scenario A could support DES and JDEM, producing important results shedding light on the nature of dark energy, but it is not clear that funding would be available for supporting LSST. There would be an order-of-magnitude increase in the sensitivity for particles that make up dark matter, but the large next-generation dark matter search experiment would have to be delayed.

In Scenario A, the US would play a leadership role through LHC experiments at the Energy Frontier, would continue to play a major role at the Cosmic Frontier, but would be significantly weakened at the Intensity Frontier. The investments in future facilities at Fermilab and DUSEL that would give the US a world leadership role in research at the Intensity Frontier would slow down significantly, with only the muon-to-electron conversion experiment under way during this time at Fermilab. These delays would mean that the physics capabilities of the Fermilab accelerator complex would lie unused for years. Only a fraction of the potential scientific productivity of Fermilab would be realized. Scenario A would permit investment in accelerator R&D to enable the US to remain at the forefront of accelerator technologies, but the program would be more limited than in the other funding scenarios.

At this funding level the program could not support the present scientific workforce. The field would have to contract significantly more than it already has, reducing the number of graduate students and negatively affecting productivity.

Overall, while this funding level could deliver significant science, there would be outstanding scientific opportunities that could not be pursued. It would sharply diminish the US capability in particle physics from its present leadership role.

8.3 BUDGET SCENARIO C: THE FY2007 LEVEL INCREASING AT 6.5 PERCENT PER YEAR

Budget Scenario C would support a world-class program of scientific discovery at all three frontiers in the decade ahead. It would provide strong support for the development of future research capabilities and of the scientific work force. Programs could move forward at a more efficient pace, with reduced costs, more timely physics results and increased scientific impact.

At the Energy Frontier, this budget scenario would extend the discovery potential of the Fermilab Tevatron Collider by supporting operation in FY2010. Budget scenario C would provide robust funding for exploitation of the LHC physics potential. It would increase operations funding for US groups working in Europe on the LHC and provide the needed personnel support at both universities and national laboratories for LHC detector and machine upgrades.

Progress toward a future lepton collider is a very high priority of the field worldwide. Should results from the LHC show that the ILC is the lepton collider of choice, funding in this scenario would support R&D and enable the start of construction of an ILC abroad. If LHC results point to another lepton collider technology, its R&D would advance. Increased funding for muon collider R&D would lead to an earlier feasibility determination for a neutrino factory and perhaps a muon collider.

Scenario C would significantly advance the exploration of physics at the Intensity Frontier. Construction of a new high-intensity proton source at Fermilab, which would support both neutrino physics and precision searches for rare decays, would be complete. Scenario C would enable an earlier construction start than would Scenario B and would shorten the construction time. It would also advance the design and construction of a beamline to DUSEL and would reduce the overall cost and risk of both these projects. Efforts to develop the technology for large-scale liquid argon or water Cerenkov detectors for neutrino physics and proton decay would benefit greatly from increased funding, leading to an earlier construction start, shorter construction period and reduced risk for a large underground detector at DUSEL. Scenario C would enable the high-sensitivity neutrino experiment to operate during the decade, providing great sensitivity to matter-antimatter asymmetry in neutrinos. Scenario C would also enable new rare *K*-decay experiments highly sensitive to new physics.

At the Cosmic Frontier, Scenario C would advance the exploration of dark energy by enabling the timely completion of the two most sensitive detectors of dark energy, the JDEM space mission and the ground-based LSST telescope. Scenario C enables strategic, large-scale investments in exciting projects at the boundary between particle physics and astrophysics, the study of high-energy particles from space. Without these investments, the US will likely lose leadership in this rapidly developing area.

Budget scenario C would provide needed additional funds to advance accelerator R&D and technology goals. These goals go well beyond preparation for possible participation in ILC. Accelerator goals for the field include advancing the development of key enabling technologies such as superconducting rf technology, high-field magnet technology, high-gradient warm rf accelerating structures, rf power sources, and advanced accelerator R&D, all of which could greatly benefit from increased funding.

Increased funding in Scenario C would allow a robust detector R&D program in the U.S. to prepare for future experiments at both the energy and intensity frontiers.

Budget Scenario C provides desperately needed resources to rebuild university and laboratory infrastructure that has eroded during lean funding years and would allow retention and hiring of needed laboratory and university technical staff. This budget scenario would provide additional support for university groups, further addressing the pressing needs enunciated in several recent reports, among them the National Academy's "Rising Above the Gathering Storm."

