Physics Advisory Committee Meeting

November 3-5, 2008

Comments and Recommendations

Strategic Planning

The Laboratory's mid-term strategy is evolving well on all three frontiers identified in the Particle Physics Project Prioritization Panel (P5) report to HEPAP—the Energy Frontier, the Intensity Frontier and the Cosmic Frontier. The current highest priority of the Fermilab program is the Energy Frontier. The Tevatron is the world's highest energy accelerator. Even after this is no longer true, the Energy Frontier will remain the largest component of the Laboratory program, centered on strong efforts on the CMS experiment and its upgrade. The current neutrino programs, and efforts in cosmology and dark matter are producing first-rate science at the Intensity and Cosmic Frontiers. Plans for future accelerator-based experiments at Fermilab are an important part of the strategic plan for the next decade. Future efforts at the Cosmic Frontier also offer exciting opportunities for important discoveries. The Committee did not discuss the Laboratory's plans for reclaiming the Energy Frontier at Fermilab, and the very long-term neutrino program, particularly if θ_{13} is small.

In spite of the difficult budget circumstances, the Laboratory is doing an excellent job at preserving its scientific opportunities in a wide breadth of areas. However, the Committee remains concerned about realizing all the future opportunities this program represents due to limited resources. Hard choices may have to be made in order to complete the most important components of the program in a timely fashion. The Committee notes the particular importance of maintaining detector and accelerator R&D for future projects in difficult budget times.

The Committee recognizes the size and complexity of the Fermilab enterprise, and the corresponding challenge of characterizing the technical resources available. An understanding of the opportunities and resources is essential for efficiently realizing the future program. The Directorate has initiated a comprehensive resource and schedule analysis, which is now in its second iteration. The Committee commends the Directorate for initiating this critical analysis. The Committee looks forward to hearing the results and how the analysis can be used to optimize the scientific program.

Energy Frontier

Tevatron

The Tevatron continues to break records for instantaneous and integrated luminosities, for which the Committee congratulates the Accelerator Division. This luminosity, together with

the excellent support of the Computing Division, resulted in over 100 papers submitted to ICHEP08. Particularly noteworthy are the improvement in the measurement of the top quark mass, new Higgs exclusion limits, observation of ZZ events, and the discovery of new b-quark bound states, amongst many other interesting results. The Committee commends the Tevatron experiments on their increased efforts to combine related physics results from CDF and DZero. The Committee is pleased to note the increased number of graduate students and post docs on CDF and DZero compared to the manpower predictions made in 2007. An in depth re-evaluation of the available manpower for the coming years would be welcome. The Committee looks forward to the continuing physics results from the Tevatron experiments. The Committee reiterates its support for Tevatron running through FY2010.

CMS and Its Upgrade

The Committee heard presentations on CMS and plans for its upgrade. The LHC experiments are unprecedented in scope and geographic distribution. Understanding the data and extracting physics will require new forms of collaboration. The LHC Physics Center (LPC) is one example of this new form. Fermilab is the only USCMS institution with the appropriate level of resources to create and sustain such a Center. The Committee notes the progress made in implementing the recommendations of the LPC Advisory Board. The Committee was pleased to see that the LPC is functioning well, enabling an effective route to contributions to the preparations for first physics by USCMS members.

It is essential for USCMS to provide leadership and contribute effectively to CMS detector upgrades. Fermilab, as the host institution for USCMS, has a crucial role to play. The Committee was pleased to hear that Fermilab scientists occupy important positions in the management of the CMS upgrade effort and are playing an important part in the pixel tracker and HCAL upgrades for Phase I, along with support for the muon detectors. Fermilab's role in Phase I upgrades will position USCMS to maintain leadership in Phase II upgrades.

Intensity Frontier

Neutrino Program

The Committee was impressed with the results of the current neutrino program, MiniBooNE, SciBooNE and MINOS, and looks forward to the imminent new results from these experiments. The Committee also heard with interest a status report on MINERvA and NOvA. While pleased with the overall evolution of the program, the Committee is concerned by the recent delay in the start of NOvA and the impact of the uncertainty in overall Laboratory funding on this important experiment.

Excellent progress has been made towards defining a new beamline from Fermilab to DUSEL, as well as the large detectors required to measure θ_{13}, determine the mass hierarchy, and possibly see CP violation in the neutrino sector. The Committee considers this to be an important area of research for Fermilab's future physics program. The physics sensitivity of such

a beamline/detector combination is well understood at this stage, with reasonable assumptions of systematic errors and backgrounds. The Committee was pleased to see that the formation of a formal collaboration for a large-detector experiment at DUSEL is well underway.

The Committee was impressed with the progress in the definition of MicroBooNE as a first step towards large-mass liquid-argon detectors. Fermilab engineering resources are essential, both to bring this experiment on line in a timely way, and to rapidly develop designs to increase the mass towards a 5kTonne detector and the ≈30kTonne apparatus considered for later stages of DUSEL. The Committee encourages cooperation with international LAr groups. While Fermilab scientists are actively involved in the LAr R&D, the Committee also encourages an increase in the scientific involvement in water Cerenkov detector R&D.

