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The Coming Revolution in Particle Physics

Report of the Fermilab Long Range Planning Committee

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The Coming Revolution in Particle Physics

Fermilab Long Range Planning Committee
Executive Summary Report

Fermilab's mission is to "advance the understanding of the fundamental nature of matter and energy". Humanity has long been challenged and inspired by the pursuit of such understanding. We seek answers both to timeless questions - *What is the Universe made of? What are the basic laws of nature?* - and particular puzzles for today, such as *What is the origin of the matter-antimatter imbalance in the universe? What is the origin of mass? What is dark matter? What is dark energy?* In this report we identify the major physics opportunities of the next decade and elaborate on the essential role that Fermilab should play, in providing the leadership and facilities needed to pursue them.

While scientific progress occurs mostly through slow, patient, and steady work, sometimes, revolutions in our understanding occur rapidly. Today, there is a clear sense that we are in the early stages of a revolution in elementary particle physics. The Standard Model, which has successfully described fundamental physics very well for the last three decades, is starting to give way to something new.

Soon, perhaps at the Tevatron but definitely at the Large Hadron Collider and Linear Collider, we will pull back the curtain on the origin of mass - or electroweak symmetry breaking - illuminating the fundamental question of why the electron that defines the entire visible and living universe is light, while the top quark is so heavy. The mechanism of electroweak symmetry breaking occurs at an energy scale accessible in experiments currently running or under construction. In ten years, we will have a new and more fundamental theory of matter and forces. We do not know what form this theory will take, but in the near future we will be able to probe its fundamental building blocks. Many theorists expect the unveiling of supersymmetry (SUSY), with a spectacular expansion in the spectrum of fundamental particles. Such a discovery would have a profound impact upon our understanding of the make-up of the physical world and the origin and evolution of the universe. Whatever comes, we fully expect it will lead to a dramatic departure from the list of particles and phenomena known today.

We now know that neutrinos have mass and mix among themselves, but the measurements do not fit a clear pattern. The observations could be a window on a new phenomenon occurring at very high mass scale, close to the putative scales of grand unification or quantum gravity. This path to new physics must be pursued enthusiastically since it offers the prospect of great revelations in the next decade or so.

The universe appears to be missing both mass and energy, or conversely, contains dark matter and dark energy, neither of which are understood. On the other hand, we observe extremely high energy cosmic rays, the origin of which is also a mystery.

The Fermilab long range planning study, therefore, addresses a new era in which the Standard Model of particle physics will evolve dramatically. The coming revolution will spark totally new questions that will be addressed through the exploration of new worlds of physics:

- A new world of phenomena at the electroweak symmetry breaking scale and beyond;
- A new world of neutrino masses and mixing, which may address the matter-antimatter asymmetry of our universe;
- A new world of particle astrophysics and cosmology aimed at solving three mysteries: What is dark matter? What is dark energy? What is the source of the highest energy cosmic rays?

Our primary tools of exploration will be accelerator-based particle physics experiments in which new particles can be created and new forces studied. These particles and forces are the basic building blocks of the universe and are tightly interwoven with the laws of physics. No component is superfluous. Even the top quark, with a mass twenty thousand times that of an up quark and a lifetime of only 10^{-24} seconds is indispensible. By colliding protons and antiprotons under controlled conditions with sufficient energy in the Tevatron collider, we were able to conjure up the top quark and measure its properties.

In future experiments, we will conjure up many new particles that also play a fundamental role in shaping matter as it is today. There will be new discoveries, such as the superpartners of the quarks and leptons dictated by SUSY or other particles predicted by new dynamics. Top quarks for their part will become tools to access new physics, such as the Higgs boson(s) and possible other phenomena that play an intimate role in the nature of the universe.

The observation of the relics of the Big Bang, and the implied processes by which the Universe originated, provide an alternate way to learn about the fundamental questions of nature and the universe. The existence of dark matter at the weak scale hints at a new sector of non-baryonic matter, perhaps a harbinger of the world of SUSY. If the matter-antimatter asymmetry known as CP violation showed up in neutrinos or SUSY, it would help explain the dominance of matter over antimatter in the universe. Questions such as the smallness of the cosmological constant pose profound challenges to our understanding of the quantum world, while the existence of dark energy may provide a window into the quantum vacuum or perhaps evidence for a new, ultra-light particle or extra dimensions.

These two experimental approaches, astrophysical observation and accelerator-based laboratory experiment, yield complementary views of the Universe. By comparing and contrasting their results and struggling to reconcile them in a consistent description of

nature — perhaps including predictions for phenomena not yet known to exist — particle physics makes progress.

These new physics questions demand that we forge new connections. For example, progress on understanding dark matter and dark energy will require the synergy of accelerator-based observations of the nature and behavior of matter and energy with astrophysical probes of how dark matter and energy have shaped the evolution and structure of the Universe. These experiments will be mounted on, miles above, and well below, the surface of our planet.

The present Fermilab collider-based program comprises two experiments (CDF, DZero) seeking new phenomena at the Tevatron with the highest energies currently available anywhere in the world. These will be succeeded by an experiment (BTeV) which will further probe the flavor sector of the universe and make a link to high-energy phenomena observed at the LHC or provide the first hints of new physics at the electroweak scale. Two neutrino experiments, one (MiniBooNE) operating at low energy, and a long-baseline experiment (NuMI-MINOS) using beam from the Main Injector, are examining oscillations in two of the three regions where neutrino flavor transitions may have been observed.

In astroparticle physics the program features a three-pronged approach to key features of our universe, comprising a broad optical survey (Sloan Digital Sky Survey, SDSS) sensitive to many astrophysical phenomena, a direct search for cold dark matter (Cryogenic Dark Matter Search, CDMS), and the exploration of cosmic rays at the highest energies (Auger Experiment).

The Tevatron Collider started to operate twenty years ago; the new physics questions point to a need for new accelerators, and new large experiments. While we may not know the answers to the questions we pose, we do know the tools that we need in order to address them. The LHC at CERN will start to operate later in this decade. Fermilab is committed to using it to learn everything we can about the new world of TeV-scale phenomena. However, there is a strong consensus that deep understanding of these phenomena will call for very detailed and precise measurements that can only be done at an electron-positron Linear Collider, in which Fermilab should plan to play a major role. In addition, we see a path to learn fundamentally new things about the world from experiments probing neutrino masses and mixing, with a step-by-step program of new facilities that builds on Fermilab's existing strengths in this area. We consider these three major components of the future program in turn.

The charge to the committee asked it to consider two possibilities for the Linear Collider: siting at Fermilab and siting offshore. The committee focused on the first of these. As host, Fermilab would bring enormous strengths to the Linear Collider: an excellent physical location, technical strengths that are among the best in the world, and long experience exploring physics at the energy frontier. Likewise, the Linear Collider would bring to Fermilab the opportunity to explore the revolutionary physics that we anticipate at the TeV scale. The Illinois sites are close enough to the existing Fermilab site for anyone to work at each on a daily basis. The committee concludes that Fermilab should

make bidding to host the Linear Collider in northern Illinois its highest priority for the future.

This committee explored many of the issues associated with hosting the Linear Collider and enumerated what would be needed for Fermilab to mount the strongest possible case to host the Linear Collider Project. The immediate steps that Fermilab must take include developing further the necessary expertise at Fermilab, establishing performance goals, developing design studies, and bidding to host an Engineering Test Facility that will fully demonstrate the chosen technology. The Laboratory should also develop a hosting model that would support other exciting HEP research in parallel with the Linear Collider. These efforts will require enhancing the organization within the Directorate to coordinate and direct Fermilab Linear Collider activities and to communicate to outside institutions. Regardless of its location, a successful Linear Collider initiative will require a major commitment and a full leadership role from Fermilab.

A major component of the present and future experimental thrust is neutrino physics. Fermilab hosts the national long baseline neutrino oscillation facility NuMI, which consists of an intense neutrino beam directed at a large detector underground in Minnesota. This project is close to operation and forms a strong springboard for further exploration. In the near future we will further exploit the NuMI beam; for example we can mount a new, larger detector on the surface, at a similar distance to that of the the existing detector but about 15 km from the axis of the neutrino beam. Characteristics of the neutrinos in this direction would then be exploited to measure the amount of electron neutrino present in the heaviest neutrino eigenstate, one of the key unexplored parameters of our description of neutrinos. Depending on the value of this parameter, the same technique could be used (with an enhanced flux of neutrinos if needed) to explore the ordering of masses of the three known neutrinos. With a sufficiently intense neutrino beam and with an adequate suite of experiments, the program could be carried into the observation of CP violation in the neutrino sector.
Fermilab's capabilities are uniquely suited to this physics. The committee feels that neutrino physics forms an exciting program and one that Fermilab should pursue vigorously.

Exploring the new world of neutrinos will require larger experiments and a more intense neutrino beam. The latter depends on the beam power available in the primary proton source. A subcommittee considered two proposals for improving the Fermilab proton source. One is a superconducting linear accelerator; the other is a rapid cycling synchrotron. Either could deliver the required beam power but the linac option has many other attractive features. Either would also require upgrades to the Main Injector. Such an accelerator could be designed, approved, and built by approximately the middle of the next decade. The physics case for intense neutrino beams is sufficiently compelling that the committee calls for the preparation of a Conceptual Design Report and other documentation sufficient to request a statement of Mission Need from the DOE in parallel with preparations for the Linear Collider.

Of course, the construction of the neutrino experiment and the increases of the beam intensity will need to be optimized. If Fermilab is the host for the Linear Collider and it is under construction, resource constraints will limit the scope or speed at which upgrades

to the neutrino program could be constructed. If the Linear Collider is located offshore, such constraints will be less pressing. In either case, we envisage enhancements beyond the present neutrino experimental program.

Under any scenario, Fermilab will play a critical role in the Large Hadron Collider program: accelerator, experiments, analysis, and interpretation. Fermilab has unique attributes which can lead to it being the main center for CMS physics analysis, a leader in the development of grid computing and a leader in R&D for LHC accelerator and detector upgrades. The committee strongly endorses Fermilab's commitment to LHC participation and has laid out some ways to reinforce this effort.

The physics questions and opportunities will require new ways of working involving greatly increased global collaboration. The word "*collaboratory*" is increasingly used to describe the character of the experiments for the Large Hadron Collider (LHC) and particularly aspects involving the exploitation of a worldwide Grid of computing resources. Each experiment involves more than 1500 physicists. The LHC has already attracted the largest collaboration of physicists ever to come together to build an accelerator; future projects of this scale are likely to be explicitly international in character.

Flavor physics and studies of the strong interaction are prominent components of the current and near term Fermilab program. With the enormous discovery potential of the coming decade, quark flavor physics may well become an essential probe of new physics, providing tests and constraints on theories, magnified sensitivity to new physics in many channels, and a unique and different view of the physics. The extant Fermilab accelerator infrastructure, including the Main Injector and Tevatron, together with a future proton driver, provides a unique opportunity for the future exploration of new physics through sensitive quark flavor physics probes.

Finally, the committee considered a number of other, currently modest, components of the laboratory program.

Particle astrophysics provides important new probes of fundamental physics that complement accelerator experiments and Fermilab was the first particle physics laboratory to establish an astrophysics effort. Given the discovery potential of this field and the strong astrophysics program currently in place, the committee feels that Fermilab should strive to expand its leadership role and grow its program in Particle Astrophysics.

Accelerator R&D is essential to the future accelerator-based exploration of matter and its properties, and is therefore essential to Fermilab's mission. Increased support is needed to provide timely options for an exciting long-term future beyond the LHC and Linear Collider. The committee advocates increased support for Accelerator R&D.

Other areas, such as detector R&D, computational physics, and collaboration on societally important issues such as accelerator-based medical treatment and science education, are also potentially important components of the future program.

The committee considers that for all these smaller programs there are a number of options, as indicated in the relevant sections, to significantly improve the execution of the programs; not all of those measures would demand a significant increase in resources.

If the Linear Collider comes to northern Illinois, Fermilab will have the opportunity to lead in the revolutions at the energy frontier. This future would be optimal for the science and for Fermilab. If the Linear Collider is sited offshore, Fermilab will take a preeminent role in the revolutions in the field of neutrino physics, while being a leading participant in the Linear Collider whereever it is built. Fermilab must prepare for both scenarios. As the situation evolves and becomes clearer as this decade advances, Fermilab will be poised to proceed with the Linear Collider or the Proton Driver. In either case, the two programs, together with strong LHC participation, quark flavor experiments, Astrophysics, and research into future accelerator technology will constitute a Laboratory with a vibrant program in revolutionary times.

1. Introduction

In early 2003, the Fermilab Director formed a committee (Appendix A) to examine options for the long-range future of Fermilab. Specifically, the committee was asked to respond to a charge (Appendix B), which laid out the assumptions, which were to underlie our discussions.

The committee met a few times during the spring of 2003 and formulated a plan of action. It identified a number of issues that deserved attention, and a subcommittee was formed to focus on each. We agreed that in addressing these key issues, a broader participation was appropriate. The manner in which that was achieved varied from subcommittee to subcommittee to group. In some cases the expanded membership participated in all the discussions, in others, particular presentations were solicited and heard. Some subgroups met regularly over several months, others convened only for a small number of discussions. We have attempted to list participants in Appendix C.

General presentations indicating the purpose of the work were given, for example at the Fermilab Users Annual Meeting. Towards the end of the summer some sense of direction developed and a series of open meetings was organized by the different subgroups. These meetings of two and more hour's duration gave the broader laboratory and user community a further chance to react to perceived directions and to make their opinions known. They were extremely well attended.

In all, nearly 100 people have participated in the process including the development of initial drafts and proto-recommendations. A larger number attended the various open sessions. It is therefore likely, even expected, that the general thrusts of this report are no surprise. Nevertheless, the committee met in a number of plenary closed sessions including a two-day retreat in which all the issues were discussed and a common view was developed. The Director and Deputy Director heard and interacted with the discussions in most of these meetings.

In attempting to converge, we have written the individual chapters from a slightly advocative point of view. In the final discussions and editing, we have attempted some damping. Nevertheless, if all recommendations were accepted, all positive options pursued, any reasonable budget would be exceeded. We have balanced this bottom up approach with a top down development. We tried to extract the essence and to provide a couple of balanced options based on the discussions at the retreat. Healthy, lively, and vigorous exchanges ensued which resulted in multiple rewrites and culminated in the Executive Summary for this document. It is there that you should find the most concise product of this process.

In preparing the report, we did consider the potential availability of resources. We have devoted a chapter of the report to discuss the limitations of our efforts. It should be recognized that there are large variances among the public opinions about costs of large elements of the program such as the Linear Collider, or even the Proton Driver. The evolution of the laboratory budget is also very uncertain and depends on many things

such as the success in physics terms of the current program. We have therefore taken a relatively optimistic point of view in achieving balance. It is in this sense that the report provides options. At any point in time, the Fermilab Director will need to make choices among the options; we hope that the broad thrusts, which we call out, will be helpful.

In the remainder of the report we use chapter 2 to describe the Physics Landscape 2010-2020, which is the basis for what we would like to see happen. In chapter 3 we discuss the Linear Collider, which will be a major component of the laboratory program under any scenario and, if constructed nearby, a dominant one. In chapter 4 we describe a vigorous but evolutionary program to address the key neutrino physics areas, and in chapter 5 we discuss attractive initiatives, which would provide the necessary powerful source of protons and, hence, neutrinos for this program.

A strong participation in the machine, the experiment (CMS), and the physics at the Large Hadron Collider is a constant for our deliberations and is discussed in chapter 6.

Astroparticlephysics, which was introduced to the Laboratory twenty years ago, continues to develop and its growth is discussed in chapter 7. Accelerator research and development, the underpinning of our science core is addressed in chapter 8, and its sister discipline, detector research and development, is covered in chapter 9. In chapter 10 we examine the interdisciplinary science close to our field, which encircles our laboratory. In chapter 11 we outline some of the resource issues and the extent to which they were addressed before we present the conclusions.

2. Physics Landscape 2010-2020: The Coming Revolution

In ten years the Standard Model will be extended, deepened, perhaps superseded. A new and more fundamental understanding of matter and forces will emerge. This novel theory will encompass a wide range of newly discovered phenomena, new elementary particles, new symmetries, new dynamics, and new cosmological discoveries, revealed through experiment with high energy particle accelerators, and astronomical and cosmological observations.

This will be a revolution in our understanding of nature, and it will either bring us closer to an understanding of all phenomena in nature, through ideas such as supersymmetry and superstrings, or it will cause us to scramble to find new ideas and a new sense of direction about the mysteries of nature. We are entering a dramatic and important time in the quest to understand the fundamental laws of nature and their role in shaping the universe.

At the energy frontier, defined by energy scales of order hundreds of GeV, now probed by the Tevatron, up to the scale of a few TeV soon to be probed by the Large Hadron Collider (LHC), we expect the unknown structure of the mysterious symmetry breaking of the Standard Model to be revealed. We will soon learn the answer to a question that has a fundamental bearing upon our own existence, "Why are the weak interactions weak?"

All theories of "electroweak symmetry breaking" involve many new particles. Supersymmetry (SUSY), a favored scenario amongst many theorists, represents extra (fermionic) dimensions of space, leading to a doubling of the number of known elementary particles, and ushering in many additional new particles and phenomena associated with the various symmetry breaking sectors. The possibility of additional bosonic dimensions of space, as embodied in superstring physics and accessible in certain limits, would likewise usher in an even greater multitude of new states and new phenomena. Alternatively, we may see new strong forces and a dynamical origin of mass. The wealth of new particles, parameters, and CP-phases carries important implications for precision and/or high statistics quark flavor physics experiments that are uniquely sensitive probes of new phenomena.

Particle Astrophysics will participate in a central way in this new revolution. The identity of Dark Matter, for which particle theory has provided a number of candidates, including the lightest supersymmetric particle, will be probed both by extending the observational methods of astronomy and astrophysics, as well as through the use of energy frontier accelerators, such as the Linear Collider and LHC. It is possible that in ten years we will come to understand the identity of the particles that make up dark matter, and we will come to understand the correct framework in which to phrase the question, "what is the source of the dark energy that evidently permeates the universe?"

We have already begun to see the enlargement of the Standard Model in the lepton sector. Neutrino masses and mixing angles, which in the early 1990's were unknown, must now be incorporated into our full description of nature. In a minimal scenario of Majorana masses and mixings amongst the three known left-handed neutrinos, we see that there is a strong hint of a new mass scale in nature, a very large mass scale, possibly associated with grand unification or the scale of quantum gravity, the Planck mass. We are not yet sure what the proper description of neutrino masses and mixing angles will be. Experiments may reveal additional unexpected particles coupled to the neutrino sector. New phenomena, such as leptonic CP-violation, will be major focal points of our expanding understanding of the lepton sector. There is much to be done with experiment to attack the issues that neutrinos now present.

A strong and historical synergy exists between cosmology and astrophysics on the one hand, and particle physics on the other, through the physics of the neutrino sector. The first evidence for neutrino masses has come from solar and atmospheric neutrino detectors. Large-scale structure observations have recently placed stringent limits on the sum of the light neutrino masses, and future weak lensing measurements will provide even tighter bounds.

We thus expect that, on a time scale of ten years, the character of our understanding of the physical world will have changed dramatically. If history is a guide, it is unlikely that our current view of the key issues will be the same in ten years as it is now. New questions will be spawned by this revolution. The possibility of acquiring a much deeper insight into the origin of the nature of matter, the elementary particles and the distribution of matter, the origin and evolution of the universe is at hand. We may get closer to understanding one of the deepest and most important mysteries of all, why

nature provides in its pattern the light stable particles of everyday matter, such as the electron, out of which all life and even human consciousness is made. Perhaps we may someday solve the holy grail of particle physics, to be able to compute the electron mass, and the properties of the other basic particles of nature, from fundamental principles.

Through the application of these discoveries will come new insights into the structure and origin of the universe. Some of the new results from particle astrophysics experiments have contributed significantly to plans for new accelerator experiments that are needed to make progress in understanding these phenomena, and in other cases, new astrophysics experiments will be needed. These developments have motivated direct dark matter detection experiments, such as CDMS, as well as accelerator experiments to discover supersymmetry.

Already, developments in neutrino physics and the possibility of a novel source of CP-violation in the lepton sector have spawned hopes that the cosmic matter-antimatter asymmetry may be explained through *leptogenesis.* Neutrino physics, together with the search for SUSY, offer the possibility of experimental handles on the questions of dark matter and dark energy. Without the discovery of new particles in accelerator experiments, the telescope based cosmological observations of the early universe will remain unexplained puzzles. Indeed, together with the discovery of scores of new particles that play key roles in shaping matter and the fundamental processes of nature as it is, will likely come interesting new low mass or long lived objects that play key roles in the evolution of the universe in earlier epochs. The process of understanding the laws of physics in greater detail through accelerator based high energy physics, at the Tevatron, the soon to commence LHC, and eventually a Linear Collider, will have greater impact on our understanding of cosmology and the early universe than any other conceivable experiments or observations.

All of our scientific tools must be made available for this challenging future. This will require a broadband energy frontier assault, as the LHC can provide, with the possible step in the distant future to a VLHC. It will also require a precision energy frontier assault, such as the Linear Collider (LC) can provide. It will require new high luminosity proton sources for neutrino physics, with possible applications to low energy experiments of interest to other communities, such as the nuclear and condensed matter physics communities. It will require the ongoing synergy with cosmology and astronomy, through ground-based and flown observatories, to flourish. This may call for a diverse use of the existing facilities, such as the main injector and Tevatron, for a wide range of flavor physics experiments.

We will examine the key features and golden modes of this future program from our present vantage point.

I. The Energy Frontier

The "Energy Frontier" of particle physics, and the questions it poses, are largely shaped by the missing Standard Model ingredient, the "Higgs boson." It is extremely important, however, to recognize that *we do not know* what the concept "Higgs boson" really represents. In the Standard Model, "the Higgs boson" is a simple spin-0, isodoublet particle multiplet, yet we know that the Standard Model, by itself, doesn't make sense--it requires unknown additional ingredients for "naturalness." Often the energy frontier is relegated in the media as the mere "search for the Higgs Boson," as though the problem is well-defined and closed-ended. That is **not** the reality of the situation.

