High-Energy Physics Facilities

Recommended For The

DOE Office of Science

Twenty-Year Roadmap

March 2003

March 17, 2003

Dr. Ray Orbach
Director, Office of Science
U.S. Department of Energy
Washington, D.C. 20585

Dear Ray,

I am responding to your charge letter to the High Energy Physics Advisory Panel (HEPAP) of December 18, 2002 concerning the particle physics facilities that will position the Office of High Energy Physics to be at the forefront of discovery over the next twenty years. This letter constitutes HEPAP's report, as requested in your letter.

Following receipt of your charge, I formed an HEP Facilities Committee consisting of all the members of the Particle Physics Project Prioritization Panel (P5) plus seven others, with myself as chair. The members of the Committee are found in Appendix A. Your charge letter provided the impetus for P5 to take an intensive look at the whole particle physics roadmap. This survey would otherwise have been deferred, but doing it now has proven to be an excellent precursor to P5 considering specific projects.

In January, after examining the list of facilities accompanying your letter, requests were sent for written narratives to 12 laboratories/proponents of a slightly revised list of facilities. The Committee heard brief presentations during a meeting on February 15-16 in Pittsburgh. More importantly, previously communicated questions were answered and additional issues discussed with the presenters of each of the narratives. The agenda of that meeting is found in Appendix B. The Committee interacted via email on the draft report. A final draft was produced in early March and approved by HEPAP.

Our response starts on page 2 with the broad goals of our science into which the facilities fit, as presented in the report of the Long-Range Planning Subpanel last year. This is followed by a description of the HEP roadmap of facilities, beginning on page 3. A summary of our recommendations on the twelve facilities begins on page 4. These have been reorganized and renamed from the original list. We show in particular how they tie into the overall scientific goals and the roadmap of the Subpanel. More detailed "one-pagers" for each facility are found in Appendix C. Each contains a brief description of the particular facility; the science being addressed; the collaboration; interagency and international aspects; ties and synergies with other sciences; possible competing facilities; the timeline; and the cost estimate. These one-pagers should replace those accompanying your letter. Appendix D contains a table that summarizes the results of our response.

The Goals of Particle Physics in the 21st Century

The Long-Range Planning Subpanel report was unanimously endorsed by HEPAP last year and provides a twenty-year strategic plan for U.S. particle physics. HEPAP's vision for our field, statement of our scientific goals, and five major recommendations naturally form an integral part of the twenty-year strategic plan for the whole Office of Science that you are developing.

The plan's formulation was in response to a charge from both the DOE and the NSF. It makes contact with developments and plans in other divisions of the Office of Science, and, given the global nature of the field, it also reflects projects and planning in the other regions of the world. Indeed, it is a part of a worldwide consensus on the most important opportunities and priorities. The U.S. program is to be carried out by DOE-HEP, in collaboration with NSF, NASA, and international partners from across the globe. This will become manifest as we discuss the possible facilities individually.

The vision contained in the Subpanel's report was shaped by several decades of extraordinary accomplishment that put particle physics at the threshold of a new era of discovery. On that basis, the Subpanel created a science roadmap for the field containing three interrelated long-range goals. The goals drive a diverse and interconnected research program. The overall plan ties them together in an exciting science-centered program that aims at keeping the U.S. among the world leaders in the field.

Ultimate Unification - Unification is the search for simplicity at the heart of matter and energy. The rich and complex phenomena we observe today appear to have emerged from a much simpler world at high energies that existed in the first moments of our universe. Experiments of the last few decades have confirmed that new fundamental particles reflecting this simpler world must exist at energy levels just beyond the reach of current accelerators. Our goal is to explore phenomena that will give us insight into the mechanism by which the disparate particles and forces of the universe merge into a single coherent picture.

At energies approaching a TeV, we will begin to explore an uncharted world where we know that two of the forces, electromagnetic and weak, are unified into one electroweak force. As part of this unification, the fundamental particles acquire the property of mass and their characteristic behavior under the weak and electromagnetic forces. We know that something fundamentally new and different than anything we have seen before must happen. But what is it? One often-cited example is a new kind of particle, the Higgs boson, as a remnant and thereby a signal of the unification mechanism. In addition, neutrino masses may be connected to energy scales where all the forces become one.

Hidden Dimensions - The visible world appears to have three spatial dimensions. String theories, however, predict that there are more. Some of them might be observable by kicking particles with enough energy that they could disappear into the extra dimensions. Particle accelerators would allow the discovery of such dimensions, and measurement of their shapes and sizes. In the long term, string theory may provide the ultimate unification of forces. Our goal is to explore whether there are extra dimensions and to decipher their structure.

Supersymmetry, which is strongly favored theoretically, predicts that additional dimensions with spin lead to a set of new fundamental particles, one partner for each known fundamental particle. We suspect that the entrance to the world of supersymmetry also lies at the TeV energy scale, where it plays an essential role in unification.

Cosmic Connections - Elementary particles that interact through a few fundamental forces shape the evolution, present state, and future of the universe. Recent astrophysics experiments indicate that most of the matter in the universe is dark, unlike any conventional matter here on Earth, and that empty space is filled with dark energy, pushing the universe to expand at an ever-increasing rate. Our goal is to explore the nature of dark matter and dark energy through experiments both on earth and in space.

A prime candidate for the dark matter is the lowest-mass particle of supersymmetry, left as a remnant of the early moments of the universe. If so, we will produce the dark matter particles and precisely study their properties and connections to unification and hidden dimensions at TeV-scale accelerators. In contrast, the next-generation experiment to study dark energy will be in space. Back on earth, by studying differences in the behavior of matter and antimatter in accelerator-based experiments, we hope to understand why our universe is now composed of matter, even though there were equal amounts of matter and antimatter in the very early universe.

The HEP Roadmap of Facilities

The long-term goals of particle physics are best advanced using a variety of techniques in a balanced and diverse program. Across the world, particle physicists are in the midst of planning a bold array of experimental initiatives. Some of these straddle the boundaries between particle physics, astrophysics, and nuclear physics. Most use accelerators, but essential advances will come from non-accelerator experiments on Earth and in space.

For the foreseeable future, particle accelerators will be the primary tools of the field. Indeed, a large majority of the facilities on our list are accelerators or accelerator-based projects. At accelerators we can actually produce the basic constituents of matter and study them in abundance under controlled conditions. There are many examples of the crucial role played by accelerators for our science over past decades.

TeV-scale accelerators, with the unique potential of discovering and understanding the physics of the Higgs boson and/or of supersymmetric particles, will be the principal means of reaching toward our long-term scientific goals. Exploration of physics at the highest available energies, first at Fermilab in the U.S., and then at CERN in Europe and at the proposed Linear Collider somewhere in the world, will allow us to chart this new frontier. That is why U.S. participation in the LHC was the central recommendation of the Drell Subpanel in 1994, why this was reiterated by the Gilman Subpanel in 1998, and why the Bagger-Barish Subpanel in 2002 made the highest priority of the U.S. program a high-energy, high-luminosity, electron-positron linear collider, wherever it is built in the world.

The breadth of the scientific problems we must attack requires, however, a diverse portfolio of experimental techniques. The remarkable progress in our field over the past half-century relied on accelerator facilities, both at the energy frontier and at lower energies, and on experiments carried out at reactors, in space, and underground. Some weighed tens of kilotons, others fit on a tabletop. Our appraisal of the scientific questions before us convinces us that this will also be true in the future. This diversity of approaches is part of the nature of our science. The facilities discussed here are responsive to this.

