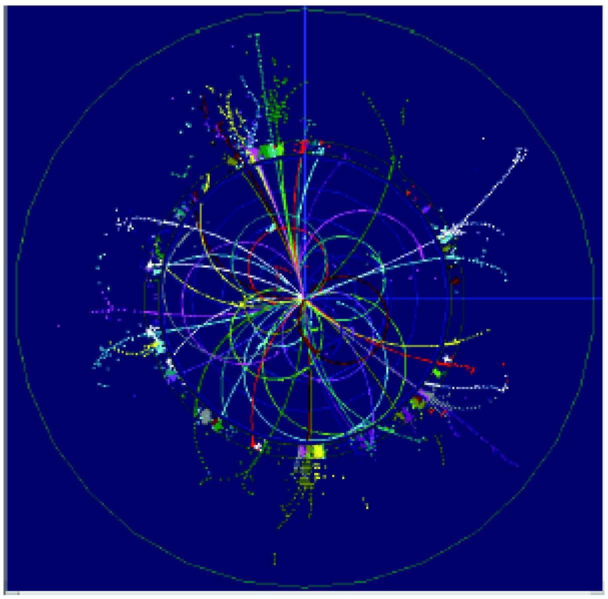
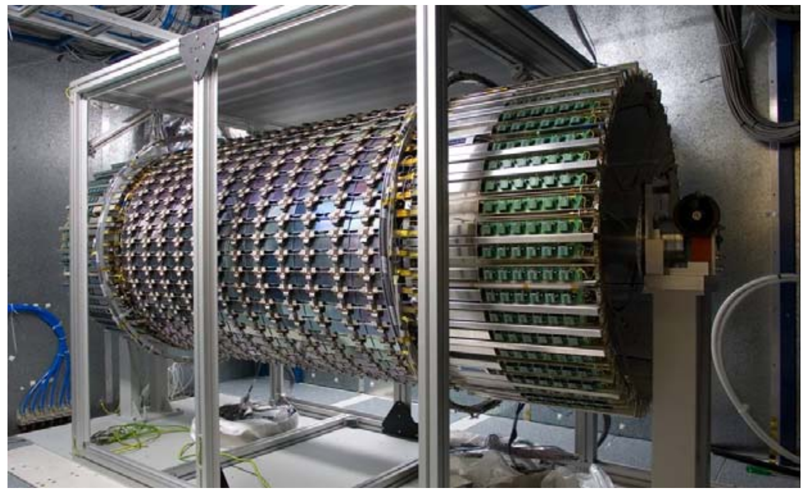
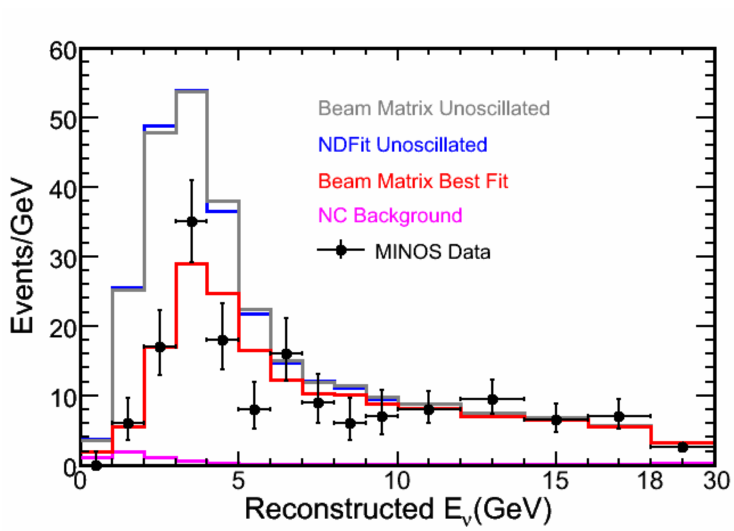
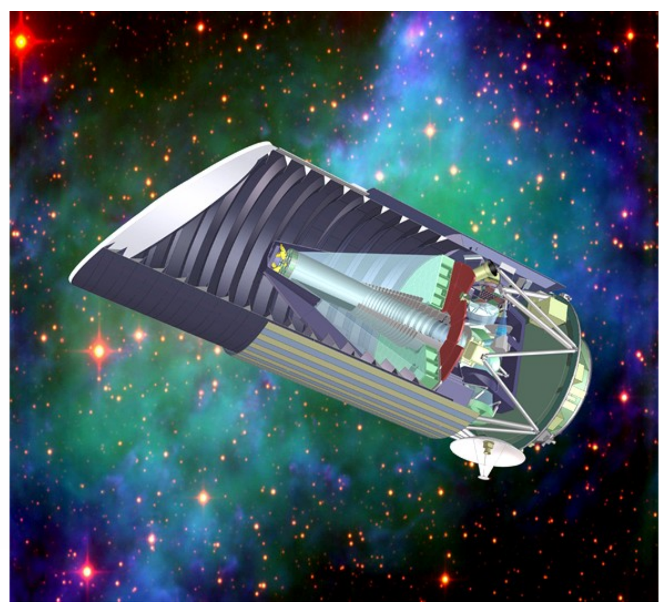
P5 Status Report: The Particle Physics Roadmap

November 2007









I. Introduction

The P5 Roadmap for Particle Physics, constructed in 2006, provides a ten-year plan for the key instruments of the field chosen for their potential to answer fundamental scientific questions. The Roadmap recommends 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. This update to HEPAP provides a status report on the Roadmap. It is based on a two-day meeting of P5 at Fermilab. The agenda for our meeting as well as the membership of the subpanel can be found in an Appendix.

Despite the remarkable progress in our understanding of the fundamental particles and forces over the past decades, many open questions remain that are key to our understanding of the universe and its evolution. We categorized these questions in the 2006 Roadmap 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?

We anticipate much progress in answering these key questions through the next generation of experiments. The major experiments were grouped into five categories in the 2006 Roadmap, as listed 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 are expected to 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 around 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.

