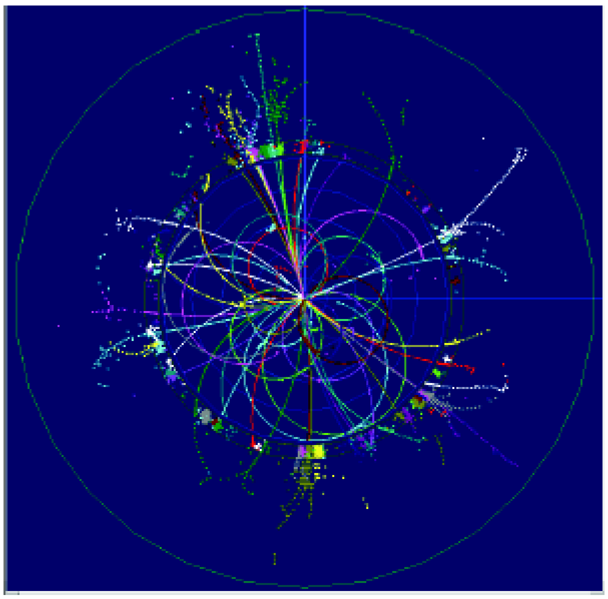
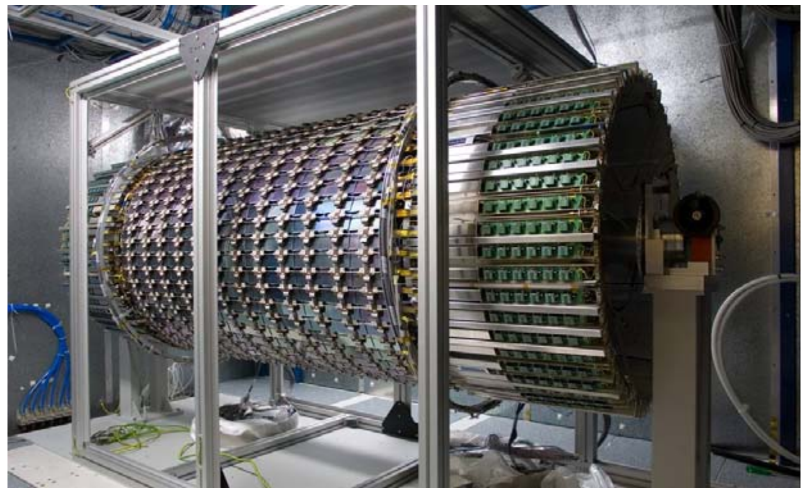
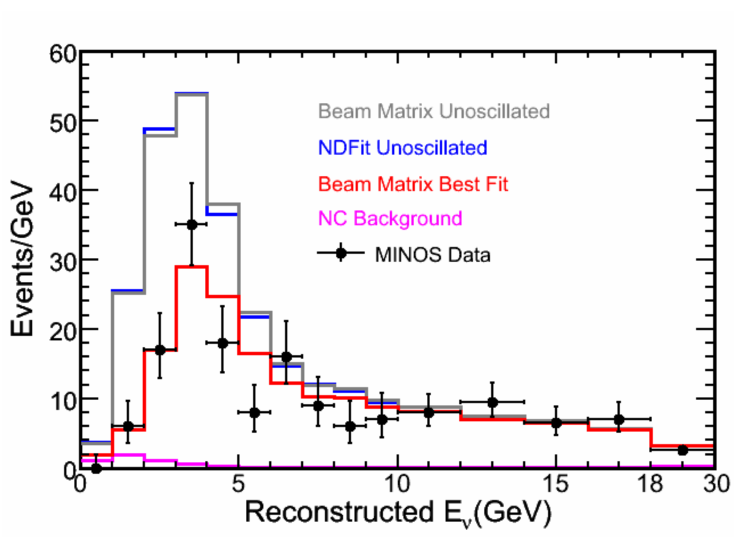
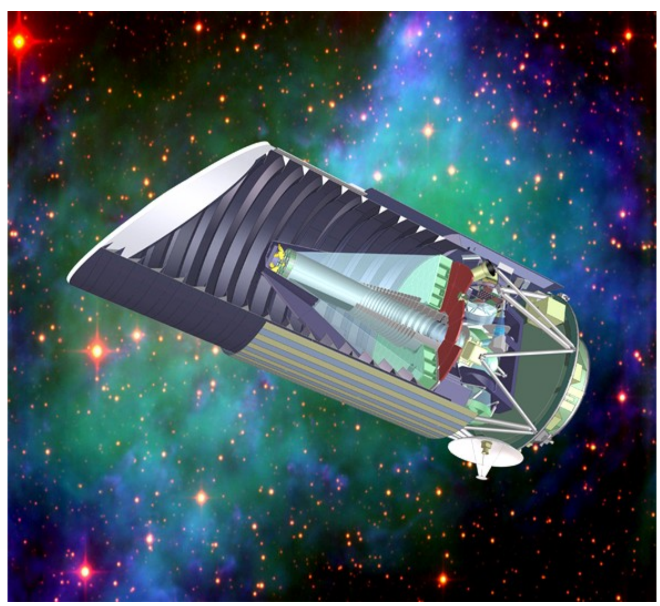
P5 Report: The Particle Physics Roadmap

October 2006









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I. Introduction

Research in particle physics strives to uncover the basic building blocks, or constituents, of the universe. We are made of such constituents, but some have had a role mainly in the early history of the universe, helping to shape the future that we now see. To complete the physics picture we must discover any as yet unobserved constituents, the rules governing the interaction of the building blocks, and the nature of the space and time in which they reside. The quantum framework of modern physics and the geometrical character of gravity with its possible extension to more dimensions is the intellectual framework that captures these concepts.

Building on many earlier discoveries, particle physics has made great strides over the past 50 years. The picture developed, although incomplete, allows us to understand much of the physics of our local neighborhood of the universe. The electromagnetic, weak, and strong forces provide an understanding of atomic phenomena, the principles underlying nuclear behavior, and after including the gravitational potential, an understanding of why the sun shines and why some stars explode. We have managed to look into the center of the sun using neutrinos. The physics of the three forces and the known constituents of matter are collectively described by a mathematical theory called the "Standard Model." The great success of this model implies that crucial, never before observed phenomena, will be seen by the experiments now being prepared to understand better the nature of the vacuum and the origin of mass.

On a larger scale we find that we live in a vast universe with an innumerable number of stars bound to their host galaxies, which are arrayed in spectacular filamentary structures of matter. The luminous structures, made of the matter and energy whose nature has been understood during the last century, is however a small fraction of the total matter. From observations of how tightly the visible matter is bound, we find that a still mysterious non-luminous component, "dark matter", must exist.

We now understand that the history of the universe spans roughly 13.7 billion years. Some objects such as quasars had their moment in time and now no longer exist; their former existence revealed by light traveling to us over billions of years. Evidence for a very dense high-energy birth, followed by expansion and cooling, is provided by cosmic microwaves, messengers from the early period of the universe. The residual matter from this epoch formed the galaxies and their groupings into large-scale structures, all expanding outwards. A surprise finding is that this expansion appears to continue at an accelerating pace. The most conservative explanation is that this is due to a second dark component that we call "dark energy".

To examine nature more deeply and continue our exploration of the physical universe requires the development of new, more powerful, scientific instruments. The roadmap we present here lays out a plan for the development and use of such instruments for particle physics research over the next decade. These instruments will take us beyond the Standard Model.

Beyond the Standard Model

The main body of this plan for particle physics summarizes the key instruments under consideration by the field and their potential to answer fundamental scientific questions. We provide priorities among options and time-scales for construction and utilization. The experiments planned address the scientific frontier as presently understood although we fully expect that new discoveries will take us beyond the questions now current in our field. A succinct summary of our present goals has been provided by the report:
Quantum Universe
The Revolution in 21st – Century Particle Physics

We repeat them here as a mission statement for the field.
1) Are there undiscovered principles of nature:
   new symmetries, new physical laws?
2) How can we solve the mystery of dark energy?
3) Are there extra dimensions of space?
4) Do all the forces become one?
5) Why are there so many kinds of particles?
6) What is dark matter? How can we make it in the laboratory?
7) What are neutrinos telling us?
8) How did the universe come to be?
9) What happened to the antimatter?

II. Executive Summary

P5 is charged to maintain the U.S. Particle Physics Roadmap for the more costly projects of our field. In this report we have constructed a new Roadmap. It includes specific recommendations for project construction and R&D for the next five years and recommendations for review dates for projects that we anticipate being ready for construction early in the next decade. These along with ongoing projects and those whose construction is nearing completion form the new Roadmap. The detailed charge to the committee can be found in Appendix 2.

Our report covers the following topics: a discussion of the major science-opportunities (Chapter III); discussion of potential projects, their costs, and assumptions regarding government agency budgets (Chapter IV); planning guidelines which follow from the science and budget projections (Chapter V); explicit recommendations for construction and reviews (Chapter VI); additional recommendations on projects and directions in the various research areas (Chapter VII); and a more extensive discussion of the various opportunities within the experimental program (Chapter VIII). The section on agency budgets provides a budget plan called the base budget, which we use for our roadmap, as well as a budget which would allow a doubling of support over 10 years, as might be appropriate for a renewed emphasis on the physical sciences and their importance to the country's economic health.

We have grouped the major science opportunities into five categories, which we list below.

1) The energy frontier projects: LHC-ILC. These have enormous discovery potential, including the possibility to discover new symmetries, new physical laws, extra dimensions of space-time, an understanding of dark matter, and improve our understanding of the nature of the vacuum and the origin of mass. The experiments at the LHC will start data taking in FY08. The ILC is under development as an International Project with strong U.S. participation.
2) A program to understand the nature of dark matter, which has been manifest to date through astrophysical measurements. Primary efforts involve laboratory programs to produce dark matter at the LHC and then analyze its properties in detail at the ILC, experiments aimed at direct detection of cosmic dark matter through scattering in materials, and measurement of particles produced by cosmic dark matter

annihilation. This field has many innovative techniques in a development phase and the Deep Underground Science and Engineering Laboratory (DUSEL), the subject of an NSF MREFC proposal, would provide a location for a large-scale dark matter scattering experiment.

3) A program to understand the nature of dark energy, which accelerates the expansion of the universe. Unlike most phenomena, dark energy can only be studied through astronomical observations at the present time; therefore the large-scale projects from the particle physics community involve interagency collaborations with the astronomy program at the NSF (toward an earth based telescope) or NASA (toward a space based telescope). The program envisions smaller (called Stage III) projects that could start data collection before the end of the decade and an ambitious earth based survey telescope and novel space based dark energy mission (called Stage IV projects).
4) Neutrino science investigations using neutrino-less double beta decay, reactor and accelerator neutrino oscillation experiments, and neutrinos from sources in space. The experiments have a broad agenda: to study the neutrino mass spectrum and mixing parameters, to determine whether neutrinos are their own antiparticles, and to study objects that act as high energy accelerators in space. A topic of particular importance is CP violation in this sector since neutrinos may have played an important role in generating the asymmetry between the quantity of matter and antimatter that we observe in the universe.
5) Precision measurements involving charged leptons or quarks. The study of these fermion systems has historically provided much of the information embodied in the Standard Model. Rare processes sensitive to potential new physics provide tests for and constraints on processes beyond the Standard Model. Such measurements could add valuable information required to understand discoveries at the energy frontier. Potentially interesting processes include measurements of the muon g-2, μ to e conversion, rare decays visible in a very high luminosity B experiment, and rare K decays using kaon beams.

In order to arrive at recommendations, we have articulated a number of planning guidelines. We summarize the key points here. They have been developed with the recent recommendations of the EPP2010 committee in mind, the goal of capitalizing on the major science opportunities before us, and the specific numbers in our base budget plan.

1) The LHC program is our most important near term project given its broad science agenda and potential for discovery. It will be important to support the physics analysis, computing, maintenance and operations, upgrade R&D and necessary travel to make the U.S. LHC program a success. The level of support for this program should not be allowed to erode through inflation.
2) Our highest priority for investments toward the future is the ILC based on our present understanding of its potential for breakthrough science. We need to participate vigorously in the international R&D program for this machine as well as accomplish the preparatory work required if the U.S. is to bid to host this accelerator.
3) Investments in a phased program to study dark matter, dark energy, and neutrino interactions are essential for answering some of the most interesting science questions. This will allow complementary discoveries to those expected at the LHC or the ILC. A phased program will allow time for progress in our understanding of the physics as well as the development of additional techniques for making the key measurements.
4) In making a plan, we have arrived at a budget split for new investments of about 60% toward the ILC and 40% toward the new projects in dark matter, dark energy, and neutrinos through 2012. The budget plan expresses our priority for developing the ILC but also allows significant progress in the other areas. We feel that the investments in dark matter, dark energy, and neutrino science in our plan are the minimum for a healthy program.
5) Recommendations for construction starts on the longer-term elements of the Roadmap should be made toward the end of this decade by a new P5 panel, after thorough review of new physics results from the LHC and other experiments.

To provide recommendations for major construction and R&D activities we have grouped the projects under consideration into several broad categories, with different degrees of priority for each group. We list groupings below in priority order. They are based on our set of planning guidelines. The activities are meant to mainly fit into a five-year timeline.

1) The highest priority group involves the investigations at the energy frontier. These are the full range of activities for the LHC program and the R&D for the ILC.
2) The second group includes the near-term program in dark matter and dark energy, as well as measurement of the third neutrino-

mixing angle. This grouping includes three small experiments: the 25 kg Cryogenic Dark Matter Search experiment, the Dark Energy Survey, and the Daya Bay reactor experiment. Also in this group is the support to develop the Stage IV dark energy experiments, the LSST and SNAP, to bring these to the "Preliminary Design Review Stage" in the case of the NSF and "CD2 Stage" in the case of the DOE over a two to three year time frame. We recommend that the DOE work with NASA to ensure that a dark energy space mission can be carried out and that the three potential approaches to the mission, one of which comes primarily from the particle physics community, have been properly evaluated. The final item in this group is the R&D funding for DUSEL, along with support by the NSF and the DOE for R&D for both a large dark matter and neutrino-less double beta decay experiment.

3) The next item is the construction of the NOνA experiment at Fermilab along with a program of modest machine improvements.
4) The final item is the construction of the muon g-2 experiment at BNL.

Matching the costs of these projects to our budget scenarios, we find that the first three groupings can be carried out in the base budget plan. Note, however, that the ILC R&D ramp up profile, chosen to match the 60% of new investment goal expressed in our planning guidelines, and the NOνA construction schedule must both be slowed with respect to the most aggressive proposals, if the costs are to be matched to the assumed annual budgets. These three groupings of projects would effectively address the nine questions presented as the mission statement for the field in our introduction. This includes near term projects as well as R&D investments for highly capable future projects, satisfying our most important science goals. We note that we have not considered and do not mention explicitly many smaller projects that should be supported but are below our cost threshold for prioritization. Our own evaluations match well the conclusions in the EPP2010 report.

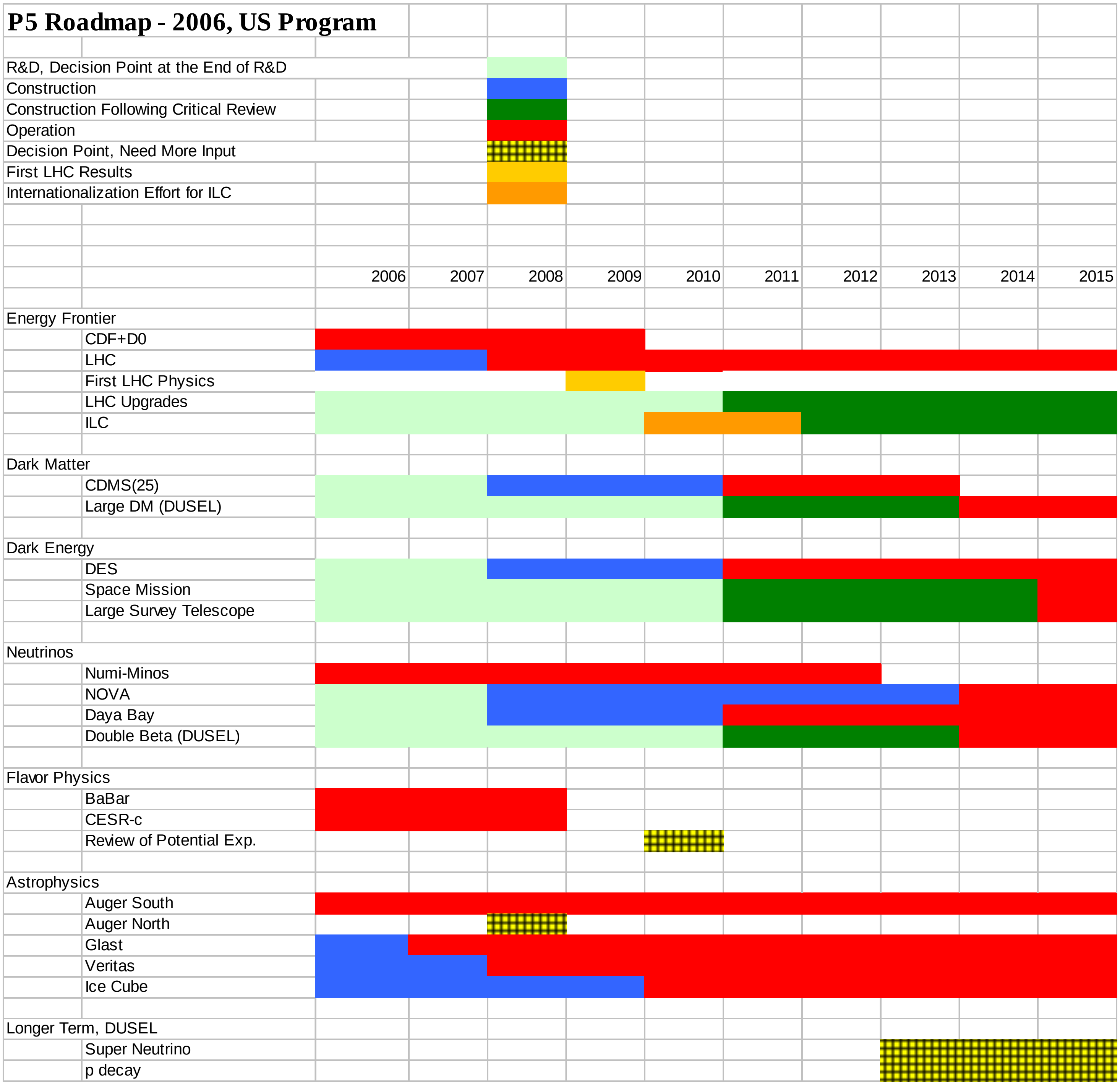
The budget that would double support over a decade would have a very significant science impact by allowing added support for the Stage IV dark energy experiments. The preparatory work for these could be completed in a more timely way, while we also pursue the other important areas in our first two groupings. In addition, the ILC R&D could be pursued more vigorously. In this scenario the muon g-2 experiment could be considered for construction.

We recommend a review by P5 toward the end of this decade to look at projects that could start construction early in the next decade. The base budget plan would allow a significant number of these to move forward to construction. The review should take into account new physics results, especially those from the LHC, results on R&D for new projects, budget and cost projections at the time, and the status of interagency agreements and MREFC plans. We list some of the areas to be examined.

1) The ILC, including a possible U.S. bid to host, and the steps needed at the governmental level for internationalization.
2) The LHC Upgrades, required for an order of magnitude luminosity increase at the LHC.
3) DUSEL and the large experiments to search for dark matter and neutrino-less double beta decay.
4) The Stage IV dark energy experiments, a large survey telescope and a dark energy space mission. Interagency agreements are crucial to these projects, which could start construction soon after review.
5) An evaluation of the status of flavor physics and the importance of further experiments across a number of possibilities such as the muon g-2, μ to e conversion, a very high luminosity B experiment, and rare K decays.

We anticipate that a separate review by P5 will be required to look at the best directions for further experiments in neutrino physics. Much work is ongoing internationally in this area with an optimum program dependent on measurements to be made by the next generation of neutrino experiments as well as results from ongoing R&D. A second important physics area that might be included in this review would be an ambitious proton decay experiment. These two projects could be the major second phase of experiments for DUSEL. The physics results over the next five to ten years will determine the best date and best set of areas to look at in such a review.

The following chart shows the resulting roadmap from the considerations discussed above. It assumes the base budget scenario discussed in greater detail in Chapter IV of this report.


P5 Roadmap - 2006, US Program
R&D, Decision Point at the End of R&D
Construction
Construction Following Critical Review
Operation
Decision Point, Need More Input
First LHC Results
Internationalization Effort for ILC
2006
2007
2008
2009
2010
2011
2012
2013
2014
2015
Energy Frontier
CDF+D0
LHC
First LHC Physics
LHC Upgrades
ILC
Dark Matter
CDMS(25)
Large DM (DUSEL)
Dark Energy
DES
Space Mission
Large Survey Telescope
Neutrinos
Numi-Minos
NOVA
Daya Bay
Double Beta (DUSEL)
Flavor Physics
BaBar
CESR-c
Review of Potential Exp.
Astrophysics
Auger South
Auger North
Glast
Veritas
Ice Cube
Longer Term, DUSEL
Super Neutrino
p decay

III. Major Science Opportunities

We summarize in this section some of the major science opportunities before us. Each science opportunity addresses in a unique way some of the nine major frontier science questions listed in the introduction. The ordering of topics below is not meant to indicate priorities among the science questions. Specific experiments are discussed in detail in subsequent sections. We base our presentation of dark energy science on the DETF report, from which we have taken much of the material for our discussion of dark energy.

1. The Energy Frontier: LHC-ILC

Over the last 50 years particle physics has achieved a remarkable understanding of the constituents of matter and the underlying dynamics describing the interactions between them. This was possible through a complementary and vigorous experimental program at various accelerator based facilities around the world, a continued increase in the available energy at the frontier, improvements in detector technology, and a steady improvement in understanding the fundamental theory. This effort has resulted in the Standard Model (SM), which is the framework for describing the forces and the constituents of matter that were present in the visible universe when it was 10^{-12} seconds old. The Standard Model is based on symmetries that we know are broken as revealed by the different behavior of the weak and electromagnetic forces (called electroweak symmetry breaking). However, we have not yet observed how these symmetries are broken. In addition, the Standard Model does not address many fundamental questions including how particles acquire their mass, or how gravity relates to the other forces. To understand how the symmetries of the SM are broken and to find the answers to these questions, we have to explore energy regimes that are beyond our current experimental reach. Fortunately general arguments and data taken to date indicate that the next step in energy will reveal key missing elements of our physics picture.

The simplest picture for the breaking of the SM symmetries involves a number of scalar fields. In this picture the lowest energy state of the universe has all of space-time filled by a field, called the Higgs field, which through interactions with the other particles generates their mass and mixings. Since particle properties (for example the electron mass) appear to be the same everywhere in the universe this field must exist everywhere, which is what we think of as the vacuum. Extensions to the simplest picture, for example in the case of additional symmetries as in Supersymmetry, can result in a number of scalar fields contributing to the vacuum. Fortunately, in all of these pictures, the Higgs field gives rise to new scalar particles (called Higgs bosons or Higgs particles) that can be produced in the laboratory. These particles have properties that reflect the mechanism by which the vacuum is generated. Using indirect measurements to date, the simplest Higgs picture would have detectable Higgs bosons at a mass near 100 GeV, while theory predicts that it is not heavier than roughly 1 TeV. From general arguments, new physics associated with the Higgs mechanism, such as Supersymmetry or extra dimensions of space, should set in at masses not larger than 1 TeV.

In addition to the successes of accelerator-based physics, the last decade has seen a revolution in astrophysics based on experimental observations of large-scale features of the universe. These observations reveal the existence of additional particles and fields not present in the SM, known as dark matter and dark energy, which comprise ~95% of the universe, as described in more detail in subsequent sections of the report. Both the missing pieces of the SM and the astrophysical observations of a dark universe clearly point to new fields, particles and underlying symmetries that can only be explored in our laboratories by probing interactions at higher energies and smaller distances.

The next step in this direction is the Large Hadron Collider (LHC), under construction at CERN in Geneva Switzerland. The LHC is our first window to unexplored energy regimes, where we expect a revolution in our understanding of physics and unequaled opportunity for discovery and new insights. The discoveries at the LHC will raise compelling questions and will signal a changed perspective on the universe. Responding to these questions will require a tool of greater sensitivity, which can probe the energy frontier with ultra-precise measurements. This is the role of the proposed International Linear Collider (ILC), which will discover the fundamental laws of nature behind the new phenomena observed at the LHC. In addition, the precision data obtainable at the ILC will allow us to telescope to even higher energy regimes via the detection of tiny quantum fluctuations. Together, measurements at the LHC and ILC will reveal the nature of the quantum universe at the next energy regime and beyond.

Physics at the Tera Electron Volt scale

Despite the remarkable progress in our understanding of the fundamental particles and forces over the past decades, many important open questions remain. They can be categorized as follows:

- The question of mass:
  How do elementary particles acquire their mass?
  How is the electroweak symmetry broken?
  Does the Higgs boson –postulated within the Standard Model- exist?

- The question of undiscovered principles of nature:
  Are there new quantum dimensions corresponding to Supersymmetry?
  Are there hidden additional dimensions of space and time?
  Are there new forces of nature?

- The question of the dark universe:
  What is the dark matter in the universe?
  What is the nature of dark energy?

- The question of unification:
  Is there a universal interaction from which all known fundamental forces, including gravity, can be derived?

- The question of flavor:
  Why are there three families of matter?
  Why are the neutrino masses so small?
  What is the origin of CP violation?

Compelling theoretical arguments and precision experimental measurements suggest that answers to at least some of these questions lie in the TeV energy range (Terascale). Currently the energy frontier is at the Tevatron collider at Fermilab, which accelerates protons and antiprotons and collides them at a center-of-mass energy of 1.96 TeV. Since the constituents in the protons and antiprotons that collide have lower energies, this machine can search for new particles up to a mass of ~0.25 TeV, depending on the total accumulated luminosity, how often the particles are produced in collisions, and the signatures they leave in the detectors. An example of this is the search for the Standard Model Higgs particle, the critical missing piece of the Standard Model. The Tevatron can probe part of the region where we expect the Higgs boson to exist, while the LHC accelerator and detectors are designed to explore the entire range. Discovering the Higgs, or whatever else breaks the symmetry of the Standard Model, is one of the main motivations for building the LHC.

The favored candidate for a theory of the symmetries underlying the Standard Model is Supersymmetry which relates fermions to bosons and predicts a supersymmetric partner for every known Standard Model particle. One of these supersymmetric particles is a likely candidate for dark matter, as discussed in the next section of this report. In addition, Supersymmetry provides the means to unify the forces at very high energy scales. Searches for supersymmetric particles are being performed at the Tevatron, and this program will be continued at the LHC. Another possibility is the existence of extra dimensions of space which could answer the question of how gravity relates to the symmetries of the Standard Model. Such additional dimensions also predict partner particles that would be observed at the LHC.

The Large Hadron Collider is a proton-proton collider with a center-of-mass energy of 14 TeV. Such a large collision energy is required in order to reach energies in the TeV range for the collisions of the elementary building blocks within the proton. In parallel, high-energy physicists throughout the world have been constructing components for two large general-purpose detectors, ATLAS and CMS, and two more specialized detectors, LHCb and ALICE, with the latter two being designed to focus on physics in the B meson sector and on heavy ion collisions, respectively. Given the high collision energy, the LHC will be an exploratory machine into the TeV energy range. Ground-breaking discoveries are expected that will shed light on the open questions outlined above. It will definitively answer the question of the existence of the Higgs particle and of TeV-scale Supersymmetry.

If the experimental program at the LHC does not confirm our current theories, it likely would point to radically different concepts, which will cause a rethinking of current theoretical ideas. No matter what the results are, it will change our picture of the universe and will have profound implications on the theoretical description of the world around us at the fundamental level.

The LHC is a discovery instrument, offering our first glimpse of the physics at the Terascale, and having the capability to detect the plethora of new particles and phenomena that we believe reside there. While the discovery of new particles helps us to understand how the universe works, the mere observation and cataloguing of them is only part of the understanding. Particles are messengers, and studying their properties and interactions lead to the discovery of new theories or new symmetries of nature. At the LHC these new particles will be produced in complicated environments along with many other particles. Measuring their properties is thus limited by that environment as well as by assumptions about how the particles are produced. For example, to determine how the Higgs particle really fits into a physics framework, what its exact properties are, and whether there is only one, requires a clean environment where measurements can be made free of theoretical assumptions. This is the hallmark of the International Linear Collider.

The ILC is a proposed high-energy e^+e^- linear collider, designed to work in concert with the LHC. The ILC would consist of two roughly 20 km linear accelerators, which would collide electrons and positrons at their intersection with initially tunable collision energies up to 0.5 TeV and upgradeable to 1.0 TeV. Since the electron is a fundamental particle, the full collision energy of the ILC would be available to study new phenomena. The electron and positron beams can also be polarized, adding resolving power to the subsequent analysis of the collisions. These machine properties result in a clean experimental environment and a complete knowledge of the quantum state of the collision. This removes theoretical or experimental ambiguities or model dependency in analysing the data. The ILC is being organized as a fully international project and its technical design is currently being coordinated worldwide by the Global Design Effort.

The ILC would have unique capabilities that would allow for the identification of the new particles observed at the LHC and the discovery of the underlying theory that gives rise to them. In the possible theoretical scenarios before us today, experiments at the ILC will be able to answer questions such as: does the Higgs have the correct properties to give the measured mass to all particles? Are there additional components to the Higgs boson that would give rise to new physics? Are the partner particles discovered at the LHC associated with Supersymmetry or extra dimensions or something else? Is the symmetry associated with the supersymmetric particles the same as that predicted by Supersymmetry? How many extra dimensions are there, what is their size and shape, and where do the elementary particles reside within them? What is the mass, spin, and couplings of the dark matter particle? Do they account for the thermal relic density of dark matter in the universe as determined by astrophysical observations? The answer to each of these questions would have profound implications on our understanding of the history of the early universe.

Some of our pressing physics questions, such as unification of the forces, may not have solutions at the TeV scale, but rather involve physics at higher energies. Finding answers to such questions through direct experimental investigations would call for energies far beyond those attainable with any particle accelerator. Access to these questions may, however, be obtained indirectly through high-precision measurements of tiny quantum fluctuations that can reveal the physics at very high-energy scales after performing well-controlled extrapolations. The prerequisite for such extrapolations is a very accurate determination of particle properties. This precision cannot be achieved at the LHC alone and requires the measurements that can be made at the ILC.

The data and knowledge that can be expected from the ILC will be complementary to and build upon the results obtained at the LHC. The LHC will unveil the existence of new phenomena, and the ILC would zoom in on properties and probe the details of these phenomena, thus revealing the underlying science. An historical example of such a synergistic relationship is provided by the measurements performed at LEP and the complementary measurements from the Tevatron. Both machines were necessary for our understanding of the constituents and forces of the Standard Model, and in particular for discovery of the top quark and tests of its role in electroweak physics.

Given our theoretical expectations for new physics at the Terascale and that the LHC will start operations at 14 TeV in 2008 and most likely will have first results on searches for new phenomena around 2009/2010, we should be in a position to make a strong physics case for the ILC around 2010. To be able to be in a position at that time to define an ILC construction project, ILC R&D will have to be pursued aggressively worldwide by the Global Design Effort in the intermediate time.

If the past is any guidance and our expectations are correct, then exploring the new energy regime of the Terascale will result in a revolutionary understanding of the universe and will guide the science of particle physics for decades to come.

2. Dark Matter

The nature and origin of dark matter is today one of the large science questions. While astrophysical observations require that it exists, and that it constitute a major portion of the mass of the universe, we do not know what it is. We do know that it is not ordinary matter. In this sense, dark matter provides the first and most robust evidence for physics beyond the Standard Model (SM) of particle physics. There are, in fact, several speculations as to the identity of dark matter coming from particle physics theory, but we will not be able to resolve these until we have more data. Thus, the detection and study of dark matter in terrestrial experiments must be one of the priorities of particle physics in the near future.

A large number of astrophysical observations provide strong evidence that roughly 23% of the energy density of the universe consists of dark matter, whose presence is inferred only from its gravitational influence:

- the observed rotational curves from galaxies contradict the expectations from Newtonian mechanics under the assumption that most of the matter of the galaxy is concentrated in the luminous central bulge.
- gravitational lensing studies show that the distortions of images of distant galaxies require large amounts of non-luminous matter.
- the observed large-scale structure of the universe cannot be explained unless the formation of galaxies is seeded by matter which interacts dominantly through gravity.
- precise measurements of anisotropies in the cosmic microwave background radiation require matter that interacts only very weakly with photons.

The evidence that dark matter exists is clear, yet the nature and origin of dark matter is unknown. Observations of the large-scale structure of the universe and measurements of the baryon abundance do not favor models in which the dark matter comes from baryonic dark matter such as non-luminous gas, brown dwarfs and so-called MACHOs (Massive Compact Halo Objects), e.g. small planets, neutron stars, black holes, etc. Measurements of neutrino properties indicate that ordinary neutrinos make only a small contribution to the total dark matter density. Furthermore, observations of the small-scale structure of the universe demand that dark matter particles are non-relativistic – this is referred to as “cold” dark matter. Attention has therefore been focused on particle models of cold dark matter, of which there are many candidates.