Any list of future facilities is a dynamic one. With time, decisions will be made to begin construction of some facilities and not of others on the list that we have now. Still other facilities may be added in response to new scientific and technical opportunities. We recognize that this is an ambitious list. However, we are part of a world community and the list needs to be viewed in an international context. Especially for the very large facilities, some will be located here and others abroad. We want to participate in the most important science, wherever the facility is located, just as our European and Asian colleagues would want to collaborate on facilities in the U.S.

Summary of Future High-Energy Physics Facilities

In discussing the scientific potential of a facility, we will use the categories suggested in your letter: *absolutely central*, *important*, and *don't know enough yet*. We apply these categorizations first to the intrinsic importance of the science being addressed. However, there are other issues that affect any specific facility on the list, such as possible scientific and technical developments and the degree to which the particular proposed facility will make decisive contributions. Therefore, for each of the proposed facilities, we give a separate rating that addresses these latter issues. For facilities that are near decision points, the ratings coincide. But for facilities far in the future, much less is known, and the ratings can be different.

To be considered *absolutely central*, we require that the intrinsic potential of the science be such as to change our view of the universe. This is an extremely high standard, at the level at which Nobel Prizes are awarded.

Our standard for facilities that we judge to be *important* is that they be world class. Moreover, a set of experiments that are individually important can together change our view of the universe. This has happened repeatedly in the past and we expect that it will be the case with the future facilities we are considering. For example, a diverse set of quark physics experiments carried out over several decades have together led to a picture in which just a few fundamental parameters explain all the weak interactions of quarks and matter-antimatter asymmetries seen in the laboratory up to now. These experiments led to absolutely central science.

We have grouped the facilities according to the physics that they address. The groupings reflect the interconnected paths on the roadmap, leading to the scientific goals described in the previous section. The first path involves unification of the weak and electromagnetic forces. It leads as well toward understanding the masses of the fundamental particles, probing for more than three spatial dimensions, and seeking to discover the new world of supersymmetry. This requires accelerators at the energy frontier, such as the LHC and the proposed Linear Collider. The second path is cosmology, which includes fundamental observations made from space. Along this path we seek to understand the history and future of the universe, including uncovering its biggest and least understood component, dark energy. The third path of quark physics requires accelerators at the luminosity frontier. It relates to unification and the question of why the universe around us is composed of matter and not antimatter. The imprint of new physics to be found at high energies should show up in precision measurements in the quark sector. The fourth path of neutrino physics requires intense neutrino sources and detectors located deep underground. The observations of oscillations of one type of neutrino into another and the implication that neutrinos have non-zero masses have been major discoveries of the past few years. They may point to physics at energy scales far beyond a TeV where all the forces are unified and at another way to understand the presence of matter rather than antimatter in our universe. A balanced approach, involving all four paths and a diverse set of facilities, is *absolutely central* to achieving our scientific goals of ultimate unification, hidden dimensions and cosmic connections.

As to readiness, we have chosen to indicate whether a project is in the *R&D phase* or the *project engineering and design phase* (PED), or is *ready for a decision on construction*.

The Energy Frontier

Linear Collider

There is now a worldwide consensus that a high-energy, high-luminosity, electron-positron linear collider (LC), operating concurrently with the LHC, is necessary to explore and understand physics at the energy frontier. The LHC and LC will provide a sweeping view and incredible precision, with the discoveries of each used to great advantage in extracting and extending the physics results of the other. A linear collider requires the technology, people, and financial resources of an international project. Building on our nation's previous investment in exploring the high-energy frontier, the Long-Range Planning Subpanel recommended, as its highest priority, that the U.S. participate in such a project, wherever it is located in the world, and that the U.S. prepare to bid to host the facility. The intrinsic science potential of the Linear Collider and the capability of the facility to achieve that science are *absolutely central*. Presently in an advanced *R&D phase* on an international basis, with the formation of an international design team it would enter the project engineering and design phase in 2006.

LHC Luminosity Upgrade

The U.S. is participating in the construction of the LHC proton-proton collider that is to begin operation in 2007 and explore the energy frontier with seven times the energy we can reach with present accelerators. U.S. physicists, supported by the DOE and NSF, comprise approximately 20% of the international detector collaborations, ATLAS and CMS. Both the U.S. accelerator and detector projects are more than 70% complete and plans are underway for U.S. scientists to play an essential role in producing science with this facility. Upgrades of the LHC are being envisaged. The more cost-effective upgrade is an increase in the luminosity, with the goal of a factor of 10 above the design value. This involves modifications of the accelerator (particularly the focusing magnets around the interaction regions) and the detectors (particularly the tracking systems). The science of extending exploration of the energy frontier with the LHC accelerator and detector luminosity upgrades is *absolutely central*. The *R&D phase* for these will need to start soon if the upgrades are to be finished by the present target date of 2014.

LHC Energy Upgrade

A challenging and more costly upgrade of the LHC would involve doubling the total collision energy from 14 to 28 TeV. This requires a multi-year shutdown of the machine during which the original magnets would be removed and a new collider, employing bending magnets with twice the field strength, would be installed and commissioned. It is possible that the physics found in the next decade at the LHC will be such that it will demand such an upgrade, but at this point we *don't know enough yet* either about the science or about the specifics of the facility that might be proposed. It will require an extensive *R&D phase*.

Cosmology

SNAP

Resolving the mystery of what is the dark energy that constitutes more than two-thirds of the energy of the present universe is one of the leading scientific questions in physics and astronomy. The particles we know describe only a tiny fraction of the composition of the universe, since neither dark matter nor dark energy is encompassed in our current picture, with the dark energy driving the universe's accelerating expansion. Thus, we are ignorant of the dominant constituents of the universe, those that shape its structure and determine its ultimate destiny. The Supernova/Acceleration Probe (SNAP) is a satellite experiment designed to take us to the next stage in exploring these mysteries. By observing thousands of Type Ia supernovae with better precision than any single supernova has ever been measured so far, SNAP will measure the history of the growth of the universe over the past 10 billion years. The science addressed by SNAP in exploring the nature of dark energy is *absolutely central*. It would be carried out by an international collaboration and would be supported by both the DOE and NASA in the U.S. The project is now in the *R&D phase*, with plans calling for project engineering and design starting in two years, and launch in 2009.

Quark Physics

BTeV

BTeV is an experiment proposed for the Tevatron Collider at Fermilab that uses cutting-edge detector technology to greatly extend the search for new physics in the decays of B mesons. The B-factories at SLAC and KEK have shown that, as predicted, there is a large matter-antimatter asymmetry (CP violation) in some B decays. Interest has turned to trying to find an asymmetry in other decays that would not follow the standard predictions and thereby point to new physics at the TeV scale. BTeV is designed to make a full set of precise measurements for the decays of the B mesons, including the B_s meson, which is not studied at the electron-positron B-factories and deliver a signal of "tagged" decays that is roughly two orders of magnitude beyond that of existing B-factories. A similar experiment, LHCb is being prepared at CERN. BTeV was given scientific approval after a rigorous review by the Fermilab Physics Advisory Committee in 2000. Continuing R&D led to refinements in the design, and budget pressures led to a reduction in the scope of the experiment that reduces costs but maintains most of the capability. Following internal Fermilab reviews, the collaboration is working on a Technical Design Report. The experiment can be built and ready to operate by 2008 and would run in an era in which the TeV scale will be under direct exploration at the LHC. Following on several generations of quark physics measurements, BTeV is an *important* experiment. It is *ready for a decision on construction*, and is being considered by P5.