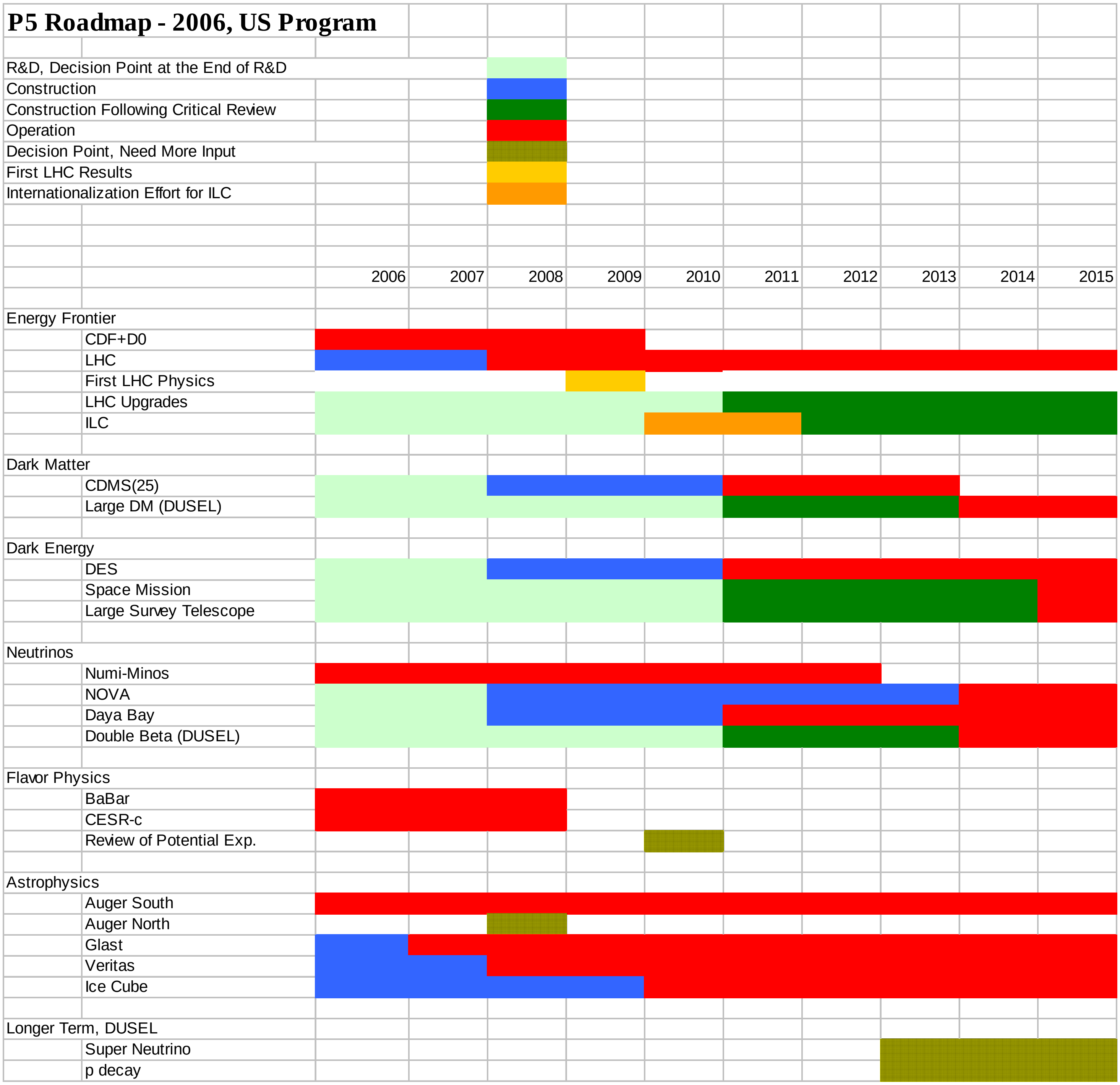
5) Precision measurements involving charged leptons or quarks. The study of these fermion systems has historically provided much of the information embodied in the Standard Model. Rare processes sensitive to potential new physics provide tests for and constraints on processes beyond the Standard Model. 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.

The detailed discussion of the science, planning guidelines, budget assumptions and specific recommendations can be found in our Roadmap report to HEPAP in October 2006. 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 gave us a base budget plan to work with. The numbers in this plan were as follows:

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

An alternative budget looked at assumed a 7% annual increase resulting in a doubling of the HEP budget over 10 years. This budget was approximately $50M per year larger than the base budget plan. We used 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.

A summary chart of the Roadmap as of 2006 is shown on the next page. In the subsequent section we start with an Executive Summary that is concerned with some developments since the 2006 Roadmap. It contains the explicit recommendations we are making to HEPAP. This is followed by very brief summaries on the status of several projects that we recommended for near-term construction starts and more detailed discussions on some of the longer-term items, which we recommended for future review. The latter depended in a number of cases on interagency agreements. As will be seen many of the near-term and long-term projects are making very good progress toward realization.


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

II. Executive Summary

We provide recommendations on three different topics in this summary. These topics are also discussed in some more depth later in the report. The first topic concerns the criteria that might lead to additional Tevatron running beyond FY09. The budget on which the 2006 Roadmap is based assumes that the funding from the shutdown of the Tevatron will be used for new projects, therefore additional running has budget implications. However, the potential for discovery at the Tevatron remains high and we therefore need to balance this against the need to move on to urgent new discovery physics in a number of areas. The second topic concerns JDEM (Joint Dark Energy Mission, a DOE-NASA partnership) where an NRC committee (the Beyond Einstein Assessment Committee) has recently made a recommendation for a first priority for a construction project in the Beyond Einstein NASA program area. The project recommended was JDEM, which figured prominently in our Roadmap for a construction start late in this decade. The last topic concerns the University program. A HEPAP subpanel (the University Grants Program Subpanel) made recommendations regarding the University program that were accepted by HEPAP in the summer of 2007. We have yet to include these recommendations into our P5 budget planning for the field.

1) Tevatron Running

The Tevatron continues to be the leading accelerator collecting data at the Energy Frontier. The preeminence of the Tevatron at the Energy Frontier will continue until the LHC experiments have collected sufficient amounts (for example 1 fb^{-1} of data for the Standard Model Higgs search) of good quality, well-understood data. This motivated our recommendation that the Tevatron continue running in 2009. At our meeting at Fermilab in September, we discussed the criteria for extending the Tevatron running beyond 2009. In this case an additional element in the discussion was the impact of additional data collection when a large amount of data had already been collected at the Tevatron. The experiments indicated that they had made significant improvements in triggering and analysis and that this trend would continue. This would give significant added value to additional data collected. The added data could be expected to allow further progress on the search for the Higgs boson, extending information over a larger mass range in the case of the simplest Higgs scenario, and providing further limits, or discovery, in the case of other models such as Supersymmetry.

Many of the improvements to the new physics searches would be best demonstrated in the Fall of 2008 after additional analysis has been completed on a larger data set. We therefore recommend that the option of continued running past 2009 be held open as a possibility and that the Fermilab management work with DOE on the implications of additional running. We recommend that funding for any additional running not come at the expense of our two highest priority areas on the Roadmap (the LHC program and ILC R&D, the program in Dark Matter and Dark Energy research, and the Daya Bay neutrino experiment) and that Fermilab in about a year carefully evaluate both the physics potential and manpower situation, which is likely very dependent on the LHC schedule, before requesting a final decision.

2) JDEM

For over two decades the DOE and the NSF have fostered research at the intersection of particle physics, astrophysics, and cosmology. The results from particle physics have often been complementary to the information resulting from astrophysical observations, as in the case of the present direct dark matter detection program, but some of the effort is also collaborative between these fields. This is particularly the case for research that has to be done using either large telescopes or in space. In the case of the DOE this research area in the past has been supported through a very small fraction of the HEP program budget. These DOE and NSF programs have produced very important physics, as exemplified by the sharing of the 2006 Nobel Prize in Physics by George Smoot, whose work on a NASA mission (COBE) was supported by the DOE.

The recent progress in understanding the geometry and history of space-time is an example of research that to date can only be done by looking out at the universe over vast distances and time scales, best studied through a collaborative effort between fields. We quote from our 2006 Roadmap:

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.