The Standard Model (SM) of particle physics provides no viable candidate for cold dark matter. Theoretical particle physics extensions to this model provide many candidates for dark matter particles, and the best-motivated ones are:

1. Axions: these particles were postulated to solve the problem of the absence of CP-violation in the strong interactions. They would have very small interaction cross-sections for the strong and weak interactions. Their masses should be extremely small, in the range 10^{-6} to 10^{-3} eV.
2. WIMPs: these “weakly-interacting massive particles” should have masses on the order of the electroweak scale, and would interact weakly, similar to the interactions expected for a heavy neutrino. There are many WIMP candidates in models of electroweak symmetry breaking that have an extra discrete symmetry, resulting in a stable lightest non-SM particle. Currently, the most studied candidates are:
   a.) neutralinos, in models where they are the lightest supersymmetric particle,
   b.) the lowest mass Kaluza-Klein excitation of the neutral gauge bosons, predicted by theories of universal extra dimensions,
   c.) the T-odd heavy photon in Little Higgs models.

Assuming that WIMPs comprise the dark matter and that they were in thermal equilibrium in the early universe, particle physicists are able to compute the relic dark matter density and compare it with WMAP measurements of the Cosmic Microwave Background Radiation, assuming that standard cosmology can be used to extrapolate back in time. In the framework of specific models one can then compute the cross-sections needed for predicting interaction rates for the terrestrial observation of dark matter particles.

There are three avenues for observing dark matter in various experiments:

1. Direct detection: WIMPs scatter elastically off of atomic nuclei whose recoil can be observed in specially designed apparatus. Axions interact with photons in a highly sensitive resonant cavity.

2. Indirect detection: WIMPs in the cosmos annihilate and the products of that interaction (high-energy photons, leptons, neutrinos, or even hadrons) are observed.
3. High-energy colliders: WIMPs are produced directly in the collisions of hadrons (Tevatron and LHC) or electrons (ILC). The Tevatron or the LHC will find evidence for dark matter particles through apparent missing energy in events with jets, leptons and/or photons. The ILC will allow precise measurements of the WIMP mass, and of the properties of other new particles. This will allow theorists to compute the relic dark matter density, at least within a given model, and relate it to astrophysical measurements.

None of these approaches alone is capable of providing a complete understanding of dark matter. The rates of direct detection experiments are proportional to the local density and velocity distribution of dark matter particles at the detector, which depend on the assumed dynamics of the galaxy. Signals of distant WIMP annihilation cannot be related to dark matter density without postulating couplings and branching ratios, which will be highly model dependent. At colliders, events with missing energy may provide evidence for new, weakly-interacting, neutral particles, but this does not prove that those particles are the same as the ones that constitute dark matter. In particular, the stability of such particles on cosmological scales cannot be verified. The clear conclusion is that a full understanding of dark matter will require data from all possible sources.

Experimental Approaches

The experimental challenge for the direct detection of WIMPs scattering in a material is formidable. The signal from elastic WIMP-nucleus scattering is both extremely rare and small in amplitude, so special techniques are needed to distinguish it from natural radioactive background. The instrumental approaches are too numerous to describe here, so only some of the highlights will be mentioned briefly.

- Very pure scintillating crystals kept at low temperatures might show an annual modulation in counting rate, which could be attributed to variations in the earth's velocity with respect to the galactic WIMP population. In fact, a signal has been reported by such a direct detection experiment, but it has been contradicted by other experiments.

- More advanced experiments use large masses cooled to mK temperatures in which two observables characterizing each recoil are recorded for every event in order to better suppress backgrounds. Some experiments can observe the ionization of the nuclear recoil as well as the phonons propagating through, for example, Ge and Si crystals, allowing them to reject photon-induced electron events on an event-by-event basis. Others can measure both ionization and scintillation light in large $CaWO_4$ crystals. At present, cryogenic experiments at subKelvin temperatures are at the forefront of direct WIMP detection and provide the strongest constraints.
- The future of this field may be dictated by the large liquified noble-gas experiments, using for example Xenon or Argon as the target mass. These techniques are in some cases in their first physics runs or at the level of working prototypes, and a vigorous R&D phase is in place right now; the hope is to build ultimately a device with a mass of a ton or more.
- Another important R&D project uses a large low-pressure TPC (time-projection chamber) to image the nuclear recoil tracks from a scatter, allowing for a direct measurement of the incoming WIMP direction. Again one plans to exploit the annual and also daily modulations of the signal. Still other methods presently in R&D using, for example, warm liquids or high-pressure gases are showing promise as alternative ways to extend the reach in sensitivity.

The field is vigorous with many parallel and competing efforts. The cryogenic experiments are leading the field in sensitivity. The present generation of discriminating experiments should reach cross-sections as low as a few 10^{-8} pb in the next couple of years, and hence will probe interesting regions for supersymmetric models of dark matter. The liquid noble-gas detectors are rapidly developing technologies and provide a promising avenue towards constructing multi-ton detectors. Intermediate and future generation experiments with detector masses in the 100 kg to one ton range and with further improvements in background rejection, are expected to reach sensitivities of about 10^{-10} pb and to probe the bulk of the theoretical parameter space for supersymmetry. One anticipates that some of the R&D efforts will coalesce, allowing larger and more robust collaborations to be formed.

Also, experiments searching for axions will explore interesting possible dark matter mass ranges and will be able to find axions that make up a sizeable fraction of the dark matter halo density. The remaining challenge will be to increase the searchable axion mass range.

Efforts to observe indirect signals of cosmic WIMPs can target a number of processes. For example, the products of WIMP-WIMP annihilation could be potentially detected through gamma ray, positron, anti-proton, anti-deuteron and neutrino signals. While the signals may be energetic, helping in the isolation from background, the rate depends on the distribution of WIMPs, which is highly uncertain and model-dependent.

The production of WIMPs at high-energy colliders is a large subject spanning three major facilities and several models of new physics. The salient points are:

- The Tevatron experiments have recorded more than 1 fb^{-1} of data and searches are underway for new particles predicted by supersymmetry, extra-dimensions, and Little Higgs models. The reach will cover only a limited region of theoretical parameter space, but in some instances it could be complementary to dark matter searches at ongoing and near future direct detection experiments.
- The LHC should start taking data in 2008. With seven times the energy and orders of magnitude higher luminosity than the Tevatron, this machine has a much greater sensitivity to new physics processes. There is a good chance that it will find evidence for dark matter particles, but it is unlikely that the precise identity of the WIMP will be established. The properties of new particles can be measured with only modest precision, and in many case studies it appears to be impossible to ascertain which theory is the correct one.
- The ILC is a proposed high-energy e^+e^- collider with strong international support. Data from such a machine can give information on the quantum numbers of the WIMP via measurements of angular distributions and the shape of the cross-section near threshold, and allow the precise measurement of the properties of other new particles. This will allow serious tests of particle physics explanations of dark matter.

If WIMPs are produced at the Tevatron or the LHC, then the ILC will be needed to delineate their properties. Only then will it be possible to tie the particle observed in collider experiments to WIMPs scattering off of nuclei in direct detection experiments, and ultimately, to the dark matter of the universe.

3. Dark Energy

Over the last several years, scientists have accumulated conclusive evidence that the cosmic expansion of our universe is accelerating. The implications of this result are profound, and most experts believe that nothing short of a revolution in our understanding of fundamental physics will be required to fully understand these observations.

The evidence comes in a variety of forms including observations of distant supernovae, galaxies and clusters of galaxies, and the cosmic microwave background. It is impossible to fit all these observations to the standard cosmological paradigm without postulating that 70% of the universe is composed of mysterious "dark energy" that drives the acceleration. With this additional component of the universe, the data all fit together beautifully.

Dark energy challenges our understanding of fundamental physics. The classic "textbook" explanation is to set Einstein's famous cosmological constant to a small positive value. While this provides a good fit to the data, it also exacerbates the notorious "cosmological constant problem" of particle physics. Simple arguments indicate that the value required by the data for the cosmological constant is 120 orders of magnitude smaller than its "natural" value in quantum field theories. How nature reduces the cosmological constant by such a large factor remains one of the great, unresolved mysteries of quantum gravity and particle physics. Many believe that the only possible resolution is for some as yet undiscovered symmetry of nature to force the value of the cosmological constant to be precisely zero, requiring then an alternative explanation for the observed dark energy phenomenon.

One such option involves postulating a new form of matter (often called "quintessence"), which happens to be in a novel state today that can drive cosmic acceleration. Closer scrutiny reveals that the parameter values required for a field theoretic description of quintessence are at least as hard to explain as those required for the cosmological constant option. For example, in many cases the mass of the quintessence particle is 35 orders of magnitude smaller than the electron mass. Such a small mass leads to major complications when quantum corrections are considered, and could imply the existence of an observable new long-range force in nature.

Given these great challenges, other options that not long ago would have seemed extremely radical are now taken seriously. Arguments have been made that exotic “axion” particles are distorting the observations, and serious consideration is being given to abandoning Einstein’s theory of gravity in order to find a better account of the observations.

With any of these options, the implications for fundamental physics are profound. The acceleration of the Universe is, along with dark matter, the observed phenomenon that most directly demonstrates that our fundamental theories of particles and gravity are either incorrect or incomplete. The problem of understanding the dark energy is called out prominently in major policy documents such as the “Quantum Universe Report” and the “Connecting Quarks with the Cosmos”, and it is no surprise that it is featured as #1 in *Science* magazine’s list of the top ten science problems of our time.

Understanding Dark Energy

Today, the most critical component of any approach to the dark energy problem is new observations. The dark energy is described by an equation of state that is different from all the other components of the universe (baryons and electrons, photons, neutrinos, and dark matter). We are very fortunate that a wide-range of new observations are possible that can measure the equation of state in the present and past history of the universe, driving significant progress in this field. Many of the top researchers from both particle physics and astronomy are being drawn to these remarkable opportunities.

The goals of a dark energy observational program may be reached through measurement of the expansion history of the universe (traditionally measured by luminosity distance versus redshift, angular-diameter distance versus redshift, expansion rate versus redshift, and volume element versus redshift), and through measurement of the growth rate of structure, which is suppressed during epochs when the dark energy dominates. All of these measurements of dark energy properties can be expressed in terms of the equation of state. If the expansion is due instead to a failure of general relativity, this could be revealed by finding discrepancies between the equation of state inferred from two types of data.

Four observational techniques would allow especially good tests of the nature of the dark energy. These are:

1) Baryon acoustic oscillations as observed in large-scale surveys of the spatial distribution of galaxies. This technique is sensitive to dark energy through its effect on the angular-diameter distance versus redshift and through its effect on the time evolution of the expansion rate.
2) Galaxy cluster surveys, which measure the spatial density and distribution of galaxy clusters. This technique is sensitive to dark energy through its effect on the angular-diameter distance versus redshift, the time evolution of the expansion rate, and the growth rate of structure.
3) Supernova surveys using Type 1a supernovae as standard candles to determine the luminosity distance versus redshift, which is directly affected by the dark energy.
4) Weak lensing surveys, which measure the distortion of background images due to bending of light as it passes by galaxies or clusters of galaxies. This technique is sensitive to dark energy through its effect on the angular-diameter distance versus redshift relation and the growth rate of structure.

Many of these techniques are rather new and they are not all at the same state of maturity. The supernova technique is at present the most powerful and best proven. However, the most incisive future measurements of dark energy will employ a number of techniques whose varying strengths and sensitivities, including different systematic uncertainities, will provide the greatest opportunity to reveal the nature of dark energy.

The scientific community is pursuing a phased program of ever more powerful measurements, allowing time to develop new ideas and new measurement techniques, at the same time that the nature of dark energy is probed. The stages of the phased program include: Stage I, which represents projects completed; Stage II, ongoing projects; Stage III near-term, medium-cost projects, which combine a number of the techniques mentioned above; and ambitious Stage IV projects that are more costly. The Stage IV projects would bring our knowledge of the dark energy equation of state to a few % accuracy at the present time in our universe and about 10 or 20% in its early history.

4. Neutrino Science

In the last eight years, we have obtained compelling evidence that, like the other constituents of matter, the neutrinos have nonzero masses. But the similarity ends there. Cosmological observations tell us that the neutrinos are at least a million times lighter than the next lightest particles – the electrons. Neutrino oscillation results tell us that, unlike the quark mixing angles, all of which are small, two of the neutrino mixing angles are very large. The electrical neutrality of neutrinos tells us that, unlike the other constituents of matter, neutrinos could be their own antiparticles. The “see-saw” hypothesis for the incredible lightness of neutrinos tells us that their masses, mixings, and particle-antiparticle properties could be the result of physics at a high mass scale very far beyond the domain of the Standard Model of elementary particle physics.

The discovery of neutrino mass has raised a number of very interesting questions about the neutrinos and their connections to the rest of physics and astrophysics. These questions include:

- *What are the masses of the neutrinos? Does the neutrino mass spectrum resemble the charged-lepton and quark spectra, or is it an inverted version of those other spectra?*

- *What is the pattern of neutrino mixing? How large is θ_{13}, the centrally important but presently-unknown mixing angle? Is the atmospheric mixing angle, known to be very large, maximal?*

- *Are neutrinos their own antiparticles?*

- *Do neutrinos violate the matter-antimatter CP symmetry? Are neutrinos the key to understanding the matter-antimatter asymmetry of the universe?*

- *Are there sterile neutrinos — neutrinos that do not experience any of the known forces of nature except gravity?*

- *What can neutrinos tell us about the models of New Physics beyond the Standard Model?*

- *What has been the role of neutrinos in shaping the universe? What would an understanding of this role tell us about the universe and about neutrinos?*

- *What can neutrinos, acting as messengers, reveal about astrophysical phenomena?*

- *What totally unanticipated further surprises do neutrinos have in store?*

The Importance of the Open Questions Regarding Neutrinos

Clearly, to understand the origin of neutrino mass, we must know how large that mass is. The see-saw mechanism suggests that neutrino mass arises from physics at the very high mass scale where the strong, electromagnetic, and weak interactions appear to become unified. From the standpoint of the Grand Unified Theories (GUTS) that describe this unification, one expects that the neutrino mass spectrum will resemble the quark and charged-lepton spectra. However, the neutrinos have already surprised us by their very large, quite un-quark-like mixing angles, and they could surprise us again by proving to have a mass spectrum that is inverted with respect to its quark and charged-lepton counterparts. In that case, there will be a nearly degenerate pair of neutrinos at the top of the spectrum, suggesting an underlying symmetry that leads to the degeneracy. Since neutrino masses may well arise from physics at the GUT energy scale, these masses may provide a view of this physics that cannot be studied directly with accelerators.

The size of θ_{13} will discriminate between models of neutrino mass. In addition, this size will greatly influence the reach that experiments must have in order to determine whether the mass spectrum is quark-and charged-lepton-like or inverted. It will also greatly influence the reach required to observe CP violation.

If neutrinos are their own antiparticles, then —

- they are the only constituents of matter with this property,
- the very strongly conserved baryon number that distinguishes neutrons from antineutrons has no leptonic analogue, and

- neutrinos have Majorana masses — masses with no analogues for quarks or charged leptons, and with a different origin than the quark and charged-lepton masses.

Establishing that neutrinos are their own antiparticles would also add credibility to the see-saw hypothesis, which predicts this property.

We would like to know why the universe contains matter but almost no antimatter. Since the known Standard-Model CP violation among the quarks cannot account for this asymmetry, we ask whether CP violation among the leptons can be the explanation. In the see-saw picture, the light neutrinos have extremely heavy "see-saw partners". These are far too massive to be produced by the accelerators of today, but they would have been produced by the hot Big Bang. *If today's light neutrinos violate CP, then quite likely so do their heavy see-saw partners.* Violation of CP in the decays of these very heavy "neutrinos" would have yielded a universe with different numbers of leptons and antileptons. Subsequent non-perturbative Standard-Model processes would have reprocessed some of this leptonic asymmetry into a baryon asymmetry, producing the matter-antimatter asymmetric universe that we see today. Interestingly, for this scenario, known as Leptogenesis, to work, the light neutrinos must have masses in the range actually suggested by the experimental data.

The LSND experiment suggests that in addition to the three active neutrinos, ν_e, ν_μ, and ν_τ, there are light "sterile" neutrinos that do not participate in any of the Standard Model interactions. *Heavy* sterile neutrinos are common in theoretical models. Are there light ones as well? To search for sterile neutrinos, one can seek neutrino or antineutrino oscillations whose wavelengths are shorter than the well-established solar and atmospheric oscillation wavelengths. An effort to do just that is in progress by the MiniBooNE experiment at Fermilab.

Neutrino oscillation experiments cannot tell us the absolute scale of neutrino mass. Perhaps experiments on the beta decay of tritium or another element will be able to do so. However, taking advantage of the influence of neutrinos on the large-scale structure of the universe, observations of this structure have already been used to obtain the very interesting upper bound of (0.4 – 2.0) eV on the sum of the masses of the light neutrino mass eigenstates. Can cosmological observations and analyses be improved to the point where cosmology provides us, not just with an upper bound, but with an actual value for this sum?

Very interesting phenomena that produce enormous amounts of energy occur in the universe. Above 10^{15} eV, the photons created by these processes do not reach us, because they get absorbed on the way. But the neutrinos can reach us, providing a means to observe these phenomena. At lower energies, the information carried by neutrinos and photons is complementary, with the neutrinos providing the best clues about the particle content of these vast astrophysical accelerators.

Neutrino physics has been characterized by surprises, including the very small masses and the very large mixing angles. It would be surprising if further surprises were not in store. Perhaps neutrinos will prove to come in sterile flavors, to have magnetic dipole moments large enough to be observed, to travel in extra spatial dimensions, or to have masses that vary with cosmological epoch and with the material environment through which the neutrinos are traveling.

How the Questions About Neutrinos Can Be Answered

A global effort to pursue the answers to the open questions has yielded a well-developed experimental program of great promise. The planned nuclear, reactor, accelerator, and underground neutrino experiments, and the astrophysical and cosmological neutrino observations, stand to teach us very interesting things about the neutrinos, about particle physics in general, and about astrophysics and cosmology.

The nuclear experiments use decays of specific nuclei with potentially favorable decay rates to search for neutrino-less double beta decay. The observation of this process, at any nonzero level, would establish that neutrinos are their own antiparticles.

The difficulty and cost associated with measuring the other neutrino properties are dependent on the unknown value of the third (and smallest) mixing angle θ_{13}. So long as $\sin^2 2\theta_{13}$, which is known only to be less than 0.12, is not less than 0.01, the value of θ_{13} can be determined by reactor $\overline{\nu}_e$ disappearance experiments, and by accelerator $\nu_\mu \to \nu_e$ appearance experiments. The reactor experiments are sensitive only to θ_{13}, therefore they can provide a clean value of this parameter. In contrast, accelerator $\nu_\mu \to \nu_e$ and $\bar{\nu}_\mu \to \bar{\nu}_e$ experiments are sensitive not only to θ_{13}, but also to the atmospheric mixing angle θ_{23}, to whether the neutrino mass spectrum is normal (i.e., quark-like) or inverted, and to whether neutrino oscillation violates CP. The quantities that will actually be measured by accelerator experiments will typically involve several underlying neutrino properties at once. These properties will then have to be sorted out. A clean measurement of θ_{13} by a reactor experiment would clearly facilitate this.

Violation of CP among the neutrinos would reveal itself as a difference between the probability for $\nu_\mu \to \nu_e$ and that for its CP-mirror-image oscillation, $\bar{\nu}_\mu \to \bar{\nu}_e$. However, a difference between these two oscillation probabilities will also be induced by the interaction between the neutrinos or antineutrinos and the matter through which they will pass on the way from their accelerator source to a distant detector where the neutrino type is determined. This matter-induced $\nu - \bar{\nu}$ asymmetry will however be quite useful, because its character depends on whether the neutrino mass spectrum is normal or inverted, thereby permitting the nature of the spectrum to be ascertained. However, to determine the nature of the spectrum via this matter effect, and to establish the presence of CP violation in neutrino oscillation, it will be necessary to disentangle the two contributions to the $\nu - \bar{\nu}$ asymmetry that is actually observed in a given experiment. The matter effect depends on the baseline over which the oscillating particles travel and on their energy in a different manner than does CP violation. Thus, accelerator experiments with different baselines and energies (for example, one in the U.S. and one in Japan) could make it possible to disentangle the two effects.

Very impressive refinements in cosmological observations of relevance to neutrinos are being made. Detectors that can study the very interesting astrophysical neutrinos with energies above 10^{15} eV are being developed. The IceCube experiment to be located under the ice at the South Pole is under construction and should be complete around the end of this decade. The need to have uninterrupted sensitivity to the neutrinos that might arrive at any moment from a supernova is being borne in mind.

5. Precision Measurements for Charged Leptons and Quarks

Processes involving charged leptons or hadrons containing a variety of quark-types have been an invaluable physics laboratory for establishing the Standard Model. Precision measurements of rare or loop-induced observables for these systems can detect the virtual effects of new physics and thus provide an effective probe of interactions beyond the Standard Model. This approach investigates higher order processes and tests for deviations from Standard Model predictions. Investigations include the radiative corrections to perturbatively calculable processes, as well as transitions that are either suppressed or forbidden in the Standard Model. Both of these scenarios carry the advantage of being able to explore the existence of new physics at very high-energy scales. In some cases, the constraints on new degrees of freedom via these indirect effects surpass those obtainable from collider searches. In other cases, entire classes of models are found to be incompatible with data. As such, the loop effects of new fundamental interactions in rare processes and precision observables are complementary to the direct search for new physics at high energy accelerators and play an important role in the search for physics beyond the Standard Model.

There are several classes of experiments that test the Standard Model at high precision, which we discuss below.

The Muon g-2 Value

The classic example of a precision observable is the anomalous magnetic moment of the muon. Magnetic moments of elementary particles receive radiative contributions that can in principle be sensitive to new physical degrees of freedom. The combination of the large mass and relatively long lifetime of the muon allows a high precision measurement of its anomalous magnetic moment $a_\mu = (g-2)/2$, where g is the standard gyromagnetic ratio relating the magnetic moment of a Dirac fermion to its spin. Experimental measurements of a_μ have reached a high level of precision, with the best value being $a_\mu = (116\ 592\ 080 \pm 58) \times 10^{-11}$ as obtained by E821 at Brookhaven. This result is roughly (1.0-2.5) standard deviations away from the present state of the art calculation of the Standard Model prediction.

An experimental result for the anomalous magnetic moment of the muon that agrees with the Standard Model expectation can be used to constrain the parameter space of new physics models, whereas a result that definitively deviates from the Standard Model can in principle be used to determine the value of some parameters in a theory. The information on new fundamental interactions gained from a_μ, especially when used in conjunction with the determination of a new physics mass scale at the LHC (or Tevatron), can be unique. In particular, it can give knowledge about a sector of the theory that is difficult to probe directly at colliders. For example, within Supersymmetry, a_μ can be used to measure $\tan\beta$, the ratio of vacuum expectation values for the two Higgs doublets in the theory.

In order to exploit the experimental precision on the anomalous magnetic moment of the muon, it is necessary to have a precise computation of the Standard Model contributions. The contributions within the Standard Model arise from the electromagnetic, electroweak, and strong interactions. The QED contributions are perturbative and a full calculation exists to fourth order in α; in addition, the leading fifth order terms have also been computed. The electroweak contributions are also perturbative and have been computed to 2-loop order in the electroweak coupling constant. The theoretical errors on both the QED and electroweak contributions are far below the level of experimental precision. The size of the Standard Model electroweak contribution is typical for generic new interactions, which appear at the electroweak scale, and are within reach of the current experimental precision.

The hadronic contributions to the Standard Model prediction of the muon anomalous magnetic moment are dominated by non-perturbative effects and contain a larger theoretical uncertainty. They arise from two classes of contributions. The first class corresponds to effects which represent the contribution of the running of the fine-structure constant, α, from low to high scales (*i.e.,* the hadronic vacuum polarization contributions). These cannot be calculated from first principles, but can be related to data via either the hadronic cross section in e^+e^- annihilation, or spectral functions in tau lepton decays, by means of a dispersion relation. The second class of hadronic contributions arises from light-by-light hadron amplitudes. These off-shell amplitudes are affected by non-perturbative strong interactions that are also not calculable in QCD and cannot be related to other experimental observables. They must be estimated within the framework of a theoretical model. It is worth noting that various computations of the light-by-light contributions are not in agreement on the size of the theoretical error for this quantity.

Heavy Flavor Physics

Precision measurements in the heavy quark sector are key to understanding the flavor structure of the Standard Model and of the new physics, which might be present at higher energy scales. CP-violating asymmetries, branching fractions, and detailed kinematic distributions of rare meson decays provide a fertile testing ground of the Standard Model. Signatures of new fundamental interactions in these processes would show up through the observation of deviations from Standard Model expectations, inconsistent measurements for a single quantity determined in different ways, or observing a process that is forbidden in the Standard Model.

A centerpiece of the search for new physics is the study of CP-violating asymmetries in meson decays. CP violation arises in the Standard Model from the existence of a phase in the CKM matrix, which governs weak decays. Unitarity imposes a relation among the elements of the CKM matrix, and this relation can be depicted as a triangle in the complex plane. The area of the triangle corresponds to the amount of CP violation. Signals for new sources of CP violation include the non-closure of the Standard Model unitarity triangle and inconsistency between separate measurements of the angles of the unitarity triangle. An observation of a new CP-violation mechanism may yield some insight into the puzzle of the abundance of matter over anti-matter in the universe and is therefore of great interest.

Rare decays, which occur only at the loop-level within the Standard Model, carry the advantage that contributions from virtual effects of new interactions can be just as large as those of the Standard Model. Hence they provide a clean signature for new physics via an enhanced or reduced rate when compared to Standard Model predictions, or from deviations in angular distributions of final state particles. The pattern of observable effects in various decay channels is highly model dependent and the measurement of several rare decay modes can provide information on the underlying theory.

Heavy flavor physics in the LHC era takes on a new context. The goal is not only to establish deviations from the Standard Model, but also to diagnose and interpret these signals in terms of the new fundamental interactions. The discovery of new physics at the LHC will lead to a determination of its mass scale. Ultra-precise heavy flavor experiments are complementary in that they will probe the flavor violation associated with the new physics and measure the new flavor parameters. The heavy flavor sector can thus play an important role in illuminating the physics of the Terascale.

Historically, rare processes in the Kaon sector have elucidated the flavor sector of the Standard Model and have provided strong constraints on models of new physics. The extremely rare decay $K \to \pi\nu\nu$, which remains to be studied experimentally, provides the theoretically cleanest determination of one of the sides of the unitarity triangle and also tests Standard Model CP-violation.

The wealth of accumulated data from the B-Factories at SLAC and KEK have furnished unprecedented knowledge on the Standard Model quark flavor sector. These experiments have established the CKM mechanism of CP-violation and have observed several rare decay modes of the B meson. Constraints from low-energy experiments and the expected higher mass scale of new physics result in the expectation that new physics effects in the B system should be small. At present, several important measurements demonstrate such small deviations from Standard Model expectations, but require more precision to reach definitive conclusions.

The charm and top-quark provide the opportunity to explore flavor physics in the charged +2/3 quark sectors. Loop-induced processes in these systems are extremely small in the Standard Model and if an effect were to be detected, it would provide an indisputable signal for new physics.

Charged-Lepton Flavor Violation

The discovery that neutrinos have mass and large mixing angles indicates that lepton flavor violation occurs within the Standard Model. However, lepton number violating processes in the charged lepton sector have yet to be observed. In the Standard Model, these processes are proportional to the small neutrino masses and the corresponding rates thus lie well below experimental sensitivity. Models with new physics contributions can give large enhancements over the Standard Model predictions and can yield event rates that are within reach of future heavy flavor factories. The decays

$\mu \to e\gamma$, $\tau \to \ell\gamma$ (with $\ell = \mu$ or e) and $\tau \to \ell\ell\ell$, as well as $\mu \to e$ conversion are all classic tests of lepton flavor violation. All receive significant enhancements in models of new physics, such as supersymmetry, and could be detected in the next round of experiments. Such an observation would reveal fundamental information on the lepton sector of the underlying theory.

Electric Dipole Moments

The electric dipole moment (EDM) of atoms, neutrons, quarks, and the electron provide a unique and sensitive test of low-energy flavor conserving CP violation. Extensions of the Standard Model often contain additional sources of CP violation, which generally give contributions to electric dipole moments. The non-observation of EDMs to-date, thus provides tight restrictions to building theories beyond the Standard Model.

IV. Projects and Budget Assumptions

1. Possible Experiments and Costs

A number of experiments have been proposed that would provide the new observations needed to make progress on the physics discussed in Chapter III. These are in various stages of development and many promising techniques are in the R&D phase and not ready for a decision on construction. In other cases the experiments are ready to move ahead but still have not completed full technical and cost evaluations. In such cases we will make recommendations on these experiments, but these recommendations are not meant to preclude completing the appropriate reviews. The experiments range in size and complexity but all have an important scientific agenda aiming to address some of the nine questions presented as the mission statement for the field in our introduction. We have been able to make use of input from the EPP2010 report, the report of the Neutrino Scientific Assessment Group (NuSAG), and the Dark Energy Task Force (DETF). We anticipate that the report of the Dark Matter Scientific Assessment Group (DMSAG) will be available soon and will provide significant guidance in planning a dark matter program.

We note that we are looking only at new experiments that would represent a sizeable expenditure. There is of course more to the program than this. In particular, we strongly value smaller projects, the development of new initiatives and associated R&D, and collaboration on projects abroad. Developments in theoretical physics are critical to a deeper understanding of physics and will be especially important in understanding new phenomena. Innovative R&D in accelerator physics has the promise to enable future scientific progress. Adequate support for the University Program, which includes approximately 100 universities nationally, is required for the success of the activities within the program, as well as to provide the highly trained scientists needed for progress in the field and for contributions to our economy.

We list the new experiments that, along with ongoing experiments and those already in construction, could be part of the particle physics roadmap. We present our assumptions about total costs for these experiments in cases where the participating agencies are clearly known. The experiments will each take a number of years to build and we do not present a cost profile.

However, detailed budget planning was undertaken, which did use such profiles, in order to insure that each fiscal year had costs that fit within the assumed budget guidelines. We only address experiments where construction activities might start within approximately the next five years and only the expenditures required from the HEP program at DOE and EPP at the NSF. NSF MREFC costs, support from other funding agencies, and foreign contributions are not included.

- The Large Hadron Collider (LHC), now nearing completion, has enormous discovery potential, including the possibility to discover new symmetries, new physical laws, first indications of dark matter particles, particles associated with extra dimensions of space-time, and improve our understanding of the nature of the vacuum and the origin of mass. The funding for the U.S. program at the LHC was planned a number of years ago. For FY07 the support for the centrally managed research programs for the ATLAS and CMS experiments add to about $64 million. Recently new groups have joined the experiments and there is a better understanding of the requirements of the program, including the needs for R&D to upgrade the LHC and its detectors. Taking into account the growth in the U.S. LHC program and inflation, an increase in operations support starting in FY08, reaching an additional $8 million per year in 2012, is required. At present an upgrade construction plan to achieve an increase in LHC luminosity of a factor of ten is being developed. It should be ready for evaluation after initial physics results are in hand. The cost of an upgrade (the U.S. contributions to the machine, ATLAS and CMS) is estimated to be somewhere in the range of $200 million to $250 million.

- The International Linear Collider (ILC), the high precision second element of an energy frontier program, is being developed through efforts spread across the globe. It should be able to follow up in great detail the initial discoveries of the LHC, creating a new picture of particle physics. It is essential for providing the physics information needed to go from first dark matter indications at the LHC to a deeper understanding of dark matter and its ties to cosmology as well as a deeper understanding of new particles and symmetries found at the LHC. Given the expected cost, it is crucial to vigorously pursue the R&D needed for reliable and minimum cost construction of the machine and its detectors. This should be done on an international basis. In addition specific studies are required for a potential site at Fermilab to enable costing and planning for a U.S. site. The ILC is the highest priority future project in the recent EPP2010 report from the National Research Council. We allocate $500 million for the relevant R&D activities over a five-year period. The goal is to produce a technical design on an international basis and once initial LHC physics results are known to initiate the next step toward realization of this accelerator.

- Three smaller scale experiments promise to advance important physics areas. These would each cost approximately $15 million to $25 million, dependent on final cost reviews and schedules. The first of these is the Dark Energy Survey, a Stage III dark energy experiment that combines measurements on baryon oscillations, cluster surveys, supernovae studies, and weak lensing to significantly improve our understanding of dark energy. The second is the next phase of the Cryogenic Dark Matter Search experiment, using a 25 kg detector deep underground to significantly extend our sensitivity for direct detection of dark matter. The third is the Daya Bay reactor experiment, located in China, which could significantly extend the reach for measuring the critical third mixing angle of the neutrino-mixing matrix.

- The value of the muon magnetic moment parameter g-2 is the most exacting low energy test of the Standard Model. It appears feasible to reduce the error on this parameter by about a factor of two, reaching an uncertainty of 0.25 parts per billion, using a muon storage ring at the Brookhaven National Laboratory. The goal of this experiment is to look for clear non-Standard Model contributions to g-2. The construction cost for the experiment, which is an extension of a very successful measurement program, along with needed improvements and operating costs for the accelerator make this an approximately $40 million to $50 million project.