Super B-Factory

Both at SLAC and at KEK, the accelerator design and physics program of a next-generation B-factory are being explored. Such a machine would possibly have over 100 times the luminosity achieved to date. The physics goals are similar to those of BTeV and LHCb in searching for new physics by examining matter-antimatter asymmetries in rare B decays with two orders of magnitude more sensitivity than at the existing B-factories. KEK and SLAC are both studying the technical issues related to designing what is expected to be a single Super-B Factory. The machine is in an early stage of the *R&D phase* and it is not yet clear that all technical problems can be solved for an electron-positron collider with a luminosity of 10^{36} cm^{-2} sec^{-1}. While there could be a physics development that would immediately make for a compelling scientific case, at this point we *don't know enough yet* as to the scientific importance beyond what will be done by experiments at hadron colliders and about the specific facility that might be eventually proposed.

CKM

Charged Kaons at the Main Injector (CKM) is an experiment to be run at the Main Injector at Fermilab to obtain approximately 100 events of the rare decay $K^+ \to \pi^+ \nu\bar{\nu}$. Here again, the goal is to use the measurement of this rate together with other measurements of rare decays and of matter-antimatter asymmetries as a sensitive probe for new physics, with specific models giving a pattern of characteristic predictions. Like BTeV, the experiment would run in an era in which new physics at the TeV scale will be under direct exploration at the LHC. CKM was given scientific approval after a rigorous review by the Fermilab Physics Advisory Committee. The K0PI0 experiment at BNL is the companion experiment measuring CP violation for the corresponding neutral K decay mode. CKM is an *important* experiment. It is *ready for a decision on construction*, and is being considered by P5.

Neutrino Physics

Double-Beta Decay Detector (Liquid Xenon)

While neutrino oscillations only measure mass differences, it is essential to get directly at the actual mass scales. Neutrinoless double-beta decay, a yet-unobserved rare decay mode of certain nuclei, has been the subject of experimental search for decades. Detection of this process would show that neutrinos are their own antiparticles, make a measurement of the scale of neutrino masses, and connect neutrino mass to the unification scale. The sensitivity needed depends on the scale and ordering of the three neutrino masses. To be sensitive to at least some of the possibilities means that fiducial masses of rare isotopes must go from kilograms to tons. The Enriched Xenon Observatory (EXO) is a program intended to make this giant leap. It is based on an entirely new technique that could confirm the existence of the exotic decay through identification of the two beta-decay electrons in the final state and also the daughter of the decay nucleus. This scheme marries high-resolution atomic spectroscopy to particle physics techniques and could provide sufficient background rejection to reach the sensitivity required.

The experiment is in the *R&D stage*, as are a number of potential competing experiments around the world using different isotopes and techniques. Further R&D is needed to prove that this and/or other techniques can achieve the sensitivity required. Neutrinoless double-beta decay is compelling science of *absolutely central* importance, but we *don't know enough yet* as to a specific proposal.

Off-Axis Neutrino Detector

The Off-Axis Neutrino Detector capitalizes on the investment being made in the beam being constructed for the MINOS detector to measure the oscillation and disappearance of muon neutrinos. Neutrinos in an off-axis beam have a narrower range of energies. A detector located in this off-axis beam could be used to detect electron neutrinos produced from a muon-neutrino beam. The facility includes a new detector, with a fiducial mass of about 50 kilotons, optimized for detection of electron neutrinos. Exposed successively to neutrino and antineutrino beams, in a five-year run its sensitivity to oscillations of muon neutrinos to electron neutrinos will be at least a factor of ten better than the current limit. An experiment involving a beam from the Japan Proton Accelerator Research Complex (J-PARC) to SuperK would pursue similar physics, but with different sensitivity due to the different distances the beams travel through the Earth. Other possibilities are being explored. Such a measurement has *important* scientific potential. We regard the Off-Axis Neutrino Detector as *important*, pending review and comparison with other proposals. The experiment would employ proven technologies, so that with a specific proposal, a decision could be made to begin *project engineering and design*.

Neutrino Super Beam (Proton Driver)

The next stage in the progression along the neutrino physics path involves a high intensity neutrino super beam, produced by a proton beam ("Proton Driver") with a beam power of a megawatt or more. There are concepts for such a beam at BNL, using an upgraded AGS as the Proton Driver, and at Fermilab, using a new proton synchrotron or superconducting proton linac in place of the present 8 GeV booster. Coupled with a long baseline and a large detector with a fiducial mass of several hundred kilotons (see the Next-Generation Underground Detector, below), the neutrino super beam would permit a comprehensive neutrino science program over a decade or more that would include the precision measurement of neutrino mass differences and oscillation parameters, plus very possibly the measurement of matter-antimatter asymmetries (CP violation) that could connect the neutrino sector to leptogenesis as a source of the baryon asymmetry of the universe. Outside the U.S., there is potential competition from Phase II of the J-PARC, coupled with a possible HyperK detector. The Proton Drivers involve technologies that are ready for *project engineering and design*. The underlying neutrino physics is of *absolutely central* scientific potential. However, more information on the neutrino oscillation parameters (sought by the Off-Axis Neutrino Detector and other experiments) could be forthcoming. Furthermore, the coupling to an appropriate detector (including its location) plus collaboration or competition from abroad are factors not known at this time, so that we *don't know enough yet* as to a specific Super Beam facility. The Long-Range Planning Subpanel assessed the time for a decision on construction as 2007.

Next-Generation Underground Detector

A large Next-Generation Underground Detector with a sensitivity a factor of approximately ten greater than what is currently available would be capable of a broad science program that includes the search for proton decay and the detection of neutrinos, whether from a beam produced by an accelerator or solar, supernova, or atmospheric neutrinos. Such a detector can form the basis of the next-generation experiment in each of these areas. The scientific potential is *absolutely central* and, in the longer term, such a detector is quite likely to be built somewhere in the world. However, there are competing technologies – some of which will require a significant *R&D phase*, only proto- collaborations at this point, and there may be competing facilities abroad. There may also be new scientific developments. Like the Super Beam, we *don't know enough yet* concerning a number of factors relevant to a specific facility.

Neutrino Factory

The final step in the progression of neutrino physics facilities is the Neutrino Factory. This is a muon storage ring, with the decay of muons in the straight sections of the ring producing intense, high-energy beams of electron and muon neutrinos and antineutrinos with drastically reduced backgrounds compared to a super beam for some types of oscillations. The Neutrino Factory is then the tool that may be needed for a complete understanding of the fundamental parameters that describe the oscillations of neutrinos. The R&D for this very challenging concept is being pursued by an international collaboration that includes the Muon Collaboration based in the U.S. The next step in that *R&D phase* centers on the Muon Ionization Cooling Experiment proposed in the U.K. Establishing the technical feasibility and a decision on construction is more than a decade away. Given our lack of knowledge of the physics results that might emerge from the Off-Axis Neutrino Detector, other neutrino experiments, and a Neutrino Super Beam, we *don't know enough yet* about the scientific potential of a Neutrino Factory or about a specific facility.

The discussion of facilities above directly responds to your request. More details on each of the facilities are contained in the twelve one-page summaries in Appendix C; a condensed version of our response is found in the table in Appendix D. We believe that the science produced from these potential facilities by the thousands of users from universities and national laboratories in the U.S., as well as their collaborators from abroad, will keep the U.S. at the forefront of particle physics over the next twenty years or more. Stimulated in part by generating our response to you, HEPAP plans to form study groups with other scientific communities to explore in more detail some of the paths in the particle physics roadmap. We would be pleased to respond if there are further questions regarding these future facilities.