"The Higgs boson" is actually a rubric, a name applied to a category of unknown phenomena, including unknown yet far-ranging new physics that will commence at or near the electroweak scale, probably with new dynamics and new symmetries, possibly associated with the physics that may ultimately link us to the remote gravitational scale, or to new strong interactions.

The Large Hadron Collider

Beyond the Tevatron program of the current decade, the LHC will be in full operation in the 2010-2020 timeframe. The "Higgs Mechanism" will emerge and the Standard Model, as we know it now, will have to evolve to something new. The next decade will thus witness a revolution in accelerator based elementary particle physics as dramatic discoveries begin to push our map of nature, and the Standard Model is dramatically modified. We will enter a new era of short-distance physics. The first new states to emerge at the energy frontier, possibly at the Tevatron, and surely at the LHC, will be part of a larger structure that contains electroweak symmetry breaking.

SUSY, if true, would represent a bold paradigm shift from what has governed physics since Einstein's 1905 paper introducing Special Relativity--SUSY would expand the defining symmetries of space and time in a radical way. SUSY must be a broken symmetry since no SUSY partner has ever been seen. SUSY can solve the naturalness problems of the Standard Model Higgs boson, provided the SUSY breaking scale is near the electroweak scale. If so, then a rich new spectroscopy of superpartners must emerge near the weak scale. For example, the Minimal Supersymmetric extension of the Standard Model (MSSM) provides superpartners for all known elementary particles and contains two fundamental Higgs doublets, leaving two CP-even and one CP-odd neutral Higgs bosons and a charged Higgs boson. It also provides novel sources of CP-violation. The lightest neutral Higgs boson has Standard Model properties and an expected mass below 135 GeV, and below 200 GeV in most extensions beyond the MSSM. Likewise, many other non-SUSY schemes, such as new strong dynamics, little Higgs theories, and extra bosonic space-time dimensions, generally predict a wealth of new particles at or near the electroweak scale.

If a fundamental Higgs boson exists, such as is predicted in the Standard Model or its supersymmetric extension, then the LHC will discover it. If there exists supersymmetry in nature, the LHC will discover some, if not many of the superpartners. If there exist new non-SUSY phenomena near the weak scale, the LHC will likely discover them. The LHC will be able to confirm or rule out electroweak scale SUSY; if SUSY is found, we can begin to pin down the exact mechanism of SUSY breaking.

The LHC will also be able to observe physics associated with alternative scenarios to SUSY. This includes the possibility of additional dimensions of space, and the observation of "Kaluza-Klein-modes," the actual motion of particles in extra compact dimensions. Alternatively, the mechanism of symmetry breaking may be dynamical, involving a new strong interaction, and the "Higgs boson" may be a bound state, again implying a rich spectroscopy of new particles. This would likely lead to an enlargement of the gauge sector, including new forces and new scales of symmetry breaking. Or it may be something unanticipated and surprising, leading to new dynamical insights and possible clues to the answers of questions about which we are presently clueless.

In almost any envisioned scenario, our understanding of nature will be rewritten by the LHC. This implies the coming of a revolution in particle physics.

The Linear Collider

With the current bounds on a weakly coupled Higgs sector, as predicted in SUSY models or in the pure Standard Model, and with its envisioned luminosity, the LC will produce tens of thousands of Higgs bosons. This is enough to prove (or disprove) that a fundamental spin-0 particle, the putative Higgs boson, forms a vacuum condensate which breaks electroweak symmetry. If decays into fermions are competitive (in the Standard Model this requires that the Higgs mass lies below below $\approx 2M_W$), then the precision LC measurements can also verify that this particle gives mass to quarks and charged leptons in the expected way.

The LC will enrich and complement, in a crucial way, the observations of the LHC. A clear example of this pertains to the spectrum of superpartners in the context of the MSSM. The LHC measures mass differences well, but it can generally only infer crudely the mass of the lightest superpartner (LSP). The LC, on the other hand, can measure the LSP mass with great precision in pair production, thereby anchoring the whole tower of superpartners.

The LC can also distinguish between different theoretical interpretations of LHC discoveries. There will be dozens of new particles whose properties will have impact upon the physics of flavor. Simulations of precision LC electroweak parameter measurements reveal that they can in principle distinguish models with SUSY from models with extra spatial dimensions or new strong dynamics. Furthermore, the LC provides the opportunity to measure precisely and directly certain of the larger Higgs-Yukawa coupling constants.

There are key capabilities of the LC that cannot be realized with the LHC program alone. For example, the LC may provide the capability to explore the first hints of Planck scale physics or SUSY breaking scale physics by measuring the suppressed operators contributing to such processes as supersymmetry breakdown. The LC also provides the opportunity to test the interrelationship between the mechanisms of SUSY breaking and the dynamics of grand unification.

Moreover, there are a number of novel physics opportunities which have a direct bearing upon cosmology can only be addressed with an LC. If new sources of CP-violation, such as demanded by baryogenesis at the electroweak scale, are present then the LC will reveal them. LC observations and measurements of the properties of weakly interacting massive particles, such as the lightest superpartner (LSP), or the lightest Kaluza-Klein excitation, can provide the critical information to determine the existence and properties of dark matter. This in turn determines the thermal relic density for comparison with cosmological dark matter density observations. This will close the circle of the problem of the identification and understanding of the nature of dark matter.

The discoveries at the Linear Collider, together with the LHC, will have profound impact upon our understanding of the universe. The most important discovery for cosmology in the foreseeable future may be the determination of whether or not SUSY exists at the weak scale, what specific kind of SUSY structure entails, and if SUSY is disproved, then what alternative physics is present at the weak scale and what may be the candidate dark matter particle(s).

The opportunities afforded by the Linear Collider are rich, of profound importance, and extremely exciting, addressing central issues common to both particle physics and cosmology.

II. Neutrino Physics

During the last several years, stunning experimental results have established that neutrinos have nonzero masses and mixings. This development opens a whole new world for us to explore. There are many important physics questions concerning neutrinos before us now that we weren't asking ten years ago.

For example, what is the zoology of neutrinos? Is the mass matrix Majorana or Dirac, determining how neutrinos oscillate from one flavor to another? Do additional sterile neutrinos exist beyond the known three left-handed neutrinos of the Standard Model? What are the elements of the leptonic mixing matrix and does it contain CP-violating phases? Are these phases detectable?

These effects arise from the depths of short distance physics. They lead to profound questions about nature: What is the deep origin and nature of neutrino flavor physics? Is this truly a window on new physics at a high mass scale, such as the GUT or Planck scale? If so, what physics is found there, and does the see-saw mechanism generate the tiny neutrino masses? What new symmetries and gauge forces play a role in neutrino

masses and mixing? What is the connection between neutrino flavor physics and quark flavor physics?

Finally, how are neutrinos, through their masses and mixing, relevant to cosmology and the early universe? Do they play a role in the formation of large scale structure? Are there cosmologically observable signatures of their masses and mixings? Was baryogenesis in the early universe made possible by *leptonic* CP violation?

It is important to identify the milestones of the global neutrino program and understand in what sequence the new may discoveries happen, and on what time scale the program can achieve the milestones.

The Standard Model can accommodate neutrino masses. The observed masses may not require new particles, beyond the three known left-handed neutrinos, being Majorana combinations of the known left-handed neutrinos. While minimal Majorana neutrino masses do not require new particles (at accessible energies), they do nonetheless represent a completely new set of parameters, and a rich and novel new set of physics questions. The natural scale of the denominator in a Majorana mass term ranges from the GUT scale to the Planck scale! Thus the origin of neutrino masses appears to reach deep into the heart of Grand Unified Theories, if not to the scale of superstrings and quantum gravity. We view the observation of neutrino masses and mixings as the observation of a new scale in physics.

If results from critical experiments such as MiniBoone reveal the need to include additional "sterile" neutrinos, we will have witnessed a remarkable extension of the Standard Model into a new terra incognita, requiring an enlargement of the elementary particles of nature.

Key experimental challenges for the future involve determining the neutrino mass splittings more precisely than they are known at present. We must determine if the atmospheric mixing angle is truly maximal, representing maximal mixing between ν_μ and ν_τ, or if it deviates somewhat from maximality. We therefore seek precise measurements of the observed mixing parameters. We also need to know the *absolute scale of neutrino mass*. This may have a significant possible interplay with the cosmological effects of neutrinos

Complex phases in the neutrino mixing matrix may lead to observable CP violation. For CP violation in neutrino oscillation to be visible in terrestrial experiments the mass splittings and mixing angles must be sufficiently large. CP violation would manifest itself as a difference between the probability $P(\nu_1 \to \nu_2)$ and the probability $P(\bar{\nu}_1 \to \bar{\nu}_2)$. The predicted difference is small -- perhaps of order 1% due to the small size of θ_{13} and of the smallness of the solar mass splitting. If there are more than three neutrinos, then the possibilities for CP violation in oscillation become richer. At present, we know only that $\theta_{13} < 0.2$.

It is of fundamental importance to the future exploration of neutrino physics to demonstrate experimentally that θ_{13} is non-vanishing, and to be able to measure it. To observe CP-violation it is therefore necessary to make a number of complementary measurements and analyze them jointly to disentangle the various neutrino properties.

The sensitivity of neutrino physics is determined by the "number of protons on target." Therefore, the future success of a comprehensive exploration of neutrino physics depends crucially upon the proton source. A new proton source, the Proton Driver, is under consideration, which would provide at least a factor of five more intensity for neutrino physics. Such a high intensity proton source could also provide intense beams for other physics.

III. Quark Flavor Physics

The field of quark flavor physics, including CP-Violation, constitutes a fundamentally important part of the Standard Model. Moreover, flavor physics offers a window on the sensitive entanglement of beyond-the-Standard-Model physics with rare processes, through quantum loop effects involving new states. Flavor physics offers sensitive indirect probes and may be the first place to reveal additional key components of the post-Standard Model physics. Indeed, many people believe that B-physics will soon reveal significant departures from the Standard Model. The main arena for quark flavor physics includes strange, charm and beauty.

Flavor dynamics and the origin of quark and lepton masses and mixings are amongst the least understood topics in the elementary particle physics. While the ultimate understanding of flavor dynamics will probably come from a more fundamental theory at very short distance scales, such as the GUT scale or the Planck scale, the study of CP-violating and rare decay processes plays a fundamentally important role in the search for the fundamental theory. In this regard, we note that lattice gauge theory has come of age and will play a key role in determining theoretical predictions of matrix elements that are measured or used in flavor physics.

In the Standard Model, flavor-changing transitions are mediated by W-bosons and are therefore governed by the electroweak scale. Effects from new physics associated, with some large scale Λ, add corrections of order $M_W{}^2/\Lambda^2$ to the corresponding amplitudes, suggesting sensitivity to new physics similar to that of precision experiments performed at the Z-peak. Because flavor-changing neutral current (FCNC) processes are suppressed by small CKM angles, loop suppression factors, helicity, and GIM, there is often magnified sensitivity to physics at and above the electroweak scale in these processes. This is the prime motivation for new high precision efforts like BTeV, LHCb, CKM and KOPIO. Once new particles are discovered at the Tevatron or LHC, it is *imperative* that we explore their mixing patterns and couplings. Flavor physics can provide valuable insight into these issues.

BTeV will thoroughly explore the sensitive arena of b-physics which can directly provide new information about post-Standard Model physics, and precision information about

parameters and dynamics. BTeV should be allowed to evolve as the program defines itself, possibly extending over a significant part of the next decade.

Signatures of new physics may emerge in *b*-physics at the Tevatron, both in Run-II and BTeV. "Golden mode" examples of such would be the observation of $B_s \to \mu^+\mu^-$. In the MSSM, for example, owing to its special two Higgs doublet structure, the branching ratio $Br(B_s \to \mu\mu)$ can have a significant enhancement over the standard value. Also, $B_s\overline{B}_s$ mixing could reveal a surprise if it departs significantly from the standard expected value. Precision measurements of such anomalies, should they arise, will be profoundly important complementary studies to the energy frontier discovery of a new spectroscopy, such as SUSY. Such modes are the sensitive probes of the interplay of beyond-the-Standard-Model physics with the flavor physics of quarks.

Heavy flavor physics is remarkably synergistic with the physics of the kaon system. Rare decays of kaons, for example, probe the details of weak interactions at the quantum level. They can be sensitive to energy scales much higher than the kaon mass itself and can thus yield fundamental insights into physics at very short distances. A remarkable historical example is the suppression of flavor-changing neutral currents, exemplified by the fact that $Br(K_L \to \mu^+\mu^-) = 7\times10^{-9}$ while $Br(K^+ \to \mu^+\nu) = 0.64$. The analysis of the K_L–K_S mass difference by Gaillard and Lee, in the Fermilab Theory Group, led to the confirmation of the GIM mechanism and predicted the mass of charm quark, $m_c \sim 1.5$ GeV, prior to its discovery. Today the focus has shifted to modes that involve CP-violation, with the top quark effects arising in loop diagrams, together with hypothetical new phenomena from physics beyond the Standard Model, playing key roles. However, the spirit of the approach remains very much the same.

Short-distance dominated decays as $K^+ \to \pi^+\nu\overline{\nu}$ and $K_L \to \pi^0\nu\overline{\nu}$ can provide excellent tools to test emergent new physics scenarios with high precision. These processes are sensitive to new physics at the level of $M_W{}^2/\Lambda^2$. Forbidden modes could be dramatic indicators of new physics. Examples include: $K_L \to \mu e$, $K^+ \to \pi^+\mu e$ and $K_L \to \pi^0\mu e$ where stringent experimental upper limits on the branching ratios exist.

There thus remain key opportunities in the area of kaon physics, including many important targets, such as the full reconstruction of the CKM triangle from s->d transitions within the kaon sector, in parallel to the same within the B-meson sector, and exploration of rare processes with sensitivity to post-Standard Model physics. Fermilab has the facility capable of providing for and hosting a range of flavor physics experiments in this arena. Fermilab should remain open to serious proposals to pursue this physics in the LHC era.

IV. The Cosmological Frontier

As we have noted, particle astrophysics experiments have provided evidence for physics beyond the Standard Model. The first evidence for neutrino masses came from solar and atmospheric neutrino detectors. Reactor neutrino experiments (KamLand) have recently

confirmed neutrino oscillations, and long-baseline neutrino experiments will soon provide more detailed measurements of the parameters of the neutrino sector. Large-scale structure from the Sloan Digital Sky Survey has recently placed stringent upper bounds on the sum of the light neutrino masses, and future weak lensing measurements will provide even tighter bounds.

Second, astrophysics and cosmology have provided strong evidence for the existence of non-baryonic Dark Matter, for which particle physics theory has provided a number of candidates, including the lightest supersymmetric particle. These developments motivated direct dark matter detection experiments, such as CDMS, as well as accelerator experiments to discover supersymmetry.

Third, distant supernova measurements, as well as the combination of cosmic microwave background (CMB) and large-scale structure surveys, have found that dark energy is causing the expansion of the universe to accelerate. This may be a signal of quantum vacuum energy, or of a new ultra-light particle, or of the breakdown of General Relativity at large distances, perhaps associated with extra dimensions. In any case, the implications for fundamental physics are likely to be profound. Future cosmology experiments, including SNAP/JDEM for supernovae as well as the development of new dark energy probes such as weak lensing and cluster abundances, will provide more powerful probes of the dark energy and begin to discriminate between the theoretical possibilities. At the same time, accelerator experiments will contribute by constraining the scale of extra dimensions and by probing physics models that must incorporate dark energy.

Fourth, the patterns of CMB temperature anisotropy and of the large-scale distribution of galaxies point to an early universe origin for structure, indicating new physics perhaps at the grand unification scale. The most popular theory for structure formation involves quantum fluctuations generated during inflation, an early period of rapidly accelerated expansion. Other possibilities, for example colliding branes in higher dimensions, are even more exotic. It may be possible to test these ideas, and probe physics at these very high energy scales, in future CMB polarization experiments.

Fifth, the origin, nature, and spectrum of the highest energy cosmic rays, with energies around 10^{11} GeV, remains a puzzle; the explanation may require physics beyond the Standard Model. The Pierre Auger Observatory will address this issue and may also provide new insights into the neutrino sector.

Fermilab is actively involved in pursuing each of these signatures of new physics, in several cases through both accelerator and astrophysics experiments. We can expect significant experimental progress on each of these questions in the years up to 2020.

V. Summary

Given that the present decade will almost certainly lead to replacement of our current understanding of nature, the Standard Model, by something new, Fermilab must position itself to be active in all aspects of the coming revolution in High Energy Physics.

Fermilab, even after the Tevatron era, will be an essential component helping to resolve the meaning and significance of, the discoveries anticipated in the forthcoming decades.

The first glimpse of the new physics may occur at the Tevatron, and certainly at the LHC.

Fermilab can optimally contribute to the future of the field at the energy frontier by becoming the site of a Linear Collider.

As revolutionary new discoveries emerge at the energy frontier, theorists will play a central role in interpreting their significance and implications. For example, theorists will give precise predictions for novel channels in b-physics and kaon physics that are sensitive to the new states. Theoretical particle physics, in the area of developing fundamental ideas and model building, perturbative QCD, lattice gauge theory and flavor physics, will remain of paramount importance in the future in interpreting and sorting out the new laws of physics.

The coming revolution will call for the detailed elaboration and analysis of new scenarios for beyond-the-Standard Model physics. This involves creative model building, as well as the detail mining of new signatures, and the implications for other arena, such as quark flavor physics and neutrino physics. This will place heavy demands upon phenomenologists in an era when many universities have ceased to be meaningfully engaged in phenomenology altogether. Fermilab is a world leading center for theoretical physics, with exceptional strength in the most relevant areas of phenomenological theory. The Theory Group will continue to play its fundamental role in advising and directing the Fermilab program.

The Tevatron, the LHC and the LC will afford the most robust handles possible to understand all the fundamental mysteries of the universe. The coming revolution of the physics at the TeV scale will provide answers to many fundamental questions such as the origin of mass, the origin of matter-antimatter asymmetry, the essence of dark matter, the nature of grand unification and its relationship to the physics at the Planck scale. The LHC will revolutionize our description of nature, and will determine the pathway to a grand synthesis of all phenomena. A TeV scale LC will multiply the LHC potential by a significant factor and add unique capability necessary to resolve the answers to these fundamental questions. Fermilab should play a leadership role as a center of US-LHC and LC physics.

The neutrino sector holds the promise of sensitivity to the highest mass scales and most basic issues in particle physics. Fermilab can pursue a world-class evolutionary program to exploit the many opportunities in this area of physics. Such a program would dovetail well with questions posed by cosmology and the observational study of the early universe. This new physics is in its infancy. It is difficult to imagine that neutrino physics will not be one of the major stars of the next decade, together with the new physics of electroweak symmetry breaking.

Astrophysics will provide new probes of fundamental physics that complement accelerator experiments. This field is undergoing a remarkable period of exciting advances and growth that should continue well into the next decade.

Studies of rare-decay and forbidden flavor physics in the strange, charm and bottom quark sectors will help unravel the mysteries presented by the first evidence of new physics, and provide key tests of new scenarios for new physics. A diverse set of possibilities for the future use of the existing Fermilab facility in quark flavor physics exists. In addition, there may arise interesting low energy applications of the facility in conjunction with the construction of a proton driver.

3. The Linear Collider

3.1 Introduction

Fermilab Director Michael Witherell, in his June 12, 2001 statement to the HEPAP Subpanel on Long Range Planning for U.S. High-Energy Physics, said: "The subpanel should recommend construction of a Linear Collider in the U.S., built as an international project, with the optimum technical design." The Director continued, and repeated at the Annual Fermilab Users' Meeting, "We propose to the U.S. and to the international HEP community that we work together to build a Linear Collider at or near the Fermilab site." The subpanel then stated within its report, dated January 2002, "We recommend that the highest priority of the U.S. program be a high-energy, high-luminosity, electron-positron Linear Collider, wherever it is built in the world. This facility is the next major step in the field and should be designed, built and operated as a fully international effort. We also recommend that the United States take a leadership position in forming the international collaboration needed to develop a final design, build and operate this machine…" Similarly strong statements were issued by advisory panels in both Europe and Asia, solidifying the worldwide consensus on the need for a Linear Collider. In early November 2003 the DOE released its twenty-year facility plan for the Office of Science, listing a Linear Collider as its highest priority mid-term project.

Following issuance of the Director's and the HEPAP Subpanel's statements several actions were taken at the laboratory, national, and international levels. Fermilab expanded its program of R&D aimed at a future Linear Collider. The primary involvement is currently with the U.S. based Next Linear Collider (NLC) Collaboration. Within this collaboration Fermilab holds major responsibilities for the development and fabrication of accelerating structures, and for siting studies. Approximately $3M per year is devoted to these activities—roughly 15% of the U.S. total investment in NLC R&D. Fermilab also remains a member of the TESLA collaboration, although the level of effort at this time is small.

On the national level the U.S. Linear Collider Steering Group (USLCSG) was formed. This group provides overall direction for the U.S. R&D program and is charged to develop the U.S. bid to host a Linear Collider. In parallel, an International Linear Collider Steering Committee (ILCSC) was formed, under the auspices of the International Committee on Future Accelerators (ICFA), to coordinate efforts across national/regional boundaries to bring a Linear Collider to reality. Fermilab has representation on both of these groups. Analogous (to the USLCSG) regional steering committees have also been formed in Europe and Asia.

Working in concert, the regional and international committees have defined performance goals for a Linear Collider. These are summarized here (and are available at http://www.fnal.gov/directorate/icfa/LC_parameters.pdf):

- Initial maximum energy of 500 GeV, operable over the range 200-500 GeV for physics running.
- Equivalent (scaled by 500 GeV/√s) integrated luminosity for the first four years after commissioning of 500 fb^{-1}.
- Ability to perform energy scans with minimal changeover times.
- Beam energy stability and precision of 0.1%.
- Capability of 80% electron beam polarization over the range 200-500 GeV.
- Two interaction regions, at least one of which allows for a crossing angle enabling $\gamma\gamma$ collisions.
- Ability to operate at 90 GeV for calibration running.
- Machine upgradeable to approximately 1 TeV.