- The NSF is moving ahead with its plans for a Deep Underground Science and Engineering Laboratory (DUSEL) as an NSF MREFC. This is an interdisciplinary laboratory fostering research in physics, astrophysics, biology, geology and engineering. The initial program would include a suite of important experiments and R&D is expected over the next few years on the construction of the lab as well as these experiments. The expected particle physics experiments that could be initiated with the lab startup would include a large dark matter detection experiment and a large neutrino-less double beta decay experiment. The cost for these would be shared equally by the MREFC and collaborating agencies. For planning purposes we estimate a cost of about $50 million as the DOE share of the dark matter experiment and $50 million for the neutrino-less double beta decay experiment. These projects are at an early stage, and do not have a well worked out cost. A goal for each would be a factor of 100 improvement in reach over existing or near term experiments. Achieving these ambitious goals will require the support of the necessary R&D to prove the potential of various options. In the case of the neutrino-less double beta decay experiment some of the options might best be supported by the DOE nuclear physics program and the financial split as well as optimum technique will have to be decided later based on the outcome of the R&D program.

- A number of ambitious initiatives are under development to provide the further observations needed to explore the dark energy phenomenon. Numerous studies have identified a Large Survey Telescope (LST) and a Dark Energy Space Mission (JDEM, jointly supported by the DOE and NASA) as providing large steps forward in the study of dark energy and tests of general relativity, our picture of inflationary cosmology, and measurement of cosmic distributions of dark matter. The particle physics community has been particularly active in developing candidates for each of these Stage IV dark energy projects, which benefit from innovative work on detectors and data acquisition techniques developed in particle physics. These two projects, the Large Synoptic Survey Telescope (LSST) and the SuperNova Aceleration Probe (SNAP) are proposed as collaborative inter-agency projects. In the case of LSST, NSF has been the lead agency with DOE providing substantial resources as the partner agency. In the case of SNAP, DOE has been the lead agency with potential partners of either NASA, an international collaboration that might include a foreign launch, or perhaps both. These experiments are ready for the next stage of design and review, which would be the "Preliminary Design Review Stage" in the case of NSF and LSST, "CD2 Stage" for the DOE parts of LSST, and "CD2 Stage" in the case of DOE and SNAP. This will allow sharpening of cost estimates, further interagency planning, further development of the collaborations, and continued work on the science potential. The cost to the DOE for the LSST construction would be $100 million with the remaining costs provided by an NSF MREFC and private sources. The cost to DOE for SNAP is not yet determined given that the discussion with potential partners has not been concluded. However, both the space launch and telescope are expensive items. The present plan envisions that the telescope cost would be shared between DOE and the partnering agencies. The logical partner to DOE would be NASA. NASA has recently announced three candidate concepts (ADEPT, Destiny, and SNAP) that it will support, jointly with the DOE, for further study for a Joint Dark Energy Space Mission. NASA has, however, not definitively indicated that it will choose to participate in the final development of an instrument and launch in the near term. Clarification of the arrangements involving NASA would greatly help the planning for this important Stage IV dark energy experiment. We anticipate a decision if NASA will develop JDEM in the near term in about a year and which mission concept is selected by the DOE and NASA approximately two years from now.

- The NOνA neutrino oscillation experiment using the NuMI beamline has been under development at Fermilab for a number of years. This experiment is complementary on a worldwide basis to the other neutrino experiments planned for early in the next decade. It would represent the next step for the U.S. in a phased international program aimed at measuring the remaining parameters of the neutrino oscillation matrix, determining the mass ordering among the neutrino mass eigenstates, and finding out whether neutrinos violate the CP symmetry. NOνA would provide unique information on the mass ordering. The U.S. cost for NOνA construction, based on DOE agreement with the proponents would be $200 million. The experiment could also benefit from modest improvements to the Fermilab accelerator complex to increase the beam intensity to about 1.2 MW. These improvements would cost approximately another $35 million. NOνA would make use of the significant investment in building the NuMI beamline at Fermilab.

- The Auger South cosmic ray experiment is made up of a very large array of detectors located in Argentina. It was constructed by an international collaboration and uses a variety of techniques to reduce systematic errors in the measurement of energies for atmospheric showers created by high-energy cosmic rays. The goal is to study the very highest energy cosmic rays and attempt to pin down specific sources. To complement the southern array, Auger North has been proposed for location in Colorado, allowing all-sky coverage for these extremely rare and poorly understood cosmic rays. The Auger South array has only recently begun to collect data and the formulation of goals and the proposed division of costs for the different members of the collaboration for Auger North are now actively under discussion. It is too early to specify either a U.S. cost for Auger North or to assign a scientific priority. We estimate that an appropriate time to review the Auger North proposal would be in FY08.

2. Agency Budgets

To arrive at a roadmap we need to make assumptions about budgets. In addition, by indicating priorities, adjustments can be made to cope with budget changes and adjustments in the profile for construction spending also allows some flexibility from year to year. In the case of the DOE, a five year funding profile in the document called "Office of Science 5-year Budget Plan: FY2007-FY2011" submitted by the DOE to Congress in early March of 2006 as part of the FY07 budget submission gives us a concrete budget plan to work with. The numbers in this plan were as follows:

FY07	FY08	FY09	FY10	FY11
$775M	$785M	$810M	$890M	$975M

In addition, the closing of PEP-II at the end of FY08 and the Tevatron at the end of FY09 (the exact date for the Tevatron to still be reviewed by P5 next year), as foreseen in the most recent P5 planning, should allow funds to flow to the exciting new projects discussed above. The recuperation of funds presently used for these programs is a crucial assumption in our planning. We assume that budgets grow by 3% per year after FY11, a roughly "flat" budget in then year dollars assuming an annual inflation rate of 3%. We use these numbers in planning our roadmap. We call this our base budget plan.

An alternative budget would assume a 7% annual increase resulting in a doubling of the HEP budget over 10 years. The numbers in such a plan, through FY11, would be:

FY07	FY08	FY09	FY10	FY11
$775M	$829M	$877M	$950M	$1016M

We use these numbers to examine what might be possible in a plan that doubles funding over a 10 year period, as might be appropriate for a renewed emphasis on the physical sciences and their importance to the country's economic health.

The NSF budget plan for EPP is less specific than that of the DOE but the NSF has a number of important objectives. There is a commitment to reserve at least 50% of the budget for university individual investigator support. There is a commitment for $18 million/year for the centrally managed LHC Research Program. There is a commitment to advance the case for DUSEL as an MREFC project with more than half of the funding to go to the initial suite of experiments located at DUSEL. DUSEL operations would be supported, beginning the last year of construction, under reasonable assumptions of budget growth. Significant funding would be provided for R&D for DUSEL and the initial suite of experiments over the next few years.

V. Planning Guidelines

We have adopted a number of planning guidelines in forming our roadmap and recommendations. We enumerate these below. They have been developed with the recent recommendations of the EPP2010 committee in mind, the goal of capitalizing on the science opportunities outlined earlier, and the specific budgetary numbers in our base budget plan. The guidelines are:

1) The LHC program is our most important near term project given its broad science agenda and potential for discovery. It will be important to support the physics analysis, computing, maintenance and operations, upgrade R&D and necessary travel to make the U.S. LHC program a success. The level of support for this program should not be allowed to erode through inflation.
2) Our highest priority for investments toward the future is the ILC based on our present understanding of its potential for breakthrough science. We need to participate vigorously in the international R&D program for this machine as well as accomplish the preparatory work required if the U.S. is to bid to host this accelerator.
3) Investments in a phased program to study dark matter, dark energy, and neutrino interactions are essential for answering some of the most interesting science questions before us. This will allow complementary discoveries to those expected at the ILC and provide nearer term projects that, along with the LHC, will train the next generation of students in particle physics. A phased program will allow time for progress in our understanding of the physics as well as the development of additional innovative techniques for making the key measurements.
4) In cases where new techniques are under development our recommendations will include rough dates for reviewing technical progress in order to select the most promising directions for new ambitious experiments.
5) In making a plan, we have arrived at a budget split for new investments of about 60% toward the ILC and 40% toward the new projects in dark matter, dark energy, and neutrinos through 2012. This excludes NSF funds made available through NSF investments in MREFC projects, which may include particle physics as part of an

interdisciplinary program involving astronomy, biology, engineering or earth sciences. The budget plan expresses our priority for developing the ILC but also allows significant progress in the other areas. We feel the investments in dark matter, dark energy, and neutrino science in our plan are the minimum for a healthy program.

6) The projects recommended for a construction start in dark matter, dark energy, and neutrino science should complete construction by approximately the end of 2012. This will allow maximum flexibility for decisions on future investments to be made toward the beginning of the next decade in the light of new science results, progress in new technologies, better definition of interagency contributions and plans, and progress on the ILC.
7) Recommendations for construction starts on the longer-term elements of the particle physics roadmap should be made around the end of this decade by a new P5 panel, after thorough review of new physics results from the LHC and other experiments. A final decision regarding possible upgrade construction for the LHC, which will likely be a high priority, should also be made at that time. We have, however, included the LHC upgrade construction (starting in FY2011) in our budget plan to be sure that funding for this can be available. In evaluating this we have kept to our funding guideline that 60% of new investment be available for the ILC. The LHC upgrade construction would fit into the remaining 40%, while still allowing significant funds for investment in next-generation dark matter, dark energy and neutrino experiments.
8) Among a range of funding options for the future provided to us, we have made our recommendations within the base budget plan. Additional discovery physics, more rapid progress on exciting projects in dark energy, as well as more rapid progress on ILC R&D would be possible with additional resources.

VI. Recommendations for Construction and Reviews

To provide recommendations for major construction and R&D activities we have grouped the projects under consideration into several broad categories, with different degrees of priority for each group. We list groupings below in priority order. They are based on our set of planning guidelines. The activities are meant to mainly fit into a five-year timeline.

1. The highest priority group involves the investigations at the energy frontier. These are the full range of activities for the LHC program and the R&D for the ILC.
2. The second group includes the near-term program in dark matter and dark energy, as well as measurement of the third neutrino-mixing angle. This grouping includes the three small experiments: DES, the 25 kg CDMS experiment, and the Daya Bay reactor experiment. Also in this group is the support for the LSST and SNAP, to bring these to the "Preliminary Design Review Stage" in the case of the NSF and "CD2 Stage" in the case of the DOE over a two to three year time frame. We recommend that the DOE work with NASA to ensure that a dark energy space mission can be carried out and that the three potential approaches to the mission have been properly evaluated. The final item in this group is the R&D funding for DUSEL, along with support by the NSF and the DOE for R&D for both a large dark matter and neutrino-less double beta decay experiment.
3. The next item is the construction of the NOνA experiment at Fermilab along with a program of modest machine improvements.
4. The final item is the construction of the muon g-2 experiment at BNL.

Matching the costs of these projects to our budget scenarios, we find that the first three groupings can be carried out in the base budget plan. Note, however, that the ILC R&D ramp up profile, chosen to match the 60% of new investment goal expressed in our planning guidelines, and the NOνA construction schedule must both be slowed with respect to the most aggressive proposals, if the costs are to be matched to the assumed annual budgets. These three groupings of projects would effectively address the nine questions presented as the mission statement for the field in our introduction. This includes near term projects as well as R&D investments for highly capable future projects, satisfying our most important science goals. We note, as mentioned earlier, that we have not considered and do not mention explicitly many smaller projects that should be supported. Our own evaluations match well the conclusions in the EPP2010 report.

The budget that would double support over a decade would have a very significant science impact by allowing added support for the Stage IV dark energy experiments. The preparatory work for these could be completed in a more timely way, while we also pursue the other important areas in our first two groupings. In addition, the ILC R&D could be pursued more vigorously.

In this funding scenario the muon g-2 experiment could be considered for construction. We are unable within the base budget to provide support for any new charged lepton or quark flavor physics project, despite the potential to add significant information in the search for physics beyond the Standard Model. This is particularly unfortunate since the g-2 experiment already provides an indication that it sees such a contribution. The charged lepton and quark flavor measurements provide important constraints that will be very valuable when trying to understand the physics at the LHC. In the case of supersymmetry, the g-2 measurement is sensitive to a number of the supersymmetric parameters and would help pin down these parameters.

We recommend a review by P5 toward the end of this decade to look at projects that could start construction early in the next decade. The base budget plan would allow a significant number of these to move forward to construction. The review should take into account new physics results, especially those from the LHC, results on R&D for new projects, budget and cost projections at the time, and the status of interagency agreements and MREFC plans. We list some of the areas to be examined.

1. The ILC, including a possible U.S. bid to host, and the steps needed at the governmental level for internationalization.
2. The LHC Upgrades, required for an order of magnitude luminosity increase at the LHC.
3. DUSEL and the large experiments to search for dark matter and neutrino-less double beta decay.
4. The Stage IV dark energy experiments, a large survey telescope and a dark energy space mission. Interagency agreements are crucial to these projects, which could start construction soon after review.
5. An evaluation of the status of flavor physics and the importance of further experiments across a number of possibilities such as the muon

g-2, μ to e conversion, a very high luminosity B experiment, and rare K decays.

We anticipate that a separate review by P5 will be required to look at the best directions for further experiments in neutrino physics. Much work is ongoing internationally in this area with an optimum program dependent on measurements to be made by the next generation of neutrino experiments as well as results from ongoing R&D. A second important physics area that might be included in this review would be an ambitious proton decay experiment. These two projects could be the major second phase of experiments for DUSEL. The physics results over the next five to ten years will determine the best date and best set of areas to look at in such a review.

VII. Further Recommendations

The next section of our report contains the detailed analysis that provides the scientific background for our roadmap. It contains a number of important, specific, recommendations. We have collected them here in order to provide a concise summary.

Recommendations Regarding the Energy Frontier: LHC - ILC

- The LHC science program should be supported so that U.S. institutions can play a major role in the exploration of the LHC physics. This includes support of theoretical and experimental efforts, computing, maintenance and operation, upgrade R&D and necessary travel support to make this program a success. The level of support should not be allowed to erode through inflation.

- The U.S. should maintain a presence in the LHC machine and contribute in such a way that the machine performance can be improved steadily to achieve the necessary luminosities.

- Participation in upgrades of the LHC machine and the detectors should be reviewed when first results from machine operation and first physics results are available. R&D for the upgrades should be pursued now, but only as part of the international R&D plans of ATLAS, CMS and the LHC machine.

- Currently the highest longer-term priority for the worldwide particle physics program is to realize the ILC. Sufficient R&D funds to achieve this goal in a timely way should be made available over the next few years.

- The level and areas of participation of the U.S. in the R&D efforts should be established within the international framework of the ILC GDE. R&D should be coordinated among the different regions of the world, to make economical use of our resources.

- Within the international ILC R&D framework, establish the corresponding milestones for contributions from the U.S., so its progress can be monitored.

- The U.S. part of the worldwide ILC R&D program should be managed like any other project in HEP, i.e. through a well defined project management structure and a multiyear plan for R&D, with monitoring through periodic reviews.

- A multiyear U.S. ILC detector R&D program, defined within the international program, and including laboratories and universities, should be established and funded at a level commensurate with other regions in the world. It should be managed centrally and monitored periodically.

- The U.S. efforts to host the ILC should be supported in a manner that will allow a credible and competitive bid to host the ILC by the U.S. soon after initial LHC results are known.

Recommendations Regarding a Dark Matter Program

- The CDMS experiment is mature, and is now dealing with the technical issues associated with advancing an already well-developed, well-understood technique into larger-scale implementation. Assuming that the experiment continues to demonstrate adequate background rejection, we recommend full support for the 25kg implementation. This is part of a vigorous international program, which will likely profit from cross checks between different techniques.
- The ADMX axion search experiment is unique and well advanced. We recommend strong support over the next five years.
- We recommend strong support of R&D toward a future dark matter experiment that will extend the cross section reach by a factor of 100 over the near-term program. Priority should be given to techniques that could be ready for construction early in the next decade. The prospective underground U.S. lab, DUSEL, would provide an ideal location for such an experiment. Where appropriate, we endorse the

concept of a consortium to study common issues and share the R&D effort.

- The field has a number of innovative projects that are interesting but are at a very early stage of development. These are worthy of some R&D support to continue development of such techniques.
- We anticipate that the DMSAG will provide much detailed guidance about the best directions for the dark matter program.

Recommendations Regarding a Dark Energy Program

- At a relatively modest cost the DES promises to achieve the DETF targets for Stage III increase in our knowledge about dark energy. It will further develop the experience and expertise using a number of techniques to measure the effects of dark energy, which will be critical to making the most of a Stage IV program, another DETF Stage III goal. We recommend that the agencies proceed with this project.
- *Phase I of a Stage IV Program:* We recommend that the agencies carry out the technical and cost studies, over the next two to three years, that would be required to confidently move forward with the construction of a Large Survey Telescope and a Dark Energy Space Mission. This will require joint planning by the NSF and the DOE in the case of the LSST to bring it to the Preliminary Design Review Stage (in the case of the NSF) and CD2 Stage (in the case of the DOE) and the DOE and NASA in the case of SNAP to bring it to the CD2 Stage (in the case of the DOE). We recommend that the DOE work with NASA to ensure that a dark energy space mission can be carried out and that the three potential approaches to the mission have been properly evaluated.
- *Phase II of a Stage IV Program:* We recommend a decision process soon after the completion of the technical and cost studies to formulate and recommend an aggressive and financially realistic Stage IV program.

Recommendations Regarding Neutrino Physics

- A detailed review of the Daya Bay project should be carried out as soon as possible. This review should focus particularly on the feasibility of the approach. It should evaluate the basis of estimates of the systematic uncertainties, along with the additional systematic uncertainties induced by moving the detectors. Conditional on a favorable review, we recommend proceeding with planning and construction.

- Proceed with the 20 kt-scale NOνA experiment. Due to long operations required, construction should not be stretched out significantly with respect to the roadmap described in Chapter VI.
- We encourage T2K and NOνA to communicate to maximize the complementarity of the two programs.
- We recommend a modest level of U.S. participation in T2K.
- We encourage international participation in NOνA Phase 1.
- The three techniques to measure neutrino-less beta decay, CUORE, EXO, and Majorana should be investigated vigorously, leading to a selection of one technique for an experiment at the 1-10 ton scale. One possible decision point is after the parallel EXO efforts in barium tagging and in the EXO-200 double beta decay search are complete. This might be as early as 2010. Should success of the techniques be demonstrated, an upgrade to the full experiment with either a 1-ton or a 10-ton detector should be considered in the context of the other possibilities available at that time and with advice from the appropriate body (*i.e.*, NuSAG). By around 2011-2013 more information is also expected to be available on the feasibility of a larger scale version of the ^{76}Ge and ^{130}Te experiments.
- The DUSEL is an intriguing scientific possibility for physics and astrophysics. We recommend that the DUSEL planning and evaluating process continue.

Recommendations Regarding Precision Measurements for Charged Leptons and Quarks

- A review of the status of flavor physics should be undertaken after results from the LHC are in hand. A decision on the muon g-2 experiment E969 should be postponed until after this review and there are further indications that new physics may be within reach of the experiment. The theoretical community should continue to work on reducing the uncertainties associated with the calculations of the hadronic contributions to the Standard Model prediction, particularly the light-by-light term.

- The review of flavor physics should also include the potential for new physics in μ to e conversion, a very high luminosity B experiment, and rare K decays. Work on options for a Super-B Factory accelerator should continue in order to better understand their potential luminosity and cost.

VIII. Detailed Discussion of Opportunities

1. The Energy Frontier: LHC-ILC

We have discussed in Chapter III the exciting physics that can be expected in the future at the Terascale energy frontier. For the reader's convenience we summarize some of the important points here.

Over the last 50 years particle physics has achieved a remarkable understanding of the constituents of matter and the underlying dynamics describing the interactions between them. This effort has resulted in the Standard Model (SM), which is the framework for describing the forces and the constituents of matter that were present in the visible universe when it was 10^{-12} seconds old. The Standard Model is based on symmetries that we know are broken. We have not yet observed how these symmetries are broken, however, many of the options open new vistas beyond the SM. To understand how the symmetries of the SM are broken we have to explore energy regimes that are beyond our current experimental reach. Fortunately general arguments and data taken to date indicate that the next step in energy, taking us to the terascale, will reveal key missing elements of our physics picture.

The next step in this direction is the Large Hadron Collider (LHC), under construction at CERN in Geneva Switzerland. The LHC is our first window to unexplored energy regimes, where we expect a revolution in our understanding of physics. The discoveries at the LHC will raise compelling questions and will signal a changed perspective on the universe. Responding to these questions will require a tool of greater sensitivity, which can probe the energy frontier with ultra-precise measurements. This is the role of the proposed International Linear Collider (ILC), which will discover the fundamental laws of nature behind the new phenomena observed at the LHC. In addition, the precision data obtainable at the ILC will allow us to telescope to even higher energy regimes via the detection of tiny quantum fluctuations. Together, measurements at the LHC and ILC will reveal the nature of the quantum universe at the next energy regime and beyond.

The LHC is a proton-proton collider with a center-of-mass energy of 14 TeV. Such a large collision energy is required in order to reach energies in the TeV range for the collisions of the elementary building blocks within the proton. In parallel, high-energy physicists throughout the world have been constructing components for two large general-purpose detectors, ATLAS and CMS. Given the high collision energy, the LHC will be an exploratory machine into the TeV energy range. It will definitively answer the question of the existence of the Higgs particle and of TeV-scale Supersymmetry.

The ILC is a proposed high-energy e^+e^- linear collider, designed to work in concert with the LHC. The ILC would consist of two roughly 20 km linear accelerators, which would collide electrons and positrons at their intersection with initially tunable collision energies up 0.5 TeV and upgradeable to 1.0 TeV. Since the electron is a fundamental particle, the full collision energy of the ILC would be available to study new phenomena. The electron and positron beams can also be polarized, adding resolving power to the subsequent analysis of the collisions. These machine properties result in a clean experimental environment and a complete knowledge of the quantum state of the collision. This removes theoretical or experimental ambiguities or model dependency in analysing the data. The ILC is being organized as a fully international project and its technical design is currently being coordinated worldwide by the Global Design Effort.

The ILC would have unique capabilities that would allow for the identification of the new particles observed at the LHC and the discovery of the underlying theory that gives rise to them. In the possible theoretical scenarios before us today, experiments at the ILC will be able to answer questions such as: does the Higgs have the correct properties to give the measured mass to all particles? Are there additional components to the Higgs boson that would give rise to new physics? Are the partner particles discovered at the LHC associated with Supersymmetry or extra dimensions or something else? Is the symmetry associated with the supersymmetric particles the same as that predicted by Supersymmetry? How many extra dimensions are there, what is their size and shape, and where do the elementary particles reside within them? What is the mass, spin, and couplings of the dark matter particle? Do they account for the thermal relic density of dark matter in the universe as determined by astrophysical observations? The answer to each of these questions would have profound implications on our understanding of the history of the early universe.

The Large Hadron Collider

The Large Hadron Collider (LHC) will start operation at full energy in 2008. At the LHC, high intensity bunches of protons will be accelerated up to 7 TeV and brought into collision to provide a center-of mass energy of 14 TeV, which is large enough to generate hard scattering processes between quarks and gluons in the TeV range. With this energy the LHC will remain the world's most powerful particle accelerator for a long time to come.

The machine is located in the LEP tunnel, which has a circumference of 27 km. It is being built by CERN, supported via its member states, however, many non-member states, including the U.S., have contributed significantly to its construction. In order to reach the bending power to keep the high-energy protons on orbit, superconducting dipole magnets providing a magnetic field of 8.3 Tesla are required. The challenge of producing about 1,200 superconducting magnets operating at a temperature of 1.9 K has been mastered; most of these magnets are at CERN and are now being installed in the tunnel.

In order to achieve the necessary collision rate required to unravel the physics expected at the LHC, the luminosity of the machine needs to reach a design value of 10^{34} cm^{-2} s^{-1}. This is being achieved by having a large number of protons in each of the 2808 bunches, which are separated in time by only 25 ns. This poses severe challenges on the performance of the detectors, since at design luminosity about 23 inelastic proton-proton interactions take place per bunch crossing. The resulting particle tracks from the 1,600 charged particles per bunch crossing in the acceptance of the ATLAS and CMS tracking detectors appear superimposed in the detector. Therefore, the detectors need to have a high granularity, need to be fast, and radiation resistant.

Despite the challenging complexity of the frontier infrastructure required for the LHC, excellent progress has been made concerning procurements, tests, and installation of the numerous components. At present it is foreseen to reach first collisions at injection energy towards the end of 2007 and to have a first commissioning and physics run in 2008.

The construction of the LHC detectors is already well advanced. Most of the components are built and are presently being installed at CERN. Groups from the U.S. have contributed significantly to the accelerator development, the detector construction, and computing. This has been a very significant effort for the last decade. The contributions of the participating U.S. groups are listed at the end of this report in Appendix 1.

Physics at the LHC

If a Standard Model Higgs boson exists, it will be detected at the LHC. Many studies performed over the past years have demonstrated that the Higgs boson can be discovered over the entire mass range, from the lower experimental bound of 114 GeV set by the LEP experiments up to the theoretically motivated upper bound of ~1 TeV. Recently, studies have established that in addition to the dominant gluon fusion production process, a Higgs boson in the difficult low mass region ($m_H < 2\ m_{Z,}$) can also be detected in the vector boson fusion mode. In this production mode the characteristic signatures of two additional jets in the forward regions of the detector and little jet activity in the central region can be exploited and additional final state signatures, *e.g.* $qqH \rightarrow qq\ \tau\tau$ also become accessible. With this additional data, the overall discovery potential can be significantly increased. Studies indicate that, if the anticipated detector performance can be reached, integrated luminosities of less than 5 fb^{-1} should be sufficient to discover the Higgs boson with a significance exceeding 5σ over the entire mass range by combining results from ATLAS and CMS, as shown in Figure 1. A 5σ discovery can be achieved with only 1 fb^{-1} if the Higgs has a mass between 150 and 400 GeV using the significant decays $H\rightarrow ZZ$. The signal for a Higgs boson with mass of about 120 GeV would need the combination of different decay channels such as $\gamma\gamma$, $\tau^+\tau^-$, bottom-antibottom, WW and ZZ and therefore will require good understanding of the detectors and backgrounds.

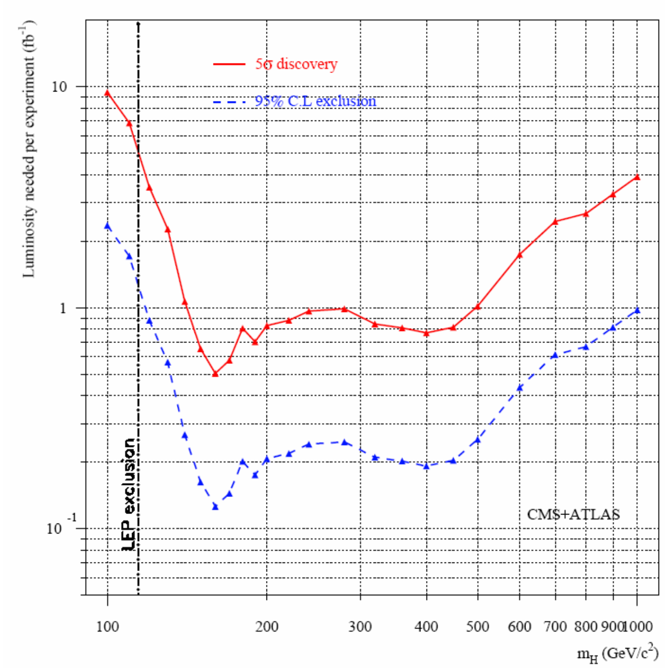


Fig. 1: The prospects for discovering a Standard Model Higgs boson in initial LHC running, as a function of its mass, combining the capabilities of ATLAS and CMS. [Ref: J.-J.Blaising, A.De Reock, J.Ellis, F.Gianotti, P.Janot, L.Rolandi and D.Schlatter, "Potential LHC contributions to Europe's future strategy at the high-energy frontier", contribution to the CERN Council Strategy Group workshop, Zeuthen, May 2006.]

First measurements of the parameters of the Higgs boson, in addition to its mass, will be performed at the LHC. The observation of the Higgs boson in different channels and production modes will allow for a first determination of ratios of couplings to gauge bosons as well to heavy fermions, which will be important to establish the Higgs-like nature of a newly discovered resonance. By using the coupling to W and Z bosons at production, measurements in the vector boson fusion modes will contribute significantly to the coupling measurements.

Data at the LHC will also allow discovery of other symmetry-breaking options, which would open new vistas beyond the Standard Model. One of the most promising options is Supersymmetry. Besides providing for an extended Higgs spectrum, discussed below, it may solve many theoretical problems, such as the origin of mass through electroweak symmetry breaking, the unification of the forces of Nature, the specific nature of dark matter in the universe, and the stability of the lightest Higgs boson mass under radiative corrections. Although the model contains many free parameters, it is highly predictive given a choice for those parameters, so it can be well tested at collider experiments.

The primary prediction of supersymmetry is the existence of a new particle for every known particle in the Standard Model; with the spin their most essential difference. For the leptons and quarks (which are fermions), there are bosonic partners called *sleptons* and *squarks*, while for the gauge bosons and Higgs particles there are fermions: the *gluino* is the supersymmetric partner of the gluon, the *charginos* are mixed states of the partners of the *W* bosons and the charged Higgs bosons, and finally, the *neutralinos* are mixed states of the partners of the photon, the *Z* boson, and the neutral Higgs bosons. The masses, decay rates, and production cross sections can be calculated unambiguously, given a choice of theoretical parameters, thus allowing clear predictions for signals at the collider experiments. Squarks and gluinos may be produced with large cross sections, thanks to their color charges. In R parity conserving models, they would decay ultimately to quarks, gluons and the lightest neutralino – the dark matter candidate, which would give a missing energy signal in a particle detector. The decay chains lead to final states that are characterized by multi-jets, leptons (which might also result from W and Z decays in the cascades), and b-quarks accompanied by large missing transverse energy. The combination of the large production cross sections with the characteristic final state signatures allows the clean separation of the SUSY events from the background from Standard Model processes. A discovery of SUSY particles with masses up to 2.5 - 3 TeV seems possible. The main signal for detecting weakly interacting SUSY particles will come from the measurement of lepton spectra and invariant di-lepton mass distributions.

Signals of low energy supersymmetry, if it is realized in nature, can be detected early on at the LHC, using deviations of inclusive spectra from Standard Model expectations. Fig. 2 shows that for a simple version of supersymmetry just 0.1 fb^{-1} ($1fb^{-1}$) of well-understood data should suffice to discover gluinos if their mass is below about 1.3 (1.7) TeV. Nonetheless, the determination of the parameters of the underlying theory is more difficult. The measurement of various kinematic endpoints of mass distributions (of leptons and b-jets) will help to determine mass differences of the new particles. How well this can be done depends on the specific realization of SUSY. It must be stressed that additional more precise measurements of SUSY production processes at a Linear Collider are essential for pinning down the underlying theory.

Regarding the Higgs sector, in the Minimal Supersymmetric Model (MSSM) there are three neutral (called *h*, *H*, and *A*) and one charged pair of Higgs boson states. The primary parameters that determine all the other masses and couplings are one Higgs boson mass (for example m_A), the third generation squark masses, and $\tan\beta$, the ratio of Higgs vacuum expectation values, which parametrizes the way the two Higgs doublets couple to up-type and down-type fermions. The precise value of tanβ is unknown, so one has to consider a range of values - the phenomenology both of the Higgs and supersymmetric sectors can differ markedly when tanβ varies from moderate values, tanβ ~1-5, to large values, tanβ ~25-60.

The MSSM Higgs sector can be fully explored at the LHC over a large fraction of the relevant parameter space with several of the five predicted Higgs bosons detected. However, there exists also an important parameter region where the LHC will detect only one of the MSSM Higgs bosons with Standard Model-like properties. The LHC may also observe a single scalar state with non-Standard-Model-like production and decay rates. In this case it would be difficult to tell from LHC data alone whether this is due to the presence of an extended Higgs sector, as predicted by the MSSM, a more complex version of supersymmetry, or whether the observed state is an admixture of a Higgs boson with a so-called radion from extra dimensions.

If no state compatible with the properties of a Higgs boson is detected at the LHC, scattering processes of W and Z bosons at high energies need to be studied in order to investigate whether there are signs of a new kind of strong interaction in the gauge boson sector. Effects of the strong interaction are expected to manifest themselves in resonant and non-resonant scattering of longitudinally polarized vector bosons at the TeV scale. Demonstrating evidence for strong electroweak symmetry breaking in the non-resonant case will not be easy at the LHC and large integrated luminosities will be needed.