Sincerely,

Fred Gilman
Chair, HEPAP

cc: John O'Fallon
Peter Rosen
Bruce Strauss

Appendix A

Members of the HEP Facilities Committee

Jonathan Bagger	Jay Marx
Eugene Beier	Rene Ong
Patricia Burchat	Ritchie Patterson
Gerry Dugan	Charles Prescott
Gary Feldman	Tor Raubenheimer
Fred Gilman (chair)	Randal Ruchti
Dan Green	Abe Seiden
Marc Kamionkowski	Marjorie Shapiro
Boris Kayser	Mel Shochet
Young-Kee Kim	Elizabeth Simmons
Bill Marciano	Stan Wojcicki

Appendix B

Agenda
HEP Facilities Committee Meeting
Omni William Penn Hotel
530 William Penn Place
Pittsburgh, PA 15219
February 15 – 16, 2003

Saturday, February 15, 2003

(HEP Facilities Committee plus Presenters and Others)

8:30 am	Welcome and Introduction	Fred Gilman
8:45 am	SNAP	Saul Perlmutter
8:55 am	Questions and Discussion	
9:15 am	Linear Collider	Tor Raubenheimer
9:25 am	Questions and Discussion	
9:45 am	LHC Upgrades	David Lissauer
9:55 am	Questions and Discussion	
10:15 am	BREAK	
10:30 am	BTEV	Sheldon Stone
10:40 am	Questions and Discussion	
11:00 am	Super B Factory	Dave Hitlin
11:10 am	Questions and Discussion	
11:30 am	CKM	Peter Cooper
11:40 am	Questions and Discussion	
12:00 pm	LUNCH BREAK	
1:30 pm	Double-Beta Decay Detector	Peter Rowson
1:40 pm	Questions and Discussion	
2:00 pm	Proton Decay Detector	Chang Kee Jung
2:10 pm	Questions and Discussion	
2:30 pm	BNL Super Beam	Tom Kirk
2:40 pm	Questions and Discussion	

3:00 pm	BREAK	
3:15 pm	Off-Axis Neutrino	Adam Para
3:25 pm	Questions and Discussion	
3:45 pm	FNAL Super Beam	Debbie Harris
3:55 pm	Questions and Discussion	
4:15 pm	Neutrino Factory	Steve Geer
4:25 pm	Questions and Discussion	
4:45 pm	Discussion	
5:30 pm	Adjourn	

Sunday, February 16, 2003

(Only HEP Facilities Committee)

9:00 am	Review and Discussion
10:30 am	BREAK
10:45 am	Structure of the Report
12:00 pm	LUNCH BREAK
1:00 pm	Writing Assignments Schedule
2:00 pm	Adjourn

Appendix C

One-Page Summaries of HEP Facilities

Linear Collider

Description: The linear collider is a high-energy, high-luminosity electron-positron colliding-beam facility. It is at the center of the world roadmap for particle physics. The underlying science motivates a strategy of building the machine to operate initially at an energy of about 500 GeV, to explore the Higgs and related phenomena, followed by an increase in energy to roughly 1 TeV.

Science: The linear collider will explore physics at the TeV scale. It will operate concurrently with the Large Hadron Collider (LHC), now under construction at CERN. The linear collider's ability to make precision measurements with a well-controlled initial state will complement the broad-brush discovery potential of the LHC. Taken together, data from the two machines will allow us to uncover the mechanism of electroweak unification, the existence of additional dimensions of space-time, and perhaps the source of the dark matter. The linear collider and the LHC will thus have a major impact on all three of the principal goals of the field, ultimate unification, hidden dimensions, and cosmic connections.

Collaboration: The physics motivation for the linear collider is the subject of a broad international consensus. Over the last decade, extensive international R&D programs, in the US, Europe, and Japan, have explored the technological options for the accelerator. This R&D work has already provided major synergistic benefits outside of particle physics: for example, it has advanced the development of X-ray free electron lasers, which can provide unique research opportunities for condensed matter physics, chemistry and materials science, and structural biology. The R&D work has reached the point where it will soon (2004) be possible to select one of two viable options – designs based on superconducting L-band or normal conducting X-band cavities – that have emerged. Physicists in the US have launched a well-organized effort to prepare for participation in that process in collaboration with their international partners.

Cost and schedule: The linear collider is envisioned as a fully international project. Construction of the collider could begin in 2009 and be completed in six to seven years. The design and construction of two detectors would proceed in parallel. A firm cost and schedule for completion of construction will be delivered as part of the pre-construction phase of the project, but present estimates (based on detailed pre-conceptual designs) place the total project cost (TPC) for construction in the U.S. at about $6B. The U.S. share of this would be made up of redirected HEP resources and incremental funding, and assuming one-third of the cost is borne by non-U.S. international partners, would total about $4B. If the collider is sited off-shore, the U.S. share would total about $2B. U.S. detector construction costs have been roughly estimated at $350M. The operating costs of the linear collider laboratory are estimated to be in the range of $300-350M per year.

Science Classification and Readiness: The project is *absolutely central* in importance to basic science: it will also be at the frontier of advanced technological development, of international cooperation, and of educational innovation. It is presently in an *R&D phase*, leading to a technology choice in 2004. Subsequently, pre-construction engineering and design for the collider could begin in 2006 and be completed in about three years, depending on how soon a site and international funding can be established. The cost to complete the engineering design and R&D through 2008 is estimated to be $1B, and as the host nation, the U.S. might supply two thirds of this cost.

LHC Luminosity Upgrade

Description: The Large Hadron Collider (LHC), an international project now under construction in Geneva, Switzerland, will extend the energy frontier by a factor of 7 when it begins operation in 2007. The ATLAS and CMS detectors have been designed to broadly study the new physics that will likely appear in such energetic collisions. U.S. physicists are taking major responsibilities for the accelerator and both experiments and will have leading roles in the scientific exploration once operation commences.

To fully capitalize on the investment and reach the major scientific goals, the LHC should be upgraded in the future. The most cost effective path is to increase the particle collision rate or luminosity. An increase by a factor of 10 will greatly extend the sensitivity of the physics studies and make accessible important channels that have very small production rates. The accelerator upgrade will involve a number of system modifications. Since the U.S. LHC responsibility is the interaction region magnets, it is natural that we focus on their upgrade. To achieve the desired luminosity, the magnets will have to be constructed from niobium-tin superconductor rather than the usual niobium-titanium. The needed R&D, which is now being organized at BNL, Fermilab, and LBNL, must begin soon.

The ATLAS and CMS detectors will have to be upgraded as well because of the increased collision rate. The most challenging system is the particle tracker, which will require much finer granularity to handle the factor of 10 increase in the number of produced particles. R&D on silicon trackers and readout electronics must begin soon if the detectors are to be upgraded by the target date of 2014.

Science: Extending the energy frontier is central to the goals of particle physics. Results from the ATLAS and CMS experiments will have major impact on all three of the principal goals of the field, ultimate unification, hidden dimensions, and cosmic connections. These experiments will be sensitive to the mechanism of electroweak unification, the existence of additional dimensions of space-time, and perhaps the source of the dark matter.

The LHC project has created an important synergy with distributed computing involving very large data samples. We anticipate that the upgraded LHC, with data of even greater complexity, will continue to provide the motivation for further developments in computing.

Collaboration and Funding: The LHC project is a truly global effort. The United States is building part of the accelerator, and U.S. physicists comprise approximately 20% of the collaborations that are designing and building the massive detectors and setting their physics priorities. Within the U.S., support is provided by an effective collaboration between the Department of Energy and the National Science Foundation.