We worked within the context described above, establishing two goals for the deliberations: First, to understand the ramifications of successfully competing to bring the Linear Collider to northern Illinois and to make recommendations on the steps that should be taken to assure the strongest possible Fermilab presentation within the U.S. bid to host: Second, to understand Fermilab's role in gaining approval for an internationally based Linear Collider, to outline options for Fermilab involvement in construction and operation (for both Illinois and non-Illinois sites), and to make recommendations on the scope of laboratory effort that should be devoted to these activities. As the discussions evolved they focused most strongly on understanding what is required to establish Fermilab as the most attractive host laboratory for a Linear Collider. Fermilab's role in a Linear Collider project located outside Illinois was also discussed, but with less emphasis.

This chapter describes the results of the LC discussions, culminating in our conclusions and recommendations to the Director.

3.2 LC Physics Opportunities

The physics program of the Linear Collider is thoroughly documented in the TESLA TDR (hep-ph/0106315), the NLC Resource Book (hep-ex/0106055, hep-ex/0106056, hep-ex/0106057, hep-ex/0106058), and the ACFA/JLC report (hep-ph/0109166). Brief summaries are contained in an earlier report to the Directorate (hep-ex/0107044) and in Chapter 2 of this report. Here we review some of the highlights.

With the envisioned luminosity the LC will produce tens of thousands of (Standard) Higgs bosons. This is enough to prove (or disprove) that the putative Higgs gives mass to the W and Z, *i.e.*, that it breaks electroweak symmetry. If decays into fermions are competitive (in the Standard Model this requires that the Higgs mass lies below below $\approx 2M_W$), then LC measurements can also verify that this particle gives mass to quarks and charged leptons.

The LC will enrich the observations of the LHC in crucial ways. A clear example pertains to the spectrum of superpartners. The LHC measures mass differences well, but it can only obtain crudely the mass of the lightest superpartner (LSP). The LC, on the other hand, can measure the LSP mass with great precision in pair production, thereby anchoring the whole tower of superpartners. Furthermore, LC measurements can distinguish between different theoretical interpretations of LHC discoveries. For example, with supersymmetry (as with any symmetry), couplings remain constrained despite spontaneous symmetry breaking. Simulations show that at the LC one reaches the precision needed to distinguish models with supersymmetry from models with extra spatial dimensions or new strong dynamics. The interplay of LHC and LC continues to be avidly developed (see http://www.ippp.dur.ac.uk/~georg/lhclc/).

The precision available at the LC provides many additional physics opportunities. Examples include the capability to

- identify dark matter. LC measurements of a weakly interacting massive particle (WIMP) can provide information to compute dark matter annihilation cross sections, and determine the thermal relic density assuming SM evolution of the universe for comparison with dark matter density results from cosmology. This will close the circle on the laboratory identification of dark matter.
- test ideas of unification by using the renormalization group to evolve measurements of couplings and masses (from LC and LHC) up to very high energies.
- explore the first hints of Planck scale physics. The evolution of the masses and couplings also elucidates the mechanism of supersymmetry breaking, possibly probing Planck-scale operators.

This brief synopsis supports the conclusion stated in Chapter 2 that the opportunities afforded by the Linear Collider are rich, of profound importance, and extremely exciting, addressing central issues common to both particle physics and cosmology. Fermilab should play a leading role and position itself to serve as host for the LC.

3.3 Accelerator R&D and Siting Studies

3.3.1 Plans for Linear Collider R&D at Fermilab through the LC technology selection

Fermilab plans to continue to operate the RF structure factory in the Technical Division in FY04 to complete the X-band RF structures required for the 8-pack test at SLAC. These FXC and FXD structures will be the first high gradient structures that Fermilab has produced with high order mode wave-guides. Following delivery of these structures, the Technical Division will continue with alternative RF designs of X-band structures and continue efforts on NLC main linac girder design. In parallel with this activity Fermilab will continue R&D at the Fermilab-NICADD Photoinjector Laboratory (FNPL) including the development of a SCRF 3rd harmonic cavity and on SCRF infrastructure appropriate for a superconducting-linac-based Proton Driver. Fermilab will also continue its collaboration with SLAC to study LC sites, based on warm or cold technologies, in both Northern Illinois and California. Considerable effort will also go into the USLCSG warm/cold comparisons leading up to the technology choice. In addition to these activities Fermilab, along with national and international partners, will start to define the goals and configurations of both a warm and a cold Linear Collider Engineering Test

Facility. This effort should be targeted at positioning Fermilab to be the site chosen for such a facility. In addition, Fermilab should support efforts by the USLCSG and ILCSC to establish a Linear Collider Global Design Organization (GDO) and should be considered as a potential site for the GDO Central Management Team.

3.3.2 Linear Collider R&D plans beyond the technology choice

It seems unlikely that governments will commit to a multi-billion dollar LC project without a complete and successful integrated systems test, including both a demonstration of the underlying technology in an operational setting, and fabrication of components in industry that meet LC performance and cost targets. It has yet to be established whether such a systems test would be based on a single, stand-alone facility, or a suite of facilities. In either event we refer to this activity as the "Engineering Test Facility" (ETF). Following the LC technology choice, Fermilab should focus on working with national and international partners to establish goals and then complete a Conceptual Design Report (CDR) for a Linear Collider Engineering ETF for the chosen technology. Siting the ETF at Fermilab would build local expertise in critical LC technologies, enabling Fermilab to be a strong partner in LC. Should Fermilab become host to the LC, this expertise will be critical. In the event of a decision that the technology be cold, an ETF sited at Fermilab might have a large overlap with development of a SCRF-based 8 GeV Proton Driver described elsewhere in this report. In any event, Fermilab will focus its accelerator R&D efforts in the Technical and Accelerator Divisions on the chosen LC technology. It is likely that Fermilab will select one or two large LC subsystems (e.g. main linac, damping rings, or sources) on which to focus its efforts and that other major LC systems (final focus, positron source, RF power source, etc) will become the focus of other collaboration partners.

3.3.3 Fermilab LC R&D Plans during Construction and Operation of the ETF

The principle role we expect the ETF to play is as a complete engineering systems test of the underlying technology for the Linear Collider. Assuming that the ETF is built at Fermilab, its installation and operation will be major activities until the construction of the full-scale LC machine is well advanced. Once an ETF is completed it would be operated initially, for at least a year, to verify its performance and reliability. It seems likely that in later years the ETF would remain a test bed for production LC components, continuing operations at least until the full LC is built and commissioned.

In parallel with construction and operation of the ETF, Fermilab will focus on preparing a "bid to host" the LC. In addition, as a member of the international LC Collaboration, it would work with industry on full-scale development of production LC components, on gaining project approval, and on establishing the LC laboratory on the selected site.

3.3.4 LC Accelerator R&D Resources

The current level of support for LC accelerator R&D at Fermilab is about $3M per year. The LC accelerator R&D effort in FY05 is expected to be similar to FY04. Ideally LC R&D would double in FY06. Construction of ETF in parallel with other LC accelerator R&D activities will require a further significant build up of Fermilab resources with approximately one third of laboratory effort devoted to LC support at the start to physical construction, assuming an Illinois site. We would imagine an effort up to 50% of the base laboratory budget for the duration of construction. Taking into account the funding of other Linear Collider efforts, which would result in activity at the host site, the activity would dominate the laboratory. In the event the Linear Collider were sited in the U.S. but outside Illinois the Fermilab effort would be perhaps 20% of the laboratory. If a Linear Collider were under construction outside the U.S. we imagine the effort being at the 10% level.

3.4 Detector R&D

LC Detector (LCD) R&D at Fermilab is carried out in collaboration with the LCRD (Linear Collider R&D – DOE sponsored) and UCLC (University Consortium Linear Collider – NSF sponsored) university groups and with laboratories and institutions in Europe and Asia who are involved in LCD R&D. The impetus for LCD R&D at an early stage is laid out in the international report, http://blueox.uoregon.edu/~lc/randd.ps(pdf). Noteworthy R&D topics that require priority include reducing the mass of Si pixels to reduce photon conversion backgrounds and multiple scattering for improved momentum resolution, designs and tests of calorimetry and software to understand the ability of energy-flow algorithms to achieve unprecedented jet-jet mass resolution, and enough R&D on large systems to assure their success by the time they have to be built. Such studies require software simulation tools that are being developed globally with US participation.

Fermilab's facilities and personnel are resources for the American and worldwide LC efforts. The modes of engagements in the LCD R&D programs range from the use of unique expertise of individuals on the staff to collaboration on a fairly broad scale between several Fermilab people (scientists and engineers) and others in North and South America, Asia and Europe. The specialized Fermilab facilities that are important to LCD R&D efforts worldwide include:

- Computing & Software Development: simulations, pool electronics, data acquisition and on-line systems support, data analysis systems, and networks;
- The Meson Test Beam Facility (MTEST/MBTF)
- ASICs Design and other electronics engineering support
- Mechanical Engineering support;
- Specialized equipment for detector construction. Fermilab acquires such equipment for its program and is continually evolving it to suit the program's needs. Current examples of such equipment includes:
    1. Lab 8 Gerber & Thermwood precision routing machines, polishing, coating, etc.;
    2. Lab 6 Scintillator and wire chamber R&D and production facilities;
    3. Lab 5 Scintillator Extrusion Facility (w/NICADD);

4. SiDet Silicon Facility for silicon R&D/production;

There are five areas of LCD R&D in which Fermilab is presently, or could become, involved. We expect to participate in two or three R&D efforts and the MTEST activities, selectively based on: available R&D funds and facilities, the LC schedule and needs, synergy with other detector work, and the interests and abilities of the Fermilab scientific staff. The areas are:

- detector and physics simulation software, which may lead naturally to calorimetric energy-flow studies and predictions for beauty and charm tagging efficiency and purity;
- pixel R&D including thinning CCDs and active pixels, and ASICs development for such detectors plus digitization and readout for pixels and other detector technologies;
- scintillator R&D associated with calorimetry and muon systems, fiber readout and the NICADD purchased scintillator extrusion machine;
- high field (4-5T) superconducting magnet studies;
- the MTEST beam facility.

The detector R&D phase needs to be completed by about the end of the current decade to support operations of a LC in the following decade. Providing a few cycles of design, prototyping, and testing requires that Fermilab ramp up detector R&D efforts over the next four years to a full complement of about 50 FTEs (physicists, engineers, computing scientists and technicians) that will be needed to carryout the studies outlined above. We assume that materials and services will be available for use by the detector R&D teams on a similar timescale.

3.5 Organization, Resources, and Governance

3.5.1 Organization

Fermilab's Linear Collider accelerator R&D effort is currently centered in the Technical Division, with participation from both the Accelerator Division and the Facilities Engineering Services Section. The laboratory has a designated coordinator for these activities who resides within the Technical Division. This designation also carries with it responsibility for liaisoning with outside institutions engaged in LC accelerator R&D. Detector R&D is centered in the Particle Physics Division with participation of the Computing Division and outside universities. Again there is within the Particle Physics Division a set of designated coordinators for these activities. Oversight is provided from within the Directorate. The role of the Directorate will need to move from oversight to more direct coordination as Fermilab initiates a more aggressive program aimed at establishing a leadership role for Fermilab in LC R&D.

3.5.2 Resources

Current resources devoted to room temperature Linear Collider R&D at Fermilab amount to roughly $3M on accelerator, and $0.5-$1M on detector R&D. If one takes credit for the entire effort being invested in superconducting RF R&D the accelerator amounts roughly double. However, the SCRF effort is currently not directed exclusively toward a Linear Collider.

As described in sections 4.3 and 4.4 we believe that establishing a leadership role in Linear Collider R&D requires a significant increase in invested resources. We have tied our estimated requirements to the initiation of ETF construction, assuming it proceeds at Fermilab, and to the start of LC construction, again assuming Fermilab is successful in a bid to become the host laboratory. Under these conditions we see resources growing to about $20M at the start of ETF construction, and to about $100M at the start of LC physical construction. At this level the LC would represent roughly 1/3 of the full laboratory effort. We believe this total investment of resources should be weighted heavily in favor of accelerator activities. During the peak construction years we expect the fraction of laboratory resources devoted to the Linear Collider would be even higher, perhaps 50%. The details here depend strongly on the global financial model established for funding the Linear Collider.

If the Linear Collider is not sited in northern Illinois, we believe Fermilab's investment would be less, in recognition of the laboratory's responsibility to continue to provide forefront high-energy hadron-based beams within the U.S. We imagine that if a Linear Collider is sited in the U.S., but not in Illinois, Fermilab will remain a major player and will be investing roughly two thirds the amounts listed above in support of Linear Collider construction activities. In the event the Linear Collider is sited offshore we would imagine Fermilab playing a less major, but still significant role, perhaps at the level of one-third the above-described support.

3.5.3 Governance

Recently groups in Europe and Asia completed studies of LC international organization. The Americans have also initiated a study, under the auspices of the USLCSG, which is near completion. All studies espouse the notion that the Linear Collider will of necessity be an international project, provisionally denoted the Global Linear Collider Project (GLCP), from inception and conclude that while the LC should be located near an existing laboratory, its organization should be independent. For the sake of discussion we refer to the European ("ECFA") model, available at
http://committees.web.cern.ch/Committees/ECFA/Cern03KalmusReport.pdf,
as representative of the consensus emerging among the three major regions that are likely to be the main contributors to the Linear Collider.

One major difference between the host and other collaborating nations is the proportion of financial commitment to the project. Within the ECFA model, the host nation/region pays a premium of about 25% of the construction cost and the balance is divided according to the GDP of the Member States including the host nation. This implies, in the current world economic situation, that the U.S. would provide slightly over half (~55%) of the total cost if it were the host, and just under 30% if it were not the host.

The GLCP is viewed as a "time-limited project", that is, it is considered to have a termination procedure that would be invoked at some point. It relies for significant infrastructure and services on a well-defined relationship with an established "host lab." The actual governance is through an international council organized on a regional basis.

Thus, the host lab role, while large, is less prominent than many U.S. physicists might have envisioned. One advantage of having a "host laboratory" is "to minimize the overhead element of the GLCP and to ensure that the full range of necessary services is available locally and does not have to be built up from a zero base." This arrangement

reduces "the cost of the ultimate closure of the GLCP by ensuring that facilities owned by it are kept to a minimum." The model also states "The GLCP and the Host Laboratory must be financially and managerially independent of each other." This model strongly implies that the host laboratory retains a diverse program, of which the "hosting function" is only one element, so that it has the opportunity to continue to exist after the GLCP is terminated.

3.6 Fermilab as Host Laboratory and a Plan of Action

Fermilab and the surrounding region have many attributes that can make it the most attractive host laboratory for a Global Linear Collider Project. Among these attributes are:

- Fermilab
  - Scientific and engineering expertise in forefront accelerator and detector technologies.
  - Significant experience in the construction and operations of large accelerator based projects.
  - The leadership mantle of high energy physics in the United States.
- Northern Illinois
  - A strong scientific base, including two national laboratories and five major research universities.
  - Geology ideally suited to a Linear Collider.
  - A transportation and utilities infrastructure system that could support Linear Collider construction and operations.
- United States
  - The wealthiest nation in the world with a tradition of undertaking cutting edge scientific projects that challenge the imagination.

In this section we describe the steps the subcommittee believes need to be taken for Fermilab to establish itself as the preferred host laboratory for a Global Linear Collider Project.

3.6.1 The U.S. as Host to the Global Linear Collider Project

Any successful U.S. bid to host an international science project such as the Linear Collider must acknowledge and address issues that are likely to be important to the international community, including:

- the need for steady, secure, and reliable funding
- ease of access to the United States for scientists from potentially every nation
- availability of work permits for spouses and other relatives accompanying international participants
- willingness to adapt to various international laws and standards that differ from U.S. standards
- willingness to divide the benefits of the project equitably among all nations based on their contributions, including contracts, positions, and scientific credit
- willingness to share decision and policy making positions equitably with other nations

If the GLCP is to go forward these issues must be addressed and resolved at very high levels of government. The U.S. High Energy Physics community must be active in explaining the issues and arguing for appropriate solutions. If Fermilab wishes to be the host institution it should take a leadership role in this effort. The resulting arrangements will benefit not only the LC, but also all future major international science projects sited in this country.

3.6.2 Fermilab Institutional Changes for Hosting the Linear Collider

Given the ECFA model for the Host Laboratory, the main change at Fermilab would likely be the establishment of an internal Linear Collider organization and the formalization of its relationship to the laboratory divisions and sections. Additional resources would have to be made available to staff the subcommittees and working groups needed to coordinate host laboratory and GLCP activities. DOE and NSF oversight of the U.S. part of the Linear Collider Project would be carried out through involvement and participation in the various oversight and management groups of the form shown in the ECFA model. Oversight of Fermilab's host role would be subject to the normal oversight of Fermilab since that role would be organized as part of the lab structure. Given the large number of foreign visitors and employees associated with the LC, Fermilab would almost certainly need to expand its Users' Office and visitor support.

Measures that can be taken immediately to prepare for this role include: 1) demonstrating U.S. and Fermilab reliability by meeting all commitments to the LHC project; 2) demonstrating an awareness of international issues in all our dealings with international partners on existing and planned Fermilab experiments; and 3) serving as an excellent and sensitive regional partner in the "Americas" region in the development of the Linear Collider project. Everything we do should demonstrate our awareness of international issues and serve as a learning experience for mastering the nuances associated with them.

Outreach

We usually think of outreach as applying to the general public. However, in this case outreach must be directed at several different constituencies, including

- The Federal government, including both the Congress and the Executive Branch
- The Illinois State government
- The High Energy Physics Community
- The scientific community beyond high energy physics
- Local universities, businesses and laboratories
- The communities nearby the Linear Collider site and Fermilab
- The broad public, especially young people

The Linear Collider is among the largest scientific projects every contemplated. Convincing a risk-averse government to support such an expensive project will be a challenge. Lingering skepticism from the SSC experience will require that we be meticulous in our arguments about scientific necessity, cost estimates, schedules, and technical solutions. Some in our government will see the international aspects of the project as representing additional complications and risks, while others will see them as opportunities. It will take a determined and broad "outreach effort" to convince all parties that the project is intellectually exciting, scientifically justified, technically sound, and can be carried out on cost and on schedule.

In addressing our national government we need to acknowledge that this research is expensive and it may not be viewed as urgent in any societal sense. We have to make our case that it is an obligation of a technically advanced society to do this kind of forefront research, and that it has an impact in exciting the minds of the public, especially the

young. It can be a "first attractor" of students to science, even if they go into areas other than particle physics.

We must also convince the government and ourselves that we will construct the project on budget, on schedule, and that it will meet its technical and scientific goals. We must demonstrate that we are willing to make a substantial contribution from our so-called "base program" to reduce the incremental costs of the project to the taxpayer. We have to show that we are willing to invest significant effort and resources in R&D without any guarantee that the project will go ahead or that it will be hosted in the U.S. We have to craft our message carefully and then enlist support, starting with our state government and Illinois Congressional representatives.

Genuine support from the international community will be critical to the success of a GLCP hosted by the U.S. Officials from the U.S. Government will have to work with their counterparts in other governments in order to establish the basis for trust and open cooperation needed for scientific participation in the U.S., even with the current backdrop of ongoing world-wide tensions.

While the U.S. high-energy physics community has endorsed the LC, only a modest fraction is currently actively participating. We need to broaden that participation, and win support even from colleagues whose chosen research program may be threatened by the investment in the LC. The Linear Collider will require from both Fermilab and the U.S. community a high degree of commitment to succeed.

The need to work on outreach to the broader scientific community is especially important. Federal government decision makers look for support in the larger scientific community for large-scale science projects. They will ask if the project has the potential for impact beyond high-energy physics. It is the scientific community outside HEP that has the knowledge to judge the scientific validity or technical viability of the LC and their opinions will be sought. We must improve our interactions with other disciplines – starting with the rest of the physics community– and, if possible, find ways to have them participate and benefit from this project.

Local universities, businesses, and laboratories can be excellent allies in the general outreach effort and can be essential contributors to the project. We have several prestigious and well-connected universities in this area. We also have Argonne National Laboratory as a potential contributor to the construction effort. And we are in the center of a high-tech area. This accumulation of scientific expertise supports Fermilab's aspiration to be the host lab and, if these institutions assist us in the outreach effort, we will benefit from their extensive contacts. We have been able to collaborate with local universities in accelerator R&D through ICAR and NICADD and this should provide a good basis for collaborating on the Linear Collider.

Outreach to our neighbors around the lab and near prospective construction sites is also crucial. Fermilab's arts programs, onsite recreation opportunities, "Ask a Scientist", Saturday morning physics, and other education and outreach activities have gone a long way toward building good community relations. However, areas of tension remain and we must continue efforts to address them through the Joint Community Task Force. Fermilab must work with people likely to be affected by the LC construction so that we propose a site that is acceptable to them.

Inreach

As an electron-positron collider, the LC represents a new direction for Fermilab. Fermilab has been engaged in physics with hadron beams for our entire history and we have developed a particular viewpoint of the value of hadron collisions that makes it hard for many to embrace the Linear Collider as the next large project. There are many aspects of this problem, some based on scientific arguments and some rooted in gut feelings. Some fear that hosting a Linear Collider at Fermilab (or even in the U.S.) will make it impossible to carry out other interesting lines of research in neutrino or heavy quark physics, or delay other prospective projects. The change in direction demands a new perspective in which the physics frontier lies in high precision measurements rather than in the highest energy collisions. Fermilab cannot succeed in the LC effort without the enthusiastic support of its staff.