P-973 Muon to Electron Conversion Experiment (Bernstein/Miller)

The Committee commends the Mu2e Collaboration for developing the proposal for searching for $\mu N \rightarrow eN$ conversion at the single event sensitivity level of 10^{-16}. The proposed level of sensitivity is four orders of magnitude below current limits, and has excellent potential for the discovery of new physics.

On September 26, in preparation for the Mu2e proposal, a Director's "pre-review" committee composed of 16 scientists and engineers was appointed to review the draft proposal. This day-long review, along with its preparatory work by a large committee, allowed for a detailed examination of the experiment and project. The input to the PAC via this report is highly appreciated. The review of the Mu2e concept was well executed, and the Committee endorses the recommendations of the report, which is available at www.fnal.gov/directorate/program_planning/directors_reviews/Mu2eReview.pdf. The Committee encourages the Mu2e proponents to review the report's specific recommendations and to respond in detail to each.

The P5 recommendation with respect to the Mu2e experiment states: "*A muon-to-electron conversion experiment at Fermilab could provide an advance in experimental sensitivity of four orders of magnitude. The experiment could go forward in the next decade with a modest evolution of the Fermilab accelerator complex. Such an experiment could be the first step in a world-leading muon-decay program eventually driven by a next-generation high-intensity proton source. The panel recommends pursuing the muon-to-electron conversion experiment, subject to approval by . . .Fermilab. . ., under all budget scenarios considered by the panel*." The Mu2e experiment outlined in the proposal to Fermilab is an appropriate response to the goals expressed above. However, it needs to be emphasized that this experiment is *extremely* challenging, and, as acknowledged by the collaboration spokespersons, a great deal of additional R&D work needs to be done to more solidly demonstrate that four orders of magnitude improvement in sensitivity over previous experiments can indeed be achieved. Of particular importance is the understanding and control of the backgrounds. They must be reduced and measured to unprecedented levels. The Committee notes that a 2005 HEPAP review of MECO stated that a

minimal goal of achieving sensitivity significantly better than 10^{-15} was essential to justify the considerable effort and expense[1].

The proposal clearly relies a great deal on past work associated with the MECO project. The proposed production, transport, and detector solenoids have undergone multiple, detailed technical reviews, and this design work can be used as a firm starting basis. The Committee agrees that this critical-path area requires extensive, immediate effort to further develop. The proposed magnet R&D program, including the input from the review report, is appropriate. Other aspects of the R&D program also appear reasonable. The Committee recommends that consideration of demonstrating the required level of extinction, a key element in background suppression, be included in the R&D plan.

The Committee recommends that, in developing a more detailed technical plan, the collaboration improve the estimation of backgrounds and thoroughly identify the critical uncertainties in the background estimates. In the present document, no uncertainties on the background estimates were presented, and some conceivable background sources including neutrons and kaons were omitted. Thorough evaluations of upper limits for the various background sources are essential. The Committee recommends that wherever possible, measurements be performed to increase the reliability of background estimates, and that a "risk" analysis be developed to estimate worst case scenarios and possible remedies. Additional R&D expenses associated with improving the reliability of the background estimates should be included in the request to the Laboratory. In addition, since the goal is to make a definitive observation of $\mu N \rightarrow eN$ conversion, it will be essential to outline a strategy for unbiased experimental determination of the level of backgrounds that the eventual experiment experiences, and to develop a plan for blind analysis of the data.

The Committee also notes that the currently estimated cost of this challenging experiment, approximately $180M (including substantial contingency), plus the cost of the additional R&D plan, is more than an order of magnitude greater than any previous search for lepton flavor violation.[2] It is of the utmost importance that the reliability of the proposal's estimates satisfies a commensurate level of scrutiny.

Impacts on the four Laboratory Divisions have been provided, and the Committee thanks the Divisions for supplying these estimates. The FTE estimates appear reasonable, although more support from the Computing Division for simulation infrastructure may be needed to help expanded collaboration effort execute the necessary detailed Monte Carlo studies.

The Committee is pleased that a number of strong groups have joined the Mu2e collaboration and encourages further growth in numbers of collaborators.

The Committee recommends that Stage I approval be granted to "P-973 – Proposal to Search for $\mu N \rightarrow eN$ with a Single Event Sensitivity Below 10^{-16}", indicating that the experiment's scientific goals and concept for the experimental technique are worthy of serious

[1] From the 2005 HEPAP RSVP report: "A sensitivity of 10^{-15} is not an adequate level for MECO."

[2] The Committee was also informed that an additional expense of a few $M are required for increasing the Booster rate from 10 to 15 Hz.

consideration for development of a complete technical plan for subsequent approval stages following further R&D.

Cosmic Frontier

The Committee heard a presentation on the Cosmic Frontier and the Fermilab Center for Particle Astrophysics (FCPA) from the Center's new Director. In addition to welcoming the new Director, the Committee would like to express its appreciation to the outgoing Acting Director, who enhanced the vitality of the FCPA and made significant advances in building a fertile and interactive environment. Through Center meetings, workshops, and a retreat, the Acting Director increased the coherency of the FPCA, and generated exciting ideas for possible future projects.

