July 8, 2011

Fermilab Physics Advisory Committee Meeting
June 21-26, 2011 – Snowmass, CO

Comments and Recommendations

(Charge elements given in italics)

Overview

The Fermilab Physics Advisory Committee (PAC) met to discuss the state of the Laboratory and its role in current and planned projects as well as to consider a Letter of Intent from the MINERvA Collaboration and a Proposal to extend MINOS running in the NOvA era.

The discussions of the Committee last summer were dominated by discussions concerning the Energy Frontier and the decision whether to extend Tevatron running for three years. This summer's discussion revolved a great deal around the Intensity Frontier and Fermilab's current and future neutrino program. The decision by the National Science Foundation (NSF) to no longer support the Deep Underground Science and Engineering Laboratory (DUSEL) has obvious impact on Fermilab's plans for the Long Baseline Neutrino Experiment (LBNE) and possible siting of direct dark-matter search experiments at DUSEL. To help the Committee with these discussions, some members of the DUSEL Program Advisory Committee joined the Fermilab Physics Advisory Committee for the first two days of the meeting. The Fermilab Physics Advisory Committee greatly appreciated the DUSEL Committee members' expert input, advice, and insights as part of the discussions and sincerely thanks them for their participation.

The meeting was also characterized by a number of inputs to the discussion being very much in a state of flux. The report of the review committee commissioned by the National Research Council to review the physics of DUSEL was not public at the time of the meeting. The Marx-Reichanadter Committee, charged by the Department of Energy (DOE) to determine cost estimates of various options for an underground facility at Homestake was available on Thursday of the meeting, with the Committee attending its presentation to HEPAP by videoconference. Promising and exciting new constraints on the key neutrino mixing parameter θ_{13} were on hand, with results from the T2K Collaboration released during the planning of the summer meeting and from updated results from the MINOS Collaboration reported on the Friday of the meeting.

Fermilab's continued leading role in exploring dark energy as part of the Cosmic Frontier was also discussed as well as other aspects of the Intensity Frontier. Excellent progress from the MINERvA and NOvA projects bodes well for the Fermilab near-term neutrino program. The Energy Frontier remains vibrant, with the CDF and DZero Collaborations continuing with an excellent array of physics results from the Tevatron's last year of operation, and CMS's ongoing spectacular operation and results from collisions at the highest energies at the CERN Large Hadron Collider.

The Committee warmly thanks the proponents of the various projects for thorough and informative presentations and documentation made available to the Committee, with impressive physics results from many Fermilab experiments across the board.

<u>Long-Baseline Neutrino Oscillations – Joint Meeting with Some DUSEL PAC Members</u>

The first day of the full Fermilab PAC meeting will include members of the DUSEL PAC. Presentations will be made on the status of the Sanford Laboratory and the Deep Underground Science and Engineering Laboratory (DUSEL), the Long Baseline Neutrino Experiment (LBNE), and the two national committee reviews in this research area.

The meeting occurs during a time of transition for US long-baseline neutrino experiment planning, with the NSF no longer planning to support DUSEL. The way forward is not yet established. We ask that, together, the two committees' members discuss the following issues and provide comments:

What will it take to keep the US program in long-baseline neutrino oscillations area vital?

What should be Fermilab's role in this effort?

In this context, also comment on the schedule and processes appropriate for decisions on the site and cavern depth for a far neutrino detector and the schedule and processes for various technology decisions.

Long-Baseline Neutrino Experiment Science, Strategy and the Role of Fermilab

The Fermilab Physics Advisory Committee reaffirms the high scientific priority of a long-baseline neutrino oscillation program that explores major aspects of the leptonic sector, in particular the neutrino mass hierarchy and CP violation. Baselines long enough for matter effects to be significant allow measurements of the detailed oscillation pattern to simultaneously determine the mass hierarchy and CP phase angle. The exact reach of this program will depend on the value of $\sin^2(2\theta_{13})$ which is still unknown. The recent data from T2K and MINOS indicate that the value of $\sin^2(2\theta_{13})$ could be of order 0.1, which if confirmed would make the case for a long baseline experiment even more compelling (see below).

The combination of the Fermilab accelerator complex, the availability of the Homestake site, and the scientific and technical expertise at national laboratories and universities place the US and Fermilab in a unique position worldwide to successfully host and lead a long-baseline neutrino experiment. The large intensity provided by Project X would further increase the reach of such a program. The underground siting of a far detector would enable other important measurements such as proton decay as well as the detection of astrophysical neutrinos.

Given the 10-year construction time-scale of a long-baseline experiment and the competitive international environment, the Physics Advisory Committee believes that there is a clear strategic advantage in proceeding expeditiously with the program, even without full knowledge of the value of $\sin^2(2\theta_{13})$. It offers the best hope to trigger a convergence of the international community on this program. The creation of a deep-underground national facility at Homestake would offer opportunities for synergy with other physics programs such as those on dark matter and double beta decay.

Future long baseline experiments using conventional neutrino beams offer the opportunity to discover CP violation in the lepton sector, provided that $\sin^2(2\theta_{13})$ is greater than 0.01. If θ_{13} is in the range currently indicated by T2K and MINOS, as might be confirmed over the next two years, such a program will be extraordinarily rich. In some parts of the phase space, NOvA and T2K may be able to give early indications of CP violation, but a dedicated program with a greatly more sensitive detector would be necessary for a definitive measurement.