Cost: Since both accelerator and detector upgrades are at the beginning of the R&D stage, reliable cost estimates are difficult to make. However based on the cost of the base LHC magnets and detectors, we estimate the U.S. share of the detector upgrades to be $100M, with an additional $50M for the accelerator components.

Science Classification and Readiness: The LHC Luminosity Upgrade is *absolutely central* to the goals of particle physics. Both the accelerator and detector upgrades are in the *R&D phase*.

LHC Energy Upgrade

Description: Extending the energy frontier is absolutely central to the goals of particle physics as is illustrated by the history of the field. The Large Hadron Collider (LHC) is an international project now under construction in Geneva, Switzerland. It will have a center-of-mass energy of 14 TeV and is expected to begin producing physics toward the end of the decade.

There are two possible upgrade paths for the LHC: a luminosity upgrade, discussed elsewhere, and an energy upgrade, discussed here. The energy upgrade could extend the reach of the LHC to 24 ~ 28 TeV depending on the details of the available technology.

We believe that an energy upgrade of the LHC or the construction of a higher energy hadron collider, with an energy of 100 ~ 200 TeV, will become a long-term objective of the field. However, upgrading the energy of the LHC or the construction of another higher energy hadron collider will require a very large investment and it is difficult to know the specific requirements until the physics discoveries from the LHC and the linear collider are known. For this reason, we urge that R&D be continued on the enabling technologies so that the energy upgrade can be considered early in the next decade. In particular, R&D on high-field superconducting magnets is essential to optimize the designs and clarify the cost drivers.

Science: Extending the energy frontier is central to the goals of particle physics. However the next energy scale that will be required will depend on the physics found at the LHC and the linear collider.

Collaboration and Funding: The LHC project is a truly global effort. It can be expected that an energy upgrade will also be a global effort.

Cost: Since both accelerator and detector upgrades are at the beginning of the R&D stage, reliable cost estimates are difficult to make. However the energy upgrade will require replacing most of the superconducting magnets and likely the vacuum system of the present LHC. Thus, the upgrade cost is similar in magnitude to that of the original cost of the LHC.

Science Classification and Readiness: At this time, we *don't know enough yet* about the scientific importance of an LHC Energy Upgrade to the goals of particle physics. The physics results from the LHC and the linear collider are needed to determine the parameters of a future accelerator at the energy frontier. The technical readiness of the LHC Energy Upgrade is also not known yet and is in an *R&D phase* to develop the required high-field superconducting magnets.

Supernova/Acceleration Probe (SNAP)

Description: SNAP is a space-based experiment designed to obtain a high statistics, calibrated data set of ~2000 Type Ia supernovae out to high redshifts. Launched into an elliptical high-earth orbit, it will repeatedly observe a 15 square degree region of the sky near the north and south ecliptic poles, returning to each field of view every four days. SNAP will have a 2-meter telescope powerful enough to detect light from very distant supernovae and a wide field of view instrumented with a half-billion pixel camera to discover and follow many supernovae at once. In addition to obtaining a high statistics sample, SNAP will use fixed filters to measure the flux of each supernova in nine separate wavelength bands between 350 and 1700 nm and will take a detailed spectrum of each supernova at its peak brightness. The camera has both optical and near-infrared detectors to observe supernova light redshifted out to z = 1.7.

Science: SNAP will investigate cosmological questions of central importance to the field of high-energy physics. Supernova studies, corroborated most recently by cosmic microwave background measurements, have demonstrated that the expansion of our Universe is accelerating and that the universe's energy density is dominated by a previously unknown component called "dark energy." SNAP's tight control of systematic errors will enable it both to measure the ratio, w, between the dark energy's pressure and energy density to a precision of 5% and also to determine the time variation, w', of this ratio. SNAP will also carry out a weak-gravitational-lensing survey covering several hundred square degrees of sky to a depth comparable to that of the Hubble Deep Field survey. This will map the distribution of dark matter through its gravitational effects; the results will be key to determining w' from the supernova data. SNAP will also support a guest survey program on other compelling areas of astrophysical interest and serve as a finder scope to the James Webb Space Telescope.

SNAP's technical abilities will be unique. There is currently no competing space-based project. Ground-based efforts are limited because the absorption of near-IR radiation by our atmosphere introduces a selection bias. SNAP, working above our atmosphere has an unimpaired ability to detect dim, distant supernovae even out to a redshift of z = 1.7 to discriminate between cosmological models. Studies of the instrument, spacecraft, orbital properties, and launch vehicle by NASA Goddard and JPL found no technological obstacles.

Collaboration & Funding: SNAP is a US project involving international collaborators (France, Sweden). The collaboration size (110 scientists/engineers) will grow as depth is added, especially in areas where NASA can provide expertise. While DOE is supporting Lehman-reviewed R&D, program managers from DOE and NASA are developing plans by which SNAP could become a joint project, drawing on the agencies' complementary strengths. NASA's AO for a "Dark Energy Probe" in the ``Beyond Einstein'' initiative calls for proposals "involving a significant NASA contribution (>25% of the total mission cost) to the existing DOE SNAP concept mission."

Cost: Ground-up cost estimates by SNAP and by NASA/Goddard for the construction and launch were ~$400M using DOE costing standards with contingency but without escalation. A JPL cost exercise, under the same rules, estimated the cost for NASA to construct SNAP at ~$600M.

Scientific Classification & Readiness: SNAP's science is of *absolutely central* importance. SNAP is in an *R&D phase* that should yield a Conceptual Design Report by the end of FY05. SNAP can be ready to initiate construction at the end of FY05 and launch in 2009.

BTeV

Description: BTeV, an approved Fermilab experiment, would make precise measurements of quark mixing and CP violation and search for new physics using enormous samples of B and B_s meson decays. It would operate at the Fermilab Tevatron, where it would take advantage of proton-antiproton collisions to produce the B and B_s mesons. BTeV's sophisticated trigger, which recognizes B and B_s mesons based on their lifetimes, would provide access to a broad range of important decay processes.

Science: The B-factories at SLAC and KEK have been spectacularly successful in observing and measuring the large CP violation in the decays of B mesons. Now, the focus is shifting to using CP violation to probe for new physics. Models with non-minimal Higgs sectors, extra dimensions, or various versions of supersymmetry all predict anomalous effects. These effects could produce unexpected results in quark mixing, CP violation, and rare decays.

BTeV is designed to make a full set of precise measurements of the decays of the B and B_s mesons. The BTeV physicists will search for the effects of new physics in two ways: they will carry out precise studies of quark mixing and CP violation in B and B_s meson decays; they will investigate rare and forbidden decays.

BTeV's studies of B_s meson decays will be sensitive to new physics affecting B_s mixing and will offer a precise, model independent measurement of the CKM angle γ. Its large data sample and excellent calorimetry could also enable BTeV to make the best measurement of the CKM angle α. These measurements are inputs to ultimate unification, and they may reveal features of hidden dimensions, for example, in the phases of couplings of supersymmetric particles. Measurements with BTeV could help distinguish among candidate models for new physics observed at the LHC.

Competition: Because it uses hadronic collisions, BTeV has access to B_s meson decays, which are out of reach for the electron-positron B-factories. BTeV also benefits from the enormous B meson production rates at hadron colliders. A similar experiment operating at the LHC hadron collider, LHCb, is in preparation at CERN. Differences in the detector designs and machine energies give BTeV and LHCb somewhat different reach in some interesting processes.