Support is increasing with the growing recognition that the LC will be central to elucidating the physics of the TeV scale. The concern that the physics case for the LC is not ironclad, and needs the early results from the LHC, is gradually subsiding as it becomes apparent that funding and technical considerations make it unlikely construction will be initiated before those results become available. Nonetheless, we should try to redirect discussion from the "energy frontier" to the "physics frontier", which we define as the place where our knowledge of physics ends, or at least becomes blurry. Full support of our staff will require a model for hosting the LC that, like the ECFA model, allows for, and in fact embraces, the idea that the host has a diverse program including components apart from the Linear Collider. It will also require that the LC be seen as the "next step" rather than as the "last step" for particle physics. Perhaps most important, full support will require a concrete plan for making the LC a reality.

3.6.3 A Plan of Action for Fermilab

Any Fermilab aspiration to become the host laboratory for a GLCP must be rooted in the establishment, both in actuality and in perception, of the necessary credentials for Fermilab (along with Illinois and the U.S.) to serve as host. We identify as the primary prerequisites:

- Demonstrated capabilities in Linear Collider technologies, including establishment of an intellectual leadership role
- Demonstrated organizational and management capabilities
- Identification of an excellent local site
- Institutional enthusiasm for the role

Of nearly equal importance will be establishing support from all affected stakeholders including: our neighbors, federal, state, and local governments, local universities and laboratories, and the U.S. and international science communities. And finally, it will be necessary for Fermilab to understand the parameters associated with the host laboratory role and how these are integrated within the laboratory's future programs.

With this in mind, we suggest that a strategic plan for establishing Fermilab as the preferred host lab for a GLCP incorporate the following elements:

- Commitment and leadership at the highest levels of Fermilab management to establish Fermilab as the preferred host.

- Development of Fermilab capability to provide technical leadership on the Linear Collider construction project, within the framework of international collaboration.

  This includes engagement in the critical accelerator technology issues and demonstration project(s). We suggest identifying a few (perhaps two) areas in which to concentrate accelerator physics effort with the goal of establishing leadership. Examples could include the main linac, the damping rings, and/or sources, areas where the laboratory currently has relevant skills and expertise. Further, we would suggest assuming leadership on the development of, with aspirations to host, the major technology demonstration project for the LC, provisionally denoted ETF.

  Within the area of detector R&D we suggest targeting a few critical areas in which we have special capabilities. Examples might include vertexing and tracking, calorimetry, and muon identification. In addition Fermilab has a unique resource in its test beams, which should be made available for LC detector R&D.

- Identification of a preferred Illinois site and development of a siting plan.

  This will require establishment of collaborations with local research institutions, and state and local governments. We suggest retaining close collaboration with those within the U.S. LC effort exploring sites outside of Illinois.

- Establishment of a realistically achievable timeline for construction and operation of a Linear Collider, in cooperation with the USLCSG and ILCSC.

- Maintenance of a strong Fermilab presence within the USLCSG and ILCSG (and their successors).

- Preparation of a bid to host the GDO Central Management Team.

- Strengthening of the Fermilab presence within the American Linear Collider collaboration.

- Development of an outreach plan addressing the following constituencies:
  - Local communities and governments
  - State government
  - Local universities and laboratories

- Establishment of a model for interaction between Fermilab as host laboratory and the Global Linear Collider Project consistent with the evolving view of the international community.

  This would include defining the preferred relationship between Fermilab (as host lab) and the international project organization, including roles and responsibilities, authorities, and the scope of work Fermilab would imagine undertaking. It is important that this model also establish a vision as to the appropriate balance between the ongoing hadron-beam-based program at Fermilab and the Linear Collider program during both the construction and operations phases.

3.7 Conclusions and Recommendations

The Linear Collider program promises to be extraordinarily exciting. It will offer studies of electro-weak symmetry breaking and the new physics that accompanies it, be it supersymmetry, extra dimensions, new strong dynamics or something else that we have not imagined. These measurements will probably be the key to distinguishing among these scenarios and may, thereby, also enable us to establish the identity of dark matter. The opportunity represented by the Linear Collider needs to be aggressively pursued, no matter where it might be sited.

Fermilab is in many ways the natural host for this extraordinary machine. It is unarguably one of the world's great laboratories for high-energy physics. The physics of the LC is a superb match to the lab's current program at the physics frontier. The LC would be one of the two largest accelerators ever brought into operation, and Fermilab would bring to it a superb technical staff, construction infrastructure, and long experience managing large projects. Preliminary site studies are promising, and the surrounding community has been generally supportive of new Fermilab projects. The establishment by Fermilab of the Linear Collider as the centerpiece within a balanced program for its long-range future will significantly enhance the prospects of such a facility being constructed.

Fermilab will host the Linear Collider only if the U.S. is attractive as a host country. A successful U.S. bid to host an international LC project must confront issues that are likely to be important to the international community, including: the need for secure and reliable funding; arrangements that enable physicists and their families to visit and live in the U.S. for extended periods without undue obstacles to obtaining visas and work permits; willingness to divide the benefits of the project equitably among all nations based on their contributions; and willingness to share decision and policy making positions equitably with other nations.

Fermilab will have the greatest chance of hosting the Linear Collider if it makes a strong and visible commitment to this goal. To succeed, Fermilab will require support from the international high-energy physics community, our colleagues in other sciences, our congressional representatives and national leaders, as well as numerous public servants in Washington, and our neighbors. We can hope for their commitment to the LC only if the lab and its staff are willing to commit to it wholeheartedly themselves. A bid to host the Global Design Organization Central Management Team would be an early sign of Fermilab's commitment.

The Fermilab program beyond the LC should be exciting and substantial and, because of uncertainties associated with the LC, flexible. However, this program must also be compatible with realistic funding scenarios.

Fermilab should act swiftly to develop its capability to provide technical leadership on the LC construction. The lab should engage in the critical accelerator technology issues. This effort should concentrate on approximately two areas that are chosen to be central to the LC and well matched to the lab's interest and expertise. Suitable possibilities might be the damping rings, the main linac, or the particle sources. Siting of a signficant technology demonstration project at Fermilab would provide a unique opportunity to develop Linear Collider expertise within the Fermilab scientific and engineering staff. The lab should assume leadership of the effort to define and develop the ETF design concept and aim to host it.

The lab should target detector R&D in a limited number of areas deemed critical to detector performance in which the lab has special capabilities. These areas might include computing and simulation, vertexing and tracking, calorimetry, muons, and test beams. This R&D should include collaborators from the U.S. and abroad.

Fermilab should also develop a LC siting plan. It should identify a preferred site in Illinois and should carry out the appropriate geological studies to establish its suitability for construction and operation of the LC. It should also establish collaborations with local institutions, state and local governments, and the surrounding communities.

Fermilab resources currently invested in Linear Collider R&D amount to approximately $4M per year. If Fermilab desires to host a Linear Collider this needs to grow significantly, by roughly a factor of five at the time of ETF construction, and an additional factor of roughly five by the start of LC construction, assuming Fermilab is the host laboratory. This investment could potentially grow by an additional 50% during the construction phase, depending upon the international financial model in place at the time. The significant majority of this funding needs to be directed toward the accelerator. In the event that Fermilab is not the host site, but U.S. is host country, we imagine Fermilab investment during the construction phase would be less, at roughly half to two thirds this level. In the event that the U.S. is not host country we imagine Fermilab investment during the construction phase at 1/3 this level.

Realization of the LC will require superb planning and coherence across the HEP community. The LC effort is currently managed nationally by the USLCSG and internationally by the ILCSC. Fermilab should strengthen its engagement with these groups. Fermilab should encourage the community to develop a realistic timeline for LC construction and operation and be an active participant in shaping that timeline.

Recently, groups in Europe and Asia completed studies of Linear Collider international organization. Both concluded that while the LC should be located near an existing laboratory, its organization should be independent of any existing lab structure. Such an arrangement will make financing and management more transparent, as desired by most governments. At the same time, it allows the host lab to retain an identity outside the LC, so that other experimental programs may be carried out in parallel with and beyond the LC program. Fermilab should engage in developing a model for its relationship with the LC project. This should include the delineation of the roles and responsibilities of the LC organization and Fermilab, the lines of authority, and the scope of work that Fermilab imagines undertaking. Success of the plan will require that it has the support of the national and international communities, and the lab should approach it with this in mind.

The Linear Collider is an expensive project, and it will come to be only if the public, our local and state communities, our national leaders, our scientific colleagues and the Fermilab staff share the excitement for the scientific opportunities. The lab should initiate outreach aimed at all of these groups.

3.7.1 Recommendations

In light of the above conclusions we recommend that:

- Fermilab reiterate its desire to serve as the host laboratory for a Linear Collider.

- A full-time person be appointed within the Directorate with responsibility for coordinating and directing all Fermilab LC activities and providing communications to outside institutions on Linear Collider. This should include both creation and execution of a strategic plan based on visible leadership and enhanced efforts in:
    - Technology R&D
    - Site studies
    - Public outreach
    - Governance models

  And incorporating
    - Establishment of a realistic timeline in consultation with the USLCSG
    - Preparation of the Fermilab component of the U.S. bid to host an international Linear Collider facility.
    - Plans for Fermilab participation in the Linear Collider in the event that the LC is sited elsewhere

- Fermilab initiate efforts to establish performance goals and develop design studies for the major test facility (ETF), or suite of facilities, required to support either a warm or cold R&D program. This should be done in collaboration with international partners, with a subsequent goal of hosting the major technology demonstration project for the chosen technology.

- Fermilab planning for a future including the Linear Collider should be based upon the host laboratory/international project model.

4. A Neutrino Program

Before Fermilab relinquishes the high-energy frontier to the LHC, it will become the leader in the world-wide effort to explore and understand the physics of neutrino oscillations. By early 2005, with both MINOS and MiniBooNE taking data, Fermilab will be able to answer some of the most pressing first-round questions raised by the discovery that neutrinos have mass. Fermilab's facilities, high-intensity neutrino beams derived from 8- and 120-GeV protons, along with the community of experimentalists and theorists engaged in this exploration, position Fermilab to lead this field, both in the sense of doing some of the most important experiments and in the sense of helping to guide the international effort. While these experiments would occupy fewer physicists than the Tevatron Collider now does, neutrino physics finds itself probing many of the most exciting current topics in our field, and Fermilab can and should have a vigorous neutrino program. In the period before it is known whether the Linear Collider would come to Fermilab, this program would focus on making best use of the existing accelerators. If the LC is not built at Fermilab, it would be high priority to extend the neutrino program. In that case the Laboratory would build on the existing facilities and mount experiments needed to address the full range of questions presented by neutrino oscillations.

Neutrino Physics

In the Physics Landscapes section of this report, we discussed the importance of neutrino physics. During the last several years, stunning experimental results have established that neutrinos have nonzero masses and substantial mixing. This development opens an exciting new world for us to explore. The Standard Model must be extended to accommodate neutrino mass terms, through the existence of either right-handed neutrinos with Dirac mass terms, or Majorana mass terms involving self-conjugate neutrinos, or both. Hence we know that this sector of the Standard Model is broken, but we don't know how it is broken. The observation that neutrino masses and mass splittings are all many orders of magnitudes smaller than those of any of the other fundamental fermions suggests radically new physics, perhaps originating at the GUT or Planck Scale, or the existence of new spatial dimensions. Whatever the origin of the observed neutrino masses and mixing, it will certainly require a profound extension to our picture of the physical world. The first steps in understanding this revolutionary new physics are to pin down the measurable parameters and to address the next round of basic questions:

Are there only three neutrino flavors, or do light, sterile neutrinos exist?

If there are only three generations, there is one angle (θ_{13}) in the mixing matrix that is unmeasured. How large is it?

Which of the two possible orderings of the neutrino mass eigenstates, the so-called "normal" hierarchy or the "inverted" hierarchy, applies?

There is one quantum-mechanical phase, called δ, in the mixing matrix that is accessible to neutrino oscillation measurements. If both θ_{13} and $\sin \delta$ are non-zero there will be CP violation in the lepton sector. Is this phase non-zero?

What precisely are the values of the neutrino masses? Are neutrino masses of the Majorana type, the Dirac type, or both?

With the exception of the last set, all of these questions can be addressed by accelerator-based neutrino oscillation experiments. The answers to these questions will guide our understanding of what lies beyond the Standard Model, and whether the new physics provides an explanation for the baryon asymmetry of the Universe (via leptogenesis), or provides deep insight into the connection between quark and lepton properties (via Grand Unified Theories), or perhaps leads to an understanding of one of the most profound questions in physics: Why are there three generations of quarks and leptons? In addition, the answers may well further challenge our picture of the physical world, and will certainly have important implications for our understanding of cosmology and the evolution of the early Universe.

As well as identifying the explicit experimental goals for the global neutrino program it is also important to understand in what sequence, and on what time scale, the goals might be achieved. For the neutrino mass hierarchy or CP violation to give observable

effects in oscillation experiments, θ_{13} would have to be non-zero. Discovering that θ_{13} is extremely small would be an important result in its own right and might indicate the presence of a new conservation law at a high mass scale.

It is of greatest importance to measure θ_{13} or determine that it is so small as to be inaccessible to foreseen experiments.

The Current Program

Planning a Fermilab neutrino oscillation program must be done in the context of the experiments world-wide that are approved and likely to operate in the next few years.

KamLAND is a 1-kiloton liquid scintillator experiment which measures the flux of electron antineutrinos from a number of nuclear reactors at varying distances from the detector. They see a deficit in flux whose magnitude is consistent with the interpretation that the electron neutrinos from the Sun are undergoing oscillations.

Super-Kamiokande is a 50-kiloton water Cherenkov detector. Events in this detector from neutrinos produced in the atmosphere by cosmic rays showed an angular dependence that can be convincingly interpreted as a dependence on the neutrino baseline. This detector also refines measurements of the solar neutrino deficit. Super-K has been rebuilt and is taking data after an accident in 2001.

SNO is a 1-kt D_2O Cherenkov detector that can separately measure charged- and neutral-current interactions of neutrinos from the sun. This experiment showed that the total flux of neutrinos matches that predicted by the solar models. Thus, the deficit of electron-type neutrinos is primarily due to their oscillation to other non-sterile neutrino flavors.

K2K directs a neutrino beam produced by 12-GeV protons from the KEK proton synchrotron towards the Super-Kamiokande detector. With a mean neutrino energy of 1.3 GeV and a source-to-detector baseline of 250 km, it is designed to test the oscillation interpretation of atmospheric muon-neutrino disappearance. Initial running before the Super-K accident indicated a deficit in muon neutrinos consistent with atmospheric measurements, and running has resumed.

MiniBooNE is a 1-kt Cherenkov detector designed to test the evidence for the transition of muon anti-neutrinos to electron anti-neutrinos found by the LSND experiment. It searches for the transition from muon neutrinos to electron neutrinos over a baseline of 540 meters in a beam with a mean neutrino energy of 0.5 GeV produced using the 8-GeV protons from the Fermilab Booster. The experiment hopes to complete the initial phase of running in 2005.

NuMI/MINOS: Beginning in 2005, the NuMI neutrino beam, produced by 120-GeV protons from the Fermilab Main Injector, will be directed at the MINOS experiment in the Soudan Mine in northern Minnesota, 735 km away. With a few times 10^{20} protons on target (about one year of running), the experiment should be able to see the signature of

oscillations. The observable energy dependence of the disappearance of neutrinos from a controlled source will represent a qualitative step forward from the Super-Kamiokande atmospheric measurements and the K2K measurements with accelerator neutrinos. In a few years of running the precision in Δm^2_{23} will improve dramatically.

CNGS has very nearly the same baseline as NuMI/MINOS, but a different strategy. Where MINOS will concentrate on the measurement of the disappearance of muon neutrinos, the CNGS experiments, ICARUS and in particular OPERA, will concentrate on the appearance of tau neutrinos. The goal is to test if the neutrino oscillations in the atmospheric regime are indeed, as inferred, dominantly transitions from muon to tau type. This approach requires use of a higher energy beam (10-30 GeV neutrinos) to allow the interactions to produce tau leptons and results in a smaller oscillation probability. About ten events are expected in five years of operation beginning in 2006.

The Foreseeable Worldwide Neutrino Program

Neutrino physics is in the midst of an experiment-driven explosion in our understanding. We can chart a specific course for a future program only under the very unreasonable assumption that each step will bring clarification without surprises. Nonetheless, current understanding does point to the set of questions posed earlier in this chapter, and, with some general guidelines, a very exciting program can be proposed and pursued. An obvious guideline is that the program as a whole should be flexible enough to react to new developments or surprises. This flexibility can be achieved via several routes, among them the design of facilities with broad capabilities or a step-by-step approach that invests resources as more is learned. In the latter case, reasonable questions are, "Would it be worth building another beam pointing at this proposed detector?" or "Would it be worth building another detector in this proposed beam?"
The accelerator-based neutrino program foreseen now has a number of well-defined components:

- Improved measurements of already observed mixing parameters.
- Clarification of the short-baseline (LSND) situation.
- Measurement of θ_{13}.
- Pursuit of the mass hierarchy and CP violation.
- Non-oscillation neutrino scattering measurements.

This effort begins with the experiments discussed as the current program, and groups around the world are designing and proposing new experiments and facilities to continue the program. We summarize the components of this foreseeable program in the sections below.

Clarification of the LSND signal

The MiniBooNE experiment will provide a definitive test of the LSND result, in which they detected electron antineutrinos in a beam that was initially muon antineutrinos. The future of neutrino physics would be considerably altered by a confirmation of the LSND result.

A positive result would make essential further exploration of what might be a new type of neutrino. Indeed the BooNE program, with a second detector at a different baseline, has already been foreseen. A favored explanation of a positive result is sterile neutrinos, and confirmation of this effect would open a whole new experimental arena. It could also affect the interpretation of long-baseline measurements and would change the optimal strategies for searching for CP violation.

In the case of a negative result from MiniBooNE, the initial prejudice would be that the original LSND result had been flawed in some way and that we have no residual evidence for a signal. A more unconventional interpretation would be that the difference stems from the fact that LSND was done with antineutrinos, the MiniBooNE measurement with neutrinos. Reconciling these two requires exotic physics, such as the violation of CPT conservation. MiniBooNE has the capability to run with antineutrinos, and could do so if the physics case is strong.

Measurements of θ_{13}

The current limit on this phenomenon is provided by the search for electron neutrino disappearance in the CHOOZ and Palo Verde reactor experiments. They found $\sin^2 2\theta_{13}$ to be less than about 0.12; the limit can depend significantly on the value of Δm^2_{13}. Improved searches can be done with higher-precision ν_e disappearance measurements, or by looking for $\nu_\mu \rightarrow \nu_e$ appearance at a combination of baseline and neutrino energy corresponding to the atmospheric mass splitting Δm^2_{23}.

NuMI/MINOS

The MINOS experiment has been optimized for the measurement of neutrino oscillations through muon neutrino disappearance, and will be without rival in the precision of its Δm^2_{23} measurement. The experiment is also expected to have somewhat greater sensitivity to the appearance of electron neutrinos than existing experiments. It is expected that after a few years running a 90% confidence level limit of about 0.06 can be obtained for $\sin^2 2\theta_{13}$.

Reactor Measurements

As mentioned above, the current limits on $\sin^2 2\theta_{13}$ come from antineutrino disappearance measurements from the CHOOZ and Palo Verde experiments. Future experiments, documented by the International Reactor θ_{13} Working Group, envisage improving the sensitivity by about an order of magnitude. They would operate with two detectors, a near detector at a distance of 100-200 meters from the reactor and a second 1 to 2 km away. A significant overburden is desirable. Some advocate that the detectors be movable to improve the ability to cross-calibrate. Such an experiment would have no direct sensitivity to the mass hierarchy or CP violation, but may be important in helping to resolve ambiguities in long-baseline accelerator experiments directed at those longer-term goals.

The location and timing of the reactor neutrino experiment depend on identifying an appropriate reactor site with a cooperative owner. Some potential sites in Illinois are being investigated, and both Fermilab and Argonne National Laboratory have provided some support. If a large-scale reactor experiment were to be mounted in Illinois, presumably the two laboratories would play useful roles.

Measurements of θ_{13} and pursuit of the mass hierarchy and CP violation

Both the mass hierarchy and the CP-violating phase δ affect the amplitude of $\nu_\mu \rightarrow \nu_e$ oscillations for long enough baselines in the Earth. This is good, because it is the pathway to measuring these fundamental quantities. However, any single $\nu_\mu \rightarrow \nu_e$ measurement will typically be faced with ambiguities, different possible sets of $\{\theta_{13}$, mass hierarchy, $\delta\}$ that give the measured amplitude.

Off-axis Neutrino Experiments

For neutrinos produced in the forward direction by pion decay, the energy of the neutrino has a broad range. On the other hand, at finite angles, it follows directly from the two-body decay kinematics that it is the angle which determines the neutrino energy, almost independently of the energy of the pion. In on-axis experiments, the backgrounds to the electrons that would signify $\nu_\mu \rightarrow \nu_e$ oscillation come from the intrinsic electron neutrinos in the beam and from the neutral pions produced by higher-energy neutrino scattering, which can masquerade as electrons. The peaked spectrum in an off-axis beam reduces both backgrounds significantly. Several proposed experiments exploit this phenomenon.

T2K

The T2K ν experiment is a second-generation long-baseline neutrino-oscillation experiment. A neutrino beam generated in the J-PARC 50 GeV high-intensity (0.75 megawatt) proton accelerator in JAERI (Tokai, Ibaraki) will be directed toward Super-Kamiokande, 295 km away from the neutrino source. The physics goal of the "first phase" is an order-of-magnitude better precision in the ν_μ disappearance measurement ($\delta(\Delta m_{23}^2)=10^{-4}$ eV^2 and δ ($\sin^2 2\theta_{23}$)=0.01). The experiment also projects a search for the ν_μ to ν_e transition a factor of more than 20 more sensitive than the CHOOZ limit. As mentioned, matter effects in a long baseline measurement both enrich and complicate the interpretation. In the case of J-PARC the matter effects are less than a 10% effect.

The present planning indicates the readiness of the beam in early 2010 if the rather aggressive funding profile can be achieved. Data-taking over a period of 4-5 years would be needed to reach the above sensitivities.