If $\sin^2(2\theta_{13})$ is smaller than 0.01, then the determination of mass ordering and CP violation becomes very difficult and possibly beyond the reach of conventional neutrino beams. Nonetheless, it would still be important to attempt to measure this key parameter to guide the scale of the next generation of even more challenging neutrino factories or beta beams. To our knowledge, there is no alternative way to achieve this that would be competitive during the considered time frame.

Given the new information from T2K and MINOS, the Laboratory should further study implications of a value of $\sin^2(2\theta_{13})$ around 0.1 for the NuMI/NOvA program and potential upgrades.

The LBNE Project

LBNE is a next generation long-baseline neutrino experiment with a flight path of 1300 km, well beyond the baselines of T2K and NOvA. Its primary goal is to probe with unprecedented sensitivity the CP-violating phase and neutrino mass hierarchy, by measuring the appearance probability of electron neutrinos and antineutrinos in corresponding muon neutrino beams. Two complementary technologies, water Cherenkov and liquid argon, are presently being considered for the far detector.

The LBNE Collaboration has done a good job of developing the physics case. The Committee recommends that more detailed studies of the implications of large values of $\sin^2(2\theta_{13})$ be performed.

Also, the Committee would like to see a more detailed physics case if T2K, NOvA, and reactor experiments were to fail to find conclusive evidence for θ_{13}. In this case, several questions arise: what would be the gain of LBNE compared to an extended NOvA program, what would be the role of Project X, and what would be the ultimate limit on $\sin^2(2\theta_{13})$ expected for LBNE?

The Committee commends the Collaboration and the project team for the progress made in the definition of the project in an uncertain environment. The contributions of the DUSEL facility team have also been critical. Further improvement of the alignment between the LBNE project team and the Collaboration is important at this phase of the project, in particular at the critical stage of technology choice.

Given the complexity and the scope of the project, the size of the Collaboration remains a concern. This may be due to the recent political and technical uncertainties about the project. The Committee encourages the Collaboration to seek additional national and international collaborating institutes.

Given the scientific importance of the LBNE primary goal, the Committee believes that LBNE is the best way for Fermilab to maintain a strong leadership role in neutrino physics. The Committee notes that the construction decision (CD-3) should occur at a time when more precise information about the value of $\sin^2(2\theta_{13})$ will be available.

LBNE and Homestake

There are strong arguments to site the far detector of a long baseline neutrino experiment deep underground. The water Cherenkov technology, which can only operate at a depth of at least 1000 m.w.e. for large detectors, has significant scientific advantages at a very deep location (see above). Cost is not very sensitive to depth, as long as one stays within the elastic limit of the rock, which is the case for a 4850 ft site at the Homestake mine. Because of their fine granularity, liquid argon detectors could possibly be located at the surface. However, for LBNE the cost savings compared to a location at 800 ft (and 4850 ft) are likely to be modest when compared to the overall project cost. In addition, the low-background physics such as proton decay and neutrino astrophysics would be impossible at the surface. Therefore, since its inception, LBNE had been envisioned having the far detector located in the Deep Underground Science and Engineering Laboratory (DUSEL) that was being studied by NSF, and detailed designs have been made for the Homestake site chosen for DUSEL.

The decision by the NSF not to pursue DUSEL perturbed the LBNE program in a major way. Fortunately, the Department of Energy reacted rapidly and formed a committee (the "Marx-Reichanadter Committee") to consider cost effective options for implementing a world class underground science program. Funds are being provided by NSF (FY11) and DOE (proposed FY12) to keep the Homestake 4850 ft level dry and safe through September 2012, while decisions are being made. The Marx-Reichanadter Committee reported at HEPAP during the Fermilab Physics Advisory Committee meeting. Its conclusions are substantially consistent with the Committee's own independent analysis and the recommendations given below.

The Fermilab PAC believes that Homestake remains a good choice for the LBNE far detector. Not only is the 1300 km baseline suitable for the program, but the project would also benefit from the significant investment already made in the facility design by NSF and DOE and in the infrastructure by the State of South Dakota and by Mr. T. Denny Sanford as a private donor.

An interesting new development (that was not available to the Marx-Reichanadter Committee) has been the realization that a liquid argon detector could be installed at 4850 ft instead of 800 ft for a very similar cost. This would improve the science reach of the liquid argon detector that requires low background and would also simplify the design. It would also decouple the underground facility aspects from the LBNE technology choice. The Physics Advisory Committee recommends that the technological choice be made under the assumption that the 4850 ft level would be used in either case.

If a Homestake location at 4850 ft were not available in a timely way, more extensive studies would be necessary to identify options for the long-baseline program. Incremental approaches based on surface detectors and existing beam lines could enable progress in the meantime, particularly if $\sin^2(2\theta_{13})$ is large.

Technology Choice

The water Cherenkov detector technology is well developed, although there are engineering challenges at the envisioned scale. Liquid argon technology offers superb tracking and lower backgrounds, but is less mature for this application. The two technologies have different strengths for the primary goal of the LBNE program and give access to complementary channels for proton decay and supernova detection. Given the promise of the liquid argon technology for this and other programs (short baseline neutrino experiments, dark matter), the Committee recommends that the liquid argon R&D (and in particular the 1 kton engineering prototype) should be vigorously pursued, independent of the eventual LBNE choice.

The Collaboration and the LBNE project team have laid out a process for arriving at a technology choice for LBNE with rigorous criteria. The Committee agrees with the general approach, but also cautions against delays that could be generated by too exhaustive a process. It is more important to arrive rapidly at a well-justified technological choice with a broad consensus of the Collaboration.