8.4 BUDGET SCENARIO D: ADDITIONAL FUNDING

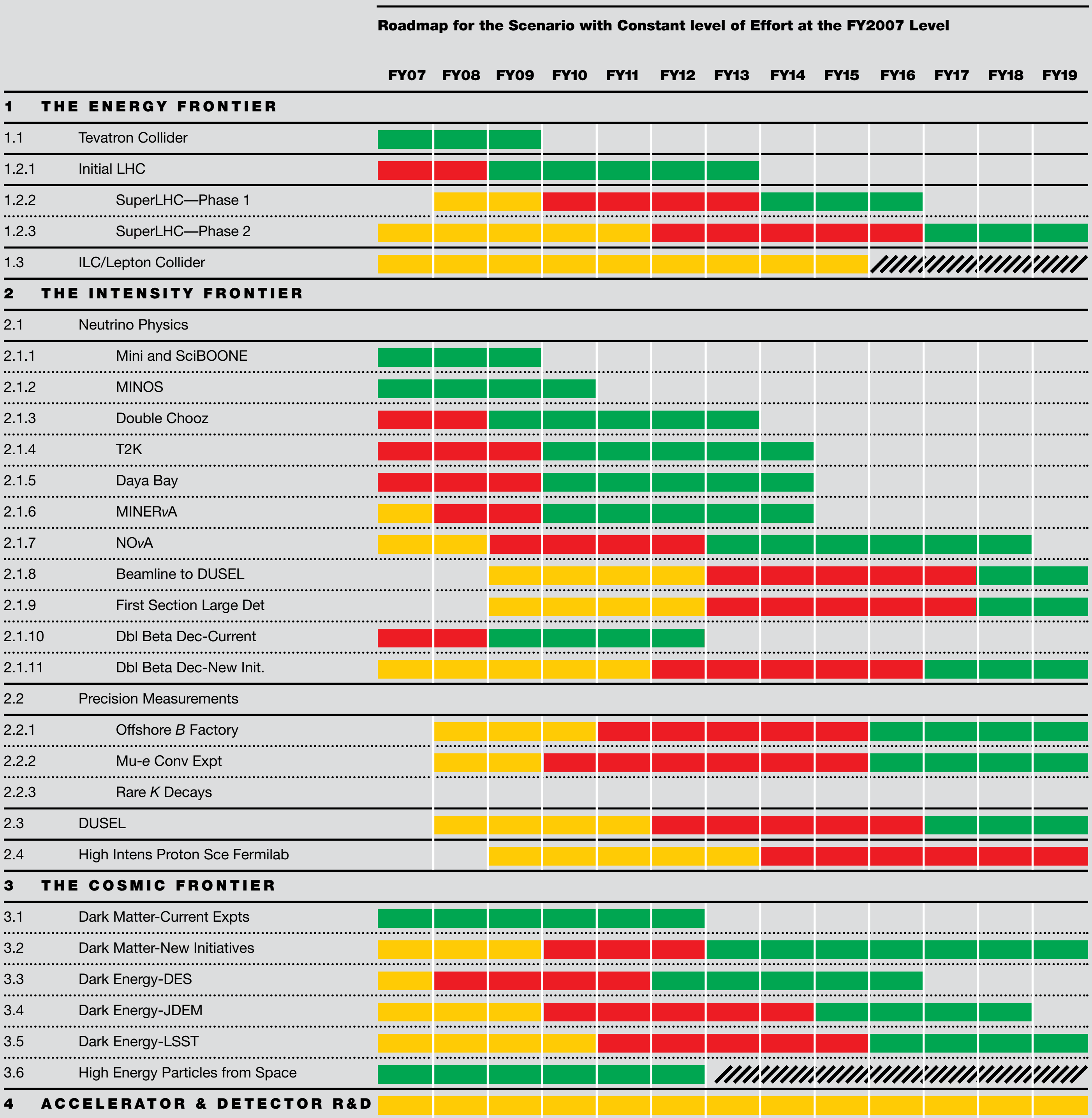
The following scientific opportunities would justify additional funding above the levels discussed:

A lepton collider will be essential for the in-depth understanding of new physics discovered at the LHC, whether it is the source of the masses of the elementary particles, new laws of nature, additional dimensions of space, the creation of dark matter in the laboratory, or something not yet imagined. Negotiations by the international community would determine the siting of the collider. A major role for the US in construction of a lepton collider would require additional funding beyond that available in budget Scenario C.