The "second phase" of experimentation considers a much-larger detector, the 1-megaton-class water Cherenkov detector called Hyper-K. An upgrade of the source from 0.75 to about 4 megawatts would be needed. The design for this facility is still preliminary.

NuMI Off –axis electron Neutrino Appearance Experiment (NOνA)

A suitable "off-axis" beam can exploited by siting a detector at about the same or slightly greater distance than that of the Soudan Mine (735 km) where the current MINOS experiment is sited. The current plans were recently submitted as a proposal to Fermilab, based on a detector of 50 kilotons. The baseline design uses liquid scintillator as the active material with readout using avalanche photodiodes. The bulk of the mass would be of a cheap material with relatively low atomic number such as particleboard. In this way, it is possible to build a relatively inexpensive detector of great size that would have good efficiency for electron identification. A fairly similar alternative would use glass-based resistive plate chambers such as are employed in the Belle Experiment. The cosmic ray backgrounds appear to be tractable with little or no overburden, so a very shallow site is adequate.

Debate continues about the optimum approach; for example work is ongoing on within the NOνA group to look at a "totally active" liquid scintillator design. Others argue that the spectacular results obtained in a liquid Argon detector by the ICARUS group motivate a large extrapolation of that technology. The issue is whether the economies of scale mitigate what, with current techniques and modest mass (half-modules of 600 tons for a total detector of 3 kt), is very expensive.

The NuMI beam will start to operate in early 2005 at 0.25 MW and over a few years hopes to ramp up to about 0.4 megawatts. The approval procedure for an experiment in the $150M class would clearly require two or three years. Obtaining the funding to construct the detector, a four year process, would be the critical issue. In five years of running it is possible to observe a signal down to $\sin^2 2\theta_{13} \sim 0.01$, very similar to the sensitivity of the JPARC experiment.

The two approaches are similar and competitive. What distinguishes the Fermilab-based approach is the longer baseline. This gives considerable sensitivity to the mass hierarchy and possibly to the CP-violating parameter δ. For the most favorable values of δ, the mass hierarchy could be resolved for $\sin^2 2\theta_{13} >\sim 0.05$.

Using the same detector and the more intense beam obtainable by injecting protons from a Proton Driver into the Main Injector would make it possible to resolve the mass hierarchy down to a value of $\sin^2 2\theta_{13} >\sim 0.017$ for the most favorable δ. The $\sin^2 2\theta_{13} >\sim 0.05$ region of sensitivity to the hierarchy would then extend over nearly half of the parameter space available to CP violation. If the Proton Driver were complete by around 2015, this sensitivity would be achieved early in the following decade. In order to resolve the mass ambiguities for all values of δ, it is likely that other measurements, perhaps from a third detector at a different baseline or angle would be necessary.

Very Long Baseline Options

The long-baseline approach described above would use a narrow-band beam and hence require multiple detectors. An alternative strategy is to use a baseline of several thousand kilometers and a broad-band beam. This approach was proposed by the Brookhaven National Laboratory (BNL) in its presentation to the HEPAP subcommittee which then reported to the Office of Science in preparation for the Facilities Report of 2003. In order to achieve the appropriate sensitivity, a 1-megawatt source and a very large detector, 0.5-1.0 megatons, is suggested. The BNL white paper referred to the UNO detector, a water Cherenkov like the Super-Kamiokande detector, though it has not yet been established that such a detector has sufficient background rejection. It is generally accepted that in such an approach, the detector would be built to also perform a proton decay search, and hence deep underground.

The siting of the National Underground Science and Engineering Laboratory (NUSEL) is uncertain, and even the concept of siting all foreseen experiments at a single laboratory is up for discussion. For one of the possible sites, the Homestake Mine in Lead, North Dakota, a study of the potential for an experiment based on a Fermilab neutrino beam has been performed. The initial studies, using the canonical 0.5-1.0 megaton detector and an intense neutrino beam based on the Proton Driver, are promising but show room for further improvement. There may be some additional benefit to a still-longer baseline.

Neutrino Factory

A neutrino factory is a muon storage ring with relatively long straight sections in which the muons decay to produce intense beams of neutrinos. Neutrino factories are attractive because, when compared with conventional neutrino beams, they yield higher signal rates with much lower background fractions and lower systematic uncertainties. These characteristics enable neutrino factory experiments to be sensitive to values of θ_{13} that are beyond the reach of any other approach. Detailed studies have shown that a non-zero value of $\sin^2 2\theta_{13}$ could be measured for values as small as $O(10^{-4})$. In addition, both the neutrino mass hierarchy and CP violation in the lepton sector could be measured over almost this entire range. Neutrino Factories provide a new sort of neutrino beam containing both electron-type neutrinos and muon-type neutrinos. This provides neutrino factory experiments with a wealth of measurements that, in addition to offering exquisite precision, also offer different signatures and systematics from Superbeam experiments.

Neutrino Factories will be needed if θ_{13} is too small to enable high-intensity conventional neutrino beams to fully explore the exciting physics, or if some new phenomenon is discovered along the way that requires the precision and flexibility provided by this new type of neutrino source. Fortunately, the high intensity Proton Driver required for a "Superbeam" at Fermilab is exactly the proton source required to drive a neutrino factory. Hence there is a natural path from Superbeams to Neutrino Factories.

Fermilab is one of the three laboratories supporting the Neutrino Factory effort, hosting the MUCOOL sub-collaboration. MUCOOL is developing the technology for a muon ionization cooling channel, one of the key subsystems required for a neutrino factory. In about 10-15 years the R&D could yield a cost-effective neutrino factory design based on

proven technology, and an associated reliable cost estimate. This is about the time at which our knowledge of θ_{13} would be enhanced by results from the next round of neutrino experiments. If desired, a neutrino factory could become a central element of the global neutrino physics plan at that time.

Neutrino Scattering Measurements

Even without oscillations, neutrinos are interesting to both the particle and nuclear physics communities, and for many years neutrino beams have been used to learn about the neutrinos themselves, about the weak interactions, and about nucleon structure. The high intensity beams developed for neutrino oscillation experiments are a resource that can also be used in furthering these other studies, and new science in this area is now possible with the NuMI and Booster beams at Fermilab. The use of modern detectors permits precision determination, for example, of the axial vector form factor of the nucleon, and of coherent neutral pion production from the nucleus. These and other processes offer significant insight into the structure and interactions of the nucleon and its constituent quarks. MINERνA will be the first experiment of this type to take advantage of these Fermilab resources.

Understanding these processes better will also advance the study of neutrino oscillations. For example, coherent neutral pion production represents a serious background in neutrino oscillation experiments looking for electrons in the final state. The Monte Carlo predictions of what to expect in "far detectors" are not now backed up by detailed measurements, and this will cause a significant systematic error until more neutrino scattering data become available. In some cases, the "near detectors" for long-baseline oscillation experiments can be enhanced into more general-purpose spectrometers for neutrino physics.

The Fermilab Neutrino Program – A Strategic Plan

While as yet there has been no global optimization of the neutrino program, the potential for Fermilab to contribute is clear. The following is a reasonable set of steps for the Laboratory to take, depending on the evolution of the physics.

- Immediately, complete the MiniBooNE measurement and chart a course for further oscillation measurements with the Fermilab Booster neutrino beam if the results lead in that direction.
- Start and execute the MINOS physics program, one that will establish the oscillation paradigm and measure θ_{23} and Δm^2_{23} with better precision. MINOS will also extend the sensitivity to the key parameter θ_{13}, to $\sin^2 2\theta_{13} \sim 0.06$.
- Starting with MINERνA, establish a program of low energy neutrino cross section measurements which will provide an underpinning for oscillation measurements and also have intrinsic interest in their own right.
- Plan and mount an experiment to measure θ_{13} with a sensitivity to $\sin^2 2\theta_{13} \sim 0.01$. This could be achieved with a large, relatively conservatively designed experiment of about 50 kt of fiducial mass and incremental upgrades to the

current proton source. If $\sin^2 2\theta_{13}$ is not too small, say 0.05, this experiment could be sensitive to the mass hierarchy. An example of a design for such an experiment is the NOνA proposal. This project could be coupled with a modest upgrade to the accelerator complex to increase the proton intensity from the Main Injector.

- Construct an upgraded proton source that would increase the intensity of the NuMI beam by about a factor of 5 and enable the second stage of the future long baseline neutrino program to achieve a sensitivity to $\sin^2 2\theta_{13}$ of ~ 0.005. If the result of the first-stage experiment was an observation, this stage would explore the mass hierarchy determination and search for CP violation in the neutrino sector. This second stage experiment might begin middle to late next decade.

At this point, "long-range planning" becomes too contingent on the scientific outcomes of too many experiments to be useful. However, the Fermilab facilities for neutrino physics with a Proton Driver would provide the foundation for a variety of directions. For example, a third experiment, the second "off-axis" detector, has the potential to resolve a number of the ambiguities inherent in the $\nu_\mu \rightarrow \nu_e$ oscillation amplitude. The beam from the new Fermilab proton source, the Proton Driver, could also be used with a much longer baseline.

- With ongoing R&D on muon cooling, the mature Fermilab proton source would be the ideal source for a Neutrino Factory.

Conclusions

The path laid out above is the most obvious and straightforward. Our long-range planning thoughts of today in no way usurp the need for bold, thoughtful, and incisive decisions and initiatives in the future.

We strongly advocate a vigorous neutrino program as an exciting component of the future Fermilab program.

5. A Proton Driver

5.1 Introduction/Physics Case:

To retain the ability to develop new initiatives and to respond to new discoveries, Fermilab must invest in a large increase in proton luminosity. The most compelling example is neutrino physics.

By the middle of this decade, Fermilab will be a world-leader in the study of neutrino oscillations. **The Booster Neutrino beam and the MiniBooNE detector, the NUMI beam line and the MINOS detector are unique Fermilab assets that can be brought to bear on these questions.** MINOS and MiniBooNE will provide crucial measurements. As described in 4, these measurements will strongly indicate the path to further experiments **but it seems likely that new long baseline neutrino experiments will be needed to understand the underlying physics.** Fermilab will have the expertise, the user community, and many facilities in place to push forward with these new experiments. However, Fermilab's current proton luminosity limits the sensitivity of both existing experiments and those envisioned for the future. A series of dramatic improvements over the past few years have produced substantial increases in the intensity of the Fermilab Proton Source. An additional factor of two increase in proton intensity may be possible but fundamental limitations of the current accelerator complex, notably the 8-GeV Booster, are being reached. Parts of the existing Linac/Booster complex are also nearly 35 years old. Maintenance and reliability are becoming a serious issue with these machines. Future new long baseline neutrino experiments will require further factors of 5-10 improvements in proton luminosity. It is clear that such experiments at Fermilab are only feasible if a major proton source upgrade is undertaken. The Proton Driver project would replace the Booster with a new 8-GeV accelerator with 0.5-2 MW beam power, a factor of 15-60 more than the current Booster. It would also make the modifications needed to the Main Injector to upgrade it to provide 120 GeV beams of 2 MW.

Many upgrades to the Proton Source and Main Injector needed for a Proton Driver project are such that they could be executed in the near term and would serve to maximize the output of the current Booster. Carrying them out as soon as possible would greatly benefit the current neutrino program. Beyond that, the future of accelerator-based neutrino oscillation physics lies with megawatt-class facilities and the new large detectors that complement them.

A Proton Driver would bring with it other advantages. It would have the capacity to support a vigorous 8-GeV fixed-target program while providing 2 MW Main Injector beams. Several Fermilab reports have discussed the physics potential of such a program. We would highlight a low-energy neutrino program – crucial if MiniBooNE confirms LSND – as well as the possibility of muon and neutron facilities. This brings the potential for expanding Fermilab's user community both within and beyond high-energy physics. A Proton Driver can also serve as a stepping-stone to future accelerators, both as an R&D test bed and as an injector, with connections to the Linear Collider, Neutrino Factories, and a VLHC**. As such, a Proton Driver directly or indirectly benefits a number of crucial long-term goals of HEP as identified in both recent HEPAP subpanel reports and the DOE Office of Science Facilities List.**

5.2 Proton Driver Technology Choices:

Requirements for a new proton source can be met by either a new proton synchrotron or a superconducting linac. Common technical elements include a new copper linac and upgrades to the Main Injector RF and beam handling systems. Preliminary cost estimates for each technology have been prepared and are in the range $300-400 M. Cost estimates have thus far not been done using the same methodology so that presentation of absolute costs or cost comparisons of the two technologies is premature.

Synchrotron-based Proton Driver: A design study for a 16-GeV synchrotron-based Proton Driver was completed in December 2000 and documented in TM-2136. A second study, requested by the lab director for an 8-GeV Proton Driver, was finished in May 2002 and documented in TM-2169, Part I. The main parameters of the second study (PD2) as well as that of the present Proton Source are listed in the following table. Compared to the existing Proton Source, the 8-GeV synchrotron would increase the number of protons accelerated per cycle by a factor of 5 and the beam power at 8 GeV by a factor of 15.

Parameters	Present Proton Source	Proton Driver (PD2)
Linac (operating at 15 Hz)		
Kinetic energy (MeV)	400	600
Peak current (mA)	40	50
Pulse length (μs)	25	90
Booster (operating at 15 Hz)		
Extraction kinetic energy (GeV)	8	8
Protons per cycle	5×10^{12}	2.5×10^{13}
Protons per hour	9×10^{16} (*)	1.35×10^{18}
Beam power (MW)	0.033 (*)	0.5

(*) Continuous operation at 5 Hz

The intensity of the 8-GeV synchrotron can be upgraded from 0.5 MW to 2 MW if the linac energy is increased from 600 MeV to 1.9 GeV. The required space for such an upgrade has been reserved in the design.

Accompanying this study, three possible upgrades of the existing linac were also investigated. Each of them could improve the performance and reliability of the present Proton Source.

- New 201 MHz front end and Tank 1 (10 MeV)
- New 402 MHz low energy section (116 MeV)

New 805 MHz superconducting high-energy section (313-500 MeV, replacing CCL stations no. 6 and 7)

Superconducting Linac

A design study for a Superconducting Linac-based Proton Driver will be completed and documented in Fermilab-TM-2169, Part II. The SC linac is designed to accelerate an H^- beam that is subsequently injected and stripped in the Main Injector. This approach avoids the space-charge tune spread in a booster synchrotron. The simplicity of design should make it simpler to operate than booster/linac combinations. Limited emittance growth in a linac means that it can deliver the high brightness, low halo beams needed for running the Main Injector at high intensity with acceptable losses. The short MI "fill time" (< 1 ms) for the SCRF linac vs. that for a 8 GeV synchrotron operating at 15 Hz (5/15 sec) means that the MI could deliver the full 2 MW of beam power at any energy from 40 to 120 GeV, and that improvements to the MI ramp time could further increase the average proton intensity.

The 8 GeV superconducting linac can also be made capable of accelerating electrons. There are many technical overlaps between the development and construction of a superconducting linac based Proton Driver and a cold technology Linear Collider. The use of SCRF in a Proton Driver also opens up a variety of other possible SCRF applications and technical collaborations at Fermilab. The committee finds these features very attractive. The primary parameters of such a machine are shown in the table below.

PRIMARY PARAMETERS	8 GeV Linac	
Linac Particle Type	H – ions or Electrons	selectable on pulse-by-pulse basis
Linac beam kinetic energy	8 GeV	
Linac Beam power	2 MW	sum of H- and e- at 8 GeV
Linac Pulse repetition rate	10 Hz	combined for H- and e-
Linac macropulse width	1 ms	
Linac current (avg. in macropulse)	26 mA	
Linac Particles per macropulse	1.56E+14 H- or e-	
Linac beam macropulse duty factor	1%	
Linac RF duty factor	1.3%	
Linac Active Length including Front End	692 m	Excludes possible expansion length
Ring circumference	3319.4m	Fermilab Main Injector
Ring Beam Energy	8-120 GeV	MI cycle time varies with energy
Ring Beam Power on Target	2 MW	~ independent of MI Beam Energy
Ring Circulating Current	2.3 A	
Ring cycle time	0.2-1.5 sec	depends on MI beam energy & flat-top
Ring Protons per Pulse on Target	1.5E+14 protons	
Ring Proton pulse length on Target	10 us	1 turn, or longer with resonant extraction
Wall Power at 10Hz Operation	12 MW	approx 2MW Standby + 1MW / Hz

A major question for the 8-GeV Linac is cost. In a linac, the expensive radio frequency (RF) systems must transfer their energy to the beam in a single pass, whereas in a synchrotron the RF costs are amortized over many thousands of beam passages through the accelerating cavities. As a result, synchrotrons have traditionally been preferred for attaining the highest beam energies while linacs due to their simplicity and relative immunity to space charge effects have traditionally been the preferred solution for the low energy (few hundred MeV) injector to such machines. Recent developments have

dramatically reduced the cost-per-GeV of superconducting linacs, and have the potential for moving the optimal technological crossover point substantially upwards in energy.

R&D:

The committee believes that there are no fundamental technical problems that preclude the construction of a new synchrotron or a superconducting linac that could serve as the required proton source. The technology for both options is available and already in use today.

The cost of the superconducting linac is sensitive to how RF power is distributed. RF distribution systems for other superconducting proton linac projects, like the SNS, use individual klystrons for each cavity. Significant cost reductions can be obtained by using the TESLA approach of distributing RF power from one high-power klystron to many superconducting cavities. The preliminary cost estimate for the SCRF Linac assumes this solution can be implemented effectively. R&D to demonstrate the feasibility of this technique is required to remove this cost uncertainty. The proposed R&D would last about 1.5 years and require approximately $1M in M&S including the RF test stand. If resources allow, additional R&D could be used to optimize the design parameters, better refine cost estimates, and reduce the overall cost for both technology options.

5.3 Changes needed in MI

The Main Injector (MI) currently operates at 2.5E13 protons per pulse (ppp) at 120 GeV/c or 150 GeV/c. Hardware upgrades in place should allow the MI to achieve its design goal of 3E13 ppp. Proton beam intensities with a Proton Driver will be considerably larger than the MI original design capability. Systematic upgrades to the MI complex are required prior to a Proton Driver to meet the demands of the anticipated FNAL physics program and to prepare the MI for a future high intensity Proton Driver.

Currently the Booster is the limiting factor in providing higher proton bunch intensity. Limitations include space charge effects at the Booster injection energy of 400 MeV, instabilities, and aperture limitations during the Booster acceleration cycle. An 8-GeV Linac would bypass the existing Booster eliminating its limitations by injecting directly into the Main Injector. The PD synchrotron option seeks to replace the existing Booster with a new 8-GeV synchrotron free of these limitations.

Higher integrated proton flux from the Main Injector will be achieved by 1) Injecting higher intensity proton bunches, 2) Stacking protons in the Main Injector in the shortest amount of time possible and 3) Reducing the Main Injector ramp time. Several additional upgrades will be required to handle higher proton intensity.

Main Injector Ramp time: Considerable increase in the integrated proton flux is possible by reducing the Main Injector cycle time. Fill time of the Main Injector is at present limited by the Booster, which cycles at 15 Hz. This limitation will remain unless the Booster is replaced by a new linac based Proton Driver. A preliminary study shows that with an 8-Gev linac the MI cycle time can be reduced from 1.9 sec to 1.0 sec by a modest

investment in the MI power supplies and RF cavities. Doubling the maximum available power supply voltage can increase the magnet ramp rate. This can be achieved by adding two dipole power supplies and one quadrupole power supply to every MI Service Building.

RF: The faster ramp rate and increased flux will require doubling the RF power in the Main Injector. The currently installed Main Injector RF cavities can handle up to 6E13 ppp. The planned proton driver will inject more than 1.5E14 ppp. At present two possible paths for the cavity upgrade are envisioned 1) double the number of RF cavities by installing new cavities at the MI30 straight section, or 2) design and build new cavities that can handle higher flux and ramp rate. R&D will be required to determine the optimal Main Injector RF cavity upgrade.

High Intensity Bunches: Higher MI bunch intensity will require additional upgrades including: 1) improvements to the damper system to reduce instabilities driven by higher intensities. 2) A Gamma-t system to control the growth of longitudinal emittance through transition. 3) Aperture improvements. At present major beam loss points at the Lambertson locations are due to limited quadrupole aperture at the injection and extraction points. New quadrupoles will be required with larger aperture but same strength. 4) Kickers also limit apertures in the MI, and improvements in the kicker areas may also be required.

Reliability: 1) The Main Injector uses recycled Main Ring quadrupoles. They have failed in the past due to insulation problems. One should consider replacing them or performing R&D to insure their reliability at higher ramp rates.

Radiation shielding and collimation: 1) A modest upgrade to the present MI40 beam dump will be required. 2) A collimation system is required to minimize uncontrolled beam losses in the machine. 3) Higher order multipole correction may become more important when the higher intensity beam starts filling a significant fraction of the MI aperture. At present MI does not have a higher order multipole correction system.

H^- Injection: The super-conducting Linac option of the proton driver will require development of a new 8 GeV H^- injection system in the Main Injector. Additionally the beam size from the 8 GeV Linac will be very small, requiring a beam painting scheme to take full advantage of the available MI aperture.

Targeting: Either option of the proton driver will require additional R&D for extracting and targeting of higher intensity beam.

5.4 Near-term plan

Providing a new proton source in a timely fashion requires an urgent commitment of resources. We believe that commitment of the necessary resources at this time can be consistent with existing laboratory commitments to Run II and to other projects. Collaboration can be a key element in obtaining resources for an early start on this project.

Collaboration on the new copper front-end linac may be possible. Although the required copper linac can be purchased commercially, collaboration on controls and instrumentation may make sense. A new medical facility is under discussion that would use a similar linac and would be located near the Fermilab site. Similarly, it may be possible to increase the collaborative effort with ANL on SCRF because of the ANL interest in the RIA project. There are other laboratories with SCRF capabilities such as SNS, JLAB, Cornell, and DESY that might be potential collaborators in an SCRF linac project. Benefits to the Proton Driver from collaboration include not only splitting off a substantial fraction of the project costs into an early subproject, but also completing the modulator and RF controls development R&D early on.