Collaboration and Funding: BTeV would operate for about three years at the Fermilab Tevatron after the conclusion of CDF and D0 operation. The collaboration now consists of 165 physicists from the US, Belarus, Italy, Russia and China, and anticipates growing to 300 to 400.

Cost: The detector has a base cost of $89.6M with a 37% contingency, for a total of $122.5M (Temple Review). The annual operating cost, including computing, extrapolated from similar currently running experiments, is estimated at $3.8M. Some international funding may be available. An award of $5M from the National Science Foundation is being used to investigate "fault tolerant, fault adaptive computing for the [BTeV] trigger."

Science Classification and Readiness: BTeV's ability to make measurements of CP violation and rare B and B_s meson decays make it *important*. The experiment is *ready for a decision on construction*, and is being considered by P5. It could begin operation by 2008.

Super B-Factory

Description: The Super B-Factory, an asymmetric e^+e^- collider with a luminosity of 10^{36} $cm^{-2}s^{-1}$, is a major upgrade of the existing SLAC B-Factory. This project includes the luminosity upgrade, to over 100 times that of the B-Factory, as well as the detector upgrade.

Science: The results of the B factories at SLAC and KEK in Japan have shown that the phase of the three-generation CKM matrix in the Standard Model can explain all the observed CP-violating effects at the current level of measurement precision. We also know that this level of CP violation is inadequate to explain the matter-antimatter asymmetry of the universe, one of the most fundamental scientific questions. By studying the pattern of CP-violating asymmetries in rare B decays and making precise measurements of CP-violating asymmetries from B mesons, the Super B-Factory can explore new physics with great sensitivity. With the Super B-Factory it is possible to distinguish the effects of supersymmetry, or other extensions of the Standard Model, through loop diagrams and see the effects of new particles that would be directly produced at high energy colliders (*e.g.* at the Tevatron or at the LHC). The physics goals are similar to those of BTeV at Fermilab, currently being considered by P5, and to those of LHCb at CERN, currently under construction. The Super B-Factory will provide a sensitive window on new physics in a manner complementary to these hadronic experiments.

Collaboration and Funding: A collaboration to pursue this project has not yet been formed. However, both SLAC and KEK have been studying the technical issues related to the accelerator and detector (KEK has been spending $1M per year in the accelerator R&D). Although SLAC and KEK are not yet working together, it is expected to be a joint effort to build a single international Super B-Factory.

Cost and Schedule: Since the project is in a very early stage of R&D, reliable cost estimates are difficult to make. The items that need significant R&D include RF system design and prototyping, feedback design and prototyping, beam dynamics studies, background simulations, vacuum chamber prototyping, and detector prototyping. The total R&D cost is estimated to be about $9M. The required R&D and follow-on construction effort could be mounted on a time scale commensurate with the shutdown of the current B-Factory complex in 2009, followed by a two-year installation of the Super B-Factory and the upgraded detector.

Science Classification and Readiness: The project is in an early stage of the *R&D phase* and it is not yet clear that all the technical problems for the accelerator with a luminosity of 10^{36} cm^{-2} sec^{-1} can be solved. While it is crucial to have future projects probing new physics in the b-quark sector, and a new physics development could immediately make for a compelling scientific case, at this point we *don't know enough yet* as to the scientific importance beyond what will be done by experiments at hadron colliders and about the specific facility that might be eventually proposed.

CKM

Description: CKM (Charged Kaons at the Main injector) is an approved Fermilab experiment to measure the branching ratio of the ultra-rare decay of a charged kaon into a charged pion and a neutrino-antineutrino pair ($K^+ \to \pi^+ \nu\bar{\nu}$). The theoretically well-understood Standard Model branching ratio for $K^+ \to \pi^+ \nu\bar{\nu}$ is about 10^{-10} and the experiment proposes to measure 100 such events in order to produce a 10% measurement. Thus, the experiment must be able to reject background kaon decays at the level of a few in a trillion. CKM proposes to do this by utilizing redundant momentum measurements of both the charged kaon and pion and highly efficient photon, electron, and muon vetoes. A previous experiment at BNL, E-787, measured two events in agreement with the theoretical predictions, demonstrating that background can be controlled to the required level. To achieve the necessary high event rates, CKM will measure kaon decay in flight from an RF-separated kaon beam. Previous experiments have studied kaon decays at rest.

Science: The measurement of $K^+ \to \pi^+ \nu\bar{\nu}$ determines the magnitude of the V_{td} element of the CKM matrix, a measure of the probability for a virtual W boson to decay into a top quark and a down antiquark. When combined with similar measurements from the study of *B* meson decays, it is sensitive to new physics at high-energy scales. In addition to its primary goal, CKM proposes to extend the sensitivity of other measurements of charged kaon decay including the study of twelve rare and ultra-rare decay modes.

Collaboration and Funding: CKM is largely a collaboration between American and Russian groups. The present collaboration has about 50 physicists from ten institutions, but expects to double its size by the time of data taking. There is no approved experiment in the world with comparable goals to those of CKM. However, recently a letter of intent was submitted to J-PARC for a stopped kaon decay experiment with the goal of measuring 50 $K^+ \to \pi^+ \nu\bar{\nu}$ decays. An experiment to measure the related $K^0 \to \pi^0 \nu\bar{\nu}$ decay, K0PI0, has been approved at BNL and is expected to be funded through the NSF MRE program. The measurement of $K^0 \to \pi^0 \nu\bar{\nu}$ provides complementary information to that from the measurement of that for $K^+ \to \pi^+ \nu\bar{\nu}$.

Cost: CKM has a cost estimate of about $100M, which includes contingency, but does not include detector, beamline, or proton source operating expenses. The proponents believe that the successful prototype tests may decrease the final cost.

Science Classification and Readiness: With regard to scientific importance, the HEP Facilities Committee regards CKM as *important*. Prototypes of the superconducting RF cavities necessary for the separated charged kaon beam have demonstrated the required field strengths. All of the CKM detector systems have been prototyped and tested with either test beam runs or data from previous experiments. Thus, CKM is *ready for a decision on construction*, and is being considered by P5.

Double-Beta Decay Detector (Liquid Xenon)

Description: EXO (Enriched Xenon Observatory) aims to use enriched liquid Xenon (^{136}Xe) to search for the neutrinoless double-beta decay reaction $^{136}Xe \rightarrow {}^{136}Ba^{++}e^{-}e^{-}$. Because that reaction is so rare, the keys to a successful experiment are a very large sample of enriched liquid Xenon (ultimately 10 tons for the full experiment) and extremely good background rejection. EXO would address the critical background rejection issue by requiring observation of the Ba ion in coincidence with $e^{-}\ e^{-}$ using atomic trapping and spectroscopy techniques. A major challenge for the experiment is procurement of the enormous sample, 10 tons, of enriched liquid Xenon required for the full experiment. A critical path item is demonstration of Ba ion trapping and identification. This novel approach is promising and worthy of pursuit.

Science: Neutrinoless double-beta decay offers a unique opportunity to probe the physics of neutrino masses, mixing and CP violation. Observation of that extremely rare reaction would provide direct evidence for lepton number violation and prove that neutrinos are Majorana fermions, *i.e.* they are their own antiparticles. Measurement of the decay rate would help pin down the neutrino mass scale, constrain mixing and CP violating phases that cannot be accessed in any other way and connect neutrino masses to the unification scale. Information gained could also help explain the observed matter-antimatter asymmetry of our Universe. How far neutrinoless double beta decay experiments need to be pushed before a signal is expected depends on the mass ordering and scale of the three known neutrino species. So, although neutrinoless double-beta decay experiments are extremely challenging, their physics motivation and payoff are compelling and absolutely central to the future goals of high energy and nuclear physics.