The study of dark energy is central to the field of particle physics. DOE is currently engaged with NASA in negotiations concerning the space-based Joint Dark Energy Mission. The panel assumed a DOE contribution to JDEM of approximately 200 million dollars, consistent with suggestions of the agencies, in each of the budget scenarios A, B, and C. A significantly larger DOE contribution, such as the 400 million dollars assumed by the NRC's Beyond Einstein Program Assessment Committee when it recommended JDEM as NASA's first Beyond Einstein mission, would justify additional funding above the overall level foreseen in the funding scenarios considered.

8.5 ROADMAP

Figure 1: Roadmap for Scenario B, the constant level of effort at the FY2007 level budget



The approximate timescales for R&D (yellow), construction (red) and operation (green) are indicated. Line 1.3 reflects the uncertain timescale of a lepton collider. If LHC results point to a 500 GeV collider, the international community could select the ILC and decide on a construction start at that time. In this roadmap we indicate a possible construction start for an international project late in the next decade with black shading. If LHC results indicate that a higher-energy lepton collider is required, R&D after 2012 or so will shift to the new technology required for such a collider. On line 3.6, High Energy Particles from Space, the program beyond 2012 will be addressed by the proposed HEPAP subpanel on particle astrophysics; this is indicated by black shading past 2013 on the roadmap.

KEY
R&D
Contruction
Operation

Chapter 9
Appendix

Appendix A
Charge to the Panel

U.S. Department of Energy
and the
National Science Foundation

JAN 23 2008

Professor Mel Shochet
Chair, HEPAP
Enrico Fermi Institute
University of Chicago
Chicago, Illinois 60637

Dear Professor Shochet:

The scientific opportunities for the U.S. high energy physics program have been identified and articulated in a number of reports generated over the last few years by the High Energy Physics Advisory Panel (HEPAP), the National Academy of Sciences (NAS), the American Physical Society (APS) and other scientific bodies. These studies have addressed and evaluated the priority of the important scientific opportunities; however, in most cases this was not done under any constraint on the resources needed. The exception to this was the Particle Physics Project Prioritization Panel (P5) report, whose first part was submitted in October 2006 and second in November 2007. The agencies have found this report to be informative and useful in their planning. However, circumstances have changed over the almost two years since the original charge was given to P5, and additional guidance is now requested by the agencies.

Since DOE/NSF issued the charge to P5 in January 2006, new information has been obtained regarding FY 2007, FY 2008 and projected out-year funding and the status of major projects. Most significant, for the strategic planning for this scientific field, is that the timescale for construction of the ILC has come into better focus in the last year. External factors, including the nature of Terascale physics to be discovered at the LHC and the internationalization required to realize the ILC, now imply an earliest possible construction start near mid-2010's. Accordingly, at the February 2007 HEPAP meeting, Dr. Raymond Orbach requested a renewed discussion of the future of U.S. particle physics during the transition period from the LHC to the ILC. He asked for a critical examination of the investments that would be needed to ensure the vitality, scientific productivity, and discovery potential of the field during the next two to three decades. There is a need at this time to understand the priorities, options, impacts and scientific deliverables for the U.S. program at various funding levels over the next ten year period.

To that end, we request that HEPAP re-examine current and planned U.S. research capabilities and assess their role and potential for scientific advancement, and determine the time and resources (the facilities, personnel, research and development and capital investments) needed to achieve the planned programs, subject to the revised budgetary and external constraints indicated above. HEPAP should then identify and evaluate the scientific opportunities and options that can be pursued at different funding levels for mounting a world-class, vigorous and productive national particle physics science program.

These evaluations should be done in the context of the increasingly necessary internationalization of particle physics while recognizing the need to maintain a healthy, flexible, domestic high energy physics infrastructure. Since Fermilab will be the single dedicated U.S. HEP user facility after 2008, it is important to understand and evaluate the role Fermilab will play in the national and worldwide context of particle physics over the next two decades. In response to Dr. Orbach's inquiry, Fermilab has completed a strategic planning process described in the *Fermilab Steering Group Report* document, the content of which should be evaluated in your planning exercise.