Synergy with Linear Collider R&D depends on the technology selection for both the Linear Collider and the Proton Driver. Following the LC technology selection there will presumably be a substantial realignment of resources pointing towards an "Engineering Test Facility" (ETF) in the chosen technology. If the cold machine is chosen, the 8-GeV Superconducting linac will have many similarities and thus could be a strong candidate to effectively merge with the ETF as the demonstration machine for a cold Linear Collider. The LC collaboration(s) have a large number of specific technical skills (including klystron development, pulsed power, LLRF/control, RF ferrite technology, RF coupler and cryogenics) which are immediately needed for development of the 8 GeV Linac. In addition, a substantial fraction of the US Linear Collider R&D budget (currently $20M/yr, and likely to increase following the technology selection) might be brought to bear on prototyping and producing components for the LC ETF / 8 GeV Superconducting Linac project.

5.5 Recommendations

1) We recommend that Fermilab prepare a case sufficient to achieve a statement of mission need (CD-0) for a 2 MW proton source (Proton Driver). We envision this project to be a coordinated combination of upgrades to existing machines and new construction.

2) We recommend that Fermilab elaborate the physics case for a Proton Driver and develop the design for a superconducting linear accelerator to replace the existing Linac-Booster system. Fermilab should prepare project management documentation including cost & schedule estimates and a plan for the required R&D. Cost & schedule estimates for Proton Driver based on a new booster synchrotron and new linac should be produced for comparison. A Technical Design Report should be prepared for the chosen technology.

6. The Large Hadron Collider

Context

The Large Hadron Collider will be the premier high-energy physics facility in the world starting around 2009. It will finally allow us to determine experimentally the mechanism responsible for EW symmetry breaking (a Standard Model or other Higgs, for example) and whether there is other new physics at the TeV scale that resolves the hierarchies and infinities of the Standard Model and which might explain cosmic dark matter (Supersymmetry, for example). These questions are a central challenge to experimental HEP. Resolving them will result in a great burst of progress, with experimental results leaping ahead of theory, and we hope, great public excitement and interest. It is critical that the US, and Fermilab in particular, play a strong role in this exciting and important enterprise. Fermilab's role should be commensurate with the scale of the lab and with our future hoped for a role in world HEP; such a role will help and support US universities in their participation in LHC.

There are two aspects of the role we expect Fermilab to play. The first involves the continuation and evolution of work that has already started and includes completion of detector construction, installation and commissioning; support for computing through the Tier1 center and other activities at Fermilab; completion of Fermilab's contribution to LHC magnet production and accelerator commissioning; and similar involvement in detector and accelerator upgrades. The second involves new activities in physics analysis and development of theory in response to the data that will be pouring out of the facility. In particular, we believe that Fermilab should strive to play a leadership role in the analysis and interpretation of the physics from CMS. Because the second collection of activities has received less attention than the first, we will begin by discussing them. It is not sufficient for Fermilab to be a very competent collaborating institution, "the best place to get your data from" or "the best place to be if you can't be at CERN". Our goal should be "the best place to be if you want to do physics." The goal may be hard to achieve, but it is worth aiming for. It should be made clear that we do this with the aim of enhancing US physics potential overall, and improving the return on US investment in CMS and LHC. We believe there is significant support for this vision from the US-CMS collaborating universities who would benefit from having Fermilab as an alternative intellectual center to CERN. We do not see any conflict between establishing a strong role for the lab, maximizing the benefit to the universities, and playing a cooperative role in the overall CMS and LHC/CERN effort. In parallel and in synergy with the analysis center, Fermilab should aim to be a leading center for LHC theory/phenomenology, a leading center for detector development and accelerator development for the LHC luminosity upgrades.

Components of a Physics Analysis Center

The long-term goal for the Physics Analysis Center should be to allow lab scientists and users to play a leadership role in LHC physics analysis. Leadership means that people will choose to do much of their work at Fermilab; that the work done here is central to CMS physics goals and gets published; that we can do analyses from start to finish; and that we operate as one of a number of intellectual centers, CERN of course being another. If we can successfully do this we will maintain the laboratory's intellectual caliber (our ability to attract the best scientists), our position within the US CMS collaboration, and within the US and world HEP communities. The corollary should be obvious. Such an analysis leadership role builds on and indeed requires a strong lab effort in support of maintenance and operations of the CMS experiment, and that the lab fulfill its mandate as a computing and data center. But these important support roles do not by themselves guarantee the achievement of leadership in physics. We believe that there is a need to work toward that goal starting now. The viability of the center will depend on

- Above all, the physicists: we will need to attract a critical mass of the best people (so good that others will want to come here to work with them). They will need expertise in analysis, software and detector, which will require people to spend time at CERN (and to have CERN-based people spend time here). A two-way flow of visitors will be natural in a world where physics analysis is multi-polar rather than CERN-centric.
- Computer infrastructure (the Tier I regional center) and computing support and expertise.
- The best buildings and facilities, and working environment and communications capabilities (videoconferencing and remote control room). Our facilities should be better than at the universities, indeed better than at CERN.
- Attention to social aspects/quality of life
- Attention to and exploitation of synergies: with the theory group, with other ongoing Fermilab experiments, with nearby universities, and with detector and accelerator work.

Good goals would be for us to host one (or more) of the physics analysis groups here, to have meetings to present/approve results here, and for people from CERN to come here, not always vice versa.

Collaboration on the LHC Accelerator

The LHC will be the most important instrument for both world and U.S. high-energy physics in the second decade of this century and will provide unique opportunities for accelerator science research. Fermilab has played a leading role in the US contribution to the design and construction of the LHC, benefiting both the LHC Project and Fermilab through R&D on advanced accelerator systems, especially high-performance magnets, for the LHC. Fermilab should fully exploit our investment by taking advantage of the opportunities that the LHC offers in the field of accelerator science and technology, and by working with CERN to ensure the maximum performance of LHC in support of high-energy physics. Fermilab, Brookhaven National Laboratory and Lawrence Berkeley National Laboratory, working with the U.S. Department of Energy, and in close collaboration with our CERN colleagues, have established the U.S. LHC Accelerator Research Program (LARP). (Efforts are under way to try to expand this collaboration to include other U.S. National Laboratories and Universities.) Through this program, U.S. accelerator specialists will continue to take an active and important role in the LHC accelerator program during its commissioning and operations, and the U.S. accelerator community will be a major collaborator in LHC performance upgrades. In particular, LARP will support U.S. institutions in LHC commissioning activities and accelerator science, accelerator instrumentation and diagnostics, and superconducting magnet R&D to help bring the LHC on and up to luminosity quickly, to help establish robust operation, and to improve and upgrade LHC performance. Furthermore, the work we do will be at the frontier of accelerator science and technology, and will thereby improve the capabilities of the U.S. accelerator community.

It is in Fermilab's long-term interest to continue the collaboration on the LHC accelerator and to lead the LARP. This leadership should manifest itself not only by Fermilab providing the Program Leader, but by Fermilab playing a leading intellectual role in all aspects of the program: commissioning of the U.S.-provided hardware for LHC, R&D on advanced beam instruments and diagnostics, participation in commissioning of the LHC with beams, fundamental accelerator physics studies using the LHC, and accelerator physics and superconducting magnet R&D for an eventual luminosity upgrade.

The Accelerator Division was involved in the LHC construction in only a minor way. For the LARP, Accelerator Division scientists and engineers must be more deeply and widely involved, if Fermilab is to lead this work in the U.S. Efforts are beginning to flesh out the LHC program in Accelerator Division, and Fermilab and Accelerator Division management should encourage and support this work. Such support may need to include providing some funds in addition to LARP-specific funds, particularly in areas of R&D that benefit the Fermilab program directly as well as contributing to the LHC.

Fermilab's Technical Division has been traditionally a leader in R&D for advanced superconducting accelerator magnets, and should be the leader in the LARP effort for the LHC luminosity upgrade. Fermilab's success in this endeavor requires continued robust funding for the base R&D program on Nb_3Sn magnets, which will provide the basic technologies on which the LARP will develop specific solutions for the LHC.

Components of an LHC Theory Center

At the start of the next decade, the LHC will be the only operating facility at the high-energy frontier and the US has already invested quite a significant portion of its HEP budget there. We expect the LHC to bring new discoveries and hopefully some interesting surprises. The US-based theory community should be in a position to play a major role in the proper understanding of LHC data. This task will be quite demanding and coordination will be very valuable.

LHC Theory Collaboration with a nucleus at Fermilab would enhance the efforts of the Fermilab CMS Physics Center, would improve coordination with efforts at other Physics centers in the US and would greatly increase the synergy between theory and experiment. The Theory Center should have connections to all LHC experiments and, be open to all US-based theorists with an interest in LHC Physics. It might allow the interaction of collider phenomenologists, experts in MC and other computational techniques and model builders

Although organized at Fermilab, the Center could, through collaboration, impact positively other laboratories and universities. Although collaboration at a distance is increasingly the norm, the center could judiciously use visitor positions.

At the start of the next decade, the high-energy frontier will be overseas, and it will be a challenge to continue playing a leading role in that domain. The LHC theory center aims to help strengthen and ensure a synergy between the theory program for this coming era and the US LHC experimental efforts. For Fermilab, in particular, it will add to the efforts of the US CMS analysis center in their goal to make Fermilab the prime place to come in order to do LHC physics analyses.

Strong and continuing support for existing commitments to CMS

Fermilab has played a leadership role in USCMS and in CMS as a whole through its management of the USCMS Detector Construction project, through its strong role in the US LHC Accelerator Project, and through its central role in the US CMS Computing and Software project. As the detector construction begins to transition into the US CMS Maintenance and Operations Program, it is essential that Fermilab continues to play a leadership role by meeting and hopefully exceeding all its ongoing commitments during the installation, commissioning, debugging, and pre-operations and operations phases of CMS.

Similarly, the Tier 1 center at Fermilab is a key element in establishing the US as an analysis power in CMS and the base from which Fermilab can build its own and USCMS analysis capability. Through its leadership and participation in the Core Software effort, and its expanding efforts in data analysis, Fermilab will create the foundation for its analysis center.

It is essential that Fermilab meet and exceed its current commitments in order to maintain its credibility as a center of CMS support and intellectual activity. Both the professional staff and the scientific staff associated with these efforts must be correctly sized to

achieve these goals, as well as the new goals in the area of physics analysis and theory discussed above.

Options

As an area of Fermilab research which is in its infancy, and as a result of the evolution in techniques and technology of collaboration there are many opportunities and options available.
We advance the following:

- The laboratory should visibly and enthusiastically embrace the LHC as a central part of its research program over the next decade, with the goal of becoming the leading US intellectual center for LHC physics, detector and accelerator work.
- The laboratory should continue to promote and support the ongoing CMS detector project, the CMS computing and software efforts, and the maintenance and operations project for CMS.
- The laboratory should encourage and support efforts underway to establish a Physics Analysis Center for the US-CMS collaboration at Fermilab. Vital components will be a contiguous and large workspace for the CMS group, for visitors, and for the theory center. It should be well equipped with excellent desktop computing, secretarial and administrative support, state of the art videoconferencing capability, ample conference rooms, access to Fermilab support staff for conferences and workshops. A users' community for this facility should be built up, together with connections between this community and the Tevatron experiments in areas of common interest (event modeling, phenomenology…). The laboratory should support a significant visitor program for CMS physicists to come to Fermilab, and expect to send Fermilab physicists to CERN and to other CMS analysis centers.
- The laboratory should take steps to increase involvement of a small number of leading Fermilab scientists in CMS physics analysis, to form the core of a future group. The physics analysis effort will start small with the expectation of ramping up by LHC turn-on.
- The laboratory should encourage exploration of the concept of a theory center to increase the synergy between experimental and theoretical programs. Ideally such a concept would sit comfortably with the existing Theory group.
- The laboratory should encourage strong involvement in detector R&D for the LHC luminosity upgrades, particularly in those areas where Fermilab and US institutions have expertise or ownership of the current CMS detector systems (scintillator calorimetry, muon chambers, silicon tracking).
- The laboratory should lead the LHC Accelerator Research Program, both by providing the Leadership personnel and by playing a leading intellectual role in all aspects of the program: hardware and beam commissioning of LHC, beam instrumentation, accelerator physics, and R&D for a luminosity upgrade
- The collaboration with CERN on the LHC accelerator should be recognized as a core activity of the laboratory and of the Accelerator and Technical Divisions in particular.

- The laboratory should continue to provide robust funding for the base program in high-field magnet R&D, in order to support the LHC-upgrade-specific magnet development.
- The laboratory should consider alternative organizational models to the current situation wherein responsibility for LHC and CMS activities are widely distributed across laboratory divisions and departments.

7. Astroparticle Physics

Summary

While Fermilab's core program involves the construction and operation of accelerators and associated experiments to study the physics of elementary particles, its mission is defined more broadly: to ``advance the understanding of the fundamental nature of matter and energy by providing leadership and resources for qualified researchers to conduct basic research at the frontiers of high energy physics and related disciplines."

The Particle Astrophysics program at Fermilab strengthens this mission. Astrophysics is providing new probes of fundamental physics that complement accelerator experiments. Moreover, this field is undergoing a remarkable period of exciting advances and growth that should continue well into the next decade, and other DOE Laboratories are moving to devote substantial resources to it. While Fermilab blazed a path into astrophysics twenty years ago, the astrophysics effort at the Laboratory remains quite modest as a fraction of the total budget. Over this period, the experimental astrophysics program at FNAL has also diversified into several areas. While the astrophysics program has achieved notable successes, in order for the program to grow it will need strong support from the Directorate and, possibly, a more coherent organizational approach.

These considerations motivate our **primary recommendations:**

1. **Fermilab should substantially expand its leadership role and program in Particle Astrophysics, which provides probes of fundamental physics that complement accelerator experiments.**

2. **The experimental Particle Astrophysics program at Fermilab should continue to aim at key questions of fundamental physics, including the nature of the dark matter, of the dark energy, and of the highest energy cosmic rays, while remaining flexible enough to pursue new astrophysics opportunities consistent with Fermilab's mission.**

3. **We encourage Laboratory management to find a mechanism to provide coherence for, attract increased support for, and manage the growth of the astrophysics program. We note that the Laboratory astrophysics community has recommended that a Fermilab Astrophysics Center should be established to help realize these goals.**

Astrophysical Evidence for Physics beyond the Standard Model

To date, Particle Astrophysics experiments have provided compelling evidence for physics beyond the Standard Model. Some of these results have led or contributed to

plans for new accelerator experiments that are needed to make progress in understanding these phenomena; in other cases, new astrophysics experiments will be needed.

An example of this synergy is the neutrino sector. The first evidence for neutrino masses came from solar and atmospheric neutrino detectors. Reactor neutrino experiments (KamLand) have recently confirmed neutrino oscillations, and long-baseline neutrino experiments will soon provide more detailed probes of the neutrino sector. Large-scale structure from the Sloan Digital Sky Survey has recently placed stringent upper bounds on the sum of the light neutrino masses, and future weak lensing measurements will provide even tighter bounds.

Second, astrophysics and cosmology have provided strong evidence for the existence of non-baryonic Dark Matter, for which particle physics theory has provided a number of candidates, including the lightest supersymmetric particle. These developments, among others, motivated direct dark matter detection experiments, such as CDMS, as well as accelerator experiments to search for supersymmetry.

Third, distant supernova measurements, as well as the combination of cosmic microwave background (CMB) and large-scale structure surveys, have found that Dark Energy is causing the expansion of the Universe to accelerate. This may be a signal of quantum vacuum energy, or of a new ultra-light particle, or of the breakdown of General Relativity at large distances, perhaps associated with extra dimensions. In any case, the implications for fundamental physics are likely to be profound. Future cosmology experiments, including SNAP/JDEM for supernovae as well as the development of new Dark Energy probes such as weak lensing and cluster abundances, will provide more powerful probes of the Dark Energy and begin to discriminate between the theoretical possibilities. At the same time, accelerator experiments will contribute by constraining the scale of extra dimensions and by probing physics models that must incorporate Dark Energy.

Fourth, the patterns of CMB temperature anisotropy and of the large-scale distribution of galaxies point to an early universe origin for structure, indicating new physics perhaps at the Grand Unification scale. The most popular theory for structure formation involves quantum fluctuations generated during inflation, an early period of rapidly accelerated expansion. Other possibilities, e.g., colliding branes in higher dimensions, are even more exotic. It may be possible to test these ideas, and probe physics at these very high-energy scales, in future CMB polarization experiments.

Fifth, the origin, nature, and spectrum of the highest energy cosmic rays, with energies around 10^{11} GeV, remains a puzzle; the explanation may require physics beyond the Standard Model. The Pierre Auger Observatory will address this issue and may also provide new insights into the neutrino sector.

Fermilab is actively involved in pursuing each of these signatures of new physics, in several cases through both accelerator and astrophysics experiments. Moreover, we can expect significant experimental progress on each of these questions in the years up to 2020.

Current and Future Astrophysics Efforts

Fermilab has a small but strong Particle Astrophysics program in place for the short term. It comprises the Theoretical Astrophysics group and groups participating in the Sloan Digital Sky Survey (SDSS, members of the Experimental and Theoretical Astrophysics groups), the Pierre Auger Observatory to study ultra-high energy cosmic rays, the Cryogenic Dark Matter Search (CDMS), and the Supernova Acceleration Probe (SNAP):

- The Theoretical Astrophysics group, established in 1983, has earned a reputation as one of the world's leading centers for the study of particle astrophysics and cosmology.
- The SDSS is the most ambitious photometric and spectroscopic survey of the Universe ever undertaken; in combination with recent CMB data, it is providing stringent constraints on the densities of dark matter and dark energy and on neutrino masses, as well as independent confirmation of the Dark Energy through the Integrated Sachs-Wolfe effect; for these reasons, it was cited as a key element in Science Magazine's Breakthrough of the Year for 2003. Fermilab scientists are playing key roles in the scientific success of the project.
- The Auger project, with Fermilab significantly involved in construction and project management, will ramp up to full scale by the end of 2005; it will uncover the nature of the ultra-high energy cosmic rays (UHECR) and in particular determine whether there is an excess of particles beyond the Greisen-Zatsepin-Kuzmin (GZK) cutoff. The Fermilab Auger group is gathering strength in analysis of the burgeoning data set.
- CDMS recently began accumulating data at the Soudan mine and expects to regain its world-leading sensitivity to dark matter particles (of which the SUSY neutralino is an excellent candidate) by the summer of 2004. Fermilab has been responsible for project management and the infrastructure of CDMS at Soudan and expects to play a significant role in the science analysis.
- In early 2004, Fermilab became a major institutional partner in SNAP, which will compete for the recently announced NASA/DOE Joint Dark Energy Mission (JDEM), planned for launch in about a decade. The Fermilab group plans to take on leadership roles in several areas of the mission, most notably in the wide area survey that will provide probes of the Dark Energy that complement the supernovae.

The groups involved in the projects above are also exploring several possible future astrophysics projects and initiatives for the intermediate term:

- The Theoretical Astrophysics group, with support from the Computing Division, is considering an expansion into computational astrophysics, with particular emphasis on N-body simulations of structure formation. Among other goals, these would support the analysis of the wide area survey of SNAP/JDEM.
- The SDSS is currently scheduled to finish data collection in mid-2005; the collaboration is now preparing proposals for a possible 3-year extension to 2008. The extension would focus on completing the contiguous northern survey, on exploring substructure in the Galactic halo (with possible implications for the

nature of the dark matter), and on carrying out a new survey for intermediate-redshift supernovae to probe the Dark Energy. Fermilab scientists are actively involved in all three facets of the SDSS extension and playing major leadership roles in two of them.

- The Auger collaboration will propose to begin construction of the northern hemisphere site in 2004-5. Fermilab recently hosted the first major meeting on Auger North and may provide project management and site administration.
- CDMS will increase its target mass by a factor of five in 2004 and run through 2005, increasing the sensitivity to dark matter particles by a factor of 100. The collaboration is exploring methods to produce cleaner detectors with improved discrimination against backgrounds, with the aim of deploying in 2006 and achieving an additional order of magnitude increase in sensitivity by 2010. Alternative technologies to achieve even higher sensitivities for the next-generation dark matter search are also being explored.
- A number of Fermilab scientists, along with several University partners, are exploring the feasibility of building a Dark Energy Camera to carry out a 4000 square degree multi-band survey in the period 2008-2012 at the Blanco 4-meter telescope at Cerro Telolo Interamerican Observatory (CTIO) in Chile. The primary aim of the survey will be to probe the Dark Energy using the abundance of galaxy clusters, weak gravitational lensing, and galaxy clustering. The Dark Energy Camera will be the largest optical camera ever built, and the Dark Energy Survey will surpass the SDSS in volume. In timescale, the Dark Energy Survey would `fill the gap' between the end of the SDSS and the launch of JDEM; Fermilab could be the lead institution in building the camera.

The above demonstrates that Fermilab has a strong astrophysics program in place for the short term and is actively developing intermediate-term projects. There is clearly a need to explore the long term in more detail. For example, one possibility currently being explored by a number of groups nationally is an `Inflation Probe', a satellite mission to hunt for CMB B-mode polarization that could provide a smoking gun for an early epoch of cosmic inflation and thereby probe physics at the Grand Unification scale.

Key Astrophysics Programs in Fundamental Physics

The Fermilab Astrophysics program, as outlined above, is structured to address three key questions of fundamental physics. These questions have all been identified by national panels (e.g., Quarks to the Cosmos, HEPAP, the DOE facilities report) as high priorities for the U.S. science program:

1. **What is the nature of the Dark Matter?**

The search for supersymmetry at the Tevatron, and in the future at the LHC and Linear Collider, will play a crucial role in probing or constraining SUSY dark matter. Similarly, direct dark matter searches with CDMS, in the course of searching for dark matter particles, will constrain SUSY models. Large-scale surveys such as the SDSS, the Dark Energy Survey, and SNAP/JDEM, provide complementary constraints on the

cosmic density of dark matter particles (through its effect on the power spectrum of galaxies and dark matter), on the properties of the dark matter (e.g., dark matter interactions alter the profiles of galaxy clusters and the properties of Galactic tidal streams), and on the density and combined masses of neutrinos.