It appears that the development of the two technologies at a similar engineering level to the preliminary design stage (CD-2) would be expensive, both in terms of engineering costs and stretching of resources. A rapid technology choice is clearly desirable. The Committee endorses the Collaboration's and project team's plan to decide the technical baseline for the project by the end of 2011. The selected LBNE baseline technology should be optimized independently of a potential second phase that was considered in the Marx-Reichanadter Committee.

<u>Dark Matter – Joint Meeting with Some DUSEL PAC Members</u>

> *Early on the second day of the meeting, direct dark-matter search experiments will be reviewed. This is also an area of research which was expected to take advantage of DUSEL. Again, please provide comments on:*
>
> > *What will it take to keep the US program in the direct searches for dark matter vital?*
> >
> > *What should be Fermilab's role in this effort?*

Dark-Matter Science, Strategy, and the Role of Fermilab

Weakly Interacting Massive Particles (WIMPs) constitute a leading candidate to explain the dark matter in the universe as a consequence of new physics at the TeV scale. The next decade promises to be very exciting, with the combination of possible WIMP detection through scattering on terrestrial targets (direct detection), annihilation in the cosmos (indirect detection), and production of WIMPS at the CERN Large Hadron Collider.

The scientific importance of direct detection has been recognized by many review committees, in particular the Dark Matter Science Assessment Group (DMSAG) and the Particle Astrophysics Science Assessment Group (PASAG). The Fermilab Physics Advisory Committee has emphasized many times the strategic importance of direct dark-matter detection for the Laboratory.

There is general consensus in the WIMP direct-detection community and by the various review committees (e.g., PASAG) for a general roadmap of the field:

- The current generation (first generation, G1) is characterized by a broad and exciting exploration of a wide variety of technologies. The hope is to characterize the background-rejection power of each technology, and to attempt to reach the 10^{-45} cm^2/nucleon cross section level (for spin-independent interactions). In the supersymmetric framework there is a significant chance of detecting a signal.

- Second generation experiments (G2, 100 kg to ton scale, depending on background rejection) would implement the most promising technologies to attempt to reach 10^{-46} cm^2/nucleon cross sections on the 2016 time scale and push the technologies to their limit. This should cover a substantial fraction of the parameter space of the simpler supersymmetric models.

- Further down-selection of technologies will occur for the third generation (G3) experiments that will aim at sensitivities of 10^{-47} cm^2/nucleon. These are targeted to be installed in the 2018 time frame and to provide results early in the next decade. This schedule requires a siting decision within the next two years, so the site-specific design requirements are known. In case a signal is observed in a G1 or G2 experiment, the G3 experiments' main goal would be to accumulate large statistics to determine the physical properties of dark matter. If no signal is detected, they would address remaining loopholes in parameter space. Irreducible background from solar neutrinos appears at a sensitivity level of 10^{-48} cm^2/nucleon. There are strong arguments to have at least two such G3 experiments in the US with different technologies, both to disentangle possibly complex new physics and to mitigate the risk of unanticipated backgrounds.

The challenge of such a program is considerable: for instance a cross section of 10^{-47} cm^2/nucleon corresponds to roughly one event per ton per year.

It has also been a budgetary challenge for the funding agencies to rapidly shift resources to the dark matter field to respond to the strong scientific recommendations of its advisory committees and the fast expansion of the community involved.

Generation 1 Experiments

The dark matter experiments to which Fermilab provides support form an impressive suite of projects, covering a diverse range of technological approaches.

After a key involvement in the very successful CDMS-II, Fermilab plays a leading role in SuperCDMS–Soudan, a 10-kg low-temperature germanium experiment, about to be deployed in the Soudan Mine using

the same infrastructure as used previously. Fermilab has provided key technical support and scientific effort for COUPP, an innovative bubble-chamber approach to WIMP direct detection. Fermilab is also getting involved in DarkSide, a liquid-argon dark-matter effort that is very synergetic to other liquid-argon efforts at the Laboratory. Finally, the thick CCDs developed for DES are being used in DAMIC, a small but innovative effort to explore the very low mass WIMP region.

These efforts are supported at Fermilab at a relatively modest level (≈8 scientific FTE, including postdocs, and 5.5 technical staff). This does not reflect the scientific importance of the dark matter program. To make progress in a timely manner, more support is needed. For example, CDMS would benefit from a larger number of scientists and from a junior staff position. A significant involvement in DarkSide would require more support. The Committee suggests that the DarkSide plans be discussed at the next meeting.

Generation 2 Experiments

It is clear that in any scenario, Homestake will not be available in time for more than one G2 experiment, with that in the Davis cavern. At the G2 stage, it makes sense to use other facilities such as SNOLAB and Gran Sasso, as is currently planned.

Given the size of the typical G2 effort, it is important that a national laboratory be involved in the project management of each G2 experiment. Fermilab will be a natural choice for some of them. Fermilab has already assumed responsibility for the overall project management for SuperCDMS in SNOLAB, with a strong contribution from the SLAC National Accelerator Laboratory. It would be natural for Fermilab to similarly support a G2 stage of COUPP (500 kg) and possibly DarkSide/MAX.

Generation 3 Experiments and Homestake

G3 dark-matter experiments are at the multi-ton scale and cost around $100M. Their scale is then well beyond what typically can be managed at the university level. It is expected that there will be one or two G3 experiments in the US. The Committee recommends that Fermilab play important scientific roles in this program and provide project leadership for at least one G3 dark-matter experiment.