A U.S. strategic plan to implement the highest priority science in the context of available funding and world-wide capabilities and collaborations must be developed. Your report should provide recommendations on the priorities for an optimized high energy physics program over the next ten years (FY 2009-2018), under the following four funding profile scenarios:

- Constant effort at the FY 2008 funding level (i.e.; funding in FY 2009 at the level provided by the FY 2008 Omnibus Bill inflated by 3.5% and thereafter inflated by 3.5% per year in the out-years)
- Constant effort at the FY 2007 funding level (i.e.; funding in FY 2009 at the level provided in FY 2007 inflated 3.5% per year over two years and thereafter inflated by a 3.5% in the out-years).
- Doubling of funding over a ten year period starting in FY 2007 (i.e.; funding in FY 2009 at the level provided in FY 2007 inflated 6.5.% per year over two years and thereafter inflated by 6.5% per year in the out-years)
- Additional funding above the previous level, in priority order, associated with specific activities needed to mount a leadership program that addresses the scientific opportunities identified in the EPP2010 report.

The report should discuss the current facilities and instrumentation that can be used to carry out parts of the planned program as well as new facilities and instrumentation that will need to be developed by the DOE and NSF in order to mount a productive, forefront program for each of the funding scenarios. The report should articulate the scientific opportunities which can and cannot be pursued, the overall level of support that is needed in the core research and advanced technology R&D programs to achieve these opportunities in the various scenarios, and the impacts on training of high energy and accelerator physicists as well as the broader scientific community. The report should also provide a detailed perspective on how the pursuit of possible major initiatives (such as DUSEL, ILC, Project X, etc.) would fit (or not) into the program you recommend in each of the scenarios.

We would appreciate the committee's preliminary comments by March 1, 2008 and a final report by April 15, 2008. We understand this is a difficult task; however, your considerations on these issues will be an essential input to planning at both the DOE and NSF.

Sincerely,

Dr. Dennis Kovar
Acting Associate Director
for High Energy Physics
Office of Science
Department of Energy

Dr. Tony Chan
Assistant Director
Mathematical and Physical Sciences
National Science Foundation

Appendix B Panel Membership

MEMBERSHIP LIST

Particle Physics Project Prioritization Panel (P5)

Charles Baltay, Chair
Yale University

Hiroaki Aihara
University of Tokyo

James Alexander
Cornell University

Daniela Bortoletto
Purdue University

James Brau
University of Oregon

Peter Fisher
Massachusetts Institute of Technology

Josh Frieman
Fermi National Accelerator Laboratory, University of Chicago

Fabiola Gianotti
CERN

Donald Hartill
Cornell University

Andrew Lankford
University of California, Irvine

Joseph Lykken
Fermi National Accelerator Laboratory

William Marciano
Brookhaven National Laboratory

Jay Marx
California Institute of Technology

Steve Ritz
NASA/GFSC, University of Maryland

Tor Raubenheimer
Stanford Linear Accelerator Center

Marjorie Shapiro
Lawrence Berkeley National Laboratory, University of California, Berkeley

Henry Sobel
University of California, Irvine

Robert Tschirhart
Fermi National Accelerator Laboratory

Carlos Wagner
Argonne National Laboratory, University of Chicago

Stanley Wojcicki
Stanford University

Mel Shochet
University of Chicago, Ex-Officio

Appendix C Agendas of the Public Meetings

FERMILAB: JAN 31–FEB 2, 2008

Thursday, Jan 31

9:00–12:00	***Panel Organizational Session (Closed)***	
	Introduction	Mel Shochet
	Charge from DOE	Dennis Kovar
	Charge from NSF	Joe Dehmer
	DOE Budget Guidance	Dennis Kovar
	NSF Budget Guidance	Jim Reidy
	Panel Organization	Charlie Baltay
	Discussion of procedures,issues,schedules,	
12:00–1:00	***Lunch***	
1:00–3:00	***Long Range Plans***	
	Fermilab Plans	Pier Oddone
	Asian Plans	Atsuto Suzuki
3:00–3:30	***Coffee Break***	
3:30–6:30	***Programs with a High Intensity Proton Source***	
	Introduction	Young-Kee Kim
	Physics	Andre deGouvea
	Neutrino Oscillations	Bonnie Fleming
	Mu to e gamma,G-2	Bill Molzon
	Rare *K* decays	Doug Bryman
	Accelerator and R&D	Steve Holmes
	Summary and Remarks	Young-Kee Kim
7:00	***Dinner Hosted by FRA***	