To maintain alignment with the P5 roadmap, it is important to remember that the Cosmic Frontier includes topics beyond dark matter and dark energy. There are many exciting possibilities at the interface between particle physics and astrophysics, and the FCPA should play a leading role in articulating to both the science community and the agencies the importance and discovery potential of this broad area.

The FCPA builds on the strong astrophysics theory group established at Fermilab in 1983, and on the following addition of experimental particle astrophysics efforts. The group explored the interconnections of inner space/outer space and defined the emergent field of particle astrophysics. The group has provided phenomenology for experiments, and a strong connection to particle theory. Past projects such as the Sloan Digital Sky Survey have been spectacularly successful.

The Committee sees the following roles for the FCPA:

1) Exploration – generating new ideas and incubating new projects.
2) Experimental projects – participating strongly or leading projects, e.g., the Dark Energy Survey.
3) National Center – representing and serving the community, including professional support, (e.g.: theory, computing, engineering, and management support), and focusing community discussions.

The Committee encourages the FCPA to continue its long-term planning. Specific questions that should be addressed include the following ones: Where does the FCPA want to be in five years? What weight is given to the above roles in the long-term plan? How are decisions to be made on which projects to pursue? What are the strategic opportunities locally within the Chicago area as well as in the larger community?

The Committee feels that the planning exercise would be particularly valuable as the new Director establishes the role of the FCPA within the Laboratory and within the larger community. Careful thought should be given now to Fermilab's interest in the upcoming 2010 Astronomy & Astrophysics Decadal Survey. This survey will determine to a very large extent

the funding priorities in this area for the next decade. It is also important for Fermilab to build a credible roadmap for its dark-matter direct search experiments and development programs for input into the new Dark Matter SAG and decisions on the first suite of experiments at DUSEL.

The Committee is concerned that the reduction of FCPA theory postdocs from five to three places the program below critical mass. A strong interaction between theory and experiment in all of the above roles leads to better projects with strong connections to particle physics. The Committee recommends restoring the number of theory positions to five.

Computational Cosmology

Computational Cosmology is an important area of growth. A computing capability of the appropriate scope and architecture will enable great advances in several areas that are very well connected to other projects in the FCPA. The recent, initial progress is a credit to the Fermilab Computing Division and members of the FCPA. The Committee is aware of related efforts at other institutions, and encourages a national perspective, as suggested in the presentation to the Committee.

The details are important, but were not presented to the Committee at this meeting. While large-scale computing for HEP experiments is very well understood at Fermilab, the architecture for computational cosmology is generally different. The immediate and longer-term implications for the Computing Division should be carefully considered in the growth plan. The Fermilab plan for these promising developments should be made with a national perspective, considering the existing resources and future plans at other institutions, in the Chicagoland area and more broadly.

JDEM Science Operations Center

The Committee was asked to comment on the progress and potential of the Laboratory's work on planning a Science Operations Center (SOC) for JDEM. In the midst of great uncertainties and changes in the overall JDEM situation, Fermilab personnel have done an excellent job advancing the planning for an SOC. The Committee agrees that hosting an SOC is an important goal for Fermilab; the Laboratory can make important contributions to the mission, and it will help ensure that Fermilab plays a leading role in JDEM science.

The Committee offers the following comments and suggestions for a Fermilab JDEM Science Operations Center:

1. The Committee very positively notes that the planning was based on conversations with, and visits to, other science operations centers. Now that JDEM is taking shape, it is especially important to continue in that mode. Specifically, in close consultation with the DOE JDEM Project Office, Fermilab JDEM SOC planners should have direct interactions with the other mission elements, including the NASA JDEM Project Office and the Science Coordination Group (SCG) Chair. An SOC has important interfaces, and the whole mission must work as

a system. An SOC design should be widely understood to be based on the needs of the integrated mission. A presentation to the SCG, and subsequent dialogue, could be quite helpful, for example.

2. The Fermilab SOC should play an active and continuous role supporting the development of mission elements with which it shares interfaces. For example, the SOC should offer to do the processing and distribution of test data starting as early as possible (e.g., subsystem prototype testing) and certainly through higher-level integration and test. The SOC planners should stay close to the mission hardware. Similarly, there should be a close connection to the groups at other institutions writing and testing the onboard software, which produces the data that will be processed by the SOC. These are all areas in which the Laboratory already has considerable expertise to offer. In addition, by playing this role, Fermilab will have clear and timely awareness of arising issues, and may therefore help the mission in other critical areas. Such "pitching in" is common in both high-energy physics and space missions, and it is critical for project success.

3. The SOC role will naturally keep Fermilab connected to JDEM science. The Committee very positively noted the Data Challenges as a feature of the current plans, and encourages wider discussions across the JDEM team at the appropriate times. SOC personnel should be actively involved in the definition, planning, and execution of all aspects of the data challenges. The SOC should play this role firmly embedded in the wider