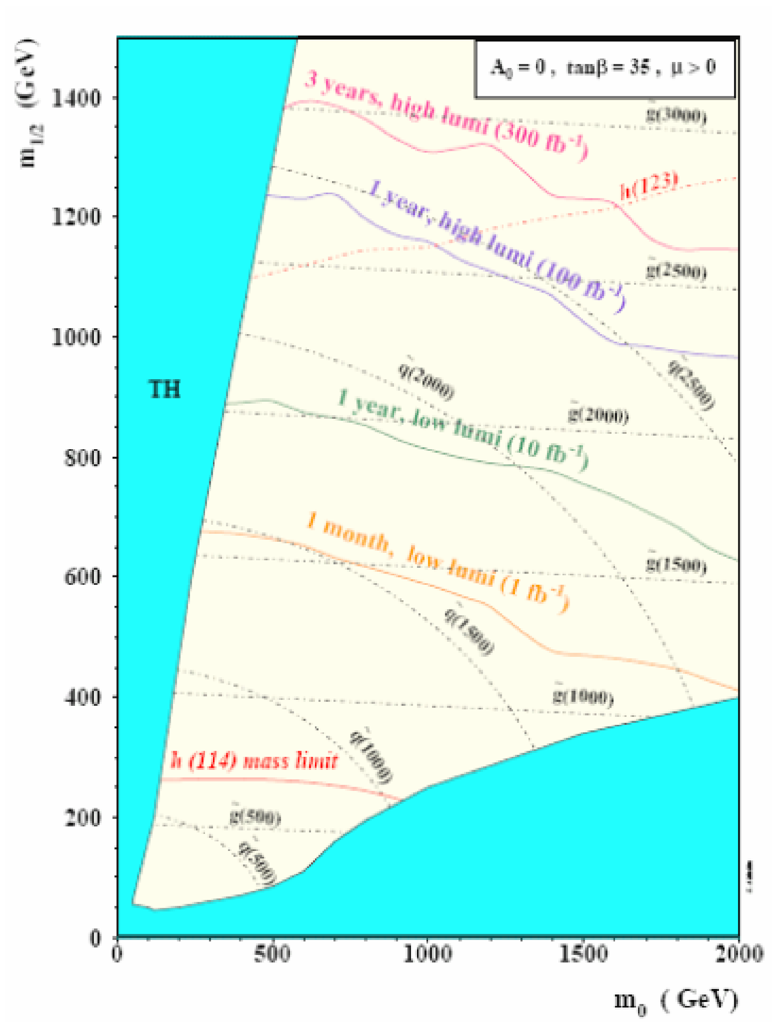


Fig. 2: The CMS reach for supersymmetric particles at the LHC, a similar reach is expected for ATLAS. The reach is essentially independent of the assumed values of tan β, A_0 and the sign of μ. [Ref: J.-J.Blaising, A.De Reock, J.Ellis, F.Gianotti, P.Janot, L.Rolandi and D.Schlatter, "Potential LHC contributions to Europe's future strategy at the high-energy frontier", contribution to the CERN Council Strategy Group workshop, Zeuthen, May 2006.]

Similar to the SUSY case, many other models of physics beyond the Standard Model can be explored at the LHC, *e.g.* resonances predicted by Technicolor Models and leptoquarks in the mass range of 1-2 TeV, new heavy vector bosons in the mass range up to 4-5 TeV, and many others.

Also the physics of the top quark plays an important role as a possible window to new physics. The top quark is the heaviest elementary particle found so far. The large expected production rate of ~ 1 top pair per second at the LHC allows for the identification of large samples of top events. They can be used to determine the mass of the top quark with an expected precision of better than 1 GeV, measurements of its couplings to the W boson at the few percent level, and the detection of possibly enhanced, non-standard flavor changing neutral current (FCNC) decays with branching ratios up to 10^{-5}. The top quark will also be used to tag more exotic phenomena, such as the production of stop squarks or as an accompanying signature of an invisibly decaying Higgs boson.

The many studies performed during the past years have demonstrated the enormous discovery potential of the ATLAS and CMS experiments. The combination of good identification and measurement of leptons (including τ leptons), the identification of b quarks, and reliable energy measurement in the calorimeters leads to very powerful detectors.

LHC Upgrades

A luminosity upgrade of the LHC, would extend the LHC physics reach by using the existing infrastructure in an optimal way. A possible upgrade scenario assumes an increase of the LHC luminosity by an order of magnitude, *i.e.* reaching peak luminosities of 10^{35} cm^{-2} sec^{-1}. Experimentation at the upgraded LHC, the so-called SuperLHC (SLHC), will be difficult: particle densities and radiation levels will be ten times higher. Other important parameters such as particle multiplicities per bunch crossing, the tracker occupancy, and the pile-up noise in the calorimeters depend on the bunch structure of the machine, for which different options are being discussed.

An energy upgrade in which the center-of-mass energy is doubled to 28 TeV is also under discussion. Such an upgrade would require replacing the ~1200 superconducting dipole magnets by stronger magnets, rendering this upgrade even more challenging and expensive. In the following discussion the focus is put on the luminosity upgrade.

Physics Motivation

An upgrade of the LHC is motivated by two scenarios: (i) depending on the nature of the discoveries made at the LHC it might be necessary to increase the luminosity to accumulate high statistics samples to study new resonances and their parameters with higher precision, (ii) if no new resonances are discovered, the accessible energy frontier must be pushed forward as much as possible. An example for scenario (i) would be a Higgs boson, for which the parameter measurement could be improved and (depending on the Higgs boson mass) constraints on the predicted Higgs boson self-coupling could be derived. The SLHC should also be able to observe for the first time several rare decay modes of the Standard Model Higgs boson, like $H \to \mu\mu$ and $H \to Z\gamma$.

In the case where no evidence for a Higgs boson resonance is found, a high luminosity machine would be vital to study as precisely as possible the high energy *WW* scattering to understand the origin of electroweak symmetry breaking. Due to the larger SLHC event rate, an excess number of events in the non-resonant W^+W^+ scattering should become significant. In addition, low-rate channels, such as the possible production of resonances in *ZZ* scattering could be observed for the first time at the SLHC.

An increase of the integrated LHC luminosity by an order of magnitude extends the LHC discovery reach in direct searches of physics beyond the Standard Model by about 20-30% in mass and allows for additional and more precise measurements. At the SLHC the mass reach for squarks and gluinos can be extended to 3 - 3.5 TeV. The mass reach for discovery of the heavier Higgs bosons *H*, *A* and $H^{\pm}$ in the MSSM can be extended by ~100 GeV.

Compositeness, another possible extension of the Standard Model, motivated by the existence of three generations of fermions, should manifest itself at the LHC through four-fermion contact interactions. Quark contact interactions should give rise to an excess of high-p_T jets above the expectations from Standard Model processes. The LHC should be able to probe compositeness scales up to 40 TeV, whereas SLHC should extend this reach to 60 TeV.

Detector Upgrades

In order to achieve the above-mentioned physics potential and thus fully profit from a luminosity upgrade of the LHC, the detector performance must be similar to that presently foreseen for the baseline ATLAS and CMS detectors. This implies in particular a fully functional Inner Detector with good tracking capabilities in an environment with much higher particle multiplicities than at the design LHC. Concerning calorimeters it should be noted that the increase of the event pile-up will deteriorate the energy resolution of electrons, photons and jets with moderate p_T and will make their identification more difficult. Following scenario (i) described above, the issue is to maintain good reconstruction capabilities and signal-to-background ratios for processes already observed at the LHC and for which improved precise measurements can potentially be achieved at the SLHC.

Most challenging is the upgrade program of the Inner Detector. Technologies that work today at inner radii of the tracker can be used for tracking at larger radii at the SLHC. However, for the inner and for the intermediate regions new technologies need to be developed. This requires that research and development activities are pursued today on various items, for example, semiconductor pixel or strip sensors, and electronics have to be developed that are more radiation tolerant. The U.S. groups have already started to play a very active role in these research and development activities.

Accelerator Upgrades

The primary road that will be followed to achieve the high SLHC luminosity is to increase the beam current and to achieve smaller transverse beam sizes. At present several options for the bunch structure in the machine, which directly impacts on the pile-up and occupancy levels in the detectors, are discussed.

An upgrade of the interaction region will be necessary to achieve significantly higher luminosities. Another motivation for such an upgrade comes from the fact that some machine elements close to the interaction point, such as focusing quadrupoles, have a radiation limit corresponding to an integrated luminosity of about 700 fb^{-1}. This limit could be reached already around the years 2013-2015, setting a possible time scale for the development and deployment of the LHC upgrade.

The luminosity upgrade might also proceed in steps. The initial phase concerns the increase of the beam current to the ultimate value, leading to a peak luminosity of $2.3 \cdot 10^{34}$ cm^{-2} s^{-1}. The baseline luminosity upgrade scenario relies on a new layout of the interaction regions (reduction of β^* and increase of the crossing angle of the beams). The corresponding peak luminosity is multiplied by a factor of two ($L = 4.6 \cdot 10^{34}$ cm^{-2} s^{-1}) provided that the bunch length can be reduced by a factor of 2 by means of a new RF system. In such a scenario, the pile-up in the detectors will be increased from an average of 23 to 88 inelastic pp reactions. This scheme is the safest option in terms of beam dynamics, machine protection, and radiation risks, but the new magnets in the interaction region and the new RF are challenging.

Further increases in luminosity involve major modifications of several LHC sub-systems and of the injector chain to exceed the ultimate beam intensity. It will also require an increased number of bunches and may not be compatible with electron cloud and long-range beam-beam effects. Different bunch spacings are being considered, of which 12.5 ns is presently favoured by the experiments and would yield a peak luminosity of $9.2 \cdot 10^{34}$ cm^{-2} s^{-1}. An alternative approach would be to have high intensity proton bunches with a bunch separation of 75 ns and a long (~15 cm) flat -instead of a Gaussian beam profile with a σ of 7.5 cm. This would lead to similar peak luminosities ($8.9 \cdot 10^{34}$ cm^{-2} s^{-1}), however, with a drastically increased pile-up of ~510 inelastic *pp* collisions per bunch crossing.

The U.S. machine physicists have via their LARP program contributed very significantly to the R&D activities on the machine side as described in the next section. At present very interesting ideas are being pursued, which would already allow for a fast upgrade of the interaction region.

The U.S. LHC program

Accelerator and Detector Contributions/Responsibilities

The U.S. contributed to the construction of both the LHC accelerator and the ATLAS and CMS detectors. Three U.S. national laboratories (BNL, FNAL, and LBNL) had major roles in the LHC construction as part of the LHC Accelerator Project. The U.S. contribution has supplied CERN with inner triplet magnet systems to collide the proton beams at the interaction points. The inner triplet systems consist of high-gradient quadrupoles provided both by Fermilab and KEK, the High Energy Accelerator Research Organization in Japan, correction coils provided by CERN, dipole magnets provided by BNL, cryogenic feedboxes provided by LBNL, and absorbers provided by LBNL to protect the superconducting magnets from collision debris. The superconducting quadrupole magnets are among the most challenging components of the machine. They provide a field gradient of up to 215 Tesla/meter over a 70 mm aperture and operate at 1.9 K, under the heat load due to secondary particles from beam-beam collisions. The LHC performance depends critically on their field quality.

The LHC is instrumented with two powerful multi-purpose particle detectors, ATLAS and CMS, designed and optimized for the discovery physics expected at these energies. About 88 institutions from across the United States participate in these LHC experiments. Both ATLAS and CMS are international collaborations, which include approximately 150 institutions with about 2,000 scientific authors each. U.S. researchers make up about 20% and 30% of the ATLAS and CMS collaboration, respectively. In fact, by 2007, more than half of all U.S. experimental particle physicists are expected to be working at the LHC. U.S. scientists and engineers have made critical contributions to the construction of these state-of-the-art detectors. Members of the U.S. team will be essential to the success of the operations and to realize the fruits of this tremendous scientific opportunity. The U.S. institutions are also leading the effort to develop distributed computer networks (Grid Computing) and perform analysis of simulated data at geographically distributed sites as required in a truly international project.

The DOE and the NSF have provided funding of $250 million and $81 million, respectively, for the fabrication of the ATLAS and CMS detectors. In addition, DOE has provided funding of $200 million for the construction of the accelerator. The U.S. LHC contribution, capped at $531 million, amounts to about 10% of the facility's overall cost of about $6 billion. The benefits of this international collaborative effort are enormous:

1) It offers unsurpassed discovery opportunities for U.S. scientists.
2) It is essential to keeping our national laboratories and universities on the cutting edge of technology and forefront research.
3) It provides transfer of the technology to industry to build new devices
4) The U.S. participation in the LHC provides an exceptional educational framework for the recruitment and training of future scientists and engineers in accelerator physics, precision particle detectors, fast data acquisition, and grid computing. This is a critical element in the scientific future of the U.S.
5) The major role of the U.S. in a world wide international collaboration for the LHC should lead to future collaborations for facilities based in the U.S. such as the ILC.

The specific U.S. contributions for ATLAS and CMS are listed in Appendix 1. In each experiment U.S. groups are responsible for components critical to every aspect of the detectors: vertex detectors, trackers, calorimeters, muon chambers, triggers, data acquisition and computing. For each component, the U.S. is contributing to the installation, pre-operations, commissioning and maintenance of the various detector components.

Maintenance and Operations of ATLAS and CMS

An essential element of the P5 roadmap is the strong support for the LHC research program and future upgrades of the LHC. Continued support of the LHC research program is critical for U.S. physicists to be able to take full advantage of the discovery potential of the LHC after their major contributions to the construction of the LHC experiments and the accelerator. The level of support must reflect the enormous interest of U.S. universities and laboratories in the LHC physics program and must account for the continued growth of the U.S. contingent in ATLAS and CMS and the upgrade program for the SLHC.

To have a successful LHC and SLHC physics program the U.S. must support the maintenance and operation (M&O) of U.S.-built components of the LHC detectors; the software, computing and physics analysis support required for data analysis; and the research and development (R&D) for future detector upgrades to handle luminosity increases. The M&O component is required to ensure the proper functioning of these complex detectors. The computing is essential to allow rapid access to data and the necessary processing capability. Preparation for the luminosity upgrade of the LHC must start now since some components of the experiment are known to have a limited lifetime in the radiation environment of the LHC.

Both ATLAS and CMS have provided accurate estimates to the DOE and the NSF of the support needed as they move from construction to maintenance and operation. The original estimates of the cost for maintenance and operation, software and computing, and R&D for the SLHC is about $33 million/year. These estimates should be updated for the increased number of collaborators and the effects of inflation. The required additional contribution has been estimated to rise from $4.4 million in FY07 to $7 million in FY11. Full participation in the LHC requires also an increase in the Core Program of about $3 million/year to account for the larger travel costs to participate in experiments based in Switzerland.

U.S. Contributions to the LHC Accelerator Upgrade

As the LHC Accelerator Project completed the deliverables for the LHC construction, the U.S. LHC Accelerator Research Program (LARP) was started. This R&D program involves four of the U.S. national laboratories (BNL, FNAL, LBNL, and SLAC). It will enable U.S. accelerator specialists to take an active and important role in the LHC accelerator during its commissioning and operations and to be a major collaborator in LHC performance upgrades. The LARP program has three main goals: advance high energy physics by speeding the LHC commissioning and improving the LHC performance, advance U.S. accelerator science and technology by conducting forefront R&D and keeping skills honed by working on the LHC, and advance international cooperation in high energy accelerators.

The LARP program is focused on beam diagnostics and instrumentation, the development of Nb3Sn quadrupoles, and the upgrade of the interaction regions at the LHC. The funding foreseen for LARP is about $12 million/year. An additional contribution of about $1 million/year for advanced instrumentation, a second generation IR upgrade, and superconducting magnets will facilitate additional leadership from the U.S. accelerator community and could have a major impact on the early commissioning of the LHC and the IR upgrade.

U.S. Contributions to the LHC Detector Upgrade R&D

Both the ATLAS and CMS collaborations are actively engaged in the R&D necessary to operate at the SLHC. They have instituted international teams with detector experts and management to determine which detector components cannot survive a factor of ten luminosity increase. It is expected that the radiation levels and data rates require novel approaches to particle detection, triggering, and data collection. The anticipated date for installation of the upgrades is around 2015.

U.S. ATLAS has already initiated a vigorous Upgrade R&D program to upgrade the detector for the expected increased luminosity. The U.S. effort involves 14 universities and 2 national labs. The areas identified for replacement are the entire inner tracking detector, which suffers the greatest amount of radiation damage, and the liquid argon on-detector electronics chain, which is also subjected to radiation. The U.S. groups were already heavily involved in the construction of these detectors, and they have the expertise to lead the required upgrades. Significant R&D has already started. The most serious challenge is presented by the inner-most tracking layer located at a radius of ~5 cm, since none of the currently used silicon technologies will function properly because of charge trapping. ATLAS is investigating the development of a novel 3-dimensional detector where charges drift to closely spaced column electrodes embedded within the bulk silicon. The novel 3-D architecture reduces the drift distances to electrodes and therefore charge trapping. At outer radii, financial constraints require the employment of more standard strip detectors. For radii between 25 cm and 65 cm detectors with n-type silicon strips on p-bulk substrates allow the collection of electrons rather than holes and good charge collection without full depletion. These detectors are potentially cheaper since they require single-sided processing but have not been traditionally used in particle physics experiments where most detectors have used p-strips on n-bulk. R&D is required to understand these novel detectors and to develop the necessary front-end electronics.

The ATLAS pixel group is considering CMOS electronics utilizing the new generation of 0.13 micron feature size electronics. For strips a new generation of SiGe bi-CMOS electronics offers high performance, radiation hardness, and low power operation. R&D is required to design and fabricate optimized front-ends. The data transmission system will also have to be redesigned for better radiation resistance, higher data rates, and better integration. Operation at the SLHC will require increased granularity, to keep the cable and cooling services from growing excessively. A new integrating structure will have to be developed to allow many modules to be serviced by common cables, cooling, and data collection.

The radiation doses received by the front-end electronics will exceed the specifications for the current ATLAS Liquid Argon (LAr) readout. This readout is a very complex system, involving 13 different technology integrated circuits, 20 different voltage regulators, spread over several circuit boards. A completely new front-end electronics will have to be designed capable of surviving high radiation doses.

The U.S. CMS SLHC R&D is ramping up but the collaboration is still focused on the completion of the current detector. The R&D is directed to the replacement of the pixel detector, the first few layers of the inner tracker, and the inclusion of tracking information in the Level 1 trigger. The CMS collaboration has held several workshops focused on the SLHC R&D. An international plan will be developed by the end of 2006.

Observations Regarding the LHC

The LHC physics program is our best window on new physics for the next decade and is the highest priority program in particle physics. Funding and priorities of the U.S. program should be maintained in such a way as to ensure the maximum physics output from this program. The infrastructure at U.S. universities and laboratories should be such that full advantage can be taken of the data from the LHC program. A critical component of this is sufficient travel support for participating groups.

Recommendations

- The LHC science program should be supported so that U.S. institutions can play a major role in the exploration of the LHC physics. This includes support of theoretical and experimental efforts, computing, maintenance and operation, upgrade R&D and necessary travel support to make this program a success. The level of support should not be allowed to erode through inflation.

- The U.S. should maintain a presence in the LHC machine and contribute in such a way that the machine performance can be improved steadily to achieve the necessary luminosities.

- Participation in upgrades of the LHC machine and the detectors should be reviewed when first results from machine operation and first physics results are available. R&D for the upgrades should be pursued now, but only as part of the international R&D plans of ATLAS, CMS and the LHC machine.

Physics at the ILC

The LHC is a discovery instrument, offering our first glimpse of the physics at the Terascale, and having the capability to detect the many new particles and phenomena that we believe reside there. While the discovery of new particles helps us to understand how the universe works, the mere observation and cataloguing of them is only part of the understanding. Particles are messengers, and studying their properties and interactions lead to the discovery of new theories or new symmetries of nature. At the LHC these new particles will be produced in complicated environments along with many other particles. Measuring their properties is thus limited by that environment as well as by assumptions about how the particles are produced. For example, to determine how the Higgs particle really fits into a physics framework, what its exact properties are, and whether there is only one, requires a clean environment where measurements can be made free of theoretical assumptions. This is the hallmark of the International Linear Collider. The LHC will unveil the existence of new phenomena, and the ILC would zoom in on properties and probe the details of these phenomena, thus revealing the underlying science.

The ILC will provide the experimental tools to study the new physics in a unique environment, with much higher precision. One of the reasons for that is that the production processes are simple and well understood, as can be seen from Figure 3, where cross sections for Standard Model (known) processes are shown, as well as predictions for Higgs production and for production of some of the supersymmetric final states. The cross sections turn on very distinctly as a function of the center-of-mass energy of the machine and the mass of the particle being produced. With precision detectors this results in unprecedented measurements of the final states. As an example of a measurement sensitive to new physics, Figure 4 shows the predicted coupling of the Higgs to the known fermions and leptons. A deviation of the measured values from linearity in this plot, would indicate new physics requiring a conceptual framework beyond the single Higgs picture.

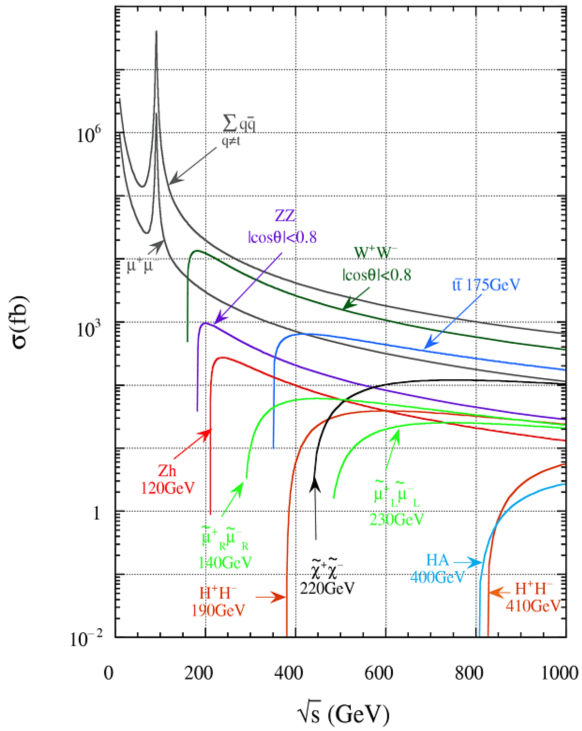


Figure 3. Cross sections for known Standard Model particles and for predicted particles as a function of ILC energy and mass of the particle.

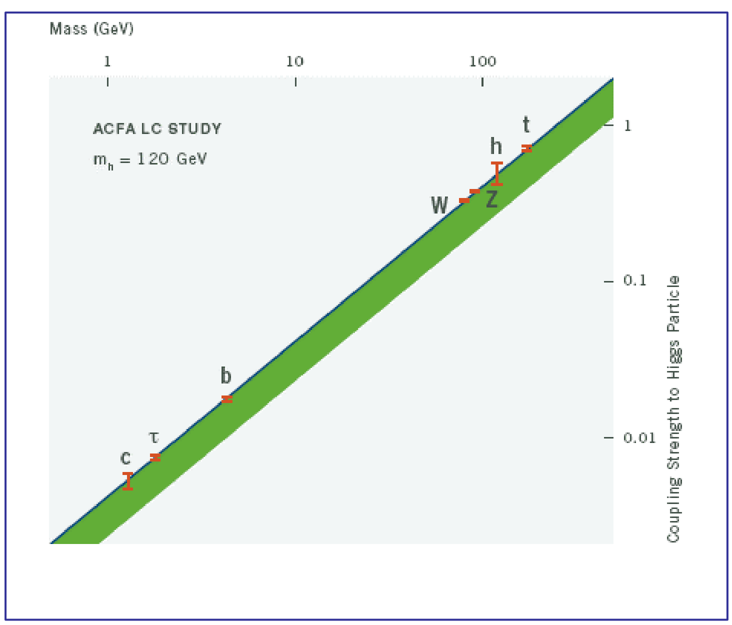


Figure 4. Expected coupling strength of the Higgs to SM fermions and bosons.

In addition to the precise measurement of the Higgs mass, the coupling to fermions and bosons, the ILC will also uniquely determine the spin of the Higgs by measuring the turn on curve as a function of the center mass energy. Other examples of measurements include determining the properties and spectrum of supersymmetric final states, thereby checking that it is actually supersymmetry that has been discovered. A disagreement with supersymmetric predictions would point to other possible explanations for the new discoveries. Since supersymmetry is a broken symmetry characterized by a number of parameters, detailed measurements are crucial for unraveling the real character of supersymmetry.

By precise measurement of the missing energy in events, the properties of a dark matter candidate particle can be identified and measured. Comparisons with cosmological observations could identify whether indeed this is the dark matter particle. Numerical examples are given in the next section on Dark Matter.

The ILC

In the ILC design, two facing linear accelerators, each less than 20 kilometers long, accelerate beams of electrons and positrons toward each other at nearly the speed of light. Each beam contains roughly 3000 bunches of twenty billion electrons or positrons which are compressed to a minuscule five-nanometer thickness at the collision point. The linear accelerators are based on high-gradient superconducting accelerating cavities and the energy of the ILC beam can be adjusted to home in on processes of interest.

The scientists proposing the ILC have striven to make it a truly international project from its inception, with the goal that the ILC would be designed, funded, managed, and operated as a fully international scientific project. At this time, the design studies are being lead by the ILC Global Design Effort team, which includes 63 scientists and engineers from around the world. This team has agreed on the baseline configuration for the particle collider and is developing an international reference design with sample sites and cost estimates for Europe, North America, and Asia.

ILC Organization and Activities

The ILC design effort is being coordinated by the ILC Steering Committee (ILCSC), a subcommittee of the International Committee on Future Accelerators (ICFA). In November 2003, the ILCSC appointed a group of 12 scientists from Asia, Europe, and North America, known as the International Technology Recommendation Panel, to evaluate proposed ILC accelerator technologies. The panel recommended the use of superconducting accelerating structures for the ILC, and both ILCSC and ICFA endorsed the recommendation in August 2004.

With an accelerating technology chosen, the ILCSC formed the ILC Global Design Effort (GDE) and appointed Barry Barish to head the effort. At present, the GDE is focusing the efforts of hundreds of accelerator scientists, engineers, and particle physicists in North America, Europe and Asia on the design of the ILC. The goal is to produce an ILC Reference Design Report (RDR) to be released in early 2007 and an ILC Technical Design Report (TDR) in 2009-2010.

The RDR is based on the Baseline Configuration that was established in August 2005. For the most part, it is based on existing technology but a number of 'alternates' have been identified that have the potential to reduce the cost or improve performance. The R&D program is aimed at both reducing the risk of the baseline components and establishing the feasibility of the alternates. The TDR will be based on the results of this R&D program. Physicists and policy-makers will use the reports to decide the future of the project on the 2010-timescale.

The GDE is advised by both the ILCSC and the Funding Agencies for Large Colliders (FALC) as shown in Fig. 5. FALC is a group of representatives from funding agencies around the world who will help develop international funding mechanisms for the ILC. FALC meets on a regular basis with members of the GDE to establish a dialogue between the funding agencies, governments, and the ILC scientific community. In addition, the ILCSC has set up a Machine Advisory Committee (MAC) that reports to the Committee on ILC accelerator issues. The ILC MAC will advise the ILCSC on the accelerator design and the R&D program that the GDE is developing.

The GDE is organized into an executive committee, which consists of the GDE leader, the three Regional Directors and the three Regional Accelerator leaders. In addition there are three subgroups that oversee the global R&D program and the design effort, as shown in the chart in Figure 5.

The international ILC R&D program is overseen by the Global R&D Board (RDB), which is responsible for assessing and providing guidance for the overall R&D program. The RDB will suggest priorities for the research facilities and R&D supporting the baseline, the R&D on alternatives to the baseline, and selective R&D that could further the field in the longer term. The mission will also include global assessments and recommended priorities for the detector R&D program and evaluation of the balance between accelerator and detector R&D.

The Design/Cost Board (DCB) is responsible for assessing and providing guidance for the overall RDR design effort program. The DCB initial goals will be to propose the overall structure and content for the Reference Design Report (RDR) document to be released in early 2007. The DCB will set goals and milestones for producing the RDR, conduct design reviews and provide guidance and assessments of the RDR effort.

The Configuration Control Board (CCB) is responsible for maintaining the baseline configuration as defined in the Baseline Configuration Document (BCD). In addition to maintaining the baseline, the CCB will assess R&D projects defined in the BCD that potentially can lead to improvements over the baseline in cost or performance. The CCB will define what needs to be demonstrated in these R&D projects, in order to be considered for a CCB action to replace the baseline.

In the U.S., the ILC program is administered by the Americas Regional Director who is appointed by the Linear Collider Steering Group of America (LCSGA), in consultation with the GDE Director. In FY06, a preliminary WBS for the Reference Design Report and R&D activities was established after discussions with international partners. Proposals from the participating U.S. laboratories were then mapped onto the WBS. This program was documented in a series of MOUs signed between the participating laboratories and the GDE. The FY06 program was reviewed in a combined DOE/NSF review in March of 2006.

A similar process was used for FY07 except additional input was available from the GDE to help prioritize the program. The Global R&D Board made a formal evaluation of the R&D proposals and the GDE Reference Design

Report management committee evaluated the accelerator design proposals. These rankings were then used by the Americas Regional Director when prioritizing the work packages that had been proposed by the participating laboratories. Again, the final agreements will be documented in a set of MOUs signed between the laboratories and the GDE and the program will be reviewed by a combined DOE/NSF committee early in 2007.

The laboratory-based proposal driven structure has been reorganized for FY08 and FY09. The Americas Regional Director has chosen a set of level-2 WBS managers who then, in consultation with the international partners, will develop proposals for the R&D and design activities across the U.S. laboratories. These proposals will then be prioritized by the Americas Regional Director and the Americas Accelerator leader working with the GDE Global R&D Board and the Executive Committee.

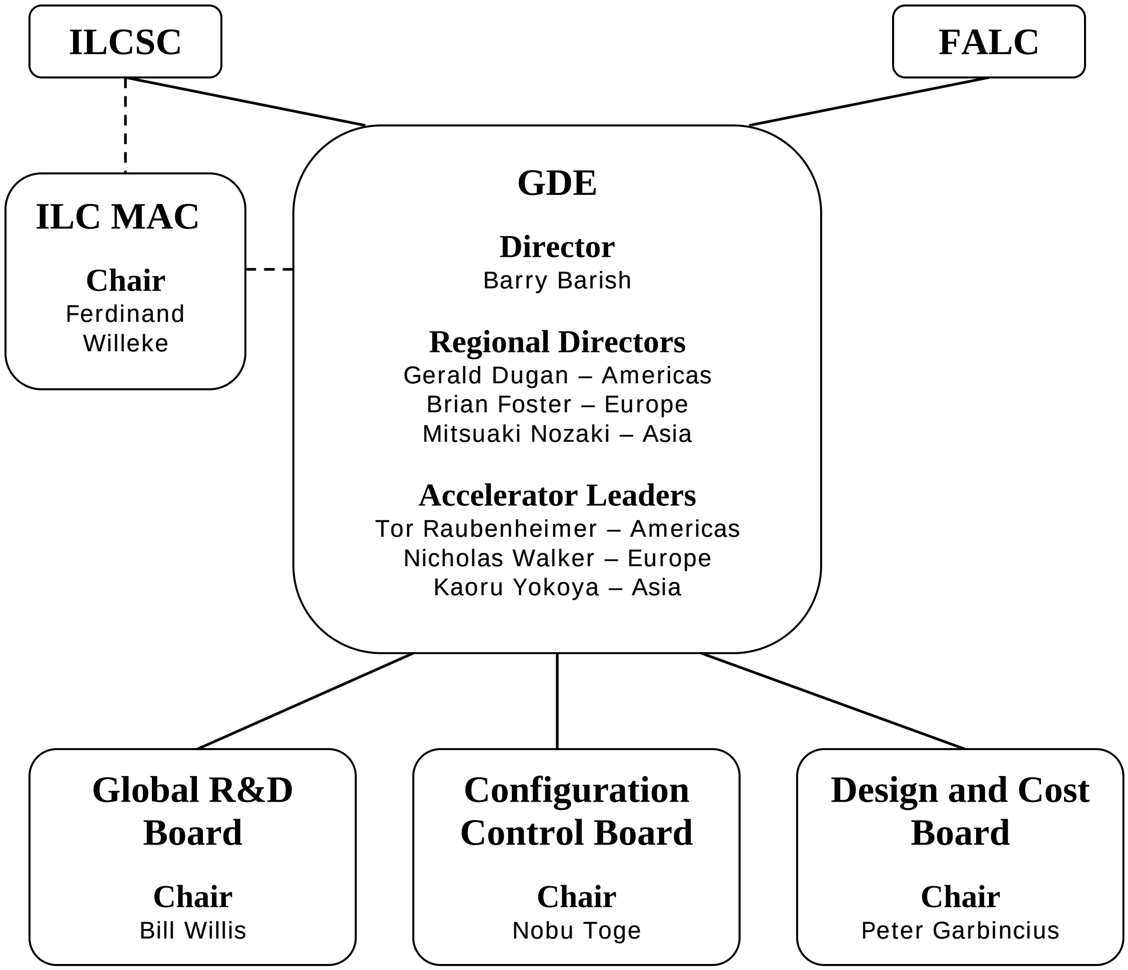


Figure 5. GDE Organization Chart

ILC Activities & Timeline

As already indicated above, the current ILC global and U.S. activities focus on delivering a Reference Design Report and the corresponding cost estimate by early 2007. This also includes a Detector Conceptual Design, based on ongoing detector concept design studies. The next step will be the Technical Design Report with a goal of completion around 2009-2010. R&D activities will continue during this whole period on accelerator and detector components. Assuming that a site will be identified and that the corresponding international agreements can be realized, construction could start around 2012-2013. Currently the estimate for construction is 7 years plus one year of commissioning to achieve an operational complex.

ILC Global Accelerator R&D Program

Two decades of accelerator R&D has made it possible to undertake a TeV-scale linear collider. The basic technologies that will be needed for the accelerator have been demonstrated. However, the R&D program is far from finished. In fact, the continuing and future R&D programs will be absolutely crucial for demonstrating critical elements of the design and optimizing it with respect to cost and performance. These R&D programs will also need to support the industrialization of the specialized components to attain the mass-production cost reductions that are required.

This highly successful linear collider R&D program continues to be primarily supported regionally by the major high-energy physics laboratories throughout the world. The largest fraction of the R&D program is focused on the superconducting (SC) cavities and cryomodules needed in the main linac. In Europe, these R&D activities are centered at DESY and the TESLA Test Facility. There is also an extensive R&D program for the European XFEL, which is based on similar although not identical technology and has important benefits for the ILC. In Asia, the R&D is centered at KEK where a new linac test facility is being constructed with locally produced SC cavities. In the U.S. the SC cavity R&D is distributed between ANL, Jefferson Lab, and Cornell University, while the cryomodule design is being done at Fermilab; all of these activities have the goal of constructing an RF unit, the basic building block of the main linac, at Fermilab.