Given that infrastructure would be developed at Homestake for the LBNE program, it may be advantageous to co-locate G3 dark matter experiments (and the ton-scale double-beta-decay experiment) with the LBNE far detector. This is aligned with the Marx-Reichanadter Committee conclusion that the location of the LBNE far detector at 4850 ft would enable the use of the 4850-ft level for dark-matter and double-beta-decay experiments. Although the estimates summarized by the Marx-Reichanadter Committee indicate that the cost of deploying these experiments in an expanded SNOLAB may be less expensive, the Committee agrees that a small cost differential may be justified, if it leads to the creation of a national underground facility with no significant delays and an equivalent or better environment for the high-priority scientific program. Clear synergies exist between low background experiments, and substantial savings could be obtained by sharing infrastructure beyond access shafts and cavities; e.g., water shield and purification plants, safety, low background counting facilities, computing support, etc.

Co-location would also enhance the intellectual interactions of the scientific teams and powerfully contribute to the training of the next generations of US low-background scientists. The investment in a common facility is further justified by the likelihood of a long-term low-background physics program over several decades. In the long run, the 7400-ft level at Homestake may become a valuable scientific asset, and although it is too expensive to develop in the short term, it may be advantageous to look for State or private resources to de-water it and provide eventual access to that deep level.

The Fermilab Physics Advisory Committee supports the concept of a national underground facility at Homestake and the siting of the G3 Dark Matter experiment(s) in this facility, if it is available in a timely fashion and enhances the Generation 3 program.

The Committee urges an evaluation of this G3 dark matter strategy in time for the siting decision of G3 in two years. This should take into account the following elements: situation at Homestake, status of LBNE, progress of dark matter technologies, double-beta-decay experiment outlook, more detailed cost estimates of the SNOLAB alternative to allow a fair comparison and confirmation of the capability of dealing with cosmogenic backgrounds at 4200 m.w.e. (Homestake 4850 ft) instead of 6000 m.w.e. (SNOLAB, Homestake 7400 ft). In parallel, the institutional aspects of a potential SNOLAB alternative might be investigated (e.g., framework of a US-Canada partnership to operate a common North American facility, liability issues, relationships with the Creighton Mine operator).

In summary, Homestake is likely to be an essential asset to assure the US leadership in underground physics for the decades to come. However, in a highly competitive international environment, this long-term national strategy should not conflict with the timely deployment of high-priority experiments such as the Generation-3 dark matter experiments.

Given the scientific importance of the dark matter program, the Committee recommends that the Laboratory make a strong effort to increase the support of the dark matter program, as it evolves through its successive generations.

Dark Energy and Other Aspects of Fermilab's Particle Astrophysics Program

Recently, the Fermilab Center for Particle Astrophysics and the Division of Particles and Fields of the APS sponsored a workshop on Experiments on the Cosmic Frontier. The PAC will hear a review of the workshop as background for a discussion of the direction of dark-energy research, in particular, the Dark Energy Survey (DES) and later experimental efforts. Again, please provide comments on:

What it will take to keep the US program in dark-energy experiments vital?

What should be Fermilab's role in this effort?

The study and characterization of dark energy continues to be a high priority of the national program, requiring multiple, complementary measurement techniques. In 2009 the Particle Astrophysics Scientific Assessment Group (PASAG), a subpanel of HEPAP, wrote: "The panel recommends the formulation of a detailed plan for achieving a comprehensive and optimal dark energy portfolio under all funding scenarios. This plan should support projects whose science reach approaches astrophysical limitations for the three primary dark-energy methods. Clearly Astro2010 is an essential component of this process." The Astro2010 "New Worlds, New Horizons Decadal Survey" identified as its highest-priority large projects for this decade the ground-based Large Synoptic Survey Telescope (LSST) and the space-based Wide-Field Infrared Survey Telescope (WFIRST), both of which have essential dark energy research goals. The LSST Camera project recently received CD-0 status from DOE and will undergo an NSF Preliminary Design Review in August. The situation for space-based dark energy studies is more complex. Keeping the US program in dark energy experiments vital requires successful completion and operation of existing projects and entrepreneurial development of new opportunities in a rapidly changing scientific and programmatic landscape.

The Fermilab dark energy program began in the 1990's with theoretical work, and continued with the Sloan Digital Sky Survey (SDSS) and SDSS-II. Since 2003 the Laboratory has played a leading role in the Dark Energy Survey (DES) that will commence operations in 2012. Also, the Laboratory has recently joined LSST and is developing a new concept for a spectroscopic follow-up to DES, the Dark Energy Spectrograph, DESpec. The Fermilab portfolio, embedded in the Fermilab Center for Particle Astrophysics (FCPA), is well aligned with the national program.

The Dark Energy Survey (DES)

The Dark Energy Survey (DES) on the 4m Blanco Telescope in Chile is an exciting Stage-III dark-energy experiment using a new, large imaging camera (DECam). Its five-year (525 nights) imaging survey of 5000 square degrees of the southern sky will enable dark-energy studies using all four principal techniques (supernovae, galaxy clusters, baryon acoustic oscillations, and weak gravitational lensing). DES is an international collaboration, with institutions from the US, the UK, Spain, Brazil, and Germany. First light is expected at the beginning of 2012, with the survey commencing in September, 2012.