2. **What is the nature of the Dark Energy?**

In the period 2005-8, a proposed Supernova component of the SDSS extension would measure 200 Type Ia supernovae in the redshift range 0.05-0.35, providing new constraints on the Dark Energy equation of state; it will also constrain systematics of the supernova population, helping to lay the foundations for SNAP/JDEM. In the period 2008-12, the proposed Dark Energy Survey will probe the Dark Energy equation of state through cluster counts, weak lensing, and the galaxy clustering power spectrum. In the period 2014-18, JDEM will provide precision constraints on the Dark Energy equation of state and its cosmic evolution via supernovae, weak lensing, and cluster counts. Measurement of Dark Energy evolution will be critical in pointing theorists down the path to future Dark Energy models (a path that Fermilab theorists helped to clear several years before the current evidence for Dark Energy emerged).

3. **What is the nature of the ultra-high energy cosmic rays?**

The Auger Observatory is designed to definitively measure the spectrum, composition, and direction of the UHECR. These results will resolve the apparent inconsistencies in the spectrum derived from current experiments and will illuminate the origin of the UHECR, in particular whether top-down scenarios, involving physics beyond the Standard Model, are needed. Auger results on UHECR propagation through the Universe will also constrain new physics (e.g., Lorentz-invariance violation). Early Auger results indicate that air showers with zenith angles all the way to the horizon can be reconstructed; this capability opens a new window to the detection of neutrinos and possibly other exotic particles.

As noted above, each of these three areas is ripe for experimental progress, and the associated Particle Astrophysics projects will make critical use of Fermilab resources and expertise. In each case, Fermilab scientists will carve out scientific leadership roles. These three areas define the baseline scope of the Astrophysics efforts going forward, but with the recognition that new opportunities consistent with the Laboratory's mission may arise.

Structuring the Astrophysics Program

While the current Astrophysics effort at Fermilab is relatively modest (experimental astrophysics projects comprise 1.4% of the annual budget), it has produced substantial successes. The `first generation' astrophysics experiments described above have now reached maturity (i.e., are taking science-quality data), providing a useful vantage point from which to consider the role Fermilab should play in the field of Particle Astrophysics.

Given the successes of the Fermilab Astrophysics program to date, the exciting prospects for near-future discoveries that will have major impact on fundamental physics in the areas described above, and the growing national support for this field, we believe the astrophysics effort at the Laboratory should be substantially expanded over the coming decade. This growth will be required to carry out the current and projected efforts described above as well as to allow for development of new initiatives. An initial estimate indicates that the program would need to expand to about 3 times its current size over the next 5-10 years in order to meet these goals.

We strongly encourage the Laboratory to find a mechanism in the near term to help manage what will be an expanding Fermilab Astrophysics program over the next decade. We note that the Fermilab Astrophysics community has recommended establishment of an interdivisional Astrophysics Center to help manage this growing and diverse program. Its goals would be, internally, to bring coherence to, nurture, and help manage the various astrophysics efforts, integrate them into the intellectual life of the Laboratory, and maximize their scientific return, and, externally, to provide leadership and external visibility for the effort and a contact point in this field for the DOE, other funding sources and institutes, and the User community. The Center could, e.g., include common space for the various efforts to foster interactions between them, and Fellowship, Visitor, and seminar programs to strengthen connections with the larger field. The Center would ideally have a leader of stature and appropriate title who could help formulate a vision and future program for astrophysics at Fermilab and who could be given (and also be able to raise) adequate resources to help carry it out.

We note that the role of the DOE in this field is growing (cf. the high priority given to SNAP/JDEM), and it is being urged to formulate a program in Particle Astrophysics. Fermilab should have in place a management structure for Astrophysics that would provide a natural conduit to this national program and to other potential funding sources.

This field is vibrant and expanding and relies strongly on University-based groups funded by multiple agencies. Yet new particle astrophysics projects are increasingly on a scale beyond that of University groups alone and will naturally exploit the technical expertise and infrastructure the Laboratory has to offer. A more unified management structure for the Astrophysics program would provide a natural contact point for the Particle Astrophysics User community at Fermilab. It would also be a logical contact point for cooperative efforts with other institutes and laboratories.

Finally, it is worth recalling that Fermilab helped start this field twenty years ago. While the current Astrophysics program is strong, others have now recognized the rich physics opportunities this field presents. Fermilab has the resources to reestablish its intellectual leadership position in Particle Astrophysics and a number of opportunities to play a major role in this field. A more unified Astrophysics program can enhance and help ensure the success of these efforts.

8. Accelerator Physics

In the view of the committee, the lack of integration of accelerator R&D into the advertised core base program of the laboratory and the inadequate level of support for accelerator R&D puts the future of particle physics in the US in jeopardy.

High Energy Particle Physics is an accelerator dependent science. Advancing the energy and intensity frontiers requires the development of increasingly sophisticated accelerators that are scientifically and technically challenging. To fulfill the laboratory's particle physics mission it is essential that the Fermilab program includes a vigorous accelerator R&D sub-program focused on (i) developing the capabilities of the present Fermilab accelerator complex, (ii) developing the accelerator concepts and technologies that might lead to new exciting particle physics facilities in the medium-term, and (iii) exploring and developing options for particle physics in the long term. It is natural that the emphasis should be on R&D toward new accelerator facilities that might eventually be built at Fermilab. It is desirable that the accelerator R&D program should also be sufficiently diverse to facilitate the emergence of new ideas.

Fermilab has some valuable accelerator R&D assets, namely: (i) a large scientific and technical staff that has experience designing, building, and operating accelerators, (ii) unique facilities that can support cutting-edge accelerator component development, (iii) an infrastructure that can provide coordination for large scale R&D projects, and (iv) active accelerator R&D groups with good track records. Despite these assets and broad recognition within the community that accelerator R&D is crucial for particle physics, accelerator R&D is not well integrated into the approved Fermilab program. Furthermore, in recent years the Fermilab base program funds have supported accelerator R&D only at the very modest level of about $6M / year. <u>This level of support is inadequate to enable the Fermilab accelerator R&D programs to provide timely options for an exciting long-term future for the laboratory</u>. It is also inconsistent with the level of support for accelerator R&D aimed at the long-term recommended by the HEPAP sub-panels on accelerator physics chaired by Tigner (1980), and Marx (1996: DOE/ER-0679).

If accelerator R&D at Fermilab is to contribute to the long-term health of particle physics at a level consistent with the Tigner- and Marx-Panel recommendations, we believe that support for this R&D must be stable (Recommendation 1) and each sub-program must be supported at a level that enables it to achieve agreed upon goals (Recommendation 2). In addition, the R&D programs should be recognized as a part of the core scientific program of the laboratory by the Directorate and the particle physics community (Recommendations 3 and 4), and new ideas and collaborative efforts should be encouraged (Recommendation 5). In addition to making these recommendations, the sub-committee has also considered the funding levels for the presently supported non-linear-collider accelerator R&D programs aimed at the medium-term and long-term. The funding profile below is intended to illustrate how support for these programs might evolve to enable their goals to be achieved on a reasonable timescale.

Recommendation 1

Accelerator R&D aimed at the long-term requires adequate and stable support if accelerator based particle physics is to have a future beyond the next 20 years.

We recommend that accelerator R&D aimed at the long-term be supported at a level that is agreed upon and support at this level is then considered a high priority.

Recommendation 2

The committee believes that the major medium-term (non-Linear Collider) and long-term accelerator R&D programs at Fermilab are well motivated, but require increased support to meet their goals.

We recommend that the approved non-Linear Collider related accelerator R&D programs at Fermilab aimed at the medium- and long-term be examined by the Director to establish for each program the goals and the level of support required to succeed in these goals. We believe this will require increasing the present support by a factor of two or more.

Recommendation 3

Lack of explicit recognition that the approved accelerator R&D projects at Fermilab are an integral part of the core scientific program limits the communities' exposure to the R&D program, and impedes a broader understanding of the appropriate level of support needed to provide for the long term future.

We recommend that the approved accelerator R&D programs at Fermilab are recognized as an integral part of the scientific program, and are advertised as such on the Comitium wall, within the program yearbook, and within Directorate-level talks.

Recommendation 4

The present peer review process for accelerator R&D gives proposed and approved accelerator R&D projects little exposure to the particle physics community.

We recommend that the Director examines the peer review and approval process for accelerator R&D aimed at the medium- and long-term, and seeks to modify the process to make the proposed and approved R&D more visible to the particle physics community.

Recommendation 5

Multi-institutional collaborations provide a fertile source of new ideas, and a way in which a broader community can contribute to accelerator R&D.

We encourage collaboration with local laboratories and universities, and in national and international collaborative efforts. We recommend that support for these collaborative efforts be detailed in an MOU negotiated between the Directorate and the proponents. We further recommend that the laboratory supports an annual accelerator R&D workshop in which new ideas can be discussed, proposals developed, and collaborations formed.

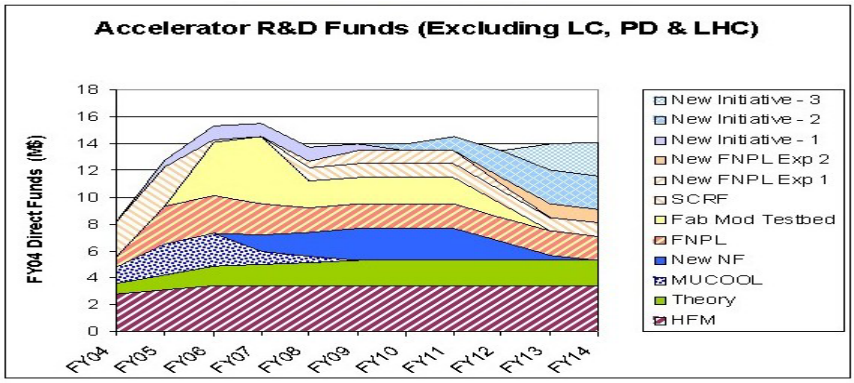
Illustrative Funding Profile

The funding profile below is based on input from the main proponents for the existing accelerator R&D programs at Fermilab, which in turn has been influenced by discussions within the framework of the Future Accelerator R&D Oversight Group (FARDOG) that have taken place over the last couple of years. The profile is intended to illustrate the level of funding that would support a healthy Fermilab non-linear-collider related accelerator R&D program.

Note that:

1. The envisioned unloaded funding to support accelerator R&D aimed at the long-term ramps up to about 14M$ which after loading is about 6% of the Fermilab base program funding. This does not include support for LC, PD, or explicit LHC upgrade R&D. This should be compared with the 4% guideline recommended by the Tigner and Marx panels.

2. The funding model assumes after an initial ramp-up of support: (i) SC magnet R&D and the FNPL infrastructure is supported at an approximately constant level throughout the time period considered. (ii) The MUCOOL program is completed within a few years, and is followed by a new Neutrino Factory related hardware R&D project, also lasting a few years. (iii) A modest and as yet unidentified new accelerator R&D initiative will begin within the next couple of years, and funding opportunities to begin modest and/or major R&D initiatives will occur once every few years.



9. Detector Physics

Today's experiments are very much enabled by past developments in detector and computing technology. Examples of this make the point obvious; to name but a few: silicon strip and pixel tracking devices, fiber readout of calorimeter signals, visible-light-photon-counters, crystals for calorimetry, deep-submicron radiation-hard electronics, and increased computing power in any number of guises.

We may expect the same dependence on developments by experiments in the future. It is reliably predicted as well, that some of today's most important technologies will need replacement in the future; e.g., the current generation of application-specific integrated circuits as older technologies disappear from the marketplace.

A number of detector-development goals are clear now, especially for LHC upgraded operation at higher luminosity and for a LC detector. In particular we will need thinner tracking devices, greater radiation hardness, and increased integration of detectors and their read-out electronics. There are also opportunities for astrophysics as outlined in that section of this report.

Detector R&D, as a dedicated line, has taken a back seat to other investments in recent times. In part this has resulted from the tension between Detector R&D, which is targeted at specific goals, as with the LHC and LC work and that which is generic. It has been argued that it should be a priority for the vitality of the Laboratory and the field and that it has been an area offering opportunities for younger physicists to mature and develop recognition. This is another element in the future health of the field. These potential attributes have not been sufficient to keep the wolves from the door.

In the past, with a multiplicity of smaller projects each seeking a slightly different experimental optimization, demands on the diverse laboratory infrastructure were continuous. Today these special Laboratory capabilities are vulnerable as a result of droughts. This is analogous to the difficulties of maintaining a strong university technical group between construction projects. The centers, which capture these capabilities, have provided critical infrastructure for detector R&D in the past but efficient and cost effective management is quite difficult. Maintaining any of these capabilities at Fermilab would require a new approach.

With this context in mind, we make the following recommendations:

1. Fermilab should attempt to focus R&D efforts. Its support should be selective. It may be that Program Planning and Division management need to instigate a semi-formal process to select the R&D, which will be supported. The equivalent of the multipurpose DOE laboratories' LDRD (Laboratory Directed Research and Development) Programs would be a mechanism by which to protect these initiatives from programmatic pressure.

Collaborations with university and other groups are often a major benefit to the R&D, as well as furthering Fermilab's traditional role in enabling research by university groups. While more leadership is a requirement for increased success, the Laboratory might consider funding some of the costs by participant fees. A recent model of this approach was that used for CERN's RD50 program. Current NICADD collaboration provides other examples of R&D support by the Laboratory.

1. The Fermilab test beams will be unique facilities required for pursuing detector R&D for the foreseeable future, especially if they remain generally available. They should be nurtured as vital user facilities.

Upgrades should be made as needed in order to provide flexible testing opportunities.

2. In the past, Fermilab's centralized facilities have played an important role in enabling detector R&D. Fermilab should seek to continue playing his role as future R&D directions, and the needs for the period covered by this report, are solidified. It is also worth noting that somedetector R&D is common to multiple future directions; e.g.,solid-state-tracking device thinning - to name just one example.

3. Fermilab should extend, formalize, and where possible, increase its role in encouraging particle physics detector R&D at universities. This could include providing opportunities, through a proposal process, for university researchers to access Laboratory equipment and receive advice from Laboratory personnel. For the Linear Collider, this is a specific task of the LC Detector Group Leader in Particle Physics Division.

10. Interdisciplinary Science, Technology and Education

10.1 Introduction

In carrying out its mission, Fermilab has developed a wide range of talents and skills that are applicable elsewhere in science and technology. In the past, Fermilab has focused its attention on its core mission, but it has occasionally committed to other enterprises. Examples are the Neutron Therapy Facility, which has treated cancer patients on site for many years, and work in designing the Loma Linda Proton Treatment Facility. Another area where Fermilab has been a leader is in science education and outreach. As we consider Fermilab's future, we must ask whether these "non-particle" science and

technology activities should be expanded, kept at about roughly the same low level, contracted, or eliminated.

We have assumed in our discussions that Fermilab is determined to retain its focus on accelerator –based particle physics and that the program we consider must, therefore, be limited in scope. We considered areas where Fermilab core competencies overlap or complement those of a non-particle science or technology area and where benefits can accrue to the laboratory, the user community, and the DOE.

One possible benefit would be to provide a more diverse portfolio of projects that might permit us to attract innovative scientists with multidisciplinary outlooks. A second possible benefit is that it might permit us to contribute to the solution of problems that are of more immediate interest to society than ones we are working on in our "pure" research. This might help us convince the public that support for our work brings immediate benefits as well as the long term benefits from fundamental research. A third reason is that smaller scale projects may provide good training for young scientists. Smaller projects offer more opportunities to try things, make mistakes, and fix them. A fourth benefit is that it could bring in alternate sources of funding It could enable us to build a network of contacts that would help us to secure external funding.

The downside of any such activity is that it can divert efforts from our core mission. It is, therefore, important to try to explain the benefits of these non-particle physics projects so that one can understand how they contribute to the overall program and to the health of the lab.

10.2 Issues

In considering the problems of having interdisciplinary science and engineering projects as part of the program, we always come back to the competition by these projects for very limited lab resources. Below we describe possible programs in radiation physics, instrumentation, and small accelerator projects. If these contribute to a vibrant workplace, help attract the best staff, and keep them energized; their value to the lab and to its HEP program could far exceed their actual cost.

In order for a program of non-particle science and engineering projects to succeed, the directorate must be able to do the following: to express clearly the advantages to Fermilab, the user community, and the DOE of limited of involvement in areas that are not particle physics; to discuss and define criteria and decision mechanisms that can be used to determine what projects Fermilab should be involved in. This should include being able to identify an initial group of outstanding projects that can be pursued and can be used as test cases; to propose an ongoing method for identifying new projects; to discuss how to fit these projects into the overall program so that they provide maximum benefit to the lab, the user community, and the DOE; and to describe mechanisms by which the progress of such projects can be tracked.

10.3 Initial Project Areas and Example Projects

We have identified the following project areas for initial consideration:

- Medical Physics/Therapy
- Computer Science and Computational Physics
- Development and use of HEP detectors for non-HEP applications
- Uses of existing and future machines for accelerator physics and instrumentation research
- Health/Radiation physics
- Education and Outreach

10.3.1 Institute for Hadron Therapy

Fermilab has been involved in medical physics and patient therapy for a long time. This area offers many possibilities for projects in medical accelerators, instrumentation, and computation. We have focused our attention on one particular project that is very closely related to our current Neutron Therapy Facility and represents the next stage in its development.

The proposed Institute for Hadron Therapy (IHT) is a multidisciplinary medical research and treatment facility whose primary mission is to compare and evaluate the roles of neutrons, protons and photons in the treatment of cancer. At the heart of the proposed facility is a 70 MeV proton linac dedicated to neutron therapy and production of radioisotopes. The new clinic would provide the space and resources needed to treat up to 1000 neutron therapy patients per year as well as imaging capabilities. In addition to providing the radioisotopes needed for in-house PET scanning, the linac would be available to produce commercial medical and/or research isotopes for sale to other institutions.

The goals and potential benefits of collaboration with this program to the lab and to society are:

- Physicians could treat up to 1500 patients annually, thus acquiring the statistics needed to evaluate the different therapies.
- Engineers would devise safe, better and more efficient collimation and patient immobilization techniques
- Physicists would develop improved dosimetry techniques and radiation detection devices.
- Software specialists would improve data acquisition software, simulation software and controls systems.
- Radiobiologists would study the biological effectiveness of different types of radiation for various cell types and fractionation schemes.
- Pharmaceutical specialists would study the role of drugs and radiation sensitizers for different types of radiation and tumors.
- Nuclear medicine specialists would use linac-generated isotopes to track the radiation-induced changes in patients' tumors.
- Accelerator physicists together with engineers would work to reduce the overall cost and complexity of the equipment to make hadron therapy more accessible. Development of spot scanning techniques would be emphasized.

- Non-medical scientists would have access to radioisotopes that could be useful in their research.

10.3.2 Initiatives in Computational Physics and Advanced Computer Science

Computing is a core competency of Fermilab and must remain so if Fermilab is to execute its own program as well as to contribute to work based at other facilities, such as the LHC. Moreover, retention of staff; ability to anticipate, prepare for, and adapt to changing technologies; and ability to maintain operational stability while adapting to change are all issues that must be addressed if computing at Fermilab is to prosper.

A carefully chosen program of R&D can address these crucial issues. While it would be very rash to predict computing trends over the 20-year time frame of this report, the initial round of projects will flow from continuation and extrapolation of existing trends. Three areas have been identified as promising ones for R&D. Each of these is an investment that has a high probability of producing results that will eventually be very useful to HEP and other laboratory scientific initiatives. They are:

- Grid Computing and Remote Resource Sharing: The Open Science Grid concept is motivated from the successful implementation and operation of several distributed computing Grids.
- Network Research: High-speed network technology has long been a core enabling expertise at Fermilab for High Energy Physics experiments and theory. Network technology is the backbone of advances in online and offline data analysis systems, Grid technology, and Lattice QCD simulation machines. Based on the accumulated expertise, proximity to world-class network infrastructure, and interest from commercial partners and funding agencies, Fermilab is well poised to become a leader in network technology research.
- Real Time Applications: The experimental program at Fermilab has deep roots in advances in Real Time Applications that form the basis of data acquisitions systems. The intriguing problem of fault tolerance in complex HEP online systems has attracted the interest of information technology scientists as well as funding from non-traditional sources.

10.3.3 Non-HEP Use of HEP Instrumentation and ASIC's

Instruments designed to detect high-energy charged particles and photons are often also effective for the detection of lower energy particles and photons. Many types of detectors that were developed for use in physics experiments have been adapted for use in other fields. For example, scintillating crystals are now ubiquitous. They are found in instruments as simple as hand-held radiation monitors and as complex as CAT and PET scanners.

Micro-pattern silicon detectors have been developed for use as vertex detectors over the last 25 years and are now part of most high energy physics experiments. These detectors are rarely used outside of high-energy physics, but could be applied with only slight modification to a variety of other applications. For instance, silicon strip detectors can make excellent x-ray detectors. Single photon counting is possible for energies above 5-10 KeV, allowing images with perfect linearity and unlimited dynamic range as well as very good spatial resolution. Near-unity quantum efficiency is also possible in many

cases. In medical imaging applications these features translate into better image contrast and resolution, and lower dose to the patient. Hybrid silicon pixel detectors coupled to fast, radiation tolerant, readout electronics have the potential to be a break-through detector for protein crystallography. The current state of the art relies on CCD detectors that are capable of neither photon counting nor fast, zero suppressed, readout.