The NASA-DOE Joint Dark Energy Mission (JDEM), aimed at a thorough study of "dark energy" and its history, is an important part of our Roadmap. A National Research Council committee was recently charged to recommend which of the five missions in NASA's Beyond Einstein Program should proceed first for a construction start around FY09. This committee chose JDEM as its top priority. This selection demonstrates the importance of JDEM to the Astrophysics community and removes one of the major elements of uncertainty in moving ahead with JDEM.

JDEM will be a very large step in scale for DOE investment at the intersection of particle physics, astrophysics and cosmology, since the cost to DOE could be comparable to that of a major accelerator and detector construction project (for example the B factory and BaBar at SLAC). The science is very compelling and we recommend that the DOE try to secure the 10 year doubling budget mentioned earlier in our introduction above. We quote from our FY06 Roadmap regarding this budget:

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.

Given the NRC Panel recommendation, the chance for such a major science impact appears to be at hand if the additional funding can be secured. We recommend that DOE continue to work intensively with NASA toward an early realization of JDEM.

3) University Program

The University Grants Program Subpanel has provided a comprehensive evaluation of the status of the university program and its critical role for the future of the field. They note the much greater diversity than in the past of the science program and its associated experimental techniques, as is in fact evident from our Roadmap. The university community will need to strongly participate in innovation for the field and in creating novel science opportunities for this scientific program to prosper. Given the importance of international participation in this program, costs for travel are expected to continue to increase. These elements are being recognized internationally. As an example Germany has recently instituted a new program involving 17 universities and two laboratories to better exploit the LHC and ILC opportunities at the energy frontier. This program is funded at a level of about 5 million euros per year for five years. Finally we mention that the field's contribution to the training of scientific personnel for the long term economic health of our country lies foremost with the universities, but is based on participation in the major science opportunities available.

The NSF is continuing on a trajectory to double funding over a ten-year period. We quote from the House Appropriations committee regarding this year's NSF budget proposal:

This level of funding will support the doubling of the NSF budget in 10 years as part of a long-term, sustained commitment to investment in basic research and development, which provides the foundation for innovation and future technologies.

Combined with the NSF's strong commitment to base grants, these sentiments bode well for our communities continued contributions to science and society.

Given the evolution of the field and its science agenda we recommend that the DOE work toward an increase in base grants by an amount of at least 9%, as is consistent with the University Grants Subpanel recommendations. This should be among the highest priorities for the field. We hope that such an increase in investment can be the initial part of a sustained commitment to investment in basic research and development through the DOE supported university base grants program, which like the NSF program provides a significant foundation for innovation and future technologies.

Our original Roadmap did not consider such university issues. A concrete funding plan should now be made. We therefore recommend that P5 look at a possible phasing and make specific recommendations early next year regarding the increase in base grant support. At that time it will be appropriate to also evaluate new ideas from Fermilab regarding a high intensity proton machine (called Project X) and also a plan for future activities being generated at SLAC, in order to understand how these can be harmonized with the Roadmap plan. We anticipate that next year updated profiles for the new construction projects DES, Daya Bay, and NOνA should be available, as well as further plans for JDEM. These should all go into a new evaluation given funding projections.

III. Progress on Projects on the Roadmap

We first summarize our 2006 Roadmap recommendations, based on the base budget scenario listed earlier. These provide recommendations for major construction and R&D activities. The projects are grouped into several broad categories, and listed in priority order. The activities listed are meant to mainly fit into a five-year timeline, with subsequent decisions for construction on new large projects combined with data collection and analysis on running experiments providing approximately a decade of discovery science.

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.

Of the projects recommended for construction, the DES, Daya Bay and NOνA experiments are all moving ahead. Work on developing the experimental equipment is progressing, as are the various international aspects. All three projects have had their CD-1 reviews in 2007 and anticipate completing the CD-2 process in the next months. Significant data collection for these experiments can be expected to start in the FY2011 to 2012 timeframe.

The CDMS experiment continues to collect data in the Soudan mine. The 25kg SuperCDMS, an upgrade of the present experiment, would consist of seven supertowers of cryogenic detectors. There is now an agreement to move ahead with 2 of the 7 super-towers needed for the experiment. Progress is constrained by limitations in funding for a broad dark matter search program that simultaneously explores many techniques (as recommended by the Dark Matter Scientific Assessment Group), and the need for CDMS to continue to demonstrate technical progress. Nevertheless the limits on direct dark matter detection are continuing to improve significantly and the experiments are now exploring a very interesting regime of cross-sections sensitive to models of Supersymmetry. We can expect continuing progress from CDMS and the other search techniques.

Another category of projects that have to be placed underground, besides the direct dark matter detection experiments, are neutrino-less double beta decay experiments. This summer the Enriched Xenon Observatory (EXO) installed a 200 kg chamber, associated clean rooms and cryogenics underground in WIPP. Still to be installed in 2008 is the TPC detector and other elements of the detection system. It is important that EXO be funded to complete their detector and start a physics run for the next few years, initiating this promising approach. The science team will simultaneously continue their R&D on Barium tagging, required to make more sensitive measurements with a larger volume of Xenon.

We list below some of the highlights for the potential longer-term construction projects:

- The ILC Engineering Design remains on track with an expected completion date of 2010. A Research Director for the ILC detector program, S. Yamada, has recently been chosen.

- R&D for the LHC Luminosity Upgrade is moving ahead internationally. A construction plan is likely in the 2011 – 2012 timeframe, with extra support for developing this plan coming from the European Union. A detailed plan for the U.S. contributions, discussed in our 2006 Roadmap, is needed on the same time-scale.

- Planning for DUSEL is moving ahead on schedule with the Homestake mine chosen as the site. A possible start date is 2011, but this is dependent on a number of factors, among them the NSF MREFC process. An early small-scale experimental program can be launched in FY2009, given special funding from the state of South Dakota and private sources.

- JDEM has been recommended for NASA priority by an NRC committee, which will hopefully clear the way for rapid progress on this project. It could start construction in the 2009-2010 timeframe but will require a selection of the final concept for the mission.

- LSST has just completed its NSF Conceptual Design Review and has been recommended to proceed to a Preliminary Design Review next. LSST is likely to be recommended by the NSF to be advanced to the Readiness Stage by Spring 08. Considerable work has been done by the project to find additional resources, both from international science funding agencies and private foundations.

We discuss in some more detail below a number of these projects as well as our response to the University Grants Program Subpanel report. We also present an outline of the Fermilab Steering Group Report, including Project X, although this is not yet ready for a thorough evaluation by P5. Finally, we also present a status report of the Tevatron program, including some analysis of the potential progress on Higgs boson detection that might be possible with a 2010 run.

Tevatron Running in 2010

The Tevatron continues to be the leading accelerator exploring the Energy Frontier. The accelerator is performing well, and a large fraction of the Run II luminosity is yet to be delivered and analyzed, as seen in Figure 1. The preeminence of the Tevatron at the Energy Frontier will continue until the LHC experiments have collected sufficient amounts of good quality, well-understood data. Given the uncertainties in the LHC schedule and on the time needed for detector commissioning, it is still not clear when the LHC will surpass the Tevatron.