Other laboratories around the world are supporting other crucial elements of the R&D program: the rf power sources are being developed at SLAC and KEK; the fundamental mode couplers are being developed at Orsay, KEK, and SLAC; elements of the positron source are being developed in the UK, ANL, LLNL, and SLAC; the damping ring components are being studied at many laboratories including INFN Frascati, KEK, Cornell, ANL, LBNL, and SLAC; and the beam delivery system components are being developed in the UK, KEK, BNL, and SLAC.

To insure that the ILC R&D maximally supports the GDE design effort, the GDE is providing global guidance for the program, setting priorities, and identifying gaps in the programs. The GDE R&D Board has created a number of task forces aimed at clarifying some of the most important topics. These include: the S0/S1 task force which is charged with identifying a path towards achieving a process for manufacturing SC cavities with a gradient greater than 35 MV/m with a high yield; the S2 task force charged with identifying the scale of a main linac test facility and making recommendations on how to construct such a facility; the S3 and S4 task forces which are charged with identifying the R&D programs for the damping rings and beam delivery systems, respectively.

The GDE is in the process of developing a globally integrated plan for the TDR and the supporting R&D. The Americas Regional Team is developing an integrated WBS for its contributions to the ILC design and R&D – other regions are expected to follow with similar descriptions – however, it should be noted that the distributed nature of the funding for the R&D presents a difficult management problem. Coordinating the different R&D programs and balancing the regional interests against the project interest is something that is not fully possible to achieve without more direct control. It is expected that this will change as the ILC project moves forward.

U.S. Program, Accelerator R&D Activities

The U.S. part of the ILC R&D program is predominantly performed within the national labs. This R&D program is an integral part of the worldwide program and the priorities and funding levels reflect the priorities of the worldwide R&D program. To set the scale, the U.S. request, based on priority projects requiring R&D, is about $105M for FY07. Most of the R&D centers on the most pressing problems, which are mostly related to the Main Linac. They involve R&D on SC cavities, with the main goal of achieving a consistent 35MV/m acceleration gradient. The next step is the construction of cryomodules and RF units, the basic building blocks for the main linac. This effort represents about 33% of all R&D in the U.S. The next big R&D items are the RF systems, the damping rings and the beam delivery system, which roughly account for another 20%. The remainder of the R&D focuses on electron and positron sources, main linac optics and beam dynamics, and global systems. An important part of the R&D program is also the "bid to host" or "R&D in the regional interest", which supports the case for locating the ILC in the U.S., by emphasizing technical capabilities in the U.S. and work on conventional facilities to prepare a U.S. site.

U.S. Detector R&D activities

The physics questions that the ILC will address require detector capabilities that are beyond the performance of current detectors. To address the anticipated final states, identify them and measure them with the precision required, essentially all charged leptons, quarks and gluons (in form of jets), photons, and W and Z bosons have to be measured extremely well. This requires excellent particle identification using vertexing detectors, excellent tracking and momentum resolution, as well as jet energy resolutions, which are only possible with new approaches toward jet energy measurements with so called Particle Flow Algorithms (PFA). The global structure responsible for the physics program, and its detectors, is the World Wide Study, which has three chairs, one from each of the three regions of the world participating in the ILC. The corresponding regional entity is the American Linear Collider Physics group, which coordinates physics and detector efforts in the U.S. region.

To achieve these advances in detector performance a well-orchestrated worldwide detector R&D program is needed. Such a program has been realized in Europe over the last few years, where it has been reviewed and funded and is addressing some of the R&D areas that need attention. Examples are sensor R&D for vertex detectors and new hadron calorimeters, which utilize the PFA method. In the U.S. such a coherent and funded program is only partially in place. There is an existing university based ILC detector R&D program, which has been funded at the level of about $0.7M/year for several year and in FY06 increased to $1.4M/year. This program is based on proposals from groups or individuals at universities. In addition there is a lab-based detector R&D program, which is weakly coupled to the university program. It is fair to say that the U.S. efforts on ILC detector R&D are lagging seriously compared to the efforts in Europe, both in terms of funding and manpower. The funding shortfall over the past few years has made it difficult to define a coherent and well-rounded program. Given that the U.S. wants to play a leading role in the ILC, this problem needs to be addressed and a well-defined U.S. ILC detector program with sufficient funding should be realized.

Observations Regarding the ILC:

The worldwide particle physics community has now embarked on a several year program of intense R&D for the ILC. This will be needed before a start of construction can be considered. In our Roadmap we have allocated approximately $500M over the next five years toward this effort, as recommended in the EPP2010 report. Since this constitutes the largest expenditure in the U.S. for any single project, funding agency oversight should be comparable to that asked of other Projects of such size, even in the R&D phase.

Recommendations

- Currently the highest longer-term priority for the worldwide particle physics program is to realize the ILC. Sufficient R&D funds to achieve this goal in a timely way should be made available over the next few years.

- The level and areas of participation of the U.S. in the R&D efforts should be established within the international framework of the ILC GDE. R&D should be coordinated among the different regions of the world, to make economical use of our resources.

- Within the international ILC R&D framework, establish the corresponding milestones for contributions from the U.S., so its progress can be monitored.

- The U.S. part of the worldwide ILC R&D program should be managed like any other project in HEP, i.e. through a well defined project management structure and a multiyear plan for R&D, with monitoring through periodic reviews.

- A multiyear U.S. ILC detector R&D program, defined within the international program, and including laboratories and universities, should be established and funded at a level commensurate with other regions in the world. It should be managed centrally and monitored periodically.

- The U.S. efforts to host the ILC should be supported in a manner that will allow a credible and competitive bid to host the ILC by the U.S. soon after initial LHC results are known.

2. Dark Matter

The nature and origin of dark matter (DM) is one of the big questions of science today. While astrophysical observations indicate that it exists, and that it constitutes a major portion of the mass of the universe, we do not know what it is. We *do* know that it is not ordinary matter. In this sense, dark matter provides the first and most robust evidence for physics beyond the Standard Model (SM) of particle physics. There are, in fact, several speculations as to the identity of the dark matter coming from particle physics theory, but we will not be able to resolve these until we have more data. Thus, the detection and study of dark matter in terrestrial experiments must be one of the priorities of particle physics in the near future.

A large number of astrophysical observations provide evidence that roughly 23% of the energy density of the universe consists of dark matter, whose presence is inferred only from its gravitational influence. The oldest and most basic evidence comes from the rotational curves of galaxies, which show that the orbital velocity of the stars or gas in a galaxy remains approximately constant over a large range of distances from the center of the galaxy. This observation cannot be explained by the gravitational effect of the luminous matter. On the basis of Newtonian mechanics, one would expect that the average orbital speed of an object at a given distance away from the bulk of the mass distribution would decrease inversely with the square root of the radius of the orbit. Therefore, if most of the mass of a galaxy were at the galactic bulge near the center, stars and gas clouds away from the central bulge would be rotating much slower than what is observed. This anomaly can be explained by postulating the existence of a kind of matter, which does not emit light with the same light-to-mass ratio as the luminous matter in the central bulge, and is not as concentrated toward the galactic center.

The existence of dark matter has been corroborated by observations unrelated to galaxy rotation curves, including gravitational lensing, measurements of the cosmic microwave background radiation, and measures of the spatial characteristics of the large scale structure of the universe.

Strong gravitational lensing is the process by which the light of a very distant, bright source is bent around a massive object, such as a galaxy, and arrives as a deformed image to the observer. The amount of bending is determined by all of the matter, including the dark matter. Weak lensing surveys measure the shapes and orientations of large numbers of distant galaxies, and requires that their orientations be locally averaged to measure the deformation of images in any region. These techniques provide a way of inferring the amount and distribution of dark matter hidden between us and the observed galaxies.

Dark matter plays a crucial role in the process of structure formation, both in explaining the tiny anisotropies observed in the cosmic microwave background radiation (CMBR) as well as the formation of large scale structures, such as galaxies, clusters and super-clusters of galaxies, and voids in the universe. The manner in which structure grows in the early universe depends on the amount and type of dark matter present. Dark matter interacts mainly through gravity, collapsing into a complex network of dark matter halos well before ordinary matter does, thereby accelerating the epoch of galaxy formation.

The evidence that dark matter exists is clear, but at present its identity is unknown and many of its properties remain speculative. There is strong evidence that it is not the ordinary matter of stars, gas, dust and planets. Two main categories of dark matter have been proposed:

i) Hot/Warm dark matter, consisting of particles that travel with relativistic velocities, such as neutrinos. Neutrinos have a very small mass and do not interact via either the electromagnetic or the strong nuclear force. However, measurements of neutrino properties indicate that ordinary neutrinos make only a small contribution to the density of dark matter. Moreover, fast-moving particles cannot explain the small-scale structure in the universe and it is therefore necessary to invoke cold dark matter.

ii) Cold (non-relativistic) dark matter could be baryonic matter such as non-luminous gas, brown dwarfs and so-called MACHOs (Massive Compact Halo Objects). Information on the baryon density of the universe comes from two independent data sources. Firstly, the abundance of primordial elements combined with predictions from Big-Bang nucleosynthesis and WMAP measurements of the CMBR show that the baryon number abundance is given by $\Omega_B h^2 = 0.0223\,^{+0.0007}_{-0.0009}$, with $h = 0.71 \pm 0.04$. Secondly, the CMBR and the Sloan Digital Sky Survey also provide measurements that yield a combined result for the matter density $\Omega_M h^2 = 0.1329\,^{+0.0056}_{-0.0075}$. From the difference between the two measurements, one obtains the non-baryonic dark matter density: $\Omega_{CDM} h^2 = 0.1106^{+0.0056}_{-0.0076}$, which is the dominant component.

Since the experimental data imply that baryonic and hot dark matter can only be a tiny part of the total, attention is focused on cold, non-baryonic dark matter. There are many well-motivated candidates for cold dark matter particles coming from theoretical particle physics, but the Standard Model itself has no suitable candidate. The best-motivated candidates are axions and WIMPs (Weakly Interacting Massive Particles).

Axions were first postulated in an attempt to solve the CP violation problem in QCD. They would have no electric charge, a very small mass and very low interaction cross-sections for strong and weak forces. Their coupling to photon pairs would be proportional to the axion mass, with a proportionality constant that is model dependent but expected to lie within a fairly narrow range. Since axions are created in a nonrelativistic state at the QCD phase transition in the early universe, and are too weakly interacting to reach thermal equilibrium with matter in the early universe, they could constitute *cold* dark matter. The axion relic density is computed to be proportional to $(10^{-5}\ \mathrm{eV}/m_a)^{7/6}$, and thus, somewhat counter-intuitively, the lighter the axion, the larger the contribution would be to the energy density of the universe. The exact contribution to the relic density is uncertain and depends on the assumptions made regarding the production mechanism. Nonetheless, one can still make the general statement that below about 1μeV the axion contribution by itself will overclose the universe. On the other hand, axion masses above 1 meV are ruled out by stellar evolution constraints, SN1987a observations, and accelerator-based searches.

WIMPs are massive, neutral weakly interacting particles with interaction cross sections sufficiently large that they would have been produced and would have annihilated for some period of time in the early universe. They would by assumption have been in thermal equilibrium, which allows a precise prediction of their relic density on the basis of standard cosmology at the early epoch. As the universe expanded and cooled down, encounters between particles became rare, and finally annihilation became too slow to keep up with the Hubble expansion. At that point, the dark matter particles dropped out of equilibrium. Integrating the Boltzman equation for the WIMP density through the time at which it fell out of equilibrium, one obtains the

WIMP relic abundance: $\Omega_\chi h^2 \approx 10^{-27} cm^3 s^{-1} / < \sigma_A v >$, where $< \sigma_A v >$ is the thermal average of the DM pair annihilation cross section times the relative velocity. If the mass and the cross-section are determined by electroweak physics, for example for m = 100 GeV, we will have $< \sigma_A v > = \pi\alpha^2 / 8m \approx 1\text{pb}$, resulting in a dark matter density compatible with WMAP measurements.

If the expansion history of the universe at or after the time of the WIMP freeze out were non-standard, then the computation of the relic density would be different. In this case, contrasting information from different DM search techniques will be essential to unravel the mystery of dark matter. It is, however, an amazing coincidence that if we assume that standard cosmology is valid up to the early universe, the estimate for the scale of new physics to explain DM coincides with the scale at which we expect new physics in relation to electroweak symmetry breaking. Hence, we expect models that provide viable WIMP candidates to involve the electroweak sector in an essential way.

Most models of electroweak symmetry breaking introduce an extra discrete symmetry such that the lightest non-SM particle is stable. The best prototypes for WIMPs are

1) The lightest "neutralino" - supersymmetric partner of a mixture of the photon, the Z gauge boson and the neutral Higgs bosons - which is stable if the discrete symmetry $R_P = (-1)^{3B+L+2S}$ ("*R*-parity") is conserved. R-parity distinguishes SM particles from their supersymmetric partners. Other supersymmetric particles such as sneutrinos, gravitinos and axinos could also provide dark matter candidates in certain SUSY models.

2) The lightest Kaluza-Klein (KK) excitation of the $U(1)_Y$ gauge boson appearing in theories of universal extra dimensions, which is stable if "KK-parity" is conserved.

3) The T-odd heavy photon in Little Higgs Models, which is stable if T-parity, is conserved. T-parity is a discrete Z_2 symmetry which distinguishes all SM particles from the new TeV scale particles.

All three of these models can predict the properties of dark matter particles consistent with astrophysical data, and so they can also predict potential signals in terrestrial experiments.

The Search for Dark Matter

The search for dark matter involves three different types of experiments: direct detection, indirect detection and production at high-energy colliders. As will become apparent in the following sections, information from all three avenues of investigation will be required for a complete understanding of the dark matter in the universe.

Direct Detection of WIMPs

The direct detection of WIMPs is based on the elastic scattering of WIMPs and atomic nuclei. Since the kinetic energy of WIMPs is quite low, only elastic scattering is relevant. This process is commonly discussed in the context of two classes of WIMP interactions with quarks: i) scalar (spin independent) interactions which increase dramatically with the mass of the target nuclei, and which typically underpin the dominant scattering process in current experiments using heavy atoms as targets; and, ii) axial-vector (spin dependent) interactions which result from couplings to the spin content of the nucleon. The spin-dependent cross-section is proportional to the nuclear spin rather than to the number of nucleons, and so little is gained by using heavier target nuclei. Most experiments rely primarily on spin-independent scattering to obtain a signal.

The rate of signal events depends on the target mass, the cross-section and on the WIMP flux, which is derived from the local halo density and velocity distribution. For plausible WIMP cross-sections and masses (ten's to hundred's of GeV), rates could be as low as a few events per ton of detector mass, per year. Clearly, a very large detector mass is required in order to produce an observable signal.

The kinetic energy spectrum of the recoil nucleus provides, unfortunately, no distinctive features. It falls exponentially with a mean value in the range of ten's of keV. The exact rate and the exponential fall-off depend directly on the WIMP mass and the nuclear mass.

The main sources for backgrounds to dark matter scattering are: photons, which will travel relatively far before producing energetic electrons in the detector material, beta electrons from ambient materials, and neutrons, which will scatter off nuclei in much the same way that WIMPs would. The means to suppress these sources of background vary considerably from experiment to experiment and are the key challenge for this kind of research. The photons, electrons, and neutrons come from natural radioactivity, to some extent induced by cosmic rays. Hence, WIMP detectors are placed deep underground to reduce the cosmic ray flux as much as possible. Active or passive shielding of the detector further reduces false events coming from cosmic rays. The detector material must be especially "clean" in the sense that radioactive elements are kept to a minimum. These measures alone are not enough to allow the direct detection of WIMPs, however, so an *active* suppression of backgrounds is necessary. In principle, a single event will be measured by two or more means, and the observables used to distinguish genuine WIMP-nucleus scattering events from backgrounds. The three observables currently employed are 1) the ionization produced by the recoiling nucleus, 2) the scintillation light accompanying the ionization, and 3) the heat produced by the recoil of the crystal against the WIMP. The heat shows up as phononic excitations of the crystal. Neutron and electron recoils will have a different profile in these three observables and hence can be discriminated by measuring at least two of them.

Very small rates and small signals make this search very challenging. Current experiments are able to probe spin-independent cross-sections on the order of 10^{-42} cm^2/nucleon, which is in the upper range of theoretical predictions. Much smaller cross-sections are typically predicted in various models, so it is imperative to improve the sensitivity by large factors – indeed, by orders of magnitude. As we describe below, current efforts and plans for the next generations of experiments will probe much larger ranges of theoretical predictions, both within supersymmetric theories and beyond.

The main uncertainty in the interpretation of results from scattering measurements is the dependence of the signal rate on the local density of dark matter and on the velocity distribution of the WIMPs near the detector on earth. Depending on the assumptions about the dynamics of our galaxy, these two quantities can have very different values than those usually assumed based on a homogenous distribution of dark matter in the disk of the galaxy.

There are many different types of detector technologies that combine one or more of the above techniques, which we review now in sequence.

Solid Scintillators at Room Temperature: Some detectors such as DAMA and NAIAD employ solid scintillators at room temperature. DAMA has reported positive evidence for WIMP scattering by exploiting the annual modulation of the WIMP velocity distribution. This signature so far is unconfirmed and difficult to reconcile with the negative results of other experiments. KIMS is another crystalline detector (CsI) in Korea similar to DAMA, which will help to confirm or refute the DAMA claims.

Ge Ionization Experiments: Some relevant results have been obtained recently using extremely pure Ge crystals. The most recent of such experiments was HDMS. Other planned experiments, which focus on neutrino-less double beta decay, such as GERDA and Majorana are based on large high-purity Ge crystals with very good shielding. If such experiments can achieve several orders of magnitude in background reduction, they will become competitive for dark matter searches.

Crystal Cryogenic Experiments: The leading experiments at the present time use large Ge or Si crystalline masses cooled to sub-Kelvin temperatures. The extremely low temperature greatly suppresses thermal noise, making it possible for a WIMP signal to stand out above the noise. A primary example of these cryogenic detectors is CDMS, which is installed in the Soudan mine. Cryogenic detectors such as CDMS benefit from a relatively low threshold for detection, typically about 10 keV, and an excellent energy resolution, better than 1% at 10 keV. They are able to identify electron backgrounds on an event-by-event basis, by measuring the charge released by the recoiling nucleus, and the heat absorbed by the crystal. Neutron and WIMP events have less charge for a given amount of heat and it is critical to minimize the ambient neutron background. The heat shows up as phonons, which are measured by superconducting transition edge sensors patterned directly on the crystal surfaces using photolithography. The detectors measure athermal phonons, which can be used to localize the event. Events that are too close to the surface of the crystal are rejected since the measured charge may be atypically small. An important feature of the cryogenic-detector program is the use of both Ge and Si crystals. Neutron backgrounds have a relatively high rate in Si, and WIMP signals have a relatively higher rate in Ge. By comparing the event rates detected in the two materials, one can demonstrate consistency with a WIMP hypothesis as opposed to neutron backgrounds alone.

Edelweiss is another cryogenic experiment run by a collaboration of British and French institutions. The detector uses Ge crystals cooled to 20 mK, installed in the underground laboratory at Modane. Like CDMS, Edelweiss measures both the ionization and the heat for each scattering event.

The CRESST experiment is run by a collaboration of German and British institutions and the Gran Sasso National Laboratory. It uses crystals made of $CaWO_4$ cooled to milli-Kelvin temperatures, which provide a measurement of the ionization and of the scintillation light produced in a scattering event.

The results obtained from these three cryogenic detectors are among the most constraining currently available (see Fig. 1). The competition among the collaborations is strong and it is difficult to say which experiment will ultimately achieve the best sensitivity, though presently CDMS holds the most stringent limits. All three collaborations are operating their second or third-generation detector, and aim for a sensitivity tending toward 10^{-8} pb = 10^{-44} cm^2.

These experiments do have some sensitivity to spin-dependent cross sections. Although Ge and Si are composed mainly of spinless isotopes, which offer no sensitivity to spin-dependent interactions, each also contains an isotope of non-zero nuclear spin, (^{73}Ge and ^{29}Si) containing a single unpaired neutron which provides some limited sensitivity. At present, CDMS has probed spin-dependent interactions down to 10^{-36} to 10^{-37} cm^2/nucleon.

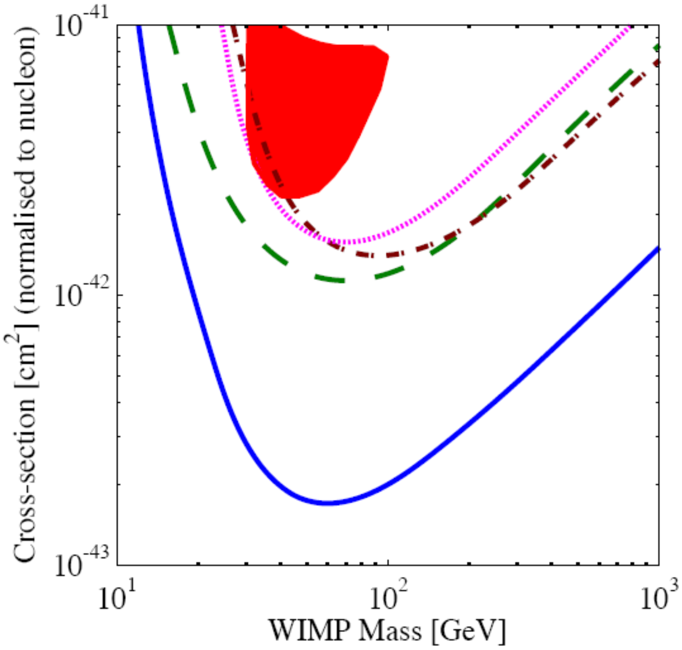


Figure 1: Direct detection limits (spin-independent) as of 2006. The solid, long-dashed, dot-dashed and dotted lines correspond to CDMS, ZEPLIN, Edelweiss and CRESST, respectively. The dark red blob is the DAMA signal region. The figure is based on plotting tools available from http://dmtools.berkeley.edu/limitplots

Liquid Noble Gas Detectors: Newer approaches use detectors based on large volumes of liquified noble gases, which are intrinsic scintillators. An external electric field can be imposed, which allows ionization electrons to be collected on electrodes and detected. This rapidly evolving technology is strongly motivated by the possibility of scaling to large volumes, and is aimed ultimately at multi-ton detector masses. The design strikes a compromise between background rejection and detector mass. Signal shape and timing provide a means to reject background. There is active R&D to improve the understanding of ionization and scintillation light yields resulting from the nuclear recoil. For detectors with TPC's, the three-dimensional localization of the signal can be used to define a fiducial volume separated from the detector edges.

Several programs around the world are very active, including XMASS in Japan, the ZEPLIN (with U.S. participation), and the XENON project operating in the Gran Sasso Lab (under U.S. leadership). These all use liquid

Xenon. Other groups are exploring the use of other noble liquids: WARP and ArDM (both in Europe) and DEAP use Argon, while the mini-CLEAN experiment is based on Neon, but will also have the ability to use liquid Argon.

There is no consensus as to which noble gas material will be optimal, and each choice has strengths and weaknesses. Kinematics, nuclear coherence, form factor suppression, the existence of unstable isotopes, methods of purification, characteristics of the scintillation signal, density, and cost are among the relevant issues. In addition, there are challenges that are common to all liquid detectors, such as passive and active shielding to reduce background events from surrounding rock, development of low-background phototubes, and liquid phase purification techniques. An emerging consortium of several experiments offers the hope of solving common problems efficiently.

At this point the liquid noble gas collaborations are mainly focused on fundamental R&D with small- and medium-scale prototypes, and so far only ZEPLIN I and WARP have reported results for dark matter searches. Their limits are about a factor of 10 and 3 larger than the 2006 CDMS limit, respectively.

Superheated Liquid Detector: One experiment, COUPP, uses a modern bubble-chamber technology. Background rejection is passive but very effective: only nuclear recoil tracks with a high ionization density will suffice to nucleate droplets in the super-heated heavy liquid. Neutron scatters tend to leave several bubbles while a WIMP scatter should leave at most one. A large target mass is relatively easy to achieve. Operational stability needs to be established. A similar experiment is PICASSO, which suspends the super-heated liquid in a gel.

Directional Experiments: Over the longer term, particularly if WIMPs are discovered, the emphasis will be on obtaining some measure of the direction of the incoming WIMP particle. This will help to provide an understanding of the local WIMP density and velocity distribution, which are crucial for relating rates to cross-sections. The velocity distribution should change noticeably throughout the year due to changes in the earth's velocity with respect to the ambient WIMP density. There will also be a day/night variation. The DRIFT experiment has an ambitious design that could achieve a directional measurement. It uses a large volume of low-pressure gas

(CS_2), which allows the nuclear recoil to leave an extended ionization track. The plan is to detect such tracks using negative-ion TPC (time projection chamber) technology; specific ionization, *dE/dx*, can also be measured. Several modules have been installed in the Boulby Mine in the U.K. and are taking data.

Current Status and Future Possibilities of Direct WIMP Searches

The sensitivity of currently planned experiments, liquid or solid, are projected to be better than the current best cross-section limits by two or three orders of magnitude, reaching about 10^{-46} cm^2/nucleon for the largest detectors envisioned. This large gain will allow probing a large range of the theoretical predictions, and is thought to be achievable within a period of about a decade. This next generation of experiments relies on large masses, on the order of 1 ton, and on improvements in background rejection. Depth is a crucial factor to reduce the flux of prompt neutrons from cosmic ray interactions. The proposed U.S. underground laboratory, DUSEL, would provide an ideal platform for housing such large dark matter experiments.

Cryogenic Solid Detectors

The CDMS Collaboration has analyzed data from two detector towers with a combined mass of 2kg installed in the Soudan mine. They conducted a blind search to avoid biases, and one event passed their selection criteria. Their result is better than originally projected, thanks to the rejection of surface events using athermal phonons, and the limit of 10^{-43} cm^2/nucleon is the lowest to date. The next run will involve five detector towers, with which they should reach a sensitivity of 10^{-44} cm^2/nucleon. Meanwhile the collaboration is proposing SuperCDMS, a phased sequence of upgrades to 25kg, 150kg, and eventually 1000kg, ultimately achieving sensitivity better than 10^{-46} cm^2/nucleon. Proposals for the 25kg detector have been submitted to NSF and DOE, and SNOLAB has strongly endorsed the proposal and asked the collaboration to define its infrastructure needs. If approved, operations would take place in 2010-2012, and the projected sensitivity for 25kg is 10^{-45} cm^2/nucleon.

Edelweiss II, operating in the Modane mine, should be able to reach a sensitivity of 10^{-44} cm^2/nucleon. CRESST is running in Gran Sasso and will also be sensitive at the 10^{-44} cm^2/nucleon level. Their low threshold for photons is a clear advantage, but detecting the very low scintillation light yield expected for WIMP signals is challenging. There are plans to combine Edelweiss and CRESST to form a large-scale (100-1000 kg) array to be called EURECA, aiming for a sensitivity of 10^{-46} cm^2/nucleon.

Liquid Noble Gas Technologies

The XENON 10 kg active target at Gran Sasso is planned to start the first physics run in 2006 and continue through 2007. Its sensitivity goal is10^{-44} cm^2/nucleon for WIMP masses above 100 GeV. A XENON 100 kg detector is in the design stage, and construction is expected in 2007/08, with the first physics run in 2008. A one-ton detector consisting of ten 100kg modules is envisioned, and expected to have a reach of 10^{-46} cm^2/nucleon. ZEPLIN II is operating underground at the Boulby mine. The detector is fully operational and physics data taking and data analysis are in progress. After the completion of the first physics run, they will continue with R&D to push the limits of performance of ZEPLIN II and to optimize the technology for the one-ton detector, ZEPLIN IV, to achieve cross section sensitivity of the order of 10^{-46} cm^2/nucleon.

WARP has operated a 3.8 kg liquid Argon prototype at Gran Sasso in 2004, and the WARP 140 kg (100 liters liquid Argon) detector is under construction with commissioning expected by the end of 2006. The 140 kg of sensitive detector volume is inside a much larger volume consisting of 9 tons of liquid Argon, used as an active veto. This design allows expansion to a 1400 kg detector, with a projected sensitivity of 10^{-46} cm^2/nucleon. The other European liquid Argon collaboration, ArDM, has similar goals, and is currently constructing a one-ton prototype at CERN.

The mini-CLEAN experiment is under construction, with 100 kg of Neon to be deployed underground in 2008. There is also a plan to expand the detector to hold 100 metric tons of liquid Neon in the near future. DEAP-1, a 10 kg prototype, is under construction and planned for deployment at SNOLAB, with a proposal to scale to a larger experiment in the future.

Direct Detection of Axions

Microwave cavity experiments exploit the resonant conversion of cosmologically produced axions to electromagnetic excitations in a cavity filled with a strong magnetic field. In this process, an incoming axion collides with a virtual photon, producing a real photon whose frequency is proportional to the axion mass. To be detectable, the frequency of the produced photon must match the resonant frequency of the cavity. Since $m_a \sim 4\mu eV$ corresponds to $f \sim 1$ GHz, these experiments push the envelope of high frequency, low noise technology.

Figure 2 summarizes the present and projected experimental situation. The region of masses and coupling strength compatible with a solution to the strong CP problem lies between the model lines labeled DFSZ and KFVZ, while the cosmologically plausible region is within the mass range from 10^{-6} to 10^{-3} eV, where microwave cavity experiments will dominate. The mass region between 0.1 and 1 meV, and couplings smaller than 10^{-11}GeV^{-1}, however, are not within the reach of planned experiments.

The ADMX experiment, sited at LLNL, uses a detector based on microwave amplifiers and has reported results from several years of initial operations. A sequence of upgrades is envisioned. The first upgrade, which involves conversion from HEMT amplifiers to SQUID amplifiers, is now underway. With improved refrigeration it is anticipated to cover the region $m_a \sim$ 2-20 μeV by 2011. Covering the next decade of axion mass requires substantial further development in both resonators and amplifiers, and is presently in a conceptual stage.

The CARRACK detector at Kyoto University uses a detector based on the principle that the photon of frequency $f=m_a$ can induce atomic transitions in a beam of atoms whose levels have been tuned by the Stark effect. Subsequent "selective ionization" of the excited atoms yields electrons that can be detected by standard techniques. This system, which may be loosely characterized as a "phototube for microwave frequencies", can in principle reach much lower noise temperatures than the microwave amplifiers, but is far more delicate and still in the developmental phase. The results of a search in a mass range close to 10μeV were reported in 2001.

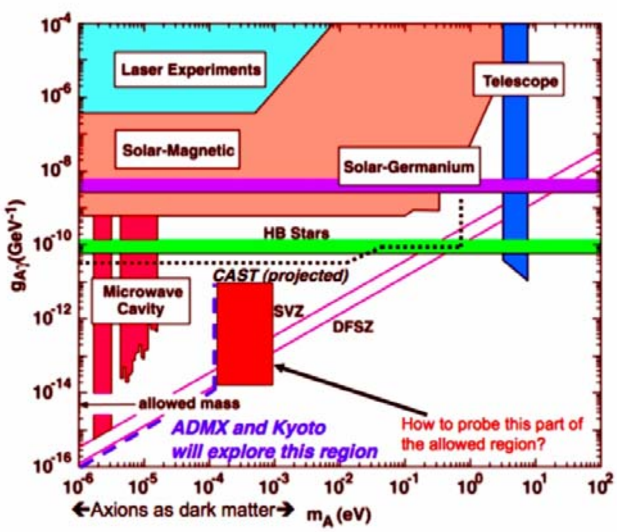


Figure 2. Reach of various axion searches in the plane of effective $a\gamma\gamma$ coupling strength (GeV^{-1}) versus axion mass (eV). See text for discussion. Ref: C. Hagmann, K. van Bibber, L.J. Rosenberg, Phys. Lett. B592, 1 (2004).

Indirect Detection of WIMPs

Indirect detection of WIMPs relies on detecting the end products of a cosmic WIMP-WIMP annihilation process. These annihilations will be more frequent where WIMPs are concentrated, namely, in the center of the galaxy, in the center of the sun, etc. There are basically four avenues for detecting cosmic WIMP-WIMP annihilations through the resulting particles produced: high-energy gamma rays, neutrinos, anti-matter, and synchrotron radiation. The idea is to observe an excess of one or more of these particles above ordinary astrophysical sources. Photons and neutrinos have an important advantage in that they point back to their source, on the other hand, positrons, anti-protons and anti-deuterons may be easier to detect and measure. Predictions for these signals are rather uncertain as they require an understanding of the concentration of WIMPs, of the end-products of their annihilation (which will vary from model to model), and of the astrophysical background. A signal in anti-matter or continuum gamma rays would likely have to be confirmed by another channel, or by direct-detection experiments, in order to be certain that WIMP annihilation is indeed the source.

At present a very promising technique seems to be the detection of high-energy gamma rays emanating from sources such as dwarf spheroidal galaxies, external galaxies, or the Milky Way halo. Experiments installed on the earth's surface identify high-energy gamma rays using the Cerenkov light produced when they shower in the atmosphere. The separation of such showers from ordinary cosmic rays is accomplished on a statistical basis using the structure of the shower. These experiments have identified several bona fide astrophysical sources of high-energy gamma rays, and the next generation might have the sensitivity to see WIMP-WIMP annihilations. HESS and VERITAS, MAGIC and CANGAROO-III are leading the next generation of these experiments.