10.3.4 Applications of Health Physics R&D at Accelerators

The field of accelerator radiation/health physics is a relatively young area of research compared to the health physics research conducted at the reactors, medical facilities and radioactive material storage facilities. The advent of high-energy, high-intensity beams requires much better understanding of radiation physics issues to design both radiologically and economically efficient machines. High energy and diverse radiation fields are common in accelerators and in space. In addition to exotic applications related to space travel there are significant needs in the satellite (communication, mapping, etc.) and aviation industries that require equipment to operate for long periods of time. There is also a significant demand in the nuclear power industry for better radiation resistant detectors. Important areas for dedicated radiation physics R&D program are: radiation damage studies, shielding studies of complex geometries, new detectors, and detailed comparisons between Monte Carlo computer codes and reality.

These R&D projects can be done at the existing HEP test beams or parasitically at the existing experiments. Each of these areas will produce results that will be very valuable for the future high-energy accelerators, have industrial applications and provide many opportunities for university research programs.

10.4 Education and Outreach

In 1983 *the Nation at Risk*, the first in a series of reports and studies critical of contemporary American precollege education, brought scientists, educators and teachers together to reexamine the way science and mathematics were taught. Shortly thereafter, under the leadership of Leon Lederman, Fermilab began a modest program to provide professional development for area science and mathematics teachers. Friends of Fermilab, a non-profit corporation created to support these efforts, worked in partnership with educators to design, fund and conduct programs to meet needs identified by the educators themselves.

This partnership has grown over 20 years to embrace annually some 20,000 students and 5,000 teachers through 26 programs. Such partnerships will remain important for the foreseeable future.

The Fermilab education programs seek to promote a life-long interest in science, raise scientific literacy and encourage young people to consider careers in science. These goals support workforce development and public support for science, important goals for Fermilab, the users and the Office of Science. We note that an important component of workforce development is pipeline programs especially those for underserved populations.

One important contribution is to help K-12 teachers and, to an appropriate degree, their students to feel part of the scientific community. This will remain on ongoing need and one with which the Lab has had great success. It could be a local program where midlevel students conduct plant population studies on the prairie restoration project or a national program like QuarkNet that brings high school teachers and their students directly into particle physics experiments. Other teachers and students could access HEP experimental data through a grid portal whether they live close to an HEP research group or not.

We envision a steady evolution of the current K-12 programs as they respond to identified needs. The Leon M. Lederman Science Center will continue to be the home for the majority of our activities. Our strategies will include researching, designing and conducting programs that maintain science education at the highest levels for all students, promoting the integration of innovative education technologies with curriculum and promoting the use of high-quality science materials and resources. Programmatic areas will include research participation, professional development workshops and institutes, "kids at Fermilab," classroom visits, the Teacher Resource Center, Web-based instructional materials and resources for scientists.

In 1983 it was not at all clear that a research facility was an appropriate setting for K-12 professional development programs. Teachers and their students have given us the answer. "Yes!" Precollege education programs work at Fermilab because it is not business as usual. Teachers come to a world-class high-energy physics research laboratory for a unique opportunity to witness science conducted at the frontiers of human understanding and to learn from leading research scientists. Students have an experience in science that broadens and enriches their attitudes and develops their appreciation for science. Students see, perhaps for the first time, what the world of science is really like, and they like what they see!

10.5 Recommendations

- Fermilab should incorporate in its research portfolio a limited, controlled selection of science and engineering projects that are outside the traditional definition of HEP activities.

- Fermilab should review, and perhaps adjust, its mechanisms to encourage, develop, and approve such projects and to track their progress from start to completion.

- Fermilab should be actively involved in the application of accelerators to the benefit of society. A possible example of this would be to collaborate with the proposed Hadron Therapy Facility in DuPage Technology Park to the north of the site and to collaborate with the planned sister facility in Taiwan.

- Fermilab should look for opportunities in R&D in Computing Science and Technology to anticipate technologies that are likely to be relevant to the program

- Fermilab should maintain its strong role in science education and outreach.

11. Resources

This planning exercise did not include detailed cost estimates for the different proposals. However, to understand the feasibility of the various options, we have done a rough budgetary analysis by extending the current laboratory budget model, which is typically used to make projections over a few years, to cover the time scale relevant to this report. The known costs of existing programs have been projected into the future, taking account of planned future changes, and the cost estimates and time profiles for new initiatives have been included to the extent that they are known.

The laboratory budget model contains approximately twenty individual items. Examples of typical components are, the CDF and DZero Run IIB Detector Upgrade MIE (Major Items of Equipment), others are Accelerator Operations and Run II Accelerator Upgrades. The general and administrative costs associated with each have been included. For some projects such as NuMI the costs are included; those associated with the directly funded LHC program are not. This convention is that agreed with the DOE as a way of defining the FNAL base budget. It is slightly less than three hundred million dollars in 2004.

The projection out to 2020 was made in actual year dollars. That is to say each item is adjusted to the actual year, including inflation. In the near term, the next few years, we are working with some understanding of what that will be, item by item. In the far future the elements are less well understood and these projections are necessarily more crude. In order to understand budget unitarity, we have compared the totals in each year with a total Laboratory budget being inflated from this year with 2% per annum and with 4% per annum. One should note the obvious; if inflation in salaries and cost of materials is 4% per annum and the Laboratory budget rises by 2% per annum, the difference of 2% per annum is the rate at which the scope of the Laboratory activities needs to decrease to match. Extended indefinitely, such a trend would ultimately compromise the ability of the Laboratory to mount any viable physics research program.

Of the current activities at Fermilab, some already have planned changes. For example, the CDF and DZero MIE project funding will end within two years. The NuMI Project will be complete in 2005. The Run II Accelerator Upgrade is expected to end by approximately 2006. The BTeV MIE is planned to complete in 2009. We don't know how long BTeV will run. However, currently the model we have has a constant Tevatron Luminosity; after three or four years of operation, the data doubling time would be correspondingly three to four years. For scale, the current operation of the Tevatron has a data doubling time of one year; CDF and Dzero have ~200 pb^{-1} in the data sets for the 2004 Spring conferences, we aim to deliver more than 250 pb^{-1} during the current fiscal year. This consideration, applied to BTeV, allows us to think of a sensible time for the Tevatron collider operations to cease with a corresponding reduction in the Accelerator Operating Costs. That reasoning does not accommodate discovery.

A number of the activities discussed in the earlier sections of the report seek boosts in funding. One clear example is the long-term accelerator research and development. Another is astroparticle physics. In each case, the discussions in the committee did support the idea that, if possible, increases should be made. In the case of the accelerator R&D this need has been identified many times. Currently the budget is approximately 2%, excluding the near term, programmatic work, with a factor of two identified as required for a healthy program. An increase in the particle astrophysics funding, from its current few million dollars by a factor between two and three was also explicitly discussed by the committee. Until now the lien on the base program of participation in the LHC program has been small. This will increase as CMS takes data and a larger number of physicists start to participate. If one takes together the ensemble of these deserving sectors of the laboratory, the total reaches fifteen to twenty percent. This is not negligible. Nevertheless, we have not attempted to "balance the budget" using these items. We have included some measure of increase but without fine-tuning.

Other activities include modest but not negligible provision for improvements to the existing accelerator complex to maintain or increase the proton source performance for currently approved experiments. A small fixed target program is also envisaged. These budget plans and commitments through the rest of this decade do not leave a large measure of flexibility.

With the discussions in the previous sections of this report, there are three elements which are potentially large cost drivers in the timeframe considered by this committee; these are a substantial neutrino experimental program, possible major upgrades to the proton source (proton driver), and the electron-positron Linear collider.

The Laboratory has received a letter of intent for an "Off-axis neutrino experiment". It is argued that a detector with a mass of about fifty thousand tonnes is required. Current cost estimates are in the range of one hundred and fifty million dollars. There are several possible strategies, which might be used in exploring the neutrino world. For example, the relative emphasis and ordering of steps with the detectors, as compared to steps with the proton source, are not yet completely clear. Nevertheless, a detector of this scale is likely needed inside the next five to twelve years.

Significant improvements to the proton source have been discussed under the rubric Proton Driver. In order to model the budget requirements, we have retained the modest line of proton source improvements. We have added approximately five hundred million actual year dollars; these are thought to cover the costs of the superconducting linear accelerator capable of at least five GeV and some appropriate upgrades to the Main Injector. These would be spread over a construction period of say five or six years. The details depend on several things, including when this might happen.

Finally, there is the Linear Collider. At this time the actual costs of such a device are the subject of much discussion. In order to avoid becoming embroiled in this issue, we have assumed that the Fermilab participation in the project would ramp up through the R&D period, and would rise further during the construction period. We were guided by extensive discussions of the Linear Collider as described in chapter 3. In the case that Fermilab is the host laboratory, it is imagined that as much as half of the laboratory

would be devoted to this during the peak construction. It should be noted that, while this would be a substantial part of the LC construction budget, the bulk of the support would come from "new money" and from other participating laboratories
in the US and abroad. If the LC is sited outside the US, Fermilab will still be a big player, but at a level of about one-third that of its contributions as host. These two situations correspond to those given to the long range planning exercise.

The Linear Collider would clearly dominate the budget in the case in which Fermilab would be the host laboratory. In addition to the LHC and Linear Collider, it would be possible to support an exciting neutrino program. That program would fall short of maximum exploitation and would be paced by a judicious deployment of resources in the direction of experiment and proton source.

In the case in which the Linear Collider is offshore, a more aggressive approach to neutrino physics could be accommodated and with a Proton Driver it would become the largest part of the program. The actual impact of the Proton Driver would depend on the construction period and its timing. A modest period with funding above the inflation-corrected budget envelope might be demanded.

As described, we have explored the resources in terms of budgets and have looked at the situation as outlined above for the options we advocate. Where the analysis is weak is in its disregard for the issue of what spectrum of Laboratory personnel would be needed. A careful study of this issue is warranted but was not possible in the timeframe of this report.

The analyses and considerations outlined above were available during the two-day retreat during which the committee discussed the main report and informed the plenary discussion of the options we advance. Given all the uncertainties, only crude conclusions can be drawn. In the case of construction of the Linear Collider at Fermilab, a growth of 4% can provide the bulk of Fermilab's imagined contribution to the construction. The other programs would necessarily be restrained, especially for the period during the construction peak. In the case in which the Linear Collider would be built offshore, a proton driver could be built by 2015 enabling a fully developed neutrino program.

12. Conclusions

In developing our report, we have been guided by a view of how physics will develop over the course of the next decade. The future is exciting. We have included a précis of that vision in the Executive Summary. In addition we lay out our conclusions, recommendations or options.

The present Fermilab collider-based program comprises two experiments (CDF, DZero) seeking new phenomena at the Tevatron with the highest energies currently available anywhere in the world. These will be succeeded by an experiment (BTeV) which will further probe the flavor sector of the universe and make a link to high-energy phenomena observed at the LHC or provide the first hints of new physics at the electroweak scale. Two neutrino experiments, one (MiniBooNE) operating at low energy, and a long-

baseline experiment (NuMI-MINOS) using beam from the Main Injector, are examining oscillations in two of the three regions where neutrino flavor transitions may have been observed.

In astroparticle physics the program features a three-pronged approach to key features of our universe, comprising a broad optical survey (Sloan Digital Sky Survey, SDSS) sensitive to many astrophysical phenomena, a direct search for cold dark matter (Cryogenic Dark Matter Search, CDMS), and the exploration of cosmic rays at the highest energies (Auger Experiment).

The Tevatron Collider started to operate twenty years ago; the new physics questions point to a need for new accelerators, and new large experiments. While we may not know the answers to the questions we pose, we do know the tools that we need in order to address them. The LHC at CERN will start to operate later in this decade. Fermilab is committed to using it to learn everything we can about the new world of TeV-scale phenomena. However, there is a strong consensus that deep understanding of these phenomena will call for very detailed and precise measurements that can only be done at an electron-positron Linear Collider, in which Fermilab should plan to play a major role. In addition, we see a path to learn fundamentally new things about the world from experiments probing neutrino masses and mixing, with a step-by-step program of new facilities that builds on Fermilab's existing strengths in this area. We consider these three major components of the future program in turn.

The charge to the committee asked it to consider two possibilities for the Linear Collider: siting at Fermilab and siting offshore. The committee focused on the first of these. As host, Fermilab would bring enormous strengths to the Linear Collider: an excellent physical location, technical strengths that are among the best in the world, and long experience exploring physics at the energy frontier. Likewise, the Linear Collider would bring to Fermilab the opportunity to explore the revolutionary physics that we anticipate at the TeV scale. The Illinois sites are close enough to the existing Fermilab site for anyone to work at each on a daily basis. The committee concludes that Fermilab should make bidding to host the Linear Collider in northern Illinois its highest priority for the future.

This committee explored many of the issues associated with hosting the Linear Collider and enumerated what would be needed for Fermilab to mount the strongest possible case to host the Linear Collider Project. The immediate steps that Fermilab must take include developing further the necessary expertise at Fermilab, establishing performance goals, developing design studies, and bidding to host an Engineering Test Facility that will fully demonstrate the chosen technology. The Laboratory should also develop a hosting model that would support other exciting HEP research in parallel with the Linear Collider. These efforts will require enhancing the organization within the Directorate to coordinate and direct Fermilab Linear Collider activities and to communicate to outside institutions. Regardless of its location, a successful Linear Collider initiative will require a major commitment and a full leadership role from Fermilab.

A major component of the present and future experimental thrust is neutrino physics. Fermilab hosts the national long baseline neutrino oscillation facility NuMI, which

consists of an intense neutrino beam directed at a large detector underground in Minnesota. This project is close to operation and forms a strong springboard for further exploration. In the near future we will further exploit the NuMI beam; for example we can mount a new, larger detector on the surface, at a similar distance to that of the the existing detector but about 15 km from the axis of the neutrino beam. Characteristics of the neutrinos in this direction would then be exploited to measure the amount of electron neutrino present in the heaviest neutrino eigenstate, one of the key unexplored parameters of our description of neutrinos. Depending on the value of this parameter, the same technique could be used (with an enhanced flux of neutrinos if needed) to explore the ordering of masses of the three known neutrinos. With a sufficiently intense neutrino beam and with an adequate suite of experiments, the program could be carried into the observation of CP violation in the neutrino sector.
Fermilab's capabilities are uniquely suited to this physics. The committee feels that neutrino physics forms an exciting program and one that Fermilab should pursue vigorously.

Exploring the new world of neutrinos will require larger experiments and a more intense neutrino beam. The latter depends on the beam power available in the primary proton source. A subcommittee considered two proposals for improving the Fermilab proton source. One is a superconducting linear accelerator; the other is a rapid cycling synchrotron. Either could deliver the required beam power but the linac option has many other attractive features. Either would also require upgrades to the Main Injector. Such an accelerator could be designed, approved, and built by approximately the middle of the next decade. The physics case for intense neutrino beams is sufficiently compelling that the committee calls for the preparation of a Conceptual Design Report and other documentation sufficient to request a statement of Mission Need from the DOE in parallel with preparations for the Linear Collider.

Of course, the construction of the neutrino experiment and the increases of the beam intensity will need to be optimized. If Fermilab is the host for the Linear Collider and it is under construction, resource constraints will limit the scope or speed at which upgrades to the neutrino program could be constructed. If the Linear Collider is located offshore, such constraints will be less pressing. In either case, we envisage enhancements beyond the present neutrino experimental program.

Under any scenario, Fermilab will play a critical role in the Large Hadron Collider program: accelerator, experiments, analysis, and interpretation. Fermilab has unique attributes which can lead to it being the main center for CMS physics analysis, a leader in the development of grid computing and a leader in R&D for LHC accelerator and detector upgrades. The committee strongly endorses Fermilab's commitment to LHC participation and has laid out some ways to reinforce this effort.

The physics questions and opportunities will require new ways of working involving greatly increased global collaboration. The word "*collaboratory*" is increasingly used to describe the character of the experiments for the Large Hadron Collider (LHC) and particularly aspects involving the exploitation of a worldwide Grid of computing resources. Each experiment involves more than 1500 physicists. The LHC has already attracted the largest collaboration of physicists ever to come together to build an

accelerator; future projects of this scale are likely to be explicitly international in character.

Flavor physics and studies of the strong interaction are prominent components of the current and near term Fermilab program. With the enormous discovery potential of the coming decade, quark flavor physics may well become an essential probe of new physics, providing tests and constraints on theories, magnified sensitivity to new physics in many channels, and a unique and different view of the physics. The extant Fermilab accelerator infrastructure, including the Main Injector and Tevatron, together with a future proton driver, provides a unique opportunity for the future exploration of new physics through sensitive quark flavor physics probes.

Finally, the committee considered a number of other, currently modest, components of the laboratory program.

Particle astrophysics provides important new probes of fundamental physics that complement accelerator experiments and Fermilab was the first particle physics laboratory to establish an astrophysics effort. Given the discovery potential of this field and the strong astrophysics program currently in place, the committee feels that Fermilab should strive to expand its leadership role and grow its program in Particle Astrophysics.

Accelerator R&D is essential to the future accelerator-based exploration of matter and its properties, and is therefore essential to Fermilab's mission. Increased support is needed to provide timely options for an exciting long-term future beyond the LHC and Linear Collider. The committee advocates increased support for Accelerator R&D.

Other areas, such as detector R&D, computational physics, and collaboration on societally important issues such as accelerator-based medical treatment and science education, are also potentially important components of the future program.

The committee considers that for all these smaller programs there are a number of options, as indicated in the relevant sections, to significantly improve the execution of the programs; not all of those measures would demand a significant increase in resources.

If the Linear Collider comes to northern Illinois, Fermilab will have the opportunity to lead in the revolutions at the energy frontier. This future would be optimal for the science and for Fermilab. If the Linear Collider is sited offshore, Fermilab will take a preeminent role in the revolutions in the field of neutrino physics, while being a leading participant in the Linear Collider whereever it is built. Fermilab must prepare for both scenarios. As the situation evolves and becomes clearer as this decade advances, Fermilab will be poised to proceed with the Linear Collider or the Proton Driver. In either case, the two programs, together with strong LHC participation, quark flavor experiments, Astrophysics, and research into future accelerator technology will constitute a Laboratory with a vibrant program in revolutionary times.

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14. Acknowledgements

The committee could not have completed this report without the broad participation of the Fermilab community. Some of those we have identified are named as members of the sub-committees. Some, including Directors of other laboratories, called in, gave us their views and helped us on our way. To all we are very grateful. The many meetings, discussions and exchanges of information could not have happened without the logistical support from Mary Cullen, Michelle Gleason and Marilyn Smith. We also would like to thank the Fermilab Director for having requested our participation in this important work.

Appendix A – Membership of the Committee

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Steve Holmes (Deputy)
Jeff Appel
Joel Butler
Marcela Carena
Josh Frieman
Steve Geer
Chris Hill
Bob Kephart
Sergei Nagaitsev
Jim Strait
John Womersley

External members
Gary Feldman, Harvard
Young-Kee Kim, Chicago
Peter Meyers, Princeton
Angela Olinto, Chicago
Ritchie Patterson, Cornell

Appendix B- Charge to the Committee

Charge to the Fermilab Long-range Planning Committee

Particle physics stands at the threshold of a new era of discovery, made possible by experiments now operating or starting up in the next few years. As the largest U.S. laboratory dedicated to High Energy Physics, Fermilab has a special responsibility to optimize the opportunities for making new discoveries about the nature of matter, energy, space, and time.

The 2001-2 HEPAP Subpanel on Long-Range Planning for U.S. High Energy Physics articulates the goals of the field well. They recommend, "that the United States take steps to remain a world leader in the vital and exciting field of particle physics, through a broad program of research focused on the frontiers of matter, energy, space, and time." The Subpanel also recommended that the U.S. participate in the Linear Collider, wherever it is built in the world, and that the U.S. prepares to bid to host such a facility. Finally, the HEPAP Subpanel argued persuasively that to address the range of compelling scientific issues the field needs a broad range of experimental strategies and techniques.

I would like the Long-range Planning Committee to develop in detail a few realistically achievable options for the Fermilab program in the next decade under each possible outcome for the Linear Collider. The goal in developing each option should be to optimize the opportunities available at Fermilab in this period for high-energy physicists to answer the most important questions in our field. The options should be guided by the priorities for the field as laid out in the HEPAP Subpanel and in the HEPAP response to the Office of Science on the facilities plan.

The committee should develop scenarios for each of the two cases spelled out by the HEPAP Subpanel.

1. A Linear Collider project will be built here, starting late in this decade with international support and organization.
2. The Linear Collider will be built offshore with substantial participation from U.S. High Energy Physics.

In either case, you should make the following additional assumptions.

1. Fermilab will have a central role in an active U.S. research program at the LHC, both as host of the US-CMS collaboration and as developer of accelerator upgrade plans.
2. Fermilab will carry out the presently approved program of experiments following approval from the national program.

The context for the plan includes the following:

1. The plan should fit into, and be a major component of, the twenty-year roadmap for the field described in recommendation two of the HEPAP Subpanel reports. Another important planning document is the recent HEPAP submission to the Office of Science for the facilities plan.

2. The initial assets that will make it possible to build a strong future with available resources are the existing facilities at Fermilab, the strengths of the existing Fermilab staff, and the active participation of a strong Fermilab user community.

I would like the Committee to give an interim progress report in time to discuss the important issues at the Aspen meeting of the Fermilab Physics Advisory Committee. We will develop a schedule for the committee to write a final report after the initial meetings to organize the work.

Appendix C – Membership of the subcommittees

Accelerator R&D: Pierre Bauer, Jerry Blazey, Court Boehn, Don Edwards, Helen Edwards, Steve Geer (Convenor), Chris Hill, Steve Holmes, Bob Kephart, Kwang-Je Kim, Sergei Nagaitsev, Philippe Piot, Panagiotis Spentzouris, Jim Strait, Mike Syphers, Alvin Tollestrup, Chris White.

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Physics Landscapes: Jeff Appel, Joel Butler, Marcela Carena, Chris Hill (convenor);

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Proton Driver: Weirin Chou**,** Dave Finley, Bill Foster, Steve Geer, Chris Hill, Bob Kephart (convenor), John Marriner, Peter Meyers, Shekhar Mishra, Sergei Nagaitsev, Victor Yarba.