Fermilab scientists conceived of DES, and have played leadership roles in many areas of the project since inception. These include DES Project Director, survey strategy, calibration, data coordination and validation, mechanical and electrical design and engineering, image simulations, DECam management, construction, testing, integration, and soon, installation, commissioning and operations support of DECam, as well as the secondary data archive. Laboratory personnel currently co-lead four of eleven Science Working Groups and are active in others.

There are three separate but coordinated DES projects: the DECam Project (DOE funded, led by Fermilab with additional hardware contributions from Spain, the UK, and collaborating universities and institutions), the Data Management System (NSF funded, led by NCSA at the University of Illinois), and the CTIO Facilities Improvement Project (NSF funded through the Cerro Tololo Inter-American

Observatory, a subsidiary of NOAO). Thus, DES is a joint DOE-NSF project, with a Joint Oversight Group (JOG).

In the June 2010 report, the Committee noted the excellent progress of the DECam project, and added the following:

> The Committee is concerned that the planning and budget for the commissioning and operations phases of the DES are not yet defined. The Committee advises the Laboratory to investigate this situation immediately. The DES is the top-priority and largest FCPA project, and so it has a large impact on the rest of the FCPA program. More generally, the Committee encourages the Laboratory to be more proactive in reviewing all high priority FCPA projects over their entire lifecycle, including all aspects of the operation plans.

The Committee heard at this meeting that funding for the DECam commissioning, while still an open issue, is likely to be resolved in the near term. A greater concern at present is the uncertainty for funding critical aspects of data processing for science analysis (Data Management System). A proposal to NSF/Astronomy was submitted in early May 2011 by NCSA, and funding of this proposal is critical for DES success. Although Fermilab's main hardware role in DES has been the DECam, the primary purpose of the Laboratory's involvement is the science. As the core science data pipeline is at risk, it is important for Fermilab to help develop contingency planning now, in case the NSF proposal is not fully funded, to ensure the scientific success of DES. At this late stage, it is important to act quickly.

LSST

After playing a leading role in multiple dark energy projects, it is logical for Fermilab to play a significant role in LSST, the major ground-based dark energy instrument after DES. The Committee was told that Fermilab is not necessarily seeking to play as prominent a role in LSST construction, but will explore areas of interest that take advantage of the Laboratory's considerable expertise and science interests.

The Committee understands that discussions are underway, and looks forward to hearing how this develops. The Committee concurs with the tradition of FCPA leadership to set directions based on the scientific interests of people at the Laboratory.

DESpec

A good example of the value of following the interests of the excellent people at the Laboratory is provided by the novel idea of the Dark Energy Spectrograph (DESpec). The DESpec concept is a multi-object spectrograph for the Blanco 4m Telescope, which is also used for DES. It would provide redshifts for tens of millions of DES and LSST galaxies. As presented, DESpec has several very attractive features. It would cover the same fields on the sky as DES and LSST, which are imaging surveys, adding considerable value to both for dark-energy studies. DESpec would leverage the DOE investment, at much lower cost because of reuse of DES infrastructure. Furthermore, the DESpec concept allows for routine swapping between DECam and DESpec, providing new capabilities for the broader, long-term Blanco 4m

observing program, including LSST follow-up observations. The design and construction timescales appear to be a good match, with DECam observations scheduled to be complete in 2017. A program of DESpec observations could potentially proceed soon thereafter, in parallel with other observing with DECam, before or around the time of LSST first light.

The Committee agrees that this is an exciting possibility, and it encourages the Laboratory to proceed quickly with a Director's Review. At least one technical issue is the spectrograph fiber density. Two additional important issues to address are: (1) discussions with NOAO (perhaps through the DES JOG) on telescope availability after 2017 and (2) coordination with BigBOSS, which is a similar proposal with some additional capabilities, led by LBNL. BigBOSS proposes to start observations in the north (NOAO has already awarded time on the Mayall Telescope at Kitt Peak, but there is not yet funding for the instrument) on a similar timescale as DESpec, and then to move to the south approximately five years later. Ideally, it would be valuable to proceed with both BigBOSS and DESpec; however given the limited telescope availability and funding constraints, the Committee encourages the DESpec group to explore ways to work together with BigBOSS and NOAO to arrive at the best approach. The Committee also encourages the group to complete the DESpec whitepaper (a draft of which was supplied to the Committee) as soon as possible.

Space-Based Dark Energy Experiments

Despite the complex history, space continues to be an important venue for dark energy research. EUCLID, a proposed European-led mission, appears to be the nearest-term possible opportunity. Given all the other excellent dark-energy activities at Fermilab, and with finite personnel and resources, a significant role in EUCLID is not likely to be a high priority for the Laboratory. However, the situation may evolve quickly, and it is difficult to predict how opportunities will unfold. Therefore, the Committee recommends that Fermilab follow progress in this area closely and keep discussions open.

Holometer

The Committee has previously heard several presentations about the Holometer project. Recently, Aaron Chou received a prestigious Early Career Award for his Holometer work, and this is being used to fund the bulk of the construction. There were no additional technical details shown at this meeting, but it was reported that within a year the experiment aims to have the world’s best limits on MHz gravitational waves, and within two years the first holographic noise results should be available. The Committee very much looks forward to hearing these exciting results at upcoming meetings.

The Cosmic Frontiers Workshop

The FCPA hosted a multi-day Cosmic Frontiers Workshop in March, bringing to the Laboratory leaders in all the main areas of activity. It was a stimulating week of information exchange, organized in innovative ways to promote discussion. An excellent summary of the meeting by the organizers has been posted on the meeting website. Looking forward, the Committee suggests a careful assessment of the lessons learned and the impacts of the workshop, as well as detailed consultation with the other

laboratories and university groups involved in Cosmic Frontier science, in planning the next steps for the field.