Experiments based in space to detect gamma rays have obvious advantages over those on the earth, since they can detect the gamma rays directly rather than through a shower in the atmosphere. This results in better charged particle rejection. The first such experiment, EGRET, observed an excess of gamma rays in an interesting range of energies during its mission. The next-generation apparatus, GLAST, has greatly improved capabilities and will be able to confirm the excess if it is real, and give a much sharper view of its energy dependence. The most striking signal for WIMP annihilation would be the essentially monoenergetic lines expected from the sub-leading processes WIMP+WIMP → 2 gammas or Z+gamma. The predictions for such processes are highly model-dependent.

Since neutrinos are not readily absorbed, they are able to escape the center of the sun where WIMP annihilations might be taking place, making neutrinos a potentially good signature for this process. Furthermore, neutrinos are able to penetrate the earth and reach large target masses below the earth’s surface. Super-Kamiokande has recently placed interesting limits on this kind of signal.

Neutrinos are very difficult to detect due to their weak interaction with matter and hence neutrino detectors require very large targets. The best approach to detecting high-energy neutrinos utilizes either large natural bodies of water or ice. The currently running Amanda experiment looks for muon tracks produced when a cosmic neutrino interacts in the highly transparent ice in the Antarctic. The Cerenkov cone produced by the muon track provides a clear signature, which can be imaged both in terms of amplitudes and the spatial evolution in time. This allows the interacting neutrino direction to be determined, allowing a verification that a potential neutrino excess comes from an expected concentration of WIMPs, such as might be the case in the sun. The next generation experiment, after Amanda, is IceCube, which can look at a much larger volume of ice and will be completed around the end of this decade. The rate at which WIMPs are captured in the sun depends on the nature of the WIMP interaction with matter. Existing bounds from direct detection experiments, still allow significant rates in IceCube, given the variation among models.

The first searches for positron and anti-proton signals were made with balloon-borne apparatus. The HEAT experiment observed an interesting excess in the positron spectrum in the mid-1990's, and similar results have been obtained by the BESS and CAPRICE experiments. PAMELA, which is a small spectrometer carried into space on a satellite launched earlier this year, should improve our understanding of these signals. The AMS-02 experiment, which is meant to operate on the international space station, would provide very accurate and detailed spectra for positrons, anti-protons and anti-deuterons. The low backgrounds in the anti-deuteron signal make this channel particularly attractive. It could result in an incisive sensitivity to WIMP annihilation.

An important feature of anti-matter experiments in space is their ability to measure energy spectra. In fact, it might be possible to use the spectra to distinguish among models. However, the predictions for signals in these experiments are susceptible to uncertainties in models for the dark matter halo, magnetic field distributions, and radiation backgrounds.

If WIMPs annihilate and produce high-energy charged particles, then these charged particles will emit synchrotron radiation as they pass through galactic magnetic fields. One can look for an excess in the spectrum of radio waves from this process; and this has been done. However, the interpretation of these results is even more problematic than signals of gamma rays or anti-matter, since in addition to the large uncertainties from the assumed halo profile, and from the modeling of the end products of WIMP annihilation, there are large uncertainties in the ambient magnetic fields required for synchrotron radiation.

The indirect-detection experiments can be viewed as a next step in astronomical investigations of dark matter. However, they are not a substitute for the direct detection of dark matter particles on earth, nor for the production of these particles in a controlled manner in collider experiments. A positive signal might indicate some sort of enhancement, such as a large spin-dependent WIMP-nucleon cross-section or a final state rich in energetic neutrinos. This information would complement the rate measurements one anticipates from the direct-search experiments, and play an important role in understanding the particle nature of dark matter. Beyond this, they provide an important tie between the particle physics investigations and the astrophysical measurements of dark matter.

What can Colliders say about Dark Matter?

At high-energy colliders, it is possible to search for dark matter in the form of missing energy signals. Many models of electroweak symmetry breaking predict new heavy colored particles. Such particles may be produced copiously at hadron colliders, such as the Tevatron and eventually the LHC, depending on their mass. If produced, they will decay to lighter particles and ultimately to the dark matter particle. The relevant signatures to search for such particle cascades at hadron colliders are then missing energy from the dark matter particle plus jets and/or leptons.

The Tevatron experiments have recorded more than 1 fb^{-1} of data and expect to collect about 4 to 8 times more in the next few years. Searches for new particles predicted by, for example, supersymmetry, extra-dimensions and Little Higgs models are underway. Searches, through many different event topologies allow for the discovery of missing energy signals that could be interpreted as the production of dark matter particles. Such large missing energy signals could probe for DM particles with masses in the few tens of GeV up to about 200 GeV.

The LHC should start taking data at high energies in 2008. With seven times the energy and orders of magnitude higher luminosity than the Tevatron, it will have much greater sensitivity to new physics processes. The LHC experiments will search for evidence for dark matter particles in events with large missing energy plus multiple jets and/or leptons, for example as expected in the cascades from heavy colored particles like gluinos and squarks. There have been several studies in the literature exploring the potential of the LHC in many different scenarios, which accommodate the measured cold dark matter relic density. If the new colored particles are within the reach of LHC, namely their masses are below a few TeV, it is likely that the LHC will find evidence for dark matter particles. In the simplest models analyzed this corresponds to WIMP masses of up to a few hundred GeV. In scenarios in which these colored particles are too heavy to be produced at the LHC, the direct production of other new, weakly interacting particles, which ultimately decay into the DM candidate, remains as a possible search channel. In these cases, multiple leptonic signals plus missing energy are the most robust option against the copious QCD backgrounds.

In many case studies it appears that the properties of the new particles can be measured with only modest precision at the LHC and hence it will be difficult to definitively establish the identity of the dark matter particle. Also, for some theoretical scenarios it can be challenging to search for dark matter at the LHC because of properties of the spectrum of colored particles that can be produced. Two interesting examples are: i) "focus point" supersymmetry, where most colored interacting particles, except the gluinos, are out of the reach of the LHC; ii) scenarios of electroweak baryogenesis, where the only light colored particles within the reach of the LHC are the scalar top superpartner with mass below the top quark mass and, possibly, the gluinos and scalar bottom quarks, with the lightest neutralino mass bounded to be below about 150 GeV. For focus point supersymmetry kinematic decay distributions can provide information about mass differences, although independent determination of all masses seems not to be possible. In the case of electroweak baryogenesis, although a new physics signal is likely to be observed, and gluino cascades can provide partial information about stop-lightest neutralino mass differences, the identification of other new weakly interacting particles relevant for understanding the composition of the dark matter candidate seems difficult at the LHC.

The ILC is a proposed high-energy e^+e^- collider with strong international support, which is planned to operate initially at a center-of-mass energy of 500 GeV with a possible upgrade to 1 TeV. In the case that the weakly interacting particles associated with the dark matter particle are within the energy reach of the ILC, these particles can be produced and their masses and couplings measured with high precision using the large missing energy signature. The availability of polarized beams and the ability to make precise measurements of cross-sections is particularly useful. The ILC also can give direct information on particle masses from the kinematic distributions of decay products and from the reconstruction of the excitation curves by measuring production processes at various center of mass energies near threshold. In the case of heavy new particles that are weakly interacting, the above measurements can constrain the heavy masses and couplings relevant to the computation of the dark matter relic density.

In the challenging supersymmetry examples presented above, e.g. the focus point and electroweak baryogenesis scenarios, the lightest chargino and lighter neutralino particles have masses below a few hundred GeV, within the ILC reach. In such cases, the ILC can provide measurements of the relevant supersymmetric parameters with sufficient accuracy that the relic density can be computed to the per cent level and compared with cosmological measurements of similar precision.

Connection of Collider Physics and Cosmology

The direct and indirect-detection experiments may well produce clear signals for dark matter particles. However, the only way to gain knowledge of the detailed properties of dark matter particles is to produce them in collider experiments. The combination of these techniques can often be used in tandem to set limits if nothing is found or develop a physics picture using all information as discoveries are made.

The Tevatron experiments might detect the production of WIMPs, if they are sufficiently light, but they will not be able to directly measure the WIMP properties. However, results from the Tevatron and from direct-detection experiments can be used to extract important information on the supersymmetric parameter spectrum. In particular, it is interesting to note that the spin-independent elastic scattering cross-sections receive important contributions from the t-channel exchange of Higgs-like particles, which might be observed or excluded in the direct searches for extended SM Higgs sectors at the Tevatron. Fig. 3 shows contours in the (μ, M_2) plane derived from the non-observation of WIMP scattering in CDMS. The green contour corresponds to the range of parameters for which the Tevatron cannot see the Higgs bosons (A,H) in the inclusive tau channel, given the current limits from CDMS. If no signal is seen by CDMS up to 2007, then the black region applies. If the lightest neutralino is the correct dark matter particle, then one must have that $M_2 < 2|\mu|$. Thus the discovery of Higgs particles at the Tevatron but the non-observation of WIMP scattering by CDMS would indicate that $|\mu|$ is large and the lightest neutralino has a very small higgsino component. Similar studies can be done for the LHC and upgraded versions of the CDMS experiment.

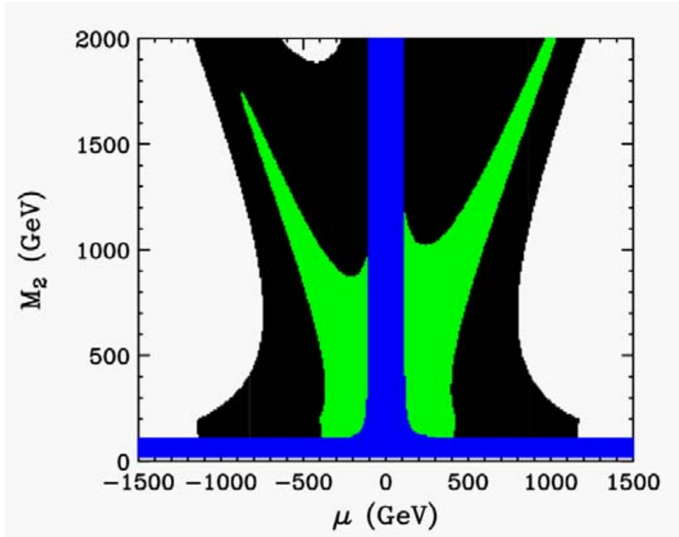


Figure 3: The regions in the Chargino-Higgsino mass parameter plane in which the possibility of discovering heavy, neutral MSSM Higgs boson at the Tevatron (4 fb-1 per experiment) through proton anti-proton → A/H X → tau+ tau- X is excluded due to current CDMS limits (light shaded/green) and the projected 2007 CDMS limits (black). The (blue) shaded region is excluded by LEP searches on charginos. [ref. M. Carena, D. Hooper, P. Skands, hep-ph/0603180]

Since a complete understanding of dark matter requires the ability to calculate the relic density and obtain a value consistent with astronomical observations, we must ask what is required for such a calculation. The detailed answer will vary from model to model, but in general one needs the WIMP mass, its spin and coupling to other particles, including information about the other particle masses.

As emphasized above, in most scenarios, the LHC will not give a precise picture of the identity of the WIMP. A quantitative and precise measurement of its properties will require the ILC. Thus we must assess how well the WIMP properties might be known once ILC data is available.

A clear illustration of the impact of ILC measurements is given in Fig. 4, which shows the relic density of neutralinos that would be computed using measurements made at the LHC and the ILC, respectively. In the case of the ILC, the accuracy on the relic abundance is better/comparable to the WMAP/prospective Planck measurements shown by the horizontal bands.

In this context the particle physics measurements will combine with the astrophysical measurements to provide a detailed picture of the dark matter.

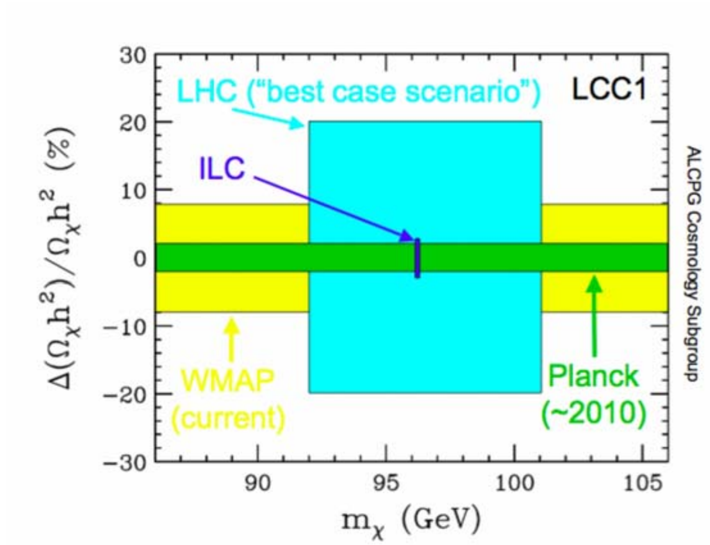


Figure 4: Accuracy in the dark matter relic abundance determination using measurements possible at the LHC and the ILC, respectively, for the supersymmetric benchmark scenario LCC1. Also shown by the light (yellow) and dark (green) horizontal bands are the measurements from WMAP and prospective Planck. Figure from a study by the ALCPG Cosmology Subgroup.

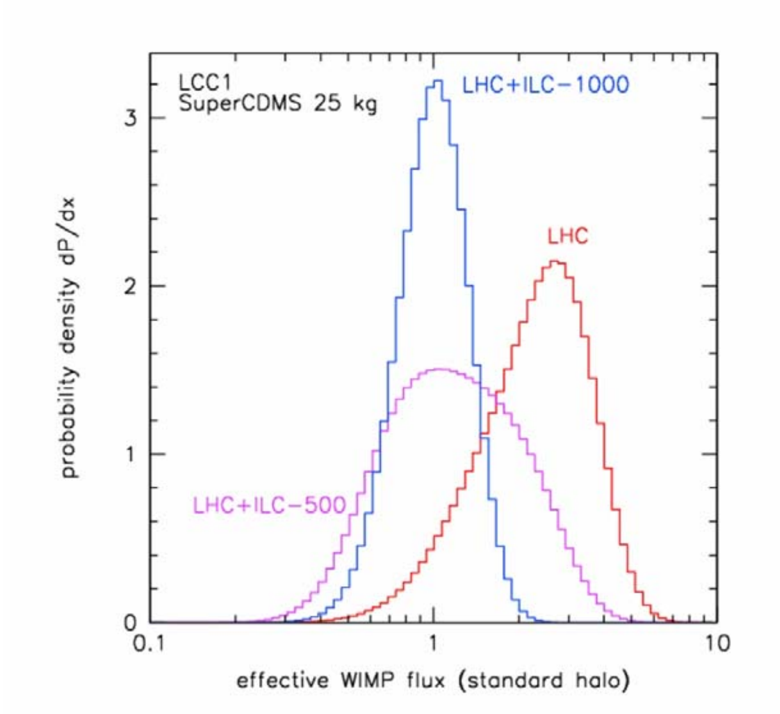


Figure 5. Effective WIMP fluxes inferred on the basis of the combination of data from SuperCDMS and the collider experiments. Here, "effective WIMP flux" means the ratio of the local flux to that expected in a reference halo model. Two versions of the ILC are shown, at 500 GeV and 1 TeV. [ref. E. Baltz, M. Battaglia, M. Peskin and T. Wizansky, hep-ph/0602187].

Information from the full set of experiments can also be used to check our local WIMP flux as illustrated by the following example. Studies show that if the LHC experiments observe events with large missing energy due to dark matter particles, then the masses of the produced WIMPs can be measured to an accuracy of about 10%. Such measurements are based on kinematic distributions of the byproducts of the decays of associated colored particles (squarks). Experiments at the ILC could measure the WIMP mass to 1% or even 0.1% depending on the specific scenario. One can compare this to the proposed direct-detection experiment SuperCDMS, which might measure the WIMP mass to 20-30% from the dependence of the recoil energy distribution on the WIMP mass, with an assumption that the velocity distribution is known to 10%. More realistically, such calculations must rely on assumptions about the local halo density. So, if the results of direct-detection experiments do not agree with the mass measurements at the LHC (and later, the ILC), this could point toward problems in our understanding of the local halo density and the velocity distribution of WIMPs. As an illustration, Fig. 5 shows how an effective WIMP flux can be inferred from the rate measurement in an experiment like SuperCDMS and the WIMP-nuclear elastic spin-independent cross-sections computed on the basis of measurements from the LHC and ILC. If, in fact, one is able to study events with missing energy at the LHC and ILC, but there is no signal from direct-detection experiments, then a strong upper bound on the local WIMP density will be obtained.

Recommendations regarding the dark matter program

The dark matter experimental program has many directions and we can expect to learn a great deal in the next few years from experiments that can directly produce dark matter particles, the LHC and eventually the ILC, a suite of possible scattering experiments, and searches for dark matter annihilation. The LHC and ILC have been discussed in the previous section of this report and the experiments that can best search for annihilation are nearing completion. We provide recommendations here for the scattering experiments only.

Recommendation

- The CDMS experiment is mature, and is now dealing with the technical issues associated with advancing an already well-developed, well-understood technique into larger-scale implementation. Assuming that the experiment continues to demonstrate adequate background rejection, we recommend full support for the 25kg implementation. This is part of a vigorous international program, which will likely profit from cross checks between different techniques.

Experiments searching for axionic cold dark matter are important since they have no counterparts in accelerator based experiments, and are likely to be the only way axions will be observed if they exist. The ADMX experiment offers essentially unique capabilities and any signal would be a triumph not only for revealing the nature of dark matter but also for understanding the strong CP problem. Coverage of the full range of plausible parameter space poses serious technological challenges, but the first order of magnitude is within reach and plans for the second order of magnitude are taking shape.

Recommendation

- The ADMX axion search experiment is unique and well advanced. We recommend strong support over the next five years.

The readiness for a large investment in WIMP detection at present is not the same for all technologies. However, research and development is intense and rapid progress is being made in many areas. A clearer picture of the capabilities of the various techniques should be available around the end of this decade. Ultimately, the ability to scale a given technology to the ton-scale will determine whether an experiment is ready for a major investment. The field will be ready for support at the $100 M level when the following conditions are satisfied. First, at least one technology must have demonstrably reached a mature stage. This means that fundamental R&D in the detector technology is complete, the physics of the signal generation is understood, background limitations are understood, and the experiment is capable of long term, unattended operation. Supporting two different types of experiments to this stage, in a worldwide context, would be very valuable and permit a comparison of alternatives. Appropriate technical reviews must have been conducted to establish readiness.

Recommendations

- We recommend strong support of R&D toward a future dark matter experiment that will extend the cross section reach by a factor of 100 over the near-term program. Priority should be given to techniques that could be ready for construction early in the next decade. The prospective underground U.S. lab, DUSEL, would provide an ideal location for such an experiment. Where appropriate, we endorse the concept of a consortium to study common issues and share the R&D effort.
- The field has a number of innovative projects that are interesting but are at a very early stage of development. These are worthy of some R&D support to continue development of such techniques.
- We anticipate that the DMSAG will provide much detailed guidance about the future directions for the dark matter program.

3. Dark Energy

In Chapter III we have discussed in detail the current situation in the area of dark energy and the scientific opportunities in that field today. For the reader's convenience we briefly repeat here the most salient points.

Over the last several years observations of distant supernovae, galaxies and clusters of galaxies, and the cosmic microwave background, provided conclusive evidence that the cosmic expansion of our universe is accelerating. The data are consistent with a standard cosmological paradigm augmented by the postulate that 70% of the universe is composed of mysterious "dark energy" that drives the acceleration.

Dark energy challenges our understanding of fundamental physics; different ideas have been put forth but none of them are wholly satisfactory. The classic explanation, consistent with the data, is to set Einstein's famous cosmological constant to a small positive value. The required value, however, would be 120 orders of magnitude smaller than its "natural" value in quantum field theories. Resolution of this discrepancy poses a serious challenge to our understanding of quantum gravity and particle physics. One possibility is that there exists an as yet undiscovered symmetry of nature which forces the value of the cosmological constant to be precisely zero, requiring then an alternative explanation for the observed dark energy phenomenon.

One such option involves postulating a new form of matter (often called "quintessence"), which happens to be in a novel state today that can drive cosmic acceleration. The parameter values required for a field theoretic description of quintessence are as hard to explain as those required for the cosmological constant option. For example, in many cases the mass of the quintessence particle is 35 orders of magnitude smaller than the electron mass leading to major complications when quantum corrections are considered.

Given these great challenges, even radical options are being seriously considered. Arguments have been made that exotic "axion" particles are distorting the observations, and serious consideration is being given to abandoning Einstein's theory of gravity in order to find a better account of the observations.

With any of these options, the implications for fundamental physics are profound. The acceleration of the Universe, along with dark matter, is the observed phenomenon that most directly demonstrates that our fundamental theories of particles and gravity are either incorrect or incomplete.

Understanding Dark Energy

The dark energy is described by an equation of state that is different from all the other components of the universe (baryons and electrons, photons, neutrinos, and dark matter). Progress in this area will be attained through a wide-range of possible new observations that can measure the equation of state in the present and past history of the universe.

The goals of a dark energy observational program may be reached through measurement of the expansion history of the universe (traditionally measured by the dependence on redshift of luminosity distance, angular-diameter distance, expansion rate and volume element), and through measurement of the growth rate of structure (suppressed during epochs when the dark energy dominates). All of these measurements of dark energy properties can be expressed in terms of the equation of state at different redshifts. If the expansion is due instead to a failure of general relativity, this could be revealed by finding discrepancies between the equation of state inferred from different types of data.

The proposed observational program focuses on four techniques, which allow especially good tests of the nature of the dark energy. They are:

1) Baryon acoustic oscillations as observed in large-scale surveys of the spatial distribution of galaxies. This technique is sensitive to dark energy through its effect on the angular-diameter distance versus redshift and on the time evolution of the expansion rate.
2) Galaxy cluster surveys, which measure the spatial density and distribution of galaxy clusters. This technique is sensitive to dark energy through its effect on the angular-diameter distance versus redshift, the time evolution of the expansion rate, and the growth rate of structure.
3) Supernova surveys using Type 1a supernovae as standard candles to determine the luminosity distance versus redshift, which is directly affected by the dark energy. Currently this is the most powerful and best-proven technique.

4) Weak lensing surveys, which measure the distortion of background images due to bending of light as it passes by galaxies or clusters of galaxies. This technique is sensitive to dark energy through its effect on the angular-diameter distance versus redshift relation and on the growth rate of structure.

Many of these techniques are rather new and not at the same state of maturity. The most incisive future measurements of dark energy will employ a number of techniques whose varying strengths and sensitivities, including different systematic uncertainties, will provide the greatest opportunity to reveal the nature of dark energy.

The current program to probe the nature of dark energy is staged thus allowing time to develop new ideas and new measurement techniques. The different stages are: Stage I, which represents projects completed; Stage II, ongoing projects; Stage III near-term, medium-cost projects, which combine a number of the techniques mentioned above; and ambitious Stage IV projects that are more costly.

The Dark Energy Task Force (DETF) has developed a methodology to assess the relative sensitivity of various proposed dark energy initiatives. The key challenge is to understand the behavior of the equation of state, the ratio of the pressure to energy density, $w = p/\rho$, as a function of time measured by the redshift z. The equation of state is parameterized as $w(a) = w_0 + (1 - a)w_a$, where $a = (1 + z)^{-1}$, w_0 is the value of w today and w_a characterizes the evolution of $w(a)$. Different theoretical models generally predict different values for $w(a)$. The DETF suggested figure of merit is defined as the reciprocal of the area of the error ellipse in the w_a - w_0 plane, the ellipse enclosing the 95% confidence limit in this 2D space. Different experiments can then be compared on the basis of this figure of merit.

DETF also suggests minimum thresholds for the new initiatives to be approved and funded. Thus Stage III proposals should gain at least a factor of 3 in the DETF figure of merit over what is expected to be known upon completion of the analysis of Stage II experiments. Similarly, Stage IV proposals should be designed to achieve at least a factor of 10 gain in the DETF figure of merit compared to the Stage II experiments. These figures of merit should be evaluated using jointly all the information expected to be known at the time of a given experiment. The Stage IV projects would then bring our knowledge of the dark energy equation of state to a few %

accuracy at the present time, and to about 10 or 20% in the early history of our universe.

Further Analysis

The DETF used the simple linear form $w(a) = w_0 + w_a(1-a)$ for the equation of state to model the dark energy properties. This leads to a two dimensional space of distinct dark energy models parameterized by w_0 and w_a. The DETF figure of merit is the inverse area of the error ellipses in the w_0-w_a space, which measures the degree to which these quantities are constrained by a given data model.

The DETF has calculated the factor by which the figure of merit improves as you go from one data set to the next (expressed as ratios of DETF figures of merit). By this measure, a factor of 3-4 improvement in the figure of merit when going from Stage III to Stage IV (as recommended by the DETF) data appears to be a realistic goal.

The DETF parameterization provides a simple, robust figure of merit for comparing the relative discriminating power of different techniques. In this section, we seek to extend (but not replace) the DETF FoM by considering the question of whether this parameterization gives a full accounting of the impact of a Stage IV program. Phase I on our dark energy roadmap (where further technical studies are done on the Stage IV projects) provides the opportunity to pursue this question in a number of ways. Based on work that is planned or currently underway, we anticipate that by the time the decision point comes at the end of Phase I we will have considerably greater information about the power of Stage IV data to discriminate among specific dark energy models (such as scalar field models or modified gravity models), as well as a better understanding of its impact on model independent parameterization spaces such as the w_0-w_a space used by the DETF.

As an illustration of the sort of insights we expect in future, we quote briefly from work that recently appeared as *astro-ph/0608269*. The authors consider a parameterization of $w(a)$ in which w takes on independent constant values in “bins” which are linearly spaced in a. In this picture the parameters are the constant values of w in each bin. The authors used nine of these parameters to consider a much larger class of functions $w(a)$ than are possible in the w_0-w_a space. By doing so the authors argue that a given experiment has more of a chance to prove what it can do. They conclude that the better Stage IV projects measure at least four parameters as well as they measure w_0 and w_a in the two dimensional parameterization.

A figure of merit similar to the one used by the DETF (given by the inverse area constrained in the nine dimensional parameter space) shows a much more dramatic impact for Stage IV data in this larger parameter space. A side-by-side comparison is given in Figure 1. In this figure B = Baryon Acoustic Oscillation data, W = Weak Lensing data and S = Supernova data. The other labels refer to specific model experiments as detailed in the DETF report. Based on these results the authors of astro-ph/0608269 argue that the DETF significantly underestimated the relative importance of Stage IV versus Stage III, but found no other important differences with the DETF results.

We expect that this and future results will clarify the full impact of Stage IV data and help address questions as to whether the cost of Stage IV projects is justified by their impact. Based on our current understanding, we are optimistic that the studies that take place during Phase 1 will greatly enhance the case for an aggressive Stage IV program.

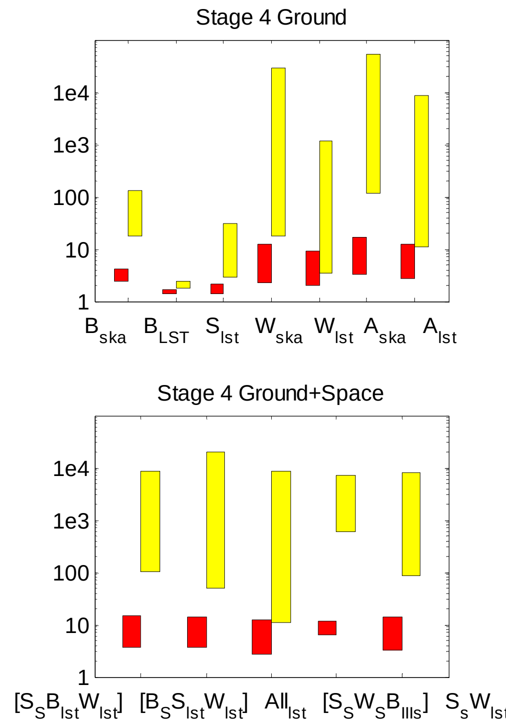


Figure 1 These plots show figures of merit calculated for DETF data models. The dark bars show the DETF figure of merit (calculated in the *w0-wa* space) and the light bars show figure of merit calculated in the higher dimensional parameter space considered in *astro-ph/0608269.* The plots are modeled after the ones in the DETF report and show the increase of the figure of merit vs Stage II. The vertical extent of the bars reflects various uncertainties in the predictions. For the better data models the new figure of merit shows at least an order of magnitude increase at Stage III and a three or four order of magnitude increase at Stage IV. Note that the Ground+Space plot shows mixtures of subsets of all possible ground-based and space-based data, resulting in some combinations that look weaker than individual Space or Ground data models.

The Experiments

The U.S. particle physics community has played a leading role in three major dark energy initiatives: the Dark Energy Survey (DES), the SuperNova/Acceleration Probe (SNAP), and the Large Synoptic Survey Telescope (LSST). The first one is a Stage III project; the last two are Stage IV projects.

The DES project is U.S. led but has collaborators from the U.K. and Spain. It is land based and proposes to use photometric techniques in four bands with a new 520 megapixel wide-field camera, covering a field of view of 3 deg squared, mounted on the existing 4m Blanco Telescope of the Cerro Tololo Inter-American Observatory (CTIO) in Chile. Photometric redshifts up to z = 1.1 should be obtained. The program plans to use all four observational techniques discussed earlier. The survey observations could start in 2009 and a five-year observational program is being planned.

The SNAP program has been planned as a joint DOE-NASA effort. It is one of several proposals submitted in response to the NASA-DOE Joint Dark Energy Mission (JDEM) space based initiative – discussed more extensively below – but the only one with significant involvement by the U.S. high-energy physics community. It is a natural follow up to the pioneering Supernova Cosmology Project that provided one of the initial evidences for an accelerating universe. SNAP will focus on two principal observational techniques: study of the redshifts and luminosities for Type 1a supernovae and observations of weak gravitational lensing. There has been interest expressed in possible collaboration by scientists in both Russia and France.

SNAP would use a 2m-diameter space telescope with a 0.7 square degree field of view. It will utilize optical and near infrared imaging and spectroscopy. SNAP would carry out detailed characterizations of 2000 Type 1a supernovae out to a red shift of 1.7. For weak lensing, a 1000 square-degree field would be covered every year.

LSST is the third dark energy initiative with significant contributions from the U.S. high-energy physics community. The expectation is that the project would be funded both by the NSF and the DOE with some additional private funds. LSST is a ground based Stage IV effort. It would use an 8.4 m telescope, to be constructed and sited at Cerro Pachon (elevation 2682 m) in Chile. LSST would be a survey instrument, able to scan 20,000 square degrees several times per month with its 3 gigapixel camera, looking at different 10 squared degrees at one time. It would reach galaxies up to a redshift of z = 3. LSST would study dark energy through baryon oscillations, supernovae, and weak lensing techniques. The expected first light is in 2013, first science observations in 2014.

LSST is one of two proposed Large Survey Telescopes (LST), the other one being the Panoramic Survey Telescope & Rapid Response System (Pan-STARRS). The latter is being developed under the leadership of the University of Hawaii's Institute for Astronomy and its design envisages employment of a large number of small mirrors with large digital cameras. It would be capable of observing the whole available sky several times per month. The initial goal of Pan-STARRS is to search for Earth approaching objects that might pose a danger in the future; the large amount of astronomical data it will obtain will be of value for a number of other astronomical studies.

At this time there are no concrete plans at the NSF for a formal comparative evaluation of LSST and Pan-STARRS. But such an evaluation is likely once the projects are more mature.

There has also been a growing interest in other countries in developing an ambitious dark energy observational program. As mentioned above, there is international interest in participating in SNAP. Japan recently approved the HSC project that aims at developing a 1.4 gigapixel camera, with a field of view of 3 square degrees, to be mounted at the prime focus of the 8.3 meter Subaru Telescope. This telescope, built by the National Astronomical Observatory in Japan, is located at Mauna Kea in Hawaii (elevation 4139 m). The survey speed, defined as the telescope aperture times the field of view, is about half that of LSST, and first light is expected in 2010. The goal will be to survey a 2000 square-degree field over a two-year period, with 150 million galaxies observed per 1000 square-degrees and reaching out to $z = 2$. This is a Stage III project, which combines studies of weak lensing, galaxy cluster counting, and baryon oscillations.

Recently, there have been some developments in the JDEM area that are relevant to the deliberations discussed in this report. On August 3, 2006, NASA announced that it had selected three proposals for advanced mission concept study for JDEM. In addition to SNAP, discussed above, NASA also selected the ADEPT and Destiny proposals for further mission concept studies. Eventual decision on a selection of a specific proposal for construction and launch (if any) would be made later, most likely in two years, when the results of these studies become available.

The Dark Energy Space Telescope (Destiny) is led by a team from the National Optical Astronomy Observatory (NOAO) and NASA's Goddard Space Flight Center and it includes a number of investigators from other institutions. The concept involves placing a 1.65 m telescope at the Earth-Sun Lagrange point, a location which would allow for stable and continuous operation of the instrument. Its two main observational techniques center on measurements of approximately 3000 Type 1a supernovae and a 1000 square degree weak lensing survey.

The third initiative selected by NASA for further mission study is the Advanced Dark Energy Physics Telescope (ADEPT), led by scientists at Johns Hopkins University and Goddard Space Flight Center. This mission would focus on using baryon acoustic oscillations as well as high-z supernovae measurements as the probe of dark energy. It would use a wide-angle telescope of about 1.3 m diameter capable of looking at about three quarters of the sky. The goal is to perform a survey of 100 million galaxies for baryon acoustic oscillation studies and also to analyze approximately 1000 high redshift supernovae.