Collaboration and Funding: Currently, EXO is in an R&D stage. Its proponents, researchers from Alabama, Caltech, U.C. Irvine, Moscow, U. Neuchatel, SLAC, Stanford and WIPP, are building a prototype experiment which should be operating within 2 years and capable of measuring ordinary 2 neutrino double-beta decay. The collaboration is relatively small by particle physics standards, but it consists of strong groups that are fully capable of completing the required R&D. If R&D is successful, a 1 ton liquid Xenon experiment will likely be proposed followed by a full 10 ton effort that would be extremely sensitive to neutrino mass and mixing. The 1 ton intermediate effort would cost about $10M - $20M. The experiment must be underground to shield from cosmic ray effects. The WIPP facility in New Mexico and Soudan Mine in Minnesota are potential sites being considered. The project would be funded through the DOE. Many other neutrinoless double beta decay candidate nuclei are being considered by other collaborations using various techniques, such as the Majorana facility being considered by NSAC. Given the importance and difficulty of observing this rare process, more than one large-scale experiment is warranted.

Cost: The cost of the full EXO program as currently envisioned would be about $100M.

Science Classification and Readiness: EXO and other large-scale neutrinoless double beta decay efforts are in an <u>*R&D phase*</u>. The physics of neutrinoless double-beta is <u>*absolutely central*</u> to furthering our knowledge of neutrinos. Until the proposed technique for measuring the decay products in coincidence is successfully demonstrated, we classify EXO (as well as competing R&D efforts using different nuclei) as <u>*don't know enough yet*</u>.

Off-Axis Neutrino Detector

Description: The Off-Axis Neutrino Detector is designed to measure one of the neutrino mixing parameters, $\sin^2(2\theta_{13})$, through the appearance of electron neutrinos (ν_e) in a beam of muon neutrinos (ν_μ). The experiment capitalizes on the existing NuMI beam operating in medium energy mode and aimed from Fermilab to the MINOS detector located underground in the Soudan Mine in northern Minnesota. The decay kinematics of the particles that give rise to the neutrino beam provide a relatively clean beam of ν_μ of well-defined energy (narrow band) and at a well-defined angle "off-axis" to the primary beam. This experiment would sit on the surface and at an angular position relative to the NuMI beam line appropriate to neutrinos of 1.5-2 GeV energy. For example a detector located 735 km from Fermilab would be positioned 10 km from the beam axis. The detector location is chosen to sit at oscillation maximum, and the neutrino energy is chosen to operate below τ threshold, to reduce experimental backgrounds. For identification of electrons, the detector would be built from proven technologies, utilizing low-density materials and fine granularity readout. A 50 kiloton detector is planned at the remote experimental site, as well as a near detector of 1% size and of the same design. The detector would be exposed to both neutrino and antineutrino beams.

Science: Recent experimental results indicate that neutrinos have very small but finite mass and mix from one type to another. The phenomena of neutrino mixing can be characterized mathematically by three mixing angles θ_{12}, θ_{23}, θ_{13}, a complex phase δ, and three mass differences. Current measurements indicate that θ_{12} and θ_{23} are large, but that θ_{13} is small. The nature of the mass hierarchy (normal or inverted) is not yet known. Among the challenges for future experiments are to determine θ_{13} and the complex phase δ. The Off-Axis Neutrino Detector is designed to measure $\sin^2(2\theta_{13})$.

Collaboration: The collaboration for this experiment currently numbers 140 physicists from 45 institutes and is drawn in part from participants in the MINOS experiment. The most advanced competitive project involves J-PARC in Japan, where an off-axis beam is planned at 2 degrees with energy centered at 0.7 GeV and aimed at SuperK. While the J-PARC-SuperK experiment will have similar goals, the very different baselines (having different matter effects) will make the two experiments complementary. Other options, including experiments near reactors, are also under consideration.

Cost: The NuMI beam will be available in 2005 independent of this experiment. A decision on the Off-Axis Detector technology is expected in 2003 and engineering validation could be completed by mid 2004. The experiment is ready for project engineering and design. Simplicity of the detector would allow construction to begin soon after approval. Preliminary studies indicate that the detector can be built at a cost of $100M, and accelerator upgrades to meet the run requirements are $20M. Operation could begin later in this decade and an 8-year experimental exposure is planned. Further beam intensity upgrades of the Fermilab accelerator complex and the availability of a proton driver would benefit this experiment.

Scientific Classification and Readiness: The measurement has *important* scientific potential. Pending review and comparison with other proposals, we regard the Off-Axis Neutrino Detector as an *important* experimental initiative. Since the experiment would use proven technologies, with a specific proposal a decision could be made to begin *project engineering and design*.

Neutrino Super Beam (Proton Driver)

Description: Neutrino Super Beam designs have been under study at Brookhaven and Fermilab, and abroad. A neutrino super beam requires an intense beam of high-energy protons, a high power target for pion production, and a pion decay channel yielding a beam of neutrinos from the decay. The Brookhaven concept proposes a 1 megawatt proton beam utilizing the existing AGS, upgraded to a 0.4 second cycle time, a new high intensity proton source, and superconducting proton linacs for injection of protons at 1.2 GeV. A neutrino beamline would be built, pointing to a detector at distance of 2000-3000 km. The Fermilab concept consists of two options, one based on a new 8 GeV proton synchrotron, and one based on a new 8 GeV superconducting proton linac, feeding the Main Injector. The synchrotron option would require upgrading the Main Injector. The Main Injector would deliver up to 120 GeV at 2 megawatts on a high power target. The more costly SC linac option would provide substantially more protons than the synchrotron option, thus easing proton economics issues. An upgraded NUMI beam line would be used.

Science: The exciting and surprising results from neutrino oscillation experiments point to an emerging critical need for an intense neutrino beam facility at an accelerator laboratory. Coupled with a long baseline and a large detector with a fiducial mass of several hundred kilotons (see the Next-Generation Underground Detector), the Neutrino Super Beam would permit a comprehensive neutrino science program that would include precision measurement of neutrino mass differences and mixing, plus very possibly measurement of CP violation (if certain mixing parameters are sufficiently large) in the neutrino sector that could be connected to leptogenesis as a source of the baryon asymmetry of the universe. The worldwide high-energy physics program will require such a facility if progress in the understanding of neutrinos is to continue.

Collaboration: If built in the U.S., the Neutrino Super Beam would be a national facility hosted at one of the two U.S. laboratories indicated above. The scientific program would require in addition a large-scale detector to investigate fully the neutrino physics. The national laboratories, U.S. university groups, and groups from the international community would be expected to participate. Outside the U.S. there is potential competition from Phase II of J-PARC, coupled with a possible HyperK detector.

Cost: Brookhaven gives a fully burdened cost estimate of $265M for the AGS upgrade, and $104M for the target and beamline. The project would require a large multi-100 kiloton class water detector, not included in this cost estimate (see Next-Generation Underground Detector). Fermilab has indicated only rough cost estimates. The proton synchrotron upgrade is $200M - $300M; The SC linac option would be $300M - $450M. The Main Injector would need upgrading as well, estimated at $40M - $60M. A large fine-grained detector in an off-axis location is needed (see Off-Axis Neutrino Detector). The cost of a future detector is not included in this estimate.