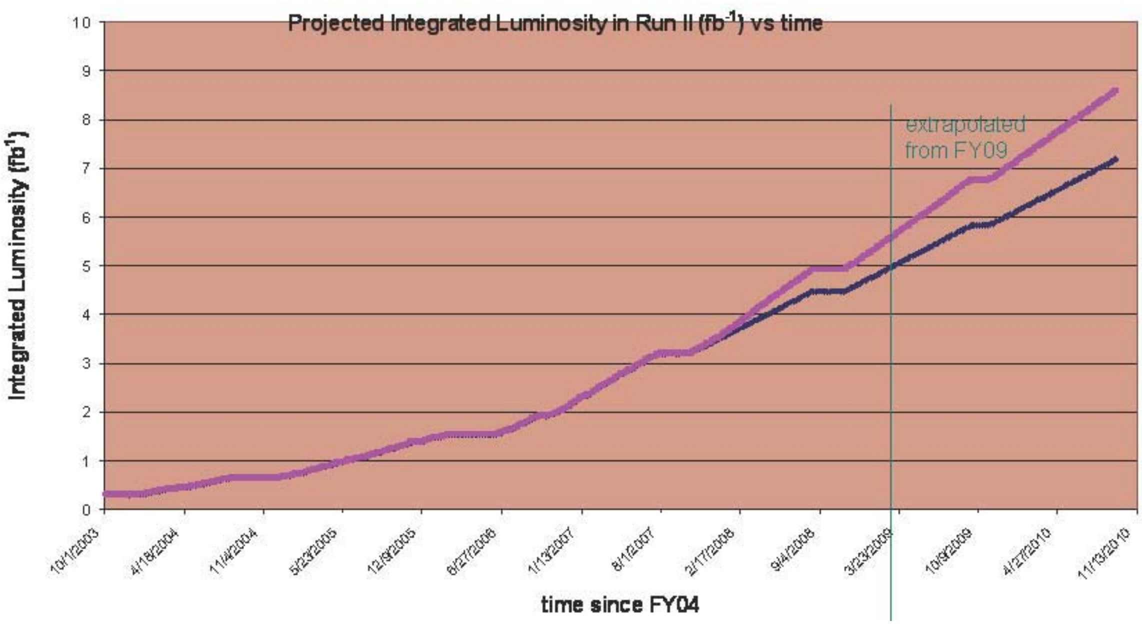


Figure 1: Range of Luminosity profiles for Run II of the Tevatron

At the July 2007 HEPAP meeting we made a recommendation that the Tevatron continue running in 2009. This recommendation was based on our sense that in 2009 the Tevatron will still be competitive and likely still ahead of the LHC. In September 2007 we met again at Fermilab to discuss the criteria for extending Tevatron running into 2010. At this meeting the lab management and the CDF and D0 collaborations made an impassioned case for this extension.

The main reason for extending the run into 2010 is the search for the Standard Model Higgs boson. While the increase in integrated luminosity from an additional year of running is modest (~ 7 fb^{-1} vs. ~ 5.5 fb^{-1}), this increase may be enough to just put the Tevatron over the “hump”, allowing exclusion of all masses below about 190 GeV, with varying sensitivity as shown in Figure 2, or possibly revealing a few standard deviation hint of a signal. Additional data will of course also allow for an extended reach in a number of beyond the Standard Model searches.

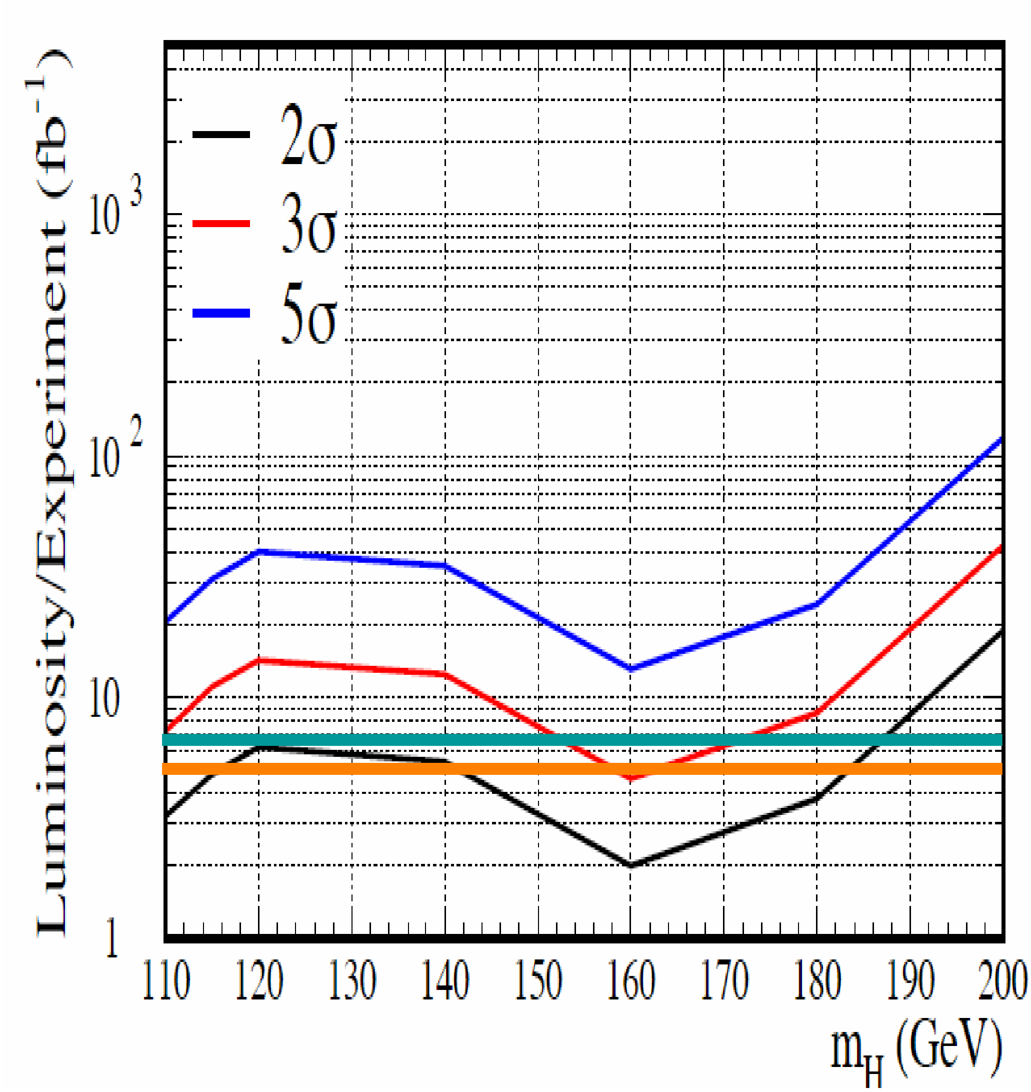


Figure 2: Projected Tevatron Higgs mass reach. Bottom (orange) line: 5.5 fb^{-1}; top (green) line 7.0 fb^{-1}. Three curves show best estimate of luminosity required for exclusion at 2, 3, or 5 standard deviations as a function of Higgs mass.

During the past year, the D0 and CDF collaborations have refocused and concentrated their resources towards the Higgs search. New triggers and new analysis techniques have been developed aimed at this very important goal. These efforts have paid off in improvements in sensitivity beyond simple square-root of luminosity scaling, as shown in Figures 3 (for a 115 GeV Higgs) and 4 (for a 160 GeV Higgs). Further improvements in analysis techniques are still being worked on.

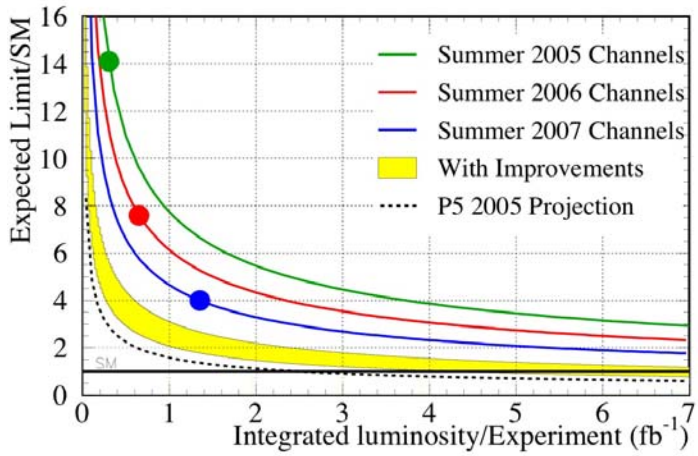


Figure 3: Expected 95% Confidence Limit on the Higgs production cross-section, in units of the SM cross-section, as a function of integrated luminosity for a Higgs mass of 115 GeV. The expectation is shown using the 2005, 2006, and 2007 analysis techniques. The yellow band shows the expectation using further improvements that are foreseen. The dashed line shows the expectation including all hoped-for improvements as presented to our committee in 2005.

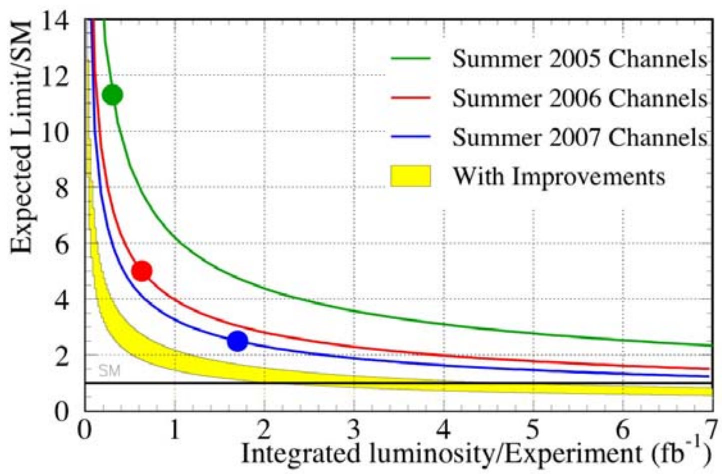


Figure 4: Expected 95% Confidence Limit on the Higgs production cross-section, in units of the SM cross-section, as a function of integrated luminosity for a Higgs mass of 160 GeV. The expectation is shown using the 2005, 2006, and 2007 analysis techniques. The yellow band shows the expectation using further improvements that are foreseen.

Because of the importance of the Higgs search, our committee recognizes that an extension in the running period may be warranted. Our recommendations are:

- The option of continued running past 2009 should be held open as a possibility and revisited in Fall 2008.
- Fermilab management should work with DOE on the implications of additional running.
- Funding for additional running should not come at the expense of the two highest priority areas in the Roadmap.
- The criteria for making a decision in Fall 2008 should include:
    - An examination of the manpower situation for the Tevatron program. This will undoubtedly depend on the LHC schedule.
    - Demonstration of significant progress in the analysis techniques that are needed to reach the desired level of sensitivity.

International Linear Collider (ILC)

Since our 2006 Roadmap recommendations there has been major progress in the ILC project. The Global Design Effort (GDE) published a Reference Design Report (RDR) in August 2007, which summarizes the physics, the accelerator and the detectors. The RDR provides a detailed technical description of the ILC, a 31 km long machine that would deliver a peak luminosity of ~$2x10^{34}$ cm^{-2} s^{-1} at the maximum center-of-mass energy of 500 GeV. The design also allows for an upgrade to a 50 km, 1 TeV machine. The cost estimate contains three elements: 4.80 Billion US$(2007) for shared value of the high technology and conventional components, 1.8 Billion US$ for site-specific costs, and labor totaling approximately 14,100 person-years. With the RDR completed the ILC project has reached another major milestone.

The GDE is now entering an engineering phase with the goal of delivering an Engineering Design Report (EDR) in 2010. To achieve this, the GDE has reorganized itself around a project management office consisting of three project managers. This central management team has been given the authority to set priorities and direct work towards the EDR. The GDE continues to work with the Funding Agencies for Large Colliders (FALC) to secure global resources for the engineering design.

The International Linear Collider Steering Committee (ILCSC) has appointed Sakue Yamada, Professor Emeritus from the University of Tokyo, as the Research Director, to coordinate the worldwide detector design and

R&D efforts. In October 2007, the ILCSC announced a call for Letters of Intent (LOIs) for the ILC detectors, to be submitted by October 2008. Yamada will start the process of reviewing the LOIs by an advisory panel, resulting in the selection of two detector concepts/collaborations capable of preparing engineering designs by 2010.

The physics case for the ILC remains strong and based on the technically driven GDE schedule, the world high-energy community is proceeding to prepare for a possible start of construction in 2012. Such a construction start date would require supporting physics results from the LHC, a global agreement to start, and the selection of a site for the ILC.

Fermilab Steering Group Report and Project X

During the spring of 2007, Fermilab charged a steering group to develop a roadmap for the accelerator-based high energy physics program at Fermilab and address a question posed by Dr. Ray Orbach at the February 2007 HEPAP meeting: "*… If the ILC were not to turn on until the middle or end of the 2020s, what are the right investment choices to ensure the vitality and continuity of the field during the next two to three decades and to maximize the potential for major discovery during that period? …*" The steering group's goal was to build their roadmap based on the recommendations of the EPP2010 National Academy of Sciences report and the P5 subpanel of HEPAP, in the context of the global particle physics program. Specifically the aim was to develop a strategic plan that:

1. Supports the international R&D and engineering design for as early a start of the ILC as possible and supports the development of Fermilab as a potential host site for the ILC;
2. Develops options for an accelerator-based high-energy physics program in the event the start of the ILC construction is slower than the technically-limited schedule; and
3. Includes the steps necessary to explore higher energy colliders that might follow the ILC or be needed should the results from LHC point toward a higher energy than that planned for the ILC.

The steering group was composed of particle and accelerator scientists from Fermilab and the national community and was chaired by Fermilab Deputy Director Young-Kee Kim. The resulting plan was released in September

2007 and can be found at: http://www.fnal.gov/pub/directorate/steering/pdfs/SGR_2007.pdf.

The plan describes Terascale physics with the LHC and the ILC as Fermilab's highest priority. While supporting ILC development, the plan also would create opportunities for exciting science at the intensity frontier, particularly in the areas of neutrino science and precision measurements involving charged leptons or quarks. If the ILC remains near the Global Design Effort's technically driven timeline with a decision to construct early in the next decade, Fermilab would continue neutrino science with the NOνA experiment, using the NuMI (Neutrinos at the Main Injector) proton plan, which is scheduled to begin operating in 2011. If the ILC construction is somewhat delayed, the steering group plan proposes SNuMI, an upgrade of NuMI to create a more powerful neutrino beam that is based on modest improvements to the existing Fermilab complex. If the ILC start is postponed significantly, the steering group plan calls for building an intense proton facility referred to as Project X. Project X consists of a superconducting linear accelerator, having characteristics similar to that of the ILC, combined with the existing Recycler Ring and the Main Injector accelerator; it is illustrated schematically in Fig. 5.

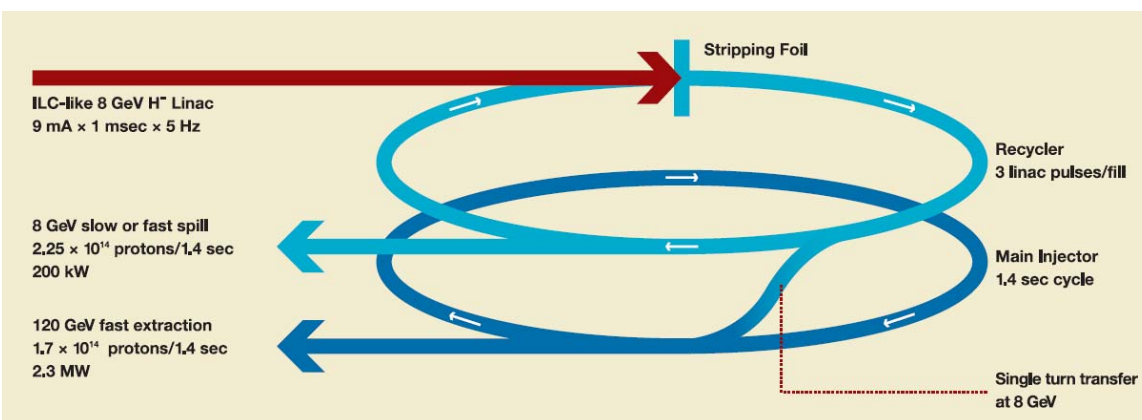


Figure 5: Schematic of Project X accelerators; from the Fermilab Steering Group Report, September, 2007.

The major component of Project X is the superconducting 8 GeV linac. Many of the technical components would be the same or similar to those used in the ILC at a scale of about one percent of a full ILC linac. Thus, building the Project X linac would offer substantial support for ILC development by accelerating the industrialization of ILC components in the U.S. and creating an engineering opportunity for ILC cost reduction and early risk mitigation. The project offers an early and tangible application for ILC R&D in superconducting technology, attracting participation from accelerator scientists worldwide and driving forward the technology for still higher-energy accelerators of the future, such as a muon collider.

Project X would provide a large increase in the proton beam power available for experiments at Fermilab; the beam power versus proton energy is illustrated in Fig. 6 for Project X, SNuMi, and NuMi. The steering group report also briefly outlines the science case for Project X. This includes enhancing Fermilab's long-baseline neutrino oscillation experiments and providing for future experiments to measure the neutrino mixing angles, mass hierarchy, and determine whether there is CP violation in the neutrino sector. The new accelerator would also provide an opportunity for new, smaller-scale experiments using intense beams generated by 8 and 800 GeV protons that would complement the long-baseline neutrino program, for example by using muons or kaons to probe physics beyond the standard model in precision experiments. A community workshop is planned to explore the utilization of Project X capabilities, which should help to develop and articulate the physics case for consideration by the next P5.

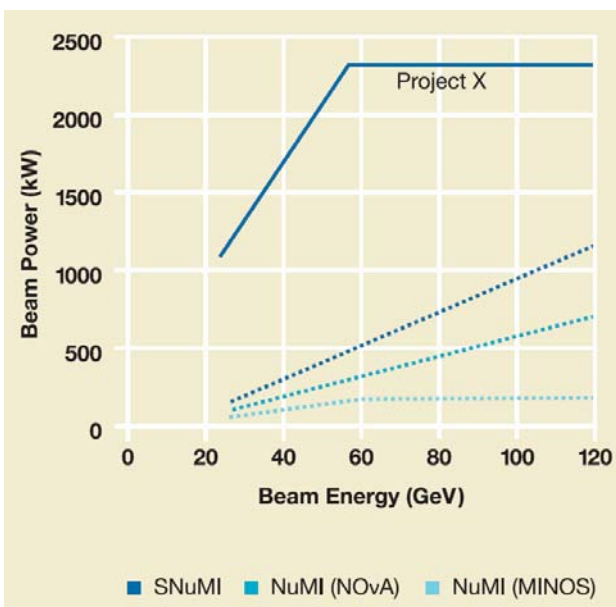


Figure 6: Proton beam power versus beam energy for Project X, SNuMi, and NuMi; from Fermilab Steering Group Report, September, 2007.

The steering group notes that a decision between the different options outlined in the report cannot be made until early in the next decade and that these programs must be considered in the context of the full particle physics program. It was estimated that Project X would be a billion dollar-class facility, but detailed estimates of the resources needed have not yet been developed. Such estimates will be needed for a construction project as will a more detailed plan of the physics goals. To prepare for a future decision, the steering group recommended that the laboratory seek R&D support to produce a complete design for Project X.

Dark Energy

Since our roadmap report and the Dark Energy Task Force (DETF) report, the case for studies of dark energy has remained compelling, and we strongly reaffirm our recommendations in this area. There have been some important developments.

First, NASA and DOE together charged an NRC study on the priorities for NASA's Beyond Einstein program, and that group issued its report in early September 2007. Beyond Einstein is NASA's roadmap program for five proposed missions at the boundary between physics and astronomy. The five missions are the Joint Dark Energy Mission (JDEM), the Laser Interferometer in Space Antenna (LISA), the Inflation Probe, the Black Hole Finder Probe, and Constellation-X. We take as a very positive sign of cooperation that NASA worked with DOE when seeking advice about its own program. There were several very important recommendations, but the first recommendation was a clear endorsement of JDEM as the highest priority: "*Recommendation 1. NASA and DOE should proceed immediately with a competition to select a Joint Dark Energy Mission for a 2009 new start. The broad mission goals in the Request for Proposal should be (1) to determine the properties of dark energy with high precision and (2) to enable a broad range of astronomical investigations. The committee encourages the Agencies to seek as wide a variety of mission concepts and partnerships as possible.*" This clears the way for NASA and DOE to develop a JDEM near-term implementation plan, including the process for selecting the JDEM mission concept. Three separate JDEM concepts were presented to the NRC panel (ADEPT, DESTINY, and SNAP) and there may be more competing for selection.

JDEM will be a very large step in scale for DOE investment at the intersection of particle physics, astrophysics and cosmology, since the cost to DOE could be comparable to that of a major accelerator and detector construction project (for example the B factory and BaBar at SLAC). The science is very compelling and we recommend that the DOE try to secure the doubling budget mentioned in the introduction to this report. We quote from our FY06 Roadmap regarding this budget: "*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.*"

Given the NRC Panel recommendation, the chance for such a major science impact appears to be at hand if the additional funding can be secured. We recommend that DOE continue to work intensively with NASA toward an early realization of JDEM.

There have also been important developments on the Large Synoptic Survey Telescope (LSST), a ground-based project planned to meet the Dark Energy Task Force (DETF) "Stage IV" criteria. The 2006 P5 roadmap recommended that DOE and NSF work together to advance this project to the next stages of design review. The LSST project has made major advances since then. We understand it had a successful NSF Conceptual Design review in September 2007 and it could be advanced to Readiness Stage by Spring 08. International participation in the R&D has expanded, including French scientists funded by CNRS. Considerable work has been done by the project to find additional resources, both from international science funding agencies and private foundations. We reiterate our recommendation that both NSF and DOE work to advance this project forward and we applaud the progress over the last year.

The Dark Energy Survey is a "Stage III" ground-based dark energy project that will make significant advances in our knowledge of the cosmological expansion history, and it will also acquire technical experience that will be critical to making the most of DETF Stage IV projects. Our 2006 roadmap recommended that the agencies proceed with this project and we are pleased that major progress has indeed been made on DES. The DOE and the NSF formed a Joint Oversight Group for DES, which has been meeting frequently since late May. A joint DOE-NSF review of DES was held at Fermilab in May, and the Review Committee recommended DOE CD-1 approval for the DECam project, which has occurred. The NSF has provided initial support for data management.

Deep Underground Science and Engineering Laboratory (DUSEL)

The report *Deep Science* (2006), resulting from an NSF-sponsored study, recommends strong U.S. support for the pursuit of deep underground science, and, in particular, for the creation in the U.S. of a world-class Deep Underground Science and Engineering Laboratory (DUSEL). In July 2007, the NSF announced a site selection: Should it decide to construct a DUSEL, the site will be the Homestake Mine in Lead, South Dakota. The DUSEL at Homestake is pictured as including laboratories at 300, 4850, and a deep facility extending from 7400 to 8000 ft. below the surface. Funds are being provided to the team that proposed the Homestake site to enable it to create a DUSEL design ready for consideration in the NSF Major Research Equipment and Facilities Construction (MREFC) competition. The earliest construction start would be in FY11. Actual initial funding needs will be determined as the design and experimental program mature but, for planning purposes, the NSF currently uses a rough target of $500M, with half going to the facility and half to the experiments. Candidate experiments include ones on dark matter detection, neutrino-less double beta decay, nuclear astrophysics, geo, solar, and supernova neutrinos, long-baseline neutrinos, and proton decay. R&D on how to accommodate a Megaton-scale neutrino and proton-decay detector would be included in the initial construction funding. As is always the case at the NSF, the MREFC would not cover operating expenses for DUSEL. The NSF Mathematical and Physical Sciences Directorate has agreed to shoulder the costs for operating the facility on its own, without contributions from the other NSF Directorates. It is generally agreed that the costs for operating the experiments themselves – whether in geosciences, engineering, biology, or physics – would be borne by the corresponding Directorate or Division.

The underground laboratory at Homestake has attracted considerable support from local sources: $70M from T. Denny Sanford, a private donor, $44M from the state of South Dakota, and $10M from another source. These funds, being administered by the South Dakota Science and Technology Authority, make possible the re-opening of the Homestake Mine and early work on its conversion into a science and engineering laboratory. Beneficial occupancy of the 4850 ft. level is anticipated by the end of calendar 2008, well in advance of any Federal DUSEL construction funding.

The precise mechanism through which the initial suite of experiments would be selected is still under consideration at NSF. Hopefully, whatever mechanism is chosen will entail considerable community involvement. While the initial suite would not include a large neutrino and proton-decay detector, there is an obvious complementarity between the interest in an intense long-baseline beam of neutrinos from Fermilab, and the interest in a DUSEL that could house the detector of these neutrinos.

Dark Matter

A broad program to understand the nature of dark matter ranked very highly in the P5 Roadmap Report and we reaffirm the importance of this effort. The steps taken toward the creation in the U.S. of a world-class Deep Underground Science and Engineering Laboratory (DUSEL) are of great relevance for the dark matter program. Such a laboratory will provide an ideal location for the next generation of ton scale dark matter direct detection experiments.

There are three approaches to the question of the nature of dark matter, all of which must be pursued if a complete and unambiguous understanding is to be achieved. These include the production of dark matter particles in the collider program, the direct observation of collisions between dark matter particles and atomic nuclei, and indirect signals of the annihilation of dark matter particles in the cosmos. The status of the collider programs, including the development of a technical design for the ILC, are discussed elsewhere in this report. We also look forward to the launch of the GLAST satellite early in 2008 and the contributions it will make to the search for indirect signals of dark matter annihilation. The direct search for axions, one of the dark matter candidates, is also continuing.

Recently, much progress can be seen in direct dark matter detection experiments, both consolidating known techniques and developing new ones. We discuss a few examples below.

CDMS is a cryogenic experiment currently collecting data in the Soudan mine. P5 endorsed an upgrade to the 25kg SuperCDMS, consisting of seven Supertowers. Subsequently, the Dark Matter Scientific Assessment Group (DMSAG) endorsed an initial phase of SuperCDMS with two Supertowers in the existing CDMS structure, with provisional recommendation for the full detector at SNOLAB contingent on the achievement of specific benchmarks. DOE and NSF are now moving ahead with funding for two Supertowers. The current experiment, CDMS II, is collecting data. These data, when published, are expected to provide the greatest sensitivity of any current experiment. The cryogenic technology employed by CDMS is now well established by several years of R&D and running experience. It may, therefore, provide one of the most reliable paths to the terrestrial observation of dark matter.

The XENON-10 experiment is a two-phase noble gas experiment installed in Gran Sasso and funded in part by the NSF and the DOE. It utilizes a new liquid-TPC technique first established in 2003. In a relatively short time, a first working detector was constructed with an active mass of 15 kg. A blind analysis completed this spring revealed ten events, which subsequently were shown to be typical background events, resulting in a limit on the order of 10^{-44} cm^2 for a WIMP mass of 100 GeV. The collaboration is constructing a larger detector with an active mass of 70 kg and components such as photomultiplier tubes with a lower natural activation. This detector should have ten times the sensitivity of the current one.

WARP is a relatively new experiment based on a mass of two-phase liquid Argon. The collaboration has made rapid progress in the last year. WARP is funded jointly by the NSF and the INFN in Italy, and is installed in Gran Sasso. A 3.2 kg prototype has been operated successfully, allowing studies of the discrimination of nuclear recoils from minimum-ionizing particles. A very good discriminating power, better than 10^{-8} has been achieved. The WARP Collaboration is now constructing a 140 kg detector at Gran Sasso, which will be commissioned in 2008. For a detector with a much larger mass, a sample of Argon depleted in its radioactive isotope would allow better sensitivity. The WARP collaboration has obtained a 5 kg mass that should be depleted by a factor of fifty, which they are working to verify. They have also announced recently the discovery of a first source of underground naturally depleted Argon, which could provide an affordable way of gathering tons of this material, relevant for the construction of a ton scale detector.

A novel ``bubble-chamber'' technique has been developed for the COUPP experiment, which has been installed underground in the NUMI beam line at Fermilab. A superheated fluid will produce a single bubble if a dark matter particle collides with an atom, in contrast to the multiple-bubble signature of a thermal neutron. Photons and electrons do not produce bubbles at all because the ionization they produce is insufficient, so a very robust and complete suppression of these primary backgrounds is achieved. An upgrade is planned which will more completely eliminate α-backgrounds from radon.

In parallel with cryogenic experiments and noble gas experiments, other novel techniques show promise and may play an important or even crucial role at a later date.

University Grants Programs

The University Grants Program Subpanel has provided a comprehensive evaluation of the status of the university program and its critical role for the future of the field. They note the much greater diversity than in the past of the science program and its associated experimental techniques, as is in fact evident from our Roadmap. The university community will need to strongly participate in innovation for the field and in creating novel science opportunities for this scientific program to prosper. Given the importance of international participation in this program, costs for travel are expected to continue to increase. These elements are being recognized internationally. As an example Germany has recently instituted a new program involving 17 universities and two laboratories to better exploit the LHC and ILC opportunities at the energy frontier. This program is funded at a level of about 5 million euros per year for five years.

More broadly, we note that the field's contributions to the training of scientific personnel for the long term economic health of our country lie foremost with the universities and are based on participation in the major science opportunities available. The importance of increasing such contributions has been given special recognition in major policy documents such as the Augustine report and the EPP2010 report and form a major part of the case made to the U.S. Congress for increased funding for research in the physical sciences. We quote from the EPP2010 report "*The success and vitality of the scientific enterprise depend on a distinctive set of institutional arrangements for training new scientists. The committee views the current role of university-based students, postdoctoral researchers, and faculty as a critical component of the particle physics enterprise that strengthens* *national capabilities in both education and science.*" The nation's major research universities have recognized these areas of common vision and interest with the funding agencies and have successfully supported (for example through consortia such as the Association of American Universities, The Science Coalition, and others) more robust funding for the physical sciences. The result has been support from both the executive and legislative branches of the government for strong growth in research budgets at the NSF and the DOE Office of Science.

The Experimental Particle Physics (EPP) and Particle and Nuclear Astrophysics (PNA) portfolios of the NSF continue to be impressively diverse and cover most of the key science areas outlined in our introduction. The EPP portfolio includes support for university scientists working on hadron physics (CDF, D0, CMS, ATLAS and LHCb), electron-positron physics (CLEO-c and BaBar), as well as neutrino physics (MINOS, NOνA, MINERνA, and MiniBooNE). The PNA portfolio includes work on dark matter (CDMS, COUPP, XENON10, DRIFT-II, ZEPLIN-II), the ultrahigh energy universe (HiRes/TA, Pierre Auger, VERITAS, and MILAGRO), and neutrinos (Double Chooz, Super-K, Borexino, and CUORE). Most of these projects are carried out in a successful collaboration with the DOE. Many are small and therefore have not explicitly appeared in our P5 Roadmap of larger projects.

Presentations to P5 on the NSF budget indicate that spending of budget increases at the NSF includes support for the training of scientific personnel at universities. We recognize the NSF position as good progress toward meeting the needs of our field and also consistent with the arguments made to increase funding in the first place. We quote from the House Appropriations committee regarding this year's NSF budget proposal: "*This level of funding will support the doubling of the NSF budget in 10 years as part of a long-term, sustained commitment to investment in basic research and development, which provides the foundation for innovation and future technologies.*"

In this regard the situation at DOE HEP gives cause for concern. Despite annual increases in the HEP budget of 4-5% for 2007 and 2008 (planned) the university program remains essentially flat. As a result immediate needs related to the LHC (especially student travel) as well as the longer-term goals outlined for the field are under unreasonable pressure. In addition the DOE HEP program risks being seen as unable to deliver on the promise of increased investment in the training of scientific personnel that has been a strong motivator for the renewed emphasis on the physical sciences.

Given the evolution of the field and its science agenda we recommend that the DOE work toward an increase in base grants by an amount of about 9%, as is consistent with the University Grants Subpanel recommendations. This should be among the highest priorities for the field. Our original Roadmap did not consider such university issues. A funding plan should now be made. We therefore recommend that P5 look at a possible phasing in of the required additional support and make concrete recommendations early next year as part of an overall budget examination. We hope that such an increase in investment can be the initial part of a sustained commitment to investment in basic research and development through the DOE supported university base grants program, which like the NSF program, provides a significant foundation for innovation and future technologies.

Appendix

Agenda: P5 Meeting at Fermilab Sept 24 and 25

Monday, September 24:

8:15 – 9:00	**Executive Session**
9:00 - 9:30	**Pier Oddone, Tevatron and Fermilab Planning**
9:30 – 10:15	**D0 Presentation, Tevatron Running Past FY09**
10:15 – 11:00	**CDF Presentation, Tevatron Running Past FY09**
11:00 – 12:15	**Executive Session**
	12:15 – 1:30 Lunch
1:30 – 2:00	**Pier Oddone, Longterm Planning**
2:00 – 3:00	**Y.Y. Kim, Steering Committee Report**
3:00 – 3:45	**John Corlett, Accelerator Advisory Committee Report**
	3:45 – 4:15 Break
4:15 – 5:00	**Mike Witherell, Beyond Einstein NRC Report**
5:00 – 5:30	**Glen Crawford, Budget Projections**
5:30 – 6:30	**Executive Session**

Tuesday, September 25:

8:30 – 9:15	**Barry Barish, Report from GDE**
9:15 – 9:45	**Jon Kotcher, DUSEL Status**
9:45 – 10:15	**Jim Whitmore, Dark Matter Program**
10:15 – 10:45	**Giorgio Gratta, EXO Status Report**
	10:45 – 11:00 Break
11:00 – 11:30	**Jim Whitmore, Base Grant Status at NSF**
11:30 – 12:00	**P.K. Williams, University Base Grants and Projects**
	12:00 – 1:00 Lunch
1:00 – 2:15	**Executive Session**
2:15 – 2:45	**Closeout with Lab Management**

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)

Michael Schmidt (Yale)

Mel Shochet (U. of Chicago) (Ex-Officio)

Harry Weerts (ANL)

Stanley Wojcicki (Stanford U.)