Even though NASA is proceeding with the initial JDEM steps, it is not yet committed to follow through with this program. There are several other missions that will compete for funding and launching opportunities: the gravitational wave detector LISA, the X-ray observatory Constellation-X, the Cosmic Inflation Probe and the Black Hole Finder. Accordingly, there is some interest among the SNAP proponents to investigate the possibility to proceed with the project without NASA involvement. Clearly that would require utilizing launching facilities outside of U.S. and hence a significantly enlarged international collaboration. The decision to go forward in the near-term with one of the five possible NASA projects is expected in about one year. If JDEM is selected it could begin construction in FY09 with a launch as early as 2013.

For completeness one should mention that there is also a third Stage IV initiative on the table (in addition to JDEM and LST), but one that is in the astronomy domain without any significant U.S. high-energy physics involvement. That initiative is the Square Kilometer Array (SKA), a proposed international effort to build a 1 square kilometer radio telescope array in a frequency range of 100 Mhz-25 Ghz, that might begin science with a partial array in 2014. Several dark energy experiments are planned for the SKA: a neutral hydrogen survey of about 10^9 galaxies for the study of baryon oscillations; shear statistics for about 10^{10} continuum detected galaxies for weak lensing; and a determination of the Hubble constant with about 1% accuracy from extragalactic maser sources.

Assessment of Current Situation

P5 is strongly enthusiastic about dark energy science and supports an aggressive experimental program. We agree with the DETF recommendations and support the pursuit of dark energy science at both Stage III and Stage IV.

We find that the DES fits nicely into our roadmap as a DETF Stage III project. At a relatively modest cost the DES promises to achieve the DETF targets for Stage III increase in our knowledge about dark energy. The DES will produce data using all four principal methods listed earlier, and thus will further develop the experience and expertise that will be critical to making the most of a Stage IV program, another DETF Stage III goal.

We also support the aggressive pursuit of a Stage IV program as designated by the DETF. As discussed above, the particle physics community's involvement in Stage IV projects is most direct with LSST and SNAP. The ongoing NASA JDEM process (of which SNAP is a part) has brought ADEPT and Destiny onto our horizon as well.

Given the high level of commitment by scientists in our community, the extraordinary importance of dark energy science, and the expected complementary nature of LSST and SNAP, P5 was tempted to make room in our roadmap to complete both of these experiments as rapidly as possible. However, due to uncertainties in overall costs, the specifics of cost sharing among the partner agencies, and the outcome of the JDEM competition we have left the final recommendations to a later review. We urge the agencies to organize this process in a way that avoids unnecessary delays.

Recommendations

- At a relatively modest cost the DES promises to achieve the DETF targets for Stage III increase in our knowledge about dark energy. It will further develop the experience and expertise using a number of techniques to measure the effects of dark energy, which will be critical to making the most of a Stage IV program, another DETF Stage III goal. We recommend that the agencies proceed with this project.
- *Phase I of a Stage IV Program:* We recommend that the agencies carry out the technical and cost studies, over the next two to three years, that would be required to confidently move forward with the

construction of a Large Survey Telescope and a Dark Energy Space Mission. This will require joint planning by the NSF and the DOE in the case of the LSST to bring it to the Preliminary Design Review Stage (in the case of the NSF) and CD2 Stage (in the case of the DOE) and the DOE and NASA in the case of SNAP to bring it to the CD2 Stage (in the case of the DOE). We recommend that the DOE work with NASA to ensure that a dark energy space mission can be carried out and that the three potential approaches to the mission have been properly evaluated.

- *Phase II of a Stage IV Program:* We recommend a decision process soon after the completion of the technical and cost studies to formulate and recommend an aggressive and financially realistic Stage IV program.

4. Neutrino Science

The exciting opportunities in Neutrino Physics have been discussed in detail in Chapter III of this report. Here we briefly summarize the main points and then discuss the projects that have been proposed to address these important questions.

The discovery of neutrino mass has raised a number of very interesting questions about the neutrinos and their connections to the rest of physics and astrophysics. These questions include:

- *What are the masses of the neutrinos?*
- *Does the neutrino mass spectrum resemble the charged-lepton and quark spectra, or is it an inverted version of those other spectra?*
- *What is the pattern of neutrino mixing? In particular, how large is θ_{13}, the presently unknown mixing angle? Is the atmospheric mixing angle, known to be very large, maximal?*
- *Are neutrinos their own antiparticles?*
- *Do neutrinos violate CP? If so, are they the key to understanding the matter-antimatter asymmetry of the universe?*
- *Are there sterile neutrinos — neutrinos that do not experience any of the known forces of nature except gravity?*
- *What can neutrinos tell us about New Physics beyond the Standard Model?*
- *What has been the role of neutrinos in shaping the universe? What would an understanding of this role tell us about the universe and about neutrinos?*
- *What can neutrinos, acting as messengers, reveal about astrophysical phenomena?*
- *What totally unanticipated further surprises do neutrinos have in store?*

A global effort to pursue the answers to the open questions has yielded a well-developed experimental program of great promise. The planning of this panel has been molded by the previous reports from EPP2010 and NuSAG, as well as the APS neutrino study.

Under consideration by this panel are three types of experiments that have been proposed to address a number of these questions:

1. Reactor neutrino experiments. These experiments seek to observe the disappearance of low energy $\bar{\nu}_e$ from a reactor on their way to detectors placed at a distance of order 1 km. These experiments are sensitive to $\sin^2 2\theta_{13}$.
2. Accelerator neutrino experiments. These experiments use the oscillation signals $\nu_\mu \to \nu_e$ and $\bar{\nu}_\mu \to \bar{\nu}_e$ over long baselines of order 100-1000 km. They are sensitive not only to θ_{13}, but also to the atmospheric mixing angle θ_{23}, to whether the neutrino mass spectrum is normal or inverted, and to whether neutrino oscillation violates CP. The quantities that will actually be measured by accelerator experiments will typically involve several underlying neutrino properties at once. These properties will then have to be sorted out. This would clearly be facilitated by a clean measurement of θ_{13} by a reactor experiment. In addition, it will require complementary accelerator neutrino experiments involving different neutrino energies and/or baselines, so that the $\nu - \bar{\nu}$ asymmetry produced by CP violation can be distinguished from that coming from mass-spectrum-dependent matter effects.
3. Neutrino-less double beta decay experiments. The observation of this process, at any nonzero level, would establish that neutrinos are their own antiparticles.

We now turn to the major projects being proposed for each type of experiment, focusing on the primary expected science return. Because of the new capabilities, which open new windows to more exotic possibilities, surprises are likely and would be very much in line with the recent history of neutrino physics. It is also important to remember that, in addition to the large projects we explicitly consider, there will be relatively small projects (such as MINERvA) that have great value and that are, as a class, vital to the overall health of the field. These projects should be judged on their individual merits. The lack of explicit inclusion of a particular small project does not mean it has low priority.

Reactor Neutrino Experiments

Nuclear reactors are a copious source of $\bar{\nu}_e$. Planned experiments are expected to be sensitive to the probability of $\bar{\nu}_e$ disappearance down to the 1-2% level. This probability is essentially given by

$$P = \sin^2 2\theta_{13} \sin^2\Delta_{31} + \cos^4\theta_{13} \sin^2 2\theta_{12} \sin^2\Delta_{21},$$

where $\Delta_{ij} = 1.27\ \Delta^2 m_{ij}\ (eV^2)L(km)/E(GeV)$, and $\Delta^2 m_{ij}$ is the difference in the square of the masses of the i-th and j-th neutrino, E is the energy of the $\bar{\nu}_e$, and L is the distance between the source and the detector. Since the first term dominates near $|\Delta_{31}| = 90^o$, and since Δ_{31} will be known quite accurately from MINOS, a measurement of the probability of $\bar{\nu}_e$ disappearance translates directly into a measurement of $\sin^2 2\theta_{13}$, independent of other neutrino mixing and mass parameters.

Thus, reactor $\bar{\nu}_e$ disappearance experiments are an important and relatively inexpensive ingredient in the world-wide neutrino research program. The unambiguous information on $\sin^2 2\theta_{13}$ that they provide is very helpful in sorting out the results from accelerator based oscillation experiments, which typically depend on combinations of parameters in the neutrino sector. Further, early information on $\sin^2 2\theta_{13}$ from reactors could be very useful for optimizing the running strategy of the accelerator based experiments, as well as influencing the planning and design of the next generation of experiments.

Since they search for a small disappearance probability, the sensitivity of reactor experiments is typically limited by systematic effects. The current most stringent limit is $\sin^2 2\theta_{13} < 0.12$, established by the CHOOZ experiment in France. This experiment used a single detector located about 1 km from two reactor cores. The most important systematic effects were due to the limited understanding of the detection efficiency, the reactor flux, and the reactor power. All new planned experiments include two or more similar liquid scintillator detectors, placed near and far from the reactors. By taking ratios of event counts in the near and far detectors, the systematic uncertainties are substantially reduced.

The upgraded CHOOZ experiment (Double CHOOZ, or DCHOOZ) will be the first one to come on line. Operations with one detector could start as early as 2007, with a second detector added by the end of 2008. DCHOOZ will reach a $\sin^2 2\theta_{13}$ sensitivity of 0.07 in one year with a single detector and 0.02-0.03 with three years of running and both detectors. Thus DCHOOZ will provide an early indication on the size of $\sin^2 2\theta_{13}$, and this information could influence the optimal running strategy for the accelerator-based experiments, NOνA/T2K. DCHOOZ is a European-led project with important contributions from the U.S.

Daya Bay

The Daya-Bay project is a collaboration of Chinese and US physicists. The reactor complex consists of two reactors at the Daya Bay site and two more at the nearby Ling Ao site, with two more reactors planned there. Daya Bay is located on the east side of the Dapeng peninsula, 55 km from Hong Kong in China.

Daya Bay is a more ambitious experiment than DCHOOZ. Its goal is to reach a $\sin^2 2\theta_{13}$ sensitivity of order 0.01 in three years of running. The better sensitivity of Daya-Bay with respect to DCHOOZ is due to the higher power of the reactors, and thus the higher neutrino flux, a larger detector volume, and to the ability to swap near and far detectors to better control systematic uncertainties. The Daya Bay collaboration plans to deploy eight detectors at several locations: two at a site near the Daya Bay reactors, two at a site near the Ling Ao reactors, and four at a far site. A plan for swapping detectors between sites is an important ingredient of the project. This plan is not yet fully worked out.

The full detector configuration could be operational in 2009. The cost of the project is not well known at this time. The majority of the cost would be borne by China, and the U.S. investment is expected to be approximately $20M.

Recommendations

- A detailed review of the Daya Bay project should be carried out as soon as possible. This review should focus particularly on the feasibility of the approach. It should evaluate the basis of estimates of the systematic uncertainties, along with the additional systematic uncertainties induced by moving the detectors. Conditional on a favorable review, we recommend proceeding with planning and construction.

Off-axis Beam Experiments

Neutrino beams at accelerators are inherently secondary beams so the energy spread is relatively large. However, due to kinematics, the energy spread is relatively smaller off the neutrino beam axis. By placing the detectors off axis, therefore, a narrow-band beam experiment is possible.

NOνA

The NuMI Off-Axis ν_e Appearance Experiment (NOνA) is a long-baseline experiment whose primary science objective is to use $\nu_\mu \rightarrow \nu_e$ oscillations to answer the neutrino mass hierarchy question: is the neutrino mass spectrum normal (*i.e.*, quark-like) or inverted? NOνA leverages the existing NuMI facility infrastructure at Fermilab. Because of the long baseline available (810 km), for L/E fixed near the oscillation maximum, the beam energy is releatively large, around 2 GeV. The large energy, together with the capability of running both neutrino and antineutrino beams, gives NOνA unique experimental access to matter effects and hence the mass hierarchy.

The measurement principle is straightforward: count the number of neutrino- and anti-neutrino-induced events. Because the measurement is based on neutrino mixing, the results are sensitive to a combination of fundamental mixing parameters. This is both an opportunity and a challenge. For a given $\sin^2(2\theta_{13})$, the relative rates from neutrino and antineutrino beams will be influenced by both the CP-violating phase, δ, and the sign of the mass difference, Δm^2. The two effects can add together in either direction or work against each other. The basic detector element is a 4cm x 6cm x 1600cm liquid scintillator cell read out by APDs. The far detector, to be sited in Ash River, MN, is approximately 100m long, with a total target mass of approximately 20 ktons. There is also a smaller (~100 ton) nearby detector, whose primary function is to measure the neutrino beam fluxes, to be sited in the existing MINOS access tunnel.

A new off-axis neutrino beam is also currently under construction in Japan, to be directed at the existing Super-Kamiokande detector. The primary science objective of this experiment, known as T2K, is to measure $\sin^2(2\theta_{13})$. Because of the shorter baseline and lower beam energy T2K phase 1 will not be able to determine the mass hierarchy or establish CP violation. Thus, the information from T2K and NOνA will be complementary. Combining the results will help to untangle the different effects of the unknown neutrino properties.

The statistical power of NOνA can be improved by either increasing the detector mass or the neutrino beam intensity, or both. Thus, NOνA represents a phased approach. For our evaluation of the science reach, we considered the capabilities of NOνA Phase 1 as follows: 6 years of running with a total of $60\text{x}10^{20}$ protons on target (POT) with half neutrino and half antineutrino running, 20 kton far detector, Δm_{32}^2 $=2.7\text{x}10^{-3}$ eV^2, $\sin^2(2\theta_{23})=1.0$. Note that NuMI upgrades are required to reach that neutrino beam intensity. The Table shows the numbers of expected neutrino- (antineutrino-) induced events for representative values of the mixing parameters. The number of background events is 12 (7.4).

$\sin^2(2\theta_{13})$, hierarchy	$\delta=0$	$\delta=\pi/2$	$\delta=\pi$	$\delta=3\pi/2$
0.02, normal	26 (8.5)	13 (11)	23 (7.5)	36 (4.3)
0.12, normal	141 (46)	111 (54)	134 (43)	164 (36)
0.02, inverted	14 (11)	6.8 (17)	17 (13)	24 (7.0)
0.12, inverted	83 (66)	65 (80)	89 (69)	107 (55)

Table 1 Numbers of neutrino- (antineutrino-) induced events in NOνA phase I, on top of a background of 12 (7.4) events, for representative values of the relevant mixing parameters. Source: NOνA collaboration.

The NOνA experiment itself will cost approximately \$200M and the accelerator upgrades are expected to cost an additional \$30M-\$40M.

If $\sin^2(2\theta_{13})$ is greater than ~0.01 it will be positively measured by the next round of reactor and off-axis beam experiments. If it is greater than ~0.04, NOνA will be able to resolve the mass hierarchy for a range of δ, as shown in the figures below. This range grows to half the possible values of δ as $\sin^2(2\theta_{13})$ grows to ~0.10. If $\sin^2(2\theta_{13})$ is too small for NOνA Phase 1 to determine the mass hierarchy, then an upgraded NOνA or an alternative approach will be necessary.

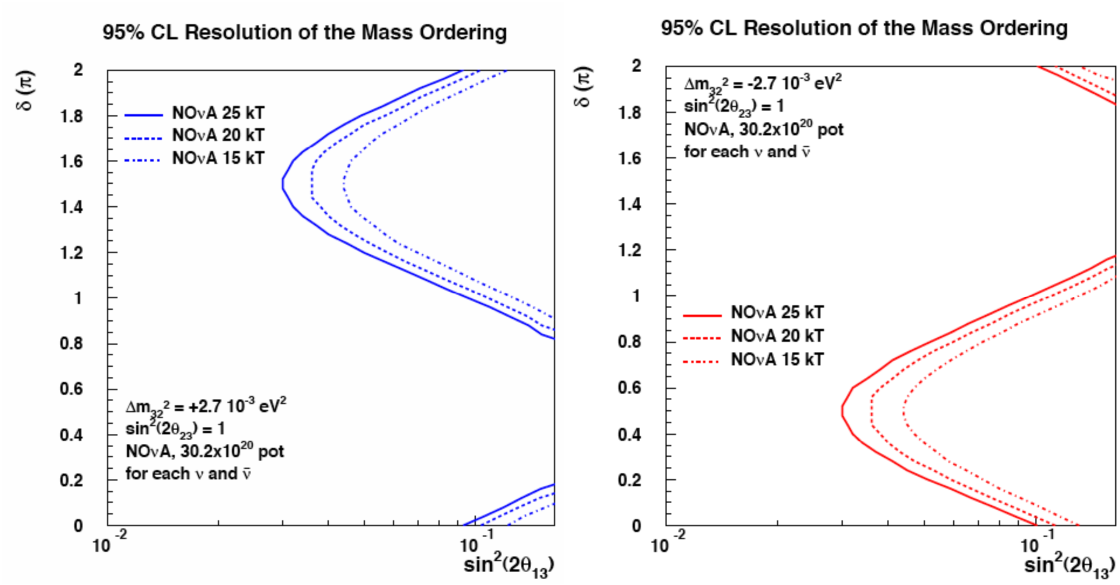


Figure 2 The regions of parameter space for which NOνA Phase 1 can determine the mass hierarchy for normal (left plot) and inverted (right plot) hierarchy. Currently, we know that $\sin^2(2\theta_{13})$ is less than 0.12.

Findings

- NOνA's θ_{13} reach is comparable to, and somewhat better than, the expected reach for both T2K and Daya Bay, determined in a completely different way.
- For "large" $\sin^2(2\theta_{13})$, the mass hierarchy can be determined over ~1/2 the δ range. Note that, for the statistics of NOνA Phase 1, the ability to measure δ is very small and very roughly independent of $\sin^2(2\theta_{13})$. It will not be possible to determine that CP violation is nonzero to 2σ or greater with NOνA Phase 1 for most values of the neutrino mixing parameters.
- A determination of the mass hierarchy will be a crucial input to the global effort to establish non-vanishing CP violation in the longer-term future. Both the hierarchy-dependent matter effect and CP violation produce an asymmetry between neutrino and antineutrino oscillation. To demonstrate that CP is violated and to determine the hierarchy, one must separate these two sources of whatever asymmetry is observed. With its longer baseline and higher energy, the U.S. program will be in a unique position to determine the hierarchy, thereby making it possible for the global neutrino effort to establish the existence of leptonic CP violation.
- By providing unique information about the mass hierarchy, NOνA adds important value to other components of the neutrino roadmap in

addition to the search for CP violation, and it informs the next steps. For example, if the next-generation neutrino-less double-beta decay experiments were to see a signal, without NOνA it would be impossible to tell whether this is due to an inverted mass hierarchy with low mass pedestal or a normal hierarchy with large mass pedestal. More importantly, if NOνA determines that the mass hierarchy is inverted, then a null result from the neutrino-less double-beta decay experiments (with effective mass sensitivity down to ~0.01eV) will be unambiguous: conventional Majorana neutrino masses would be ruled out, neutrinos would most likely be Dirac particles, and there would be no compelling motivation for future neutrino-less double beta decay experiments. Conversely, if NOνA determines that the mass hierarchy is normal, then even larger neutrino-less double beta decay experiments would be well motivated. Thus, without a resolution of the mass hierarchy, there will certainly be an inherent ambiguity in a null result from the next round of neutrino-less double-beta decay experiments.

- For a limited range of "small" $\sin^2(2\theta_{13}) \sim 0.02\text{-}0.04$, information from NOνA could be very important to plan the next phase of the worldwide neutrino program. For example, if $\sin^2(2\theta_{13}) = 0.02$ and the CP-violating phase $\delta = 3\pi/2$, then information from NOνA, combined with results from T2K, would indicate that we are in a region of parameter space where some additional running at the existing facilities would resolve the mass hierarchy. For other values of the parameters, results from NOνA plus T2K would show that more powerful facilities would be needed.
- Without NOνA, the world loses the chance to look for evidence of CP violation in the leptonic sector for a long period of time. To be sure, there is a risk that NOνA Phase 1 will provide no new information, depending on unknown mixing parameters. Still, we find the science case is compelling: there is no other planned program worldwide that will have the potential to resolve the mass hierarchy and start to probe for CP violation in the leptonic sector.
- Additional NOνA physics topics include
  - A measurement of $\sin^2(2\theta_{23})$, to a precision that is about a factor 10 better than the eventual measurement by MINOS, using quasi-elastic ν_μ charged-current events.
  - A search for sterile components of the neutrino mass eigenstates by looking for disappearance of neutral current

interactions in the far detector. Similarly, if MiniBooNE has a positive result, the NOνA near detector could be moved along the MINOS tunnel to reconfirm and further investigate the result.
  - Measuring the neutrino light curve from a galactic supernova, should one occur during the NOνA lifetime.
  - Checks of the neutrino neutral current cross-sections around 2 GeV, along with a possible $\sin^2(\theta_W)$ measurement, using the near detector.
- While the physics case for NOνA Phase 1 is the primary consideration, there are also a number of practical considerations:
  - An upgrade path, if called for, is provided by the NOνA facility. By increasing the detector mass and/or beam intensity, better sensitivity will be possible.
  - A number of associated benefits, such as having test beam capability in the U.S. for detector development during the next decade, are enabled by the NOνA program.
  - Not proceeding with NOνA would again let science leadership move out of the U.S. after considerable investment in infrastructure for accelerator-based neutrino physics, in particular the NuMI facility.

Recommendations

- Proceed with the 20 kt-scale NOνA experiment. Due to long operations required, construction should not be stretched out significantly with respect to the roadmap described in Chapter VI.
- We encourage T2K and NOνA to communicate to maximize the complementarity of the two programs.
- We recommend a modest level of U.S. participation in T2K.
- We encourage international participation in NOνA Phase 1.

Neutrino-less Double Beta Decay Experiments

At present the only feasible way to determine whether neutrinos are Majorana particles (that is, they are their own antiparticles) is through searching for neutrino-less double beta decay using unstable nuclei. This would likely be the first evidence for violation of total lepton number. The rate for such a process depends on material-specific nuclear matrix elements.

Modeling of these matrix elements introduces an uncertainty that is difficult to estimate, and should neutrino-less double beta decay be observed one would wish to repeat the experiment using different elements in order to check the accuracy of these calculations. The rate is also proportional to the square of the ``effective neutrino mass'', that is, the combination $m_{eff}=\Sigma_i m_i U_{ei}^2$, where U_{ei} is the mixing parameter of the electron neutrino with the mass eigenstate, i. The relationship is shown in Figure 2 as a function of the lightest mass eigenstate. Note that cancellations can occur in m_{eff}, so that even if neutrinos are Majorana particles, m_{eff} could be zero or too small to measure. However, for Majorana neutrinos, an inverted hierarchy, and no light sterile neutrinos, m_{eff} is at least 0.01 eV. NuSAG has identified this value as a worthwhile, if challenging, goal. For a normal, nondegenerate spectrum, the typical effective mass is of order the solar oscillation scale. There are currently no viable proposals to reach such sensitivity. The effective mass could be significantly larger, even for a normal hierarchy, if neutrinos are degenerate. A subset of the Heidelberg-Moscow collaboration has claimed evidence for neutrino-less double beta decay of ^{76}Ge at a rate corresponding to m_{eff} in the range (0.11 - 0.56) eV (95% c.l.), with a best value of 0.39 eV. This controversial result would indicate a degenerate neutrino spectrum.

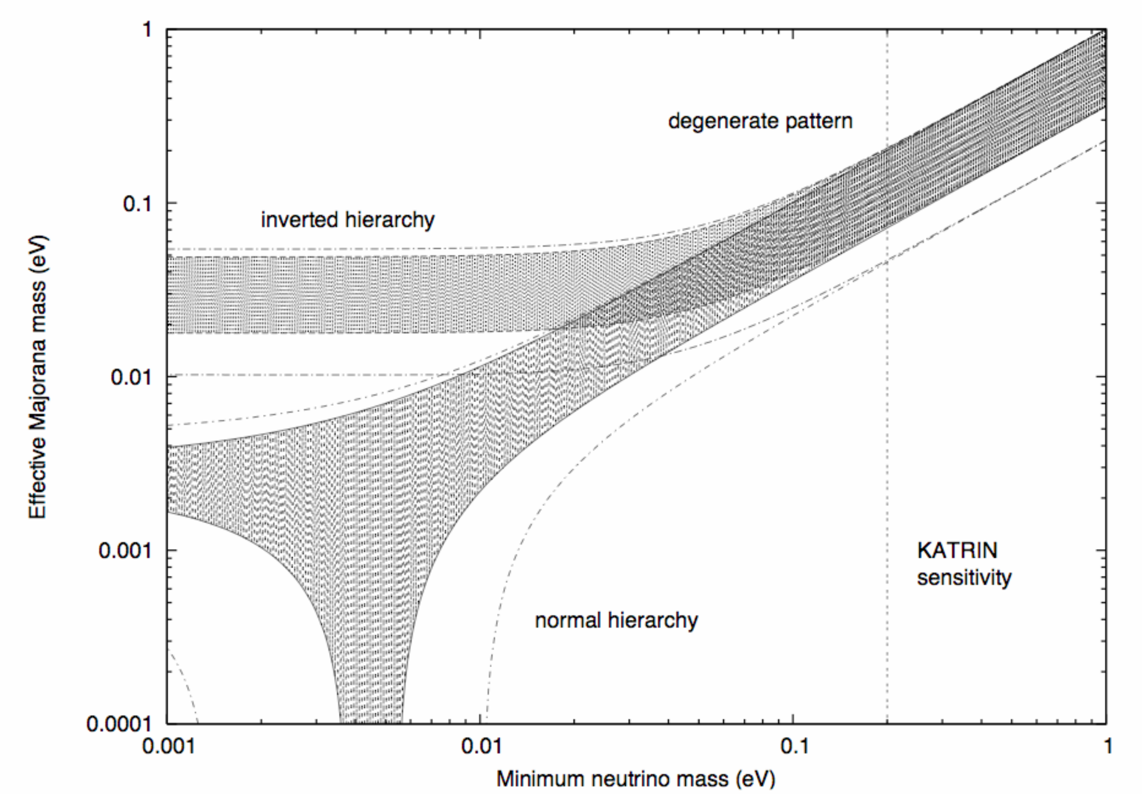


Figure 3 The relation between the effective Majorana mass and the mass of the lightest mass eigenstate. The shaded areas indicate the allowed effective Majorana mass values using the best-fit oscillation parameters. The dot-dash lines indicate how the allowed regions grow when the 95% CL uncertainties in the oscillation parameters are taken into account. The sensitivity of the planned KATRIN β-decay experiment is also shown. Source: NuSAG report 1 (2005).

There are a large number of experiments, using a diversity of techniques, that have proposed future stages with sensitivity to the inverted hierarchy region. Three of these were selected by NuSAG to have highest funding priority. These are CUORE, EXO, and Majorana.

The CUORICINO experiment, currently running in Gran Sasso, uses 12 kg of ^{130}Te and will be sensitive with four years of data to m_{eff} in the 0.6 eV range. This experiment is planned to be upgraded in 2010 to CUORE, which will use 200 kg of ^{130}Te. The projected sensitivity of CUORE after 4 years of running is around 0.1 eV. The U.S. participation in this effort is fairly small.

The EXO-200 experiment, which uses 200 kg of Xenon with enriched concentration of ^{136}Xe, is to begin operations in WIPP in 2008-2009, and to run for 2 years. The projected sensitivity is to an effective neutrino mass of about 0.3 eV, competitive with current bounds and in the range of the claimed Heidelberg-Moscow signal. The EXO experiment is exploring several approaches for reducing the backgrounds by tagging the daughter barium ion, with the goal of achieving access to the inverted hierarchy neutrino mass regime. EXO was judged by NuSAG to offer the potential for a unique and cost-effective approach, but the barium tagging still faces considerable technological challenges. The full EXO experiment is estimated to cost $28.5M for a 1-ton experiment with sensitivity to m_{eff} in the 0.05 eV range and $105M for a ten ton experiment with sensitivity in the 0.01 eV range. Because of the potential efficiency of the background rejection scheme, the moderate depth of WIPP should suffice even for a ton or ten-ton scale EXO.

There are currently two planned experiments using ^{76}Ge crystals as both source and detector. GERDA is a European experiment in Gran Sasso using a Cryogenic liquid shield for background rejection, while Majorana is a DOE nuclear funded experiment using electroformed copper shielding. Majorana could be sited in SNOLAB or perhaps DUSEL for later stages, and needs at least 4500 mwe of depth. GERDA is already funded with infrastructure construction beginning this year. Majorana is essentially ready for a DOE CD-1 review. The two experiments are currently cooperating on information exchange and are considering eventually joining forces on a ton scale experiment using the best techniques developed by each experiment. Such a 1000 kg experiment could reach an effective mass of 0.05 eV, which probes part of the nondegenerate inverted hierarchy region. The initial configuration of Majorana has 60 kg, at a projected cost of about $30M, and is planned to start in 2010, with first results in 1-2 years. Expansion to 120 or 180 kg is to be considered in future stages. By 2013 sensitivity to the 0.3 eV m_{eff} range is expected. An even larger, 480 kg, version is estimated to cost around $125M.

Recommendations

- The three techniques to measure neutrino-less beta decay, CUORE, EXO, and Majorana should be investigated vigorously, leading to a selection of one technique for an experiment at the 1-10 ton scale. One possible decision point is after the parallel EXO efforts in barium tagging and in the EXO-200 double beta decay search are complete.

This might be as early as 2010. Should success of the techniques be demonstrated, an upgrade to the full experiment with either a 1-ton or a 10-ton detector should be considered in the context of the other possibilities available at that time and with advice from the appropriate body (*i.e.*, NuSAG). By around 2011-2013 more information is also expected to be available on the feasibility of a larger scale version of the ^{76}Ge and ^{130}Te experiments.

DUSEL

In response to community expressions of interest in the establishment of a U.S. underground facility for physics and other sciences, the National Science Foundation is considering the creation of a Deep Underground Science and Engineering Laboratory (DUSEL). A multi-step planning and evaluation process that could lead to the funding of a DUSEL is underway. In the NSF vision communicated to P5, the DUSEL would consist initially of a basic laboratory containing experiments that would include a large-scale dark matter direct detection experiment, a large-scale neutrino-less double beta decay probe, and a third physics experiment such as one on solar neutrinos or one measuring nuclear reaction rates under very low background conditions. Initial funding would include $(150 – 200)M in support of these experiments. It would also encompass R&D on a megaton-scale proton-decay and neutrino detector, and on a large cavern that could house such a detector. The cavern R&D could embrace modest exploratory excavation.

The proposed DUSEL would have a number of attractive features:

- It would create a significant U.S. underground science program. Such programs elsewhere (those at Gran Sasso and Kamioka, for example) have been very fruitful. They have led to dramatic findings on supernovae and neutrino flavor change, and significant limits on proton decay and neutrino-less double beta decay.

- The physics experiments that would be part of the initial DUSEL, especially those on dark matter and double beta decay, are ones of great scientific interest. The component of the initial DUSEL funding that would be in direct support of these experiments would greatly facilitate their actually getting done. Furthermore, it is consistently asserted that the existing underground facilities are quite full (we have not been able to check this assertion independently), so that a DUSEL would provide needed space for very important experiments. Not all experiments would need the great depth that is generally contemplated for the DUSEL. But some of them, such as the very-large-scale dark matter search, would benefit greatly from it.

- A DUSEL would provide the U.S. with the flexibility to create a megaton-class proton- decay and accelerator neutrino detector for a second-phase of experiments. Such a detector would seek evidence of grand unification, and evidence that the matter-antimatter asymmetry of the universe arose through leptonic CP violation.

The work toward a technical design for DUSEL will be funded in FY07 with a Preliminary Design Review expected in early FY08. The goal is to receive NSF approval in FY08 with construction starting in FY10. In view of the existence of other projects already in the queue for MREFC funding, as well as other new projects that will compete for MREFC funding on the same timescale as DUSEL, the start of DUSEL may be later than the desirable start dates of the relevant dark matter and double beta decay experiments, if this schedule is not adhered to.

The DUSEL facility operating costs are unclear at this time, but they are likely to be significant. An estimate given to P5 is 10% of the total construction cost per year, or about $40M per year. These costs will be borne by the relevant NSF established program areas, which also must fund other valuable particle physics projects and the precious R&D for future innovation that is critical to the long-term health of the field. Thus, a viable DUSEL program should include a plan for increased funding of the established program areas by the time DUSEL construction is complete.

Recommendation

- The DUSEL is an intriguing scientific possibility for physics and astrophysics. We recommend that the DUSEL planning and evaluating process continue.

The Longer-term Neutrino Oscillation Program Context

Strategies for going beyond Phase 1 of both NOνA and T2K to a stage with considerable sensitivity to CP violation are being explored for the timeframe beyond 2015. In the U.S., this exploration is being carried out by the Workshop on Long Baseline Neutrino Experiments. This months-long study is focusing primarily on two options: One of these involves upgrading the NuMI-NOνA facility by adding detection capability at a location to be chosen to optimize the physics reach. This location might be further off axis than NOνA will be, at the 2nd atmospheric oscillation maximum, or at the location of NOνA, or closer to the neutrino source than NOνA. In addition, if sufficient funding is available, the intensity of the NuMI beamline could be increased through the construction of a new proton source. The other option entails building at Fermilab a new broad-band [(0 – 6) GeV] neutrino beam that would travel well over 1000 km to a megaton-class water Cerenkov detector in a large cavern in a DUSEL, or to a suitable liquid argon detector. The beam requirements are under study. The detector would serve not only as a neutrino detector, but also as a proton decay detector, sensitive to different decay rates and modes, depending on whether it is a water Cerenkov or liquid argon detector.