Science Classification and Readiness: The science potential of the underlying neutrino physics for the Neutrino Super Beam is <u>*absolutely central*</u>. More information on the neutrino oscillation parameters could be forthcoming. The coupling to an appropriate detector (including its location) plus collaboration or competition from abroad are not known at this time, so that we <u>*don't know enough yet*</u> as to a specific Neutrino Super Beam facility. The Proton Drivers employ technologies that are ready for <u>*project engineering and design*</u> (with some modest R&D remaining). A decision on construction could be taken later in this decade.

Next-Generation Underground Detector

Description: A Next-Generation Underground Detector would support a diverse science program and concepts for such a detector are being developed in the U.S. and abroad. One example of such a facility is the proposed UNO detector (Underground Nucleon Decay and Neutrino Observatory), a next-generation underground water Cerenkov detector that probes physics beyond the sensitivity of the highly successful Super-Kamiokande detector in Japan, utilizing well-tested technologies. The total mass of the proposed UNO detector is 650 kilotons; the active volume is 440 kilotons, 20 times larger than the Super-Kamiokande detector. Smaller detectors based on other materials, such as scintillating liquids or liquid argon, are also being considered. The optimal detector depth to perform the full proposed scientific program of UNO is about 4000 meters-water-equivalent or deeper. A number of detector sites in the U.S. are being considered: the Hendersen, Homestake and Soudan mines, the San Jacinto mountains and the Waste Isolation Pilot Plant (WIPP).

Science: A next-generation underground detector will serve as a multi-purpose laboratory for the search for the long-sought-after decay of the nucleon and for a rich study of neutrinos from a broad range of sources -- neutrinos from accelerator sources such as a Super Beam, described above; neutrinos from supernovae as far away as the Andromeda galaxy; supernova relic neutrinos; atmospheric neutrinos; and solar neutrinos. If realized, a next-generation underground detector could result in great discoveries with far reaching impact on astrophysics, nuclear physics and particle physics, for decades to come.

Collaboration and Funding: Both the Department of Energy and the National Science Foundation are viewed as possible funding sources for a next-generation underground detector. Detectors similar to UNO are being discussed in Japan (Hyper-Kamiokande) and Europe (Frejus). There are preliminary discussions among interested parties to consider forming an international collaboration for a next-generation water Cerenkov detector, wherever it might be built.

Cost: Preliminary estimates indicate that the UNO detector would cost approximately $500M, but are site dependent. The major components of the cost are the photomultiplier tubes, excavation and water purification system. Other options remain to be costed.

Science Classification and Readiness: We view the scientific potential of a next-generation underground detector to be *absolutely central*. The UNO detector technology is well tested and may not require significant R&D. However, there are competing technologies, some of which will require a significant *R&D phase*, only proto- collaborations at this stage, and possible competing facilities abroad. There may also be new scientific developments. Thus, we *don't know enough yet* concerning a number of factors relevant to a specific facility. For the UNO detector, rigorous professional civil and mechanical engineering design of the UNO depends on the choice of final site. The detector could be completed within 10 years of groundbreaking.

Neutrino Factory

Description: A Neutrino Factory is designed to create a very intense and very pure beam of both electron and muon neutrinos and anti-neutrinos at relatively high energies. The components of the facility are a proton source (considered elsewhere as a distinct facility), a target system, a collection and cooling apparatus for muons, a muon accelerator and a muon storage ring. The facility could lead, ultimately, to a muon collider. Note that this facility could supply beam to off-axis or long baseline neutrino oscillation experiments simultaneously.

Science: From the decay of the muons in the straight sections of the muon storage ring, the Neutrino Factory would provide the most intense and cleanest neutrino and antineutrino beams envisioned at present. The primary aim of the facility is to enable the very accurate measurement of the elements of the neutrino-mixing matrix. In particular, if the parameter θ_{13} is very small, this may be the tool that is needed to address the full neutrino-mixing matrix, including CP violation in the neutrino sector. As discussed for the Super Beam, this scientific goal is of great importance because the observed baryon asymmetry of the universe cannot be explained by the CP violation observed to date in the quark sector. An additional source of CP violation is required, which could be associated with the neutrino sector.

Collaboration: A strong international collaboration of 130 professionals from 33 institutions, 11 foreign, is in place to perform the R&D needed to prepare for construction. In particular, $8.5M is needed to support the U.S. share of R&D for the Muon Ionization Cooling Experiment (MICE) that will be performed at Rutherford Laboratory when adequate funding becomes available. A proposal has been made to the NSF to secure additional funds for the R&D program of MICE.

Cost and Schedule: This facility is still in an R&D phase. Assuming that the R&D schedule is technically limited, the R&D can be completed in FY07. At that time a sound cost estimate would also be available after a complete design study was completed. At present, the cost is estimated to be $1.7B, without contingency, EDIA, or G&A. A decision to construct could be made in 2008 and construction could begin in 2013. The estimated cost does not include the proton driver associated with the neutrino super-beam facilities. The drivers of Fermilab, BNL, or J-PARC are possibilities, although an upgrade up to a beam power of 4 MW is very desirable. The neutrino factory would be a natural next step beyond the super-beam because of the greater physics reach available due to higher beam intensities and greatly reduced backgrounds because of the purity of the beams. It is unlikely that more than one neutrino factory will be built worldwide, so that competition is not an issue.

Scientific Classification and Readiness: Given our lack of knowledge of the physics results that may emerge from the Off-Axis Neutrino Detector, other neutrino experiments, and a Neutrino Super Beam, we *don't know enough yet* about the scientific potential of the Neutrino Factory or about a specific facility. The Neutrino Factory is in the early part of an *R&D phase*.

Appendix D

HEP Facilities Summary Table

Project	Type	Physics	Cost	Scientific Potential	Proposed Facility	State of Readiness	Possible Time Scale
Linear Collider	Facility	Energy Frontier	\$5B – \$7B	Absolutely Central	Absolutely Central	R&D	2015 Operation
LHC Luminosity Upgrade	Facility	Energy Frontier	\$150M (US Part)	Absolutely Central	Absolutely Central	R&D	2014 Operation
LHC Energy Upgrade	Facility	Energy Frontier	Unknown	Don't Know Enough Yet	Don't Know Enough Yet	R&D	Decision in Next Decade
SNAP	Experiment	Cosmology	\$400M – \$600M	Absolutely Central	Absolutely Central	R&D	2009 Launch
BTEV	Experiment	Quark Physics	\$120M	Important	Important	Ready for Decision on Construction	2008 Operation
CKM	Experiment	Quark Physics	\$100M	Important	Important	Ready for Decision on Construction	2008 Operation
Super-B Factory	Facility	Quark Physics	Unknown	Don't Know Enough Yet	Don't Know Enough Yet	R&D	Decision Later This Decade
Double-Beta Decay	Experiment	Neutrino Physics	\$100M	Absolutely Central	Don't Know Enough Yet	R&D	2005 Prototype
Off-Axis Neutrino Detector	Experiment	Neutrino Physics	\$120M	Important	Important	Project Engineering and Design	2010 Operation
Neutrino Super Beam	Facility	Neutrino Physics	\$250M – \$500M (Accelerator and Beam Only)	Absolutely Central	Don't Know Enough Yet	Project Engineering and Design	Decision Later This Decade
Underground Detector	Facility	Neutrino Physics and Proton Decay	\$500M	Absolutely Central	Don't Know Enough Yet	R&D	Decision Later This Decade
Neutrino Factory	Facility	Neutrino Physics	Unknown	Don't Know Enough Yet	Don't Know Enough Yet	R&D	Decision in Next Decade