Near-Term Experiments in Neutrino Physics

The PAC will hear a review of the current and near-term experimental neutrino physics program at Fermilab and around the world, a summary of the Short-Baseline Neutrino Experiments Workshop recently held at Fermilab, and about the NOvA Near Detector plans and options.

Please comment on:

1. *Status and plans for the Fermilab near-term neutrino program.*
2. *The significance of the recent antineutrino results from MiniBooNE and MINOS and relevance for possible further antineutrino-mode running in the Booster Neutrino Beam and NuMI beam.*
3. *The progress of the MINERvA Experiment.*
4. *The progress of the NOvA Project.*

The status and plans for the near-term Fermilab neutrino oscillation program were put into perspective in a presentation to the Committee. MINOS and MiniBooNE have presented a variety of suggestive, but not definitive results which require further investigation. New results are expected during the coming months. MicroBooNE, expected to start data taking in 2013, is aimed at understanding the low energy MiniBooNE excess as well as giving further validation of techniques involved in developing large scale liquid argon detectors. The implications of recent results from T2K providing an indication of possible ν_e appearance were discussed in terms of the potential impact on the future programs of NOvA and LBNE. New results from reactor experiments are also expected during the coming year. MINERvA is on track to provide cross-section measurements useful for many of the existing and planned oscillation experiments. Overall, Fermilab has a varied and vital neutrino physics program aimed at addressing crucial issues in this field. The breadth of this program is unique, and should be exploited in a coherent way. It brings together the many elements essential for the long-term neutrino future of the Laboratory.

The Committee also heard a report about the short baseline neutrino oscillation workshop. Many possible approaches to clarifying the present suite of puzzling neutrino experiment results (including that from LSND, the MiniBooNE low-energy excess, and unexpected differences between neutrinos and antineutrinos) were discussed. The Committee endorses a key finding, the importance of pursuing those new experiments that can provide definitive (i.e. > 5 sigma) results to address the present indications of anomalies.

The Committee was very impressed by the progress over the past year achieved by the MINERvA Collaboration. They completed the full detector installation in March, 2010, and the experiment is now successfully taking data. Preliminary physics results have already been presented at NuInt11. We congratulate the MINERvA Project on receiving the 2011 DOE Secretary's Award of Achievement. Unfortunately the NuMI beam line has been affected recently by target problems and, as of the date of the

meeting, MINERvA had received only 1.5×10^{20} POT, which is about 30% of their expected exposure for this run. The Committee endorses the Laboratory priority for neutrino running for MINERvA. Updated MINOS results from analysis of their additional antineutrino data should be available soon, and the plan should be reviewed if there are interesting developments from MINOS.

The Committee was impressed also by the progress achieved by NOvA. The far detector building has been completed and is ready for occupancy as of April, 2011. The Near Detector on Surface (NDOS), which was designed to prototype all detector systems prior to installation at Ash River, has been completed. Commissioning and data collection is ongoing and is providing critical insight on many installation and integration issues. The successful completion of the NOvA Project could leave some contingency that could be available to strengthen the project. Several options are under consideration, including the possible integration of SciNOvA into the NOvA project (see the November 2010 Physics Advisory Committee report).

MINER*ν*A Letter of Intent to Add H2/D2 Target to Their Physics Program (E-938)

> *The Laboratory has received a Letter of Intent from the MINERvA Collaboration to operate the existing liquid helium target with liquid hydrogen and deuterium for cross section measurements in the NOvA era. Effort will be required to understand the safety and other issues associated with operating the target with these fluids underground, and with providing appropriate venting and possibly other needs to make this possible. Therefore, the physics justification of running with such targets must be significant and the data must be useful as part of the Laboratory's future program. Of course, it will come at the expense of having less helium-target data as well. The Laboratory would appreciate any comments on this possible expansion of the physics goals of E-938. What priority should such a proposal have for the engineering needs required to fully understand the implications of proceeding?*

The Letter of Intent from MINER*ν*A presents physics motivations for running the experiment with deuterium or hydrogen in the existing cryogenic target. Currently, the target is foreseen to use helium following safety checks, and data will be taken with the low energy beam up to the shutdown in 2012, when modification of the NuMI beam will be made for NO*ν*A. MINER*ν*A then proposes to use deuterium for the full NO*ν*A era, corresponding to three years of neutrino and three years of antineutrino running. If the use of deuterium is not found to be possible, then their alterative choice would be to use hydrogen.

Since the NO*ν*A beam energy will be higher, the exclusive final states that are being studied with the low energy beam will be less easily accessible, and the physics interest will move to inclusive final states: measuring deep inelastic scattering of neutrinos off the range of heavy nuclear targets that are included in the apparatus, and comparing them to the deuterium target measurements. The ratios of structure functions will be determined, and nuclear effects can be disentangled.

The physics case for deuterium running is considered by the Committee to be interesting, with the attractive feature of the cancellation of systematic effects being made possible by comparing the

measurements on different targets in the same experiment. The statistical precision is significant for the discrimination among different models in the neutrino mode, although it is less convincing for the predicted antineutrino exposure. The case for operating with hydrogen (if deuterium turns out not to be possible) is less compelling.