Friday, Feb 1

9:00–12:00	***Linear Collider***	
	Overview	Barry Barish
	Physics and Detectors	John Jaros
	The US ILC Effort	Mike Harrison
	Global ILC Design	Marc Ross
	CLIC and Other Options	Tor Raubenheimer
12:00–1:00	***Lunch***	
1:00–3:00	***Tevatron Run Extension***	
	Machine Prospects	Roger Dixon
	The CDF Experiment	Rob Roser
	The D0 Experiment	Darien Wood
3:00–4:30	***Town Meeting***	
4:30–6:00	***Panel Executive Session***	

Saturday, Feb 2

9:00–12:00	***Panel Executive Session***	

SLAC: FEB 21–23, 2008

Thursday, Feb 21

9:00–10:00	***Executive Session***	
10:00–11:00	***Super B Factories***	
	Theoretical Motivation (15 min)	Zoltan Ligeti
	US Participation at Frascati (15 min)	David Hitlin
	US Participation at KEK (15 min)	Tom Browder
11:00–12:00	***DUSEL***	
	View from the NSF (20 min)	Jonathan Kotcher
	Facility,Experiments,Detectors (20 min)	Kevin Lesko
12:00–1:00	***Lunch***	
1:00–5:00	***Neutrino Physics***	
	Overview (30 min)	Peter Myers
	The NOvA Experiment (20 min)	Gary Feldman
	The Fermilab Program (20 min)	Gina Rameika
	Other Fixed Target Expts (15 min)	Heidi Shellman
	Coffee Break	
	Water C Det at DUSEL (20 min)	Milind Diwan
	Liquid Argon Detectors (20 min)	Bonnie Fleming
	Advanced Neutrino Sources (20 min)	Steve Geer
	Reactor Experiments (15 min)	Robert McKeown
	Double Beta Decay (15 min)	Giorgio Gratta
5:00–6:00	***Executive Session***	

Friday, Feb 22

8:00–10:00	***Dark Energy Experiments***	
	Overview (40 min)	Josh Frieman
	Supernovae (20 min)	Alex Kim
	Weak Lensing (20 min)	Bhuvnesh Jain
	Baryon Oscillations (20 min)	Martin White
10:00–10:30 Coffee Break		
10:30–12:00 Particle Astrophysics		
	High Energy Gamma Rays (25 min)	Roger Blandford
	H E Cosmic Rays and Neutrinos (25 min)	Angela Olinto
	CMB Experiments (25 min)	Scott Dodelson
12:00–1:00	***Lunch***	
1:00–2:00	***Executive Session***	
2:00–3:00	***European Plans and Views***	
	Rolf Heuer	

3:00–4:00	***SLAC Plans and Views***
	Steve Kahn, Persis Drell
4:00–4:30	***LBL Plans and Views***
	Jim Siegrist
4:30–6:00	***Town Meeting***

Saturday, Feb 23

9:00–12:00	***Executive Session***

BROOKHAVEN: MARCH 6–8, 2008

Thursday, March 6

9:00–11:00 — ***Executive Session***

11:00–12:00 — ***Dark Matter Experiments***

Hank Sobel

12:00–1:00 — ***Lunch***

1:00–2:30 — ***Generic Accelerator R&D***

Introduction (5min)	Maury Tigner
Lasers and Plasmas (25 min)	Wim Leemans
Muon Colliders (25 min)	Bob Palmer
Generic R&D Needs (25 min)	Maury Tigner

2:30–5:00 — ***LHC and Super LHC***

Physics Motivation (30 min)	Ian Hinchcliffe
Coffee Break	
ATLAS (20 min)	Abe Seiden
CMS (20 min)	Joel Butler
Accelerator (20 min)	Steve Peggs

5:00–7:30 — ***Executive Session***

Friday, March 7

8:30–9:00 — ***University Program Concerns***

Wesley Smith

9:00–9:30 — ***Brookhaven Plans&Views***

Steve Vigdor

9:30–10:00 — ***Cornell Plans&Views***

Jim Alexander

10:00–10:30 — ***Argonne Plans&Views***

Jim Proudfoot

10:30–12:00 — ***Executive Session***

12:00–1:00 — ***Lunch***

1:00–4:30 — ***Executive Session***

4:30–6:00 — ***Town Meeting***

Saturday March 8

9:00–12:00 — ***Executive Session***

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