The findings of the Workshop on Long Baseline Neutrino Experiments will be considered in the international context by the Neutrino Scientific Assessment Group (NuSAG) later this year. In addition, plans for the future will certainly be influenced by shorter-term experimental findings.

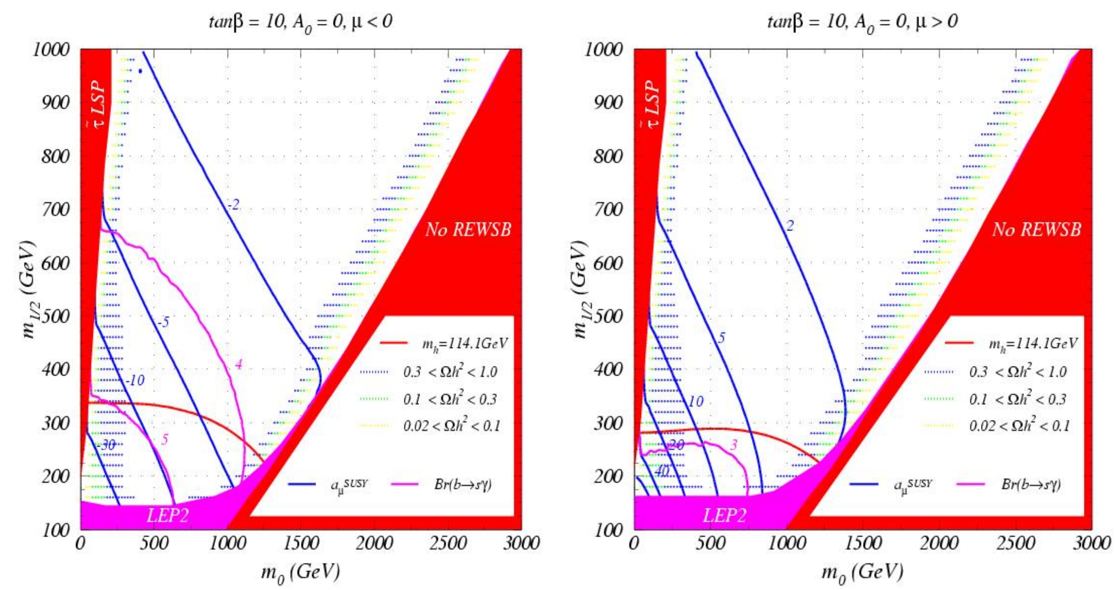
In Japan, there is consideration of a post-T2K-Phase-1 very intense (4 MW) beam from Tokai to a new, megaton-scale detector 295 km away. Since this baseline, the same as that of T2K-1, is fairly short, there is also the idea of possibly dividing the large detector in half, and placing half of it in Korea, more than 1000 km from the source. At this greater distance, CP violation effects will be much larger than at 295 km. Comparing results from the two distances will facilitate the separation of the matter effect from CP violation. The physics reach of this approach is under study.

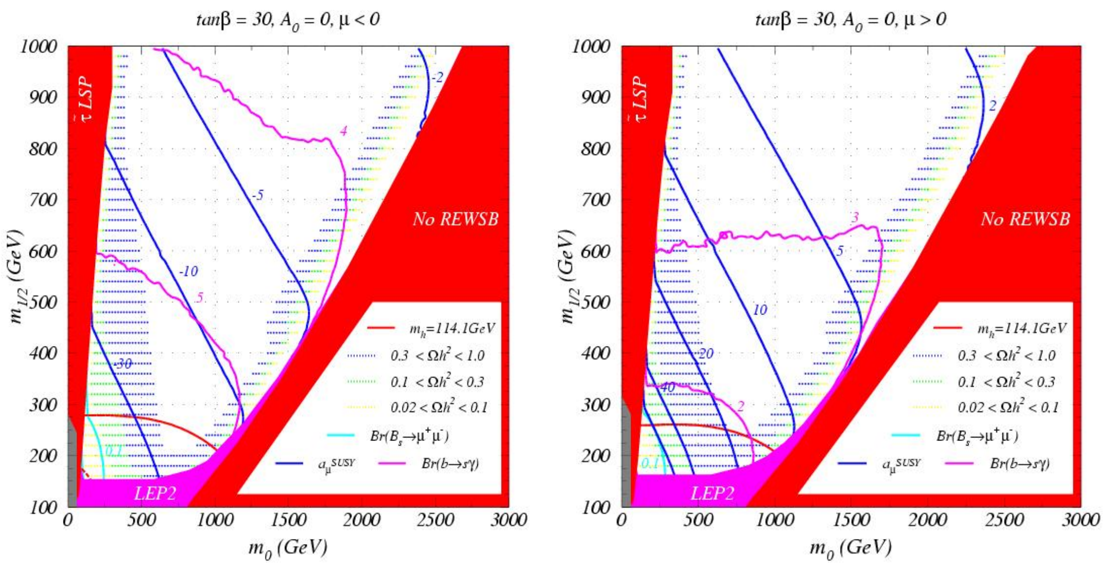
In Europe, there is exploration of the possibility of building at CERN a Superconducting Proton Linac (SPL) that could produce a 4 MW neutrino beam with an energy of (260 – 350) MeV that would travel 130 km to a megaton-class detector in a new laboratory in the Frejus tunnel. One would not expect this very low energy beam to be sensitive to the mass hierarchy. There is also exploration of the more distant possibility of a very powerful neutrino facility using Beta Beams (pure ν_e or $\bar{\nu}_e$ beams from stored, accelerated nuclei that undergo beta decay). The SPL could be a part of the complex that produces the Beta Beams, and, eventually, Beta Beam, SPL, and atmospheric neutrino data might be used to determine the mass hierarchy.

The ultimate neutrino facility may be a Neutrino Factory based on a storage ring for muons whose daughter neutrinos form a very intense, and effectively flavor-pure, beam. The Neutrino Factory, Beta Beams, and related facilities are the focus of an International Scoping Study. International planning and coordination will be important for these ambitious projects.

5. Precision Measurements for Charged Leptons and Quarks

The remarkable success of the Standard Model suggests that the quest for physics beyond the Standard Model using accelerators should be directed either at higher energy scales, such as the LHC and ILC, or at small effects in low-energy precision observables. The latter approach involves higher order processes that probe for deviations from Standard Model predictions due to the virtual effects of new fundamental interactions. Such tests of the Standard Model through quantum corrections provide a powerful tool for probing the high energy scales possibly related to electroweak symmetry breaking and the flavor problem. This is illustrated in the figure below, which displays the parameter space in Supersymmety that is presently probed by the higher order process g-2 of the muon and the rare decay b $\rightarrow$ sγ as well as direct collider searches at LEP2.





Summary of constraints on the MSUGRA model of Supersymmetry in the mass plane of the common scalar (m_0) and common gaugino ($m_{1/2}$) mass. Regions of the Cold Dark Matter relic density, and contours of m_{Higgs} >114.1 GeV, the muon anomalous magnetic moment a_μ (x 10^{10}), and the branching fraction for $b \rightarrow s\gamma$ (x 10^4) are displayed and labeled as indicated. The region excluded by searches at LEP and by theoretical constraints are labeled. The value of tan β (the ratio of vacuum expectation values for the two Higgs doublets) and the sign of μ (the Higgsino mixing parameter) are as indicated. Source: H Baer *et al*., JHEP **0207**, 050 (2002).

g-2 of the Muon

The classic example of a precision observable is the anomalous magnetic moment of the muon $a_\mu = (g-2)/2$. Comparison of experiment with theory for this quantity tests the Standard Model at the quantum loop level. Experimental measurement of a_μ has reached a high level of precision, with the current value being $a_\mu = (116\ 592\ 080 \pm 54 \pm 33) \times 10^{-11}$ as obtained by E821 at Brookhaven. This result is roughly (1.0-2.5) standard deviations away from the present calculation of the Standard Model prediction.

In order to exploit the experimental precision on the anomalous magnetic moment of the muon, it is necessary to have a precise computation of the Standard Model contributions. These are usually denoted as $a_\mu^{SM} = a_\mu^{QED} + a_\mu^{EW} + a_\mu^{had}$. The values of the various Standard Model contributions are collected in the Table below. The QED contributions are perturbative and a full calculation exists to fourth order in α; in addition, the leading fifth order terms have also been computed. The QED contribution is given by $a_\mu^{QED} = (116\ 584\ 719 \pm 0.1 \pm 0.4) \times 10^{-11}$ with the first error corresponding to the accuracy of the complete 4th and partial 5th order computation, and the second error representing the uncertainty in the value of α. The electroweak contributions are also perturbative and have been computed to 2-loop order in the electroweak coupling constant. They give a contribution $a_\mu^{EW} = (154 \pm 1 \pm 2) \times 10^{-11}$, with the first error corresponding to the uncertainty in the calculation and the second error resulting from the uncertainty in the Higgs mass. The errors on both the QED and electroweak contributions are far below the present level of experimental precision. The size of the Standard Model electroweak contribution is typical for generic new interactions, which appear at the electroweak scale and are within the sensitivity of the current experimental precision.

The hadronic contributions to the Standard Model prediction of the muon anomalous magnetic moment, a_μ^{had}, are dominated by non-perturbative effects and contain a large theoretical uncertainty. They arise from two classes of contributions. The first class corresponds to effects that represent the contribution of the running of α from low to high scales (*i.e.,* the hadronic vacuum polarization contributions). These cannot be calculated from first principles, but can be related by means of a dispersion relation to data on either the hadronic cross section in e^+e^- annihilation, or spectral functions in tau lepton decays. At leading order in the strong coupling constant, the e^+e^- data yields $a_\mu^{had}(VP) = (6963 \pm 62 \pm 36) \times 10^{-11}$, whereas the tau data gives $a_\mu^{had}(VP) = (7110 \pm 51 \pm 28) \times 10^{-11}$. Here, the first error is from experiment and the second is from theory. The tau and e^+e^- data do not yield consistent results. Improvements in the electron-positron data sample at low energies are expected from several sources over the next few years. The corresponding error on $a_\mu^{had}(VP)$ is predicted to decrease to roughly 40×10^{-11} in the coming year, with further improvements on a longer timescale being difficult to estimate. The vacuum polarization contribution is large enough that higher order 3-loop contributions have been computed and yield a result which is $(-98 \pm 1) \times 10^{-11}$, using the low-energy e^+e^- data. The second class of hadronic contributions is given by light-by-light hadron amplitudes. These off-shell amplitudes are affected by non-perturbative strong interactions that are not calculable in QCD and cannot be related to other experimental observables. They must be estimated within the framework of a theoretical model. Recent evaluations give $a_\mu^{had}(LBL) = (120 \pm 35) \times 10^{-11}$. Various computations of the light-by-light contributions are not in agreement on the size of the theoretical error for this quantity and it is not clear how the calculations of this term can be improved. The table below summarizes the contributions to the muon g-2. In calculating a total we have used the e^+e^-vacuum polarization value, however, we also list in the table the vacuum polarization term determined from tau data for reference.

Contributions to a_μ (in units of 10^{-11})

E821	116 592 080 $\pm$ 54 $\pm$ 33
SM QED	116 584 719 $\pm$ 0.1 $\pm$ 0.4
SM EW	154 $\pm$ 1 $\pm$ 2
SM hadronic $(VP)_{e+e-}$	6 963 $\pm$ 62 $\pm$ 35
SM hadronic $(VP)_\tau$	7 110 $\pm$ 51 $\pm$ 28
Three loop (VP)	-98 $\pm$1
SM hadronic (LBL)	120 $\pm$ 35
SM Total $_{e+e-}$	116 591 858 $\pm$ 72 $\pm$ 35 $\pm$ 3

The Brookhaven E969 collaboration has submitted a proposal to increase the level of precision on the measured value of a_μ. The proposal plans to increase the muon flux by a factor of 5 over that obtained by E821 by installing (i) an open end inflector magnet and (ii) four times the number of quadrupole magnets in the forward pion decay beam. Combined with upgraded detectors, electronics, and NMR system, the collaboration expects to reach an accuracy of $\Delta a_\mu = \pm 25 \times 10^{-11}$.

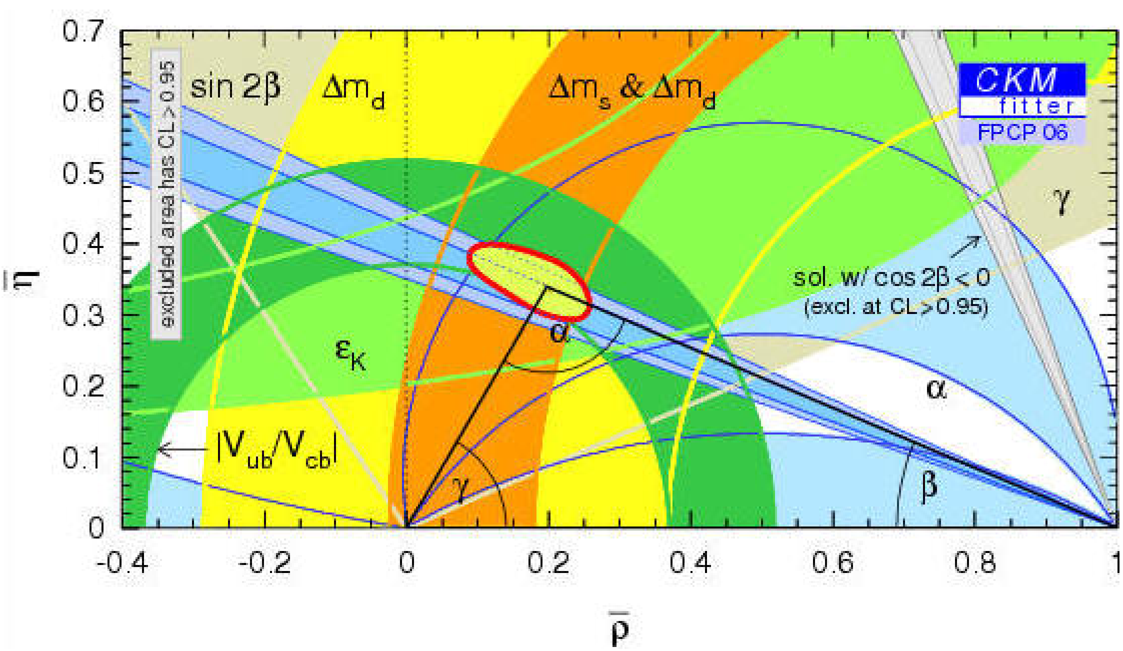
P5 believes the E969 proposal constitutes an impressive improvement on the measurement on this fundamental quantity, but is concerned whether the theoretical uncertainty (particularly both classes of hadronic contributions) can reach the same level of accuracy.

Recommendation

- A review of the status of flavor physics should be undertaken after results from the LHC are in hand. A decision on the muon g-2 experiment E969 should be postponed until after this review and there are further indications that new physics may be within reach of the experiment. The theoretical community should continue to work on reducing the uncertainties associated with the calculations of the hadronic contributions to the Standard Model prediction, particularly the light-by-light term.

Heavy Flavor Physics

Precision measurements in the heavy quark sector are key to understanding the flavor structure of the Standard Model and of the new physics that might be present at higher energy scales. A centerpiece of the search for new physics is the study of CP-violating asymmetries in meson decays. CP violation arises in the Standard Model from the existence of a phase in the CKM matrix, which governs weak decays. Unitarity imposes a relation among the elements of the CKM matrix, and this relation can be depicted as a triangle in the complex plane. The current fit of the global data set on the unitarity triangle is shown in the figure below.



The wealth of accumulated data from the B-Factories at SLAC and KEK has furnished unprecedented knowledge on the Standard Model quark flavor sector. BaBar and Belle have established the CKM mechanism of CP-violation and have observed several rare decay modes of the B meson. At present measurements of some processes, such as CP violation in $b \rightarrow s$ transitions, demonstrate small deviations (at the level of roughly 2.5σ) from Standard Model expectations, but more precision is required to reach definitive conclusions. The expected statistical errors for these penguin modes are expected to reach 7-15% per channel by the end of 2008.

The LHCb experiment will begin operation in 2008 at the LHC. The detector will collect 10^{12} B hadrons in 10^7 seconds at luminosities of 10^{32} $cm^{-2}s^{-1}$. Such large data samples will enable LHCb to make many unique direct measurements, particularly in the B_s system, and on the angle γ of the unitarity triangle.

The possibility of a very high luminosity e^+e^- Super-B Factory is presently under study. Two techniques of accelerator design for achieving high luminosities are being investigated: (i) a conventional storage ring with super-high currents in the rings, and (ii) employing ideas from the ILC design to inject high luminosity beams to a storage ring. Luminosities on the scale of 10^{36} $cm^{-2}s^{-1}$ could be delivered by a full-scale version of either design, or a smaller scale project could deliver 10^{35} $cm^{-2}s^{-1}$. With a 10 ab^{-1} data sample, the statistical error on the CP violating asymmetries in the $b \to s$ penguin transitions could be reduced to roughly 2-4% in each channel. This would correspond to a 5σ signal for new physics in interesting modes if the contributions from new interactions are a 20% effect.

Historically, rare processes in the Kaon sector have elucidated the flavor sector of the Standard Model and have provided strong constraints on models of new physics. The extremely rare decay $K \to \pi\nu\nu$ provides the theoretically cleanest determination of the CKM element V_{td} and probes CP violation, however it remains to be studied experimentally. A new experiment to measure this decay is being proposed at KEK and, if approved, expects to observe 50 events in the charged channel.

Loop-induced decays, CP violation, and meson mixing in the D-meson sector have yet to be observed, yet they provide an important opportunity to explore flavor physics in the charged +2/3 quark sector. These processes are extremely small in the Standard Model due to an efficient GIM mechanism, and if a signal were to be detected, it would provide indisputable evidence for new physics. Limits on these processes have been placed by the B-Factories and CLEO-c. Future improvements, or observation of these modes, could be obtained at a Super-B Factory or a charm-tau factory, which is presently under discussion in China. Rare processes involving the top-quark are being probed at the Tevatron with additional sensitivity expected at the LHC.

Heavy flavor physics in the LHC era takes on a new context. The goal is not just to establish deviations from the Standard Model, but also to diagnose and interpret these signals in terms of new fundamental interactions. The discovery of new physics at the LHC will lead to a determination of its mass scale. Ultra-precise heavy flavor experiments can then probe the flavor violation associated with the new interactions and determine the flavor parameters within the new theory. The heavy flavor sector can thus play an important and complementary role in illuminating the physics of the Terascale.

Lepton Flavor Violation

With the discovery of neutrino masses and mixings, it is clearly established that lepton flavor is not conserved. This indicates that lepton flavor violation occurs in the charged lepton sector as well, although it has yet to be observed. The rates for such processes in the absence of new physics are miniscule, being proportional to the tiny neutrino mass. However, many models with new physics contributions can give large enhancements and can yield event rates that are within reach of future experiment.

Such processes in the muon sector include $\mu \to e\gamma$, $\mu \to eee$, and $\mu \to e$ conversion, with the radiative decay channel being the historical benchmark mode for Lepton Flavor Violation searches. The present bound on the branching fraction for $\mu \to e\gamma$ is 10^{-11} from LAMPF. MEG at PSI is expected to improve the sensitivity to 10^{-13} by 2009, with the possibility of a further factor of 10 improvement in the rate by 2012 being under study. In addition, the possibility of performing a MECO-like experiment at Fermilab, which would probe $\mu \to e$ conversion to the sensitivity of roughly 10^{-17}, is currently under investigation. These levels of sensitivity would probe favored ranges of parameter space in supersymmetric Grand Unified Theories.

Rare tau decays, such as $\tau \to \ell\gamma$ (with $\ell = \mu$ or e) and $\tau \to \ell\ell\ell$, can be probed at Heavy Flavor factories. The current upper bounds from PEP-II and KEK-B on the branching fraction for these processes are of order 10^{-7}. It is expected that future Super-B Factories with 10 ab^{-1} of integrated luminosity can improve this sensitivity by two orders of magnitude. In the absence of new fundamental interactions, the branching fraction for the decay $\tau \rightarrow \mu\gamma$, for example, is of order 10^{-54}, well below the reach of experiment. However, contributions from supersymmetric models can yield a rate in the range $10^{-(7-10)}$ for this channel, which is within reach of future experiment.

Recommendation

- The review of flavor physics should also include the potential for new physics in μ to e conversion, a very high luminosity B experiment, and rare K decays. Work on options for a Super-B Factory accelerator should continue in order to better understand their potential luminosity and cost.

Electric Dipole Moments

The electric dipole moment (EDM) of atoms and particles, such as the neutron and electron, provide important and sensitive probes of the fundamental origin of CP violation. The present experimental bounds on the EDM of the neutron, electron, and Mercury are $d_n < 3 \times 10^{-26}$ *e cm*, $d_e < 2 \times 10^{-27}$ *e cm*, and $d_{Hg} < 2 \times 10^{-28}$ *e cm*, respectively. The experimental sensitivity is expected to increase by roughly two orders of magnitude in the next 5 years.

The Standard Model possesses two possible sources of CP violation: the phase in the CKM quark mixing matrix, and the QCD vacuum angle θ_{QCD}. The CKM phase contributes to electric dipole moments only at three-loop order in perturbation theory and requires mixings through all three generations. As such, its contribution is highly suppressed and the Standard Model electroweak expectation for the neutron EDM is $d_n \sim 10^{-32}$ *e cm*. In contrast, the contribution from the QCD vacuum angle is $d_n \sim 3 \times 10^{-16}\, \theta_{QCD}$ *e cm*, which represents a potential background to any non-Standard model effects. However, this contribution can potentially be distinguished from new sources of CP violation in the weak sector by observing EDMs in atoms with paired versus unpaired electrons.

Extensions of the Standard Model often contain additional sources of CP violation that generally give large contributions to electric dipole moments. For example, supersymmetric theories generally possess a number of phases associated with the supersymmetry breaking sector, which contribute to EDMs at the one-loop level and exceed the experimental bound. The non-observation of EDMs to-date, thus place stringent limits on CP violating extensions of the Standard Model.

EDM experiments are small and inexpensive compared to the projects being considered in this report and hence are not under consideration by P5. Nonetheless P5 recognizes their unique contribution toward determining the origin of CP violation.

Appendix 1. U.S. Contributions to the LHC Experiments

U.S. Contributions to ATLAS

Subsystem	Specific deliverables
Pixels	Complete Mechanical Support system Design and production of electronics and optical links Assembly of detectors for the forward disks
Silicon Strip Detectors	Design and fabrication of front end chips Assembly of detector modules
Readout Drivers (RODs)	Design and fabrication of entire set of RODs
Transition Radiation Tracker (TRT)	Assembly of Barrel TRT Design and fabrication of front-end electronics Design and procurement of components of the RODs
Liquid Argon Calorimeter	Design and fabrication of Barrel Cryostat Design and fabrication of all Signal and High Voltage Feedthroughs Design and fabrication of the Barrel Motherboards Design of and fabrication of one half of the preamps Design and fabrication of the Front-end Boards and optical links Design and fabrication of the trigger sums Design and fabrication of the Forward Calorimeter (FCAL) Electromagnetic sections
Tile Calorimeter (TileCal)	Design and fabrication of Extended Barrel TileCal including the Intermediate TileCal, Cryostat Scintillators and Minimum Bias Trigger Scintillators Design and fabrication of all the Front-end electronics Purchase and test a fair share of photomultiplier tubes.

Muon Spectrometer	Design and fabricate 240 Inner and Outer Monitored Drift Tubes (MDTs) including the alignment system for the entire endcap Design and fabricate front-end electronics for the barrel and endcap including the Chamber Service Modules Design and fabricate Cathode Strip Chambers including all the electronics
Trigger/Data Acquisition	Design and fabricate Region of Interest Builder Contribute to the Level 2 hardware and software
Core Software	Developed overall ATLAS software framework called ATHENA Developed the Event Data Model Developed various aspects of database projects (this done to insure easy access to the huge amount of data when the beams collide) Developed a Production and Analysis system (PanDA) Developed the ATLAS Detector Description Contributions to the Open Science Grid (and predecessors)
Computing Facilities	Development and operation of the largest Tier 1 computing center in ATLAS Deployment and operation of five Tier 2 Computing centers

U.S. contributions to CMS

Subsystem	Specific deliverables
Hadron Calorimeter (complete system)	Design, construction from scintillators to data acquisition system, installation and operation
Endcap Muon System (complete system)	Design, construction from chambers to readout, installation and operation
Tracker Outer Barrel	Assembly and installation of 100 m^2 of silicon strips
Forward Pixels (complete system)	Design, construction from pixels silicon to readout installation and operation
Electromagnetic Calorimeter	Design and purchase of laser calibration system, optical data fiber purchase and installation
Trigger (complete system)	Design construction and commissioning of the calorimeter level 1 trigger
Data Acquisition System	Design and purchase of the event builder switch controllers
Magnet (complete system)	Design and purchase of the endcap steel magnet return yoke
Computing Facilities	Creation of the CMS national US Tier1 computing center at Fermilab and the 7 Tier 2 centers around the country
Core Software	Rewrite of the Event Data Model and the computing Framework
Analysis and operation centers	Creation of the LHC Physics Center and remote Operations Center at Fermilab

Appendix 2. Charge to P5, Membership, and Agendas for Public Meetings

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

January 23, 2006

Professor Abraham Seiden
Chair, P5 Subpanel of HEPAP
University of California, Santa Cruz
1156 High Street
Santa Cruz, California 95064

Dear Professor Seiden:

We would like to thank you and all the members of your P5 Subpanel for the hard work that you have already devoted to addressing the first set of questions that we asked you concerning the Tevatron and PEP-II programs. Your thoughtful advice will be extremely useful in our planning and will help us make the most of our physics program over the next few years.

We would now like to return to the original goal of P5, We would like you to propose a detailed roadmap for the U.S. high energy physics program for the period of roughly the next ten years, with particular focus on the decisions needed in the next five years, and mindful of the international context. This roadmap should lay out the most compelling scientific opportunities that can be addressed in that timeframe. In addition, we would like a specific prioritization of the major elements of the roadmap. This prioritization should assume a future yearly budget envelope which will be provided by the funding agencies. It should focus upon maximizing the physics return within the constraints, prioritizing the following elements as potential areas of study:

- Operations of existing facilities, consistent with your recommendations concerning the Tevatron and PEP-II
- U.S. contributions to LHC operations, computing, and upgrades
- The elements of a neutrino program under consideration by the Neutrino Scientific Assessment Group (NuSAG), including: neutrinoless double beta decay experiments, reactor experiments, off-axis detectors, and high intensity, long-baseline neutrino experiments.
- International Linear Collider
- Deep Underground Science and Engineering Laboratory (DUSEL) and associated scientific experiments
- Next-generation dark matter experiments
- Dark energy experiments

- Other major proposals which the agencies request to be included.

It is clear that all of the above initiatives are scientifically excellent, but that taken together, they will more than saturate the available resources; while all routes on the roadmap should be quite interesting, not all will be taken. The prioritization should therefore recommend, on the basis of their relative scientific merit, the relative priority and time ordering of projects; suggest which ones may require additional funding; and lay out decision points. You should consider the scientific potential of each initiative, the timeliness of its scientific output together with the likely costs to the U.S., and how well it fits into a coherent program (including the evolution of our facilities infrastructure). Where relevant, the Subpanel should consider the impact of potential program decisions taken elsewhere within the international HEP community, their relation to the programs of related fields such as nuclear physics and astrophysics, and their broader impact on science and society. The forthcoming National Academy of Sciences' EPP 2010 committee (Elementary Particle Physics in the 21st Century) will provide important strategic context to your roadmap, as well as other Scientific Advisory Groups that are assisting the High Energy Physics Advisory Panel (HEPAP) and the Astronomy and Astrophysics Advisory Committee (AAAC).

We will be happy to provide any additional information needed on schedules and costs of the various initiatives in each of these areas and any other information such as driving factors of our facilities infrastructure.

The DOE and the NSF would like a draft report regarding the prioritized projects and programs along with an updated roadmap for the U.S. particle physics program, by July 2006 with a final report by September 2006.

Thank you in advance for your dedication to addressing these important and challenging questions.

Sincerely,

Dr. Robin Staffin
Associate Director
Office of High Energy Physics
Office of Science
Department of Energy

Dr. Michael Turner
Assistant Director
Mathematical and Physical Sciences
National Science Foundation

cc: HEPAP Chair

P5 Members

Abe Seiden (UCSC) Chair
Hiroaki Aihara (University of Tokyo)
Andy Albrecht (UCDavis)
Jim Alexander (Cornell)
Daniela Bortoletto (Purdue)
Claudio Campagnari (UCSB)
Marcela Carena (FNAL)
William Carithers (LBNL)
Dan Green (FNAL)
JoAnne Hewett (SLAC)
Boris Kayser (FNAL)
Karl Jakobs (University of Freiburg)
Ann Nelson (U. of Washington)
Harrison Prosper (Florida State U.)
Tor Raubenheimer (SLAC)
Steve Ritz (NASA/GSFC & U. of Maryland)
Michael Schmidt (Yale)
Mel Shochet (U. of Chicago) (Ex-Officio)
Harry Weerts (ANL)
Stanley Wojcicki (Stanford U.)

Monday, March 27

9:00-9:45AM	DOE Comments & Discussion of Charge	R. Staffin, DOE
9:45-10:30	NSF Comments & Discussion of Charge	J. Dehmer, NSF
10:40-11:15	NSF Astronomy, Interagency Projects	D. Lehr
11:15-11:55	NASA, Interagency Projects	M Salamon/P. Hertz
12:00-1:30PM	LUNCH	
1:30-2:15	DOE Budget Discussion	G. Crawford, DOE
2:15-3:00	NSF Budget Discussion	R. Ruchti, NSF
3:15-4:15	g-2 Presentation	L. Roberts/W. Marciano
4:15-5:00	Discussion of g-2	J. Hewett
5:00-5:45	Roadmap Discussion	A. Seiden

Tuesday March 28

8:30-9:00AM	Report of ILC-LHC Group	H. Weerts
9:00-9:30	Report of Neutrino Group	S. Ritz
9:30-10:15	Status Report, Dark Energy Task Force	A. Albrecht
10:15-10:30	Group Discussion of Dark Energy Issues	
10:30-11:00	Report of Dark Matter Group	M. Carena
11:00-12:00PM	LHC Presentation	H. Gordon
12:00-1:30	LUNCH	
1:30-3:00	Budget Group Presentation & Discussion of How to Proceed, Excel Spreadsheet	D. Green
3:00	ADJOURN	

Tuesday, April 18

9:00-10:00AM	Fermilab Long-Term Plans	Pier Oddone
10:00-10:45	EXO Double Beta Decay Experiment	Giorgio Gratta
10:45-11:00	Break	
11:00-12:00	Daya Bay Reactor Experiment	Kam-Biu Luk
12:00-1:30PM	Lunch	2nd Floor Crossover
1:30-2:30	NOvA	Gary Feldman
2:30-3:15	DUSEL, Homestake Proposal	Kevin Lesko
3:15-4:00	DUSEL, Henderson Proposal	Chang Kee Jung
4:00-4:15	Break	
4:15-5:00	Neutrino Plans in Japan	Hiro Aihara
5:00-6:00	NUSAG Report	Peter Meyers (Closed)
6:00-7:00	Reception and Informal Community Discussion	2nd Floor Crossover

Wednesday April 19

8:30-9:00AM	DUSEL Science Agenda	Bernard Sadoulet
9:00-9:30	Combining Data on Dark Matter	Ted Baltz
9:30-10:10	CDMS and Other Cryogenic mK Detectors	Blas Cabrera
10:10-10:50	Xenon and Other Cryogenic Liquid Detectors	Elena Aprile
10:50-11:10	Committee Discussion of Neutrinos	(Closed)
11:10-12:00PM	Discussions of Neutrinos with Lab Management	(Closed)
12:00-1:00	Lunch	2nd Floor Crossover
1:00-2:00	Combined Neutrino Discussion	(Closed)
2:00-3:00	Dark Matter Discussion	(Closed)

Tuesday, April 20

8:00-8:30AM	Continental Breakfast	
8:30-9:00	Planning Activities in Europe	Karl Jacobs
9:00-9:45	Report from the AAAC	Garth Illingworth
9:45-10:30	ILC Update from the GDE	Barry Barish
10:30-10:45	Break	
10:45-11:25	US ILC Accelerator R&D	Jerry Dugan (by phone)
11:25-11:45	US ILC Detector R&D	Jim Brau
12:00-1:00PM	Lunch	
1:00-2:00	SNAP	Saul Perlmutter
2:00-3:00	Dark Energy Survey	Josh Frieman, Brenna Flaugher
3:00-3:15	Break	
3:15-4:15	LSST	Steve Kahn
4:15-4:45	Dark Energy Program in Japan	Hiro Aihara
4:45-5:15	Dark Energy Program in Europe	Stavros Katsanevas
5:15-6:00	SLAC Plans	Jonathan Dorfan
6:00-7:15	Reception and informal community discussion outside the ROB	

Wednesday April 21

8:00-8:30AM	Continental breakfast	
8:30-8:50	Status and Plans for PEP-II	John Seeman
8:50-9:15	Update on BABAR Physics and Prospects	David MacFarlane
9:15-9:30	Status of the Belle Program	Hiro Aihara
9:30-10:30	Discussion with SLAC management [closed]	
10:30-11:30	Report of the Dark Energy Task Force [closed]	
11:30-12:00PM	Executive session [closed]	
12:00-1:00	Lunch [Committee]	
1:00-3:00	Executive session [closed]	