The Committee recommends that the investigation of the engineering issues associated with the use of the deuterium target proceed, as long as that engineering effort can be provided without serious impact on the high-priority programs such as MicroBooNE. It is recommended that the possibility of taking data with a liquid argon target also be investigated further, as this is a key choice for many proposed future neutrino experiments, and measurements of the corresponding cross-sections would be valuable.

MINOS+ Proposal to Run in the NO*ν*A Era (P-1016)

*The Laboratory has received a proposal from the MINOS Collaboration to collect data with the higher intensities which will be available in the NO*ν*A era. The extra effort required of the Laboratory for this is not great. However, there will be a modest additional cost associated with maintaining adequate spares for both MINER*ν*A and MINOS. Therefore, the physics justification of continued running must be significant and the data must be useful as part of the Laboratory's future program.*

The Committee received, and heard a presentation on the MINOS+ proposal. MINOS+ is a continuation of the MINOS experiment for the first three years of the NO*ν*A era, expected to start in 2013, when the NuMI beamline will operate in the medium energy configuration. With a higher energy neutrino beam than in the MINOS era, MINOS+ will provide further precision measurements on both ν_μ and anti-ν_μ disappearance channels, especially in the 4 to 10 GeV region. MINOS+ will make further precision measurements on atmospheric oscillation parameters as well as constrain, or find evidence for, non-Standard-Model physics.

Given the known performance of MINOS, the modest costs involved, and the interest in the physics accessible, in particular for sterile neutrino searches, the Committee recommends Stage 1 approval for MINOS+ with the understanding that it will run completely parasitically to NO*ν*A. The Committee would like to see more mature sensitivity plots for sterile neutrinos at the next Committee meeting.

Other Topics

The PAC will hear a number of presentations on other areas of the Fermilab program: the Tevatron and LHC collider physics efforts, the experiments planned on muon physics, Project X accelerator and physics, a possible future Muon Collider and its physics program, and computing support for the Fermilab physics program.

We welcome any comments the PAC may have on the Fermilab program or issues raised in any of the presentations.

CDF and DZero

Because the Tevatron run was not extended, the accelerator will shut down on September 30, 2011. This marks the end of data taking in a very successful 26-year program. By then, both CDF and DZero will have recorded about 10 fb^{-1} of data. The Committee is impressed by the excellent performance of the machine and the experiments, as well as the rate and quality of the physics results produced by both collaborations. The number of Ph.D. theses and journal papers published in the past year remains as high as usual, and it is not expected to start to decline for at least another year.

Despite a fast ramp up of the LHC program, the Tevatron remains the leader in several high profile analyses. The high precision achievable in the measurement of the top-quark and *W*-boson masses (less than 1 GeV and 15 MeV, respectively) will provide significantly improved constraints on the allowed mass region for the Standard-Model Higgs. In addition to completing these legacy measurements, the Tevatron will focus on the combination of the Standard-Model Higgs boson results, with particular emphasis to the low-mass region. The collaborations will also further explore several recently observed anomalies, such as a resonance in dijet invariant mass in the *W*+dijet CDF sample, the asymmetry in *t* anti-*t* production (not accessible in the proton-proton collisions of the LHC), and the asymmetry in the dimuon sample. The Fermilab Director has setup a task force to resolve the discrepancy between the *W*+dijet results of the two collaborations. It is important that this discrepancy is resolved as soon as possible, and the Committee looks forward to hearing about the outcome of this effort at the next meeting.

The Committee believes that the physics program at the Tevatron remains very strong, and that it is important that the collaborations optimize their efforts to complete the high-priority analyses in a timely manner. It is also essential that Fermilab provides the necessary resources to enable the CDF and DZero collaborations to do this. This includes maintaining an adequate Laboratory staffing level, providing sufficient funding to preserve a vibrant visitor's program over the next two years to complete the legacy measurements, and supplying adequate computing support including planning for the preservation of the Tevatron data, to allow for future cross checks of potential LHC discoveries.

CMS

The Large Hadron Collider will dominate the energy frontier in the next two decades. Both accelerator and detectors are off to a great start. With the instantaneous luminosity of the LHC approaching its design value, the CMS and ATLAS detectors have already integrated over 1 fb^{-1} per experiment, which enables them to lead the searches for physics beyond the Standard Model and the hunt for the Higgs boson.

Fermilab plays a central role in the CMS Collaboration. Its scientists hold several leadership positions in the management and physics organization of the experiment, and are leading a number of the high-profile analyses. The Laboratory also hosts the largest CMS Tier1 facility, and plays a central role in the CMS detector upgrades, with major responsibilities in the silicon pixel tracker and hadron calorimeter.

In addition to its direct contribution to the experiment, Fermilab provides a valuable service to the entire US CMS contingent. The LHC Physics Center (LPC) hosts about 133 physicists from various US universities, who come to the Laboratory to take shifts in the Remote Operation Center and perform

physics analysis in the highly productive environment of the LPC. That number is expected to grow to about 200 asymptotically. About one third of the CMS physics publications have LPC scientists among their lead authors.

The Committee is very impressed with the depth and breadth of the contributions of the Fermilab team to the CMS experiment, and congratulates them on their achievements. The only area of concern is that of detector upgrades. With the successful operation of the LHC, preparing for detector upgrades is becoming more urgent, and the international community is in the process of planning how the responsibilities will be shared in this area. The delay in obtaining funding, starting with CD-0 approval, can potentially compromise the leadership role of Fermilab and of the entire US LHC community in terms of detector contributions to the experiments. The Committee encourages the Fermilab management to facilitate the discussion with the funding agencies in order to resolve this situation.

Muon Collider

The Muon Collider is one of the most promising options for a multi-TeV lepton collider. Fermilab is hosting the Muon Accelerator R&D Program (MAP), an organization of over 200 participants from 15 institutions formed to address the technical challenges and feasibility issues of such a machine. This program has substantial synergy with the neutrino factory project as they would share the same front end.

During the past year, the Muon Collider community has made substantial progress in defining its organizational structure, in which Fermilab continues to play a leading role. The workshop organized by Fermilab in Telluride in June 2011 will help strengthen the Muon Collider effort providing a forum to share recent progress and to coordinate the various efforts in this field.

Progress has also been made on the detector front, in terms of developing a more realistic simulation using the ILCROOT package, as well as identifying methods to reduce the harsh backgrounds.

The Committee notes that it is important to continue and strengthen this effort within the present budget constraints, as it is an investment in the future of high energy physics and in the vitality of the Fermilab accelerator R&D program. In particular, the Committee recommends a modest increase of support for the accelerator, detector, and physics simulation studies to enable a better understanding of the physics reach of a Muon Collider experiment. A close collaboration with SLAC is likely to lead to progress in this direction and is highly encouraged. The Muon Collider leadership is also encouraged to develop a coherent plan with well-identified milestones to better demonstrate the feasibility of both accelerator and detector technology.

Muon Physics Program

Mu2e is an experiment proposing to search for lepton-flavor-violating muon to electron conversion with single-event sensitivity better than 10^{-16}, an improvement of a factor up to 10,000 over the previous measurement. The Mu2e Collaboration is now preparing for a CD-1 review.
The Committee is pleased with the progress in defining the Mu2e project management structure, as well as the technical developments in the superconducting magnets and the tracker technology. YetHowever, the Committee looks forward to hearing about realistic simulation studies to demonstrate background reduction at a level necessary to reach the design sensitivity.

The Fermilab muon g–2 experiment aims for a factor of four improvement in precision over the previous experiment (E821 at Brookhaven National Laboratory), by using a higher beam intensity and a more appropriate time structure for the beam. This proposal was recently granted Stage-1 approval by Fermilab. It was also selected for funding among three proposals submitted in 2010 to the DOE Office of High Energy Physics to carry out research at the Intensity Frontier.

The apparatus, including the superconducting muon storage ring, will be moved from Brookhaven National Laboratory to Fermilab. The option to share the accelerator facility with the Mu2e experiment has been considered for potential cost saving, but a concrete plan has yet to be developed. At this moment, the Collaboration's primary goal is to prepare the conceptual design report. The Committee looks forward to more details about controlling the experimental systematic uncertainties needed to achieve the design precision.

Project X

The Project X accelerator complex is the heart of the future Fermilab flagship projects at the Intensity Frontier. It consists of a 3-GeV continuous-beam superconducting linac with 1 mA, followed by an 8-GeV pulsed linac and the existing 120-GeV Main Injector. The 3-GeV linac will supply a total beam power of 3 MW. The beam will have a variable time structure and will be delivered simultaneously to kaon, muon, and nuclear physics experimental areas. At the same time, the Main Injector ring, with a beam power of 2 MW, will produce a high-intensity neutrino beam to LBNE.

Project X will provide extraordinary experimental opportunities, including extending the reach of long-baseline neutrino oscillation experiments as well as a potential short-baseline neutrino program, the study of charged lepton flavor violation with muons, the study of rare charged and neutral kaon decays, and will have nuclear physics and energy applications. Options for the initial suite of experiments were identified in a series of Project X physics workshops organized by Fermilab, the most recent in 2010.

The Committee was impressed with the success of the workshop series and was pleased to learn that the MOUs for the accelerator R&D program have been established. The collaboration being formed comprises several international partners, including a sizable Indian component that plans to provide valuable in-kind contributions. The Illinois Accelerator Research Center will host the accelerator teams.

Given the budget profile, the Committee is concerned that the cost and timeline of the LBNE project may seriously delay the start of Project X. The Committee encourages the Project X leadership to explore the possibility of expanding the international partnership to reduce the impact of this project on the US HEP budget, and therefore facilitate its coexistence with the LBNE project. The Committee recommends that the Fermilab management make all possible efforts to keep Project X on schedule in view of the potential international competition. A substantial delay in the start of the project would be especially detrimental to the kaon physics program in the US, which, unlike the neutrino and muon efforts, is presently absent. Fermilab should make every effort to ensure the healthy growth of US kaon program which has great physics reach. Finally, the Committee would like to better understand how the current Project X frontend can be upgraded to become the proton driver for a future neutrino factory or muon collider.

Computing

The Fermilab Computing Sector provides the computing infrastructure necessary for the success of each and every experiment at the Laboratory. The excellent service provided by this group enabled the success of the CMS Tier-1 center, the physics productivity of the LPC, the fast physics output of the Tevatron experiments, several recent advances in Lattice QCD, just to mention a few examples. The Committee commends the Computing Sector for the excellent service they provide to the Laboratory.

Due to the growing number of experiments at Fermilab, the demands on the computing sector are likely to grow in the near future. The Committee endorses the current plans for an annual review of the allocation of the Fermilab computing resources based on the science needs of each group, to help coordinate and prioritize individual requests to the computing management.

The Committee also notes that the expertise of the Computing Division would be an asset to the LSST group should the opportunity arise for Fermilab to expand its current responsibilities in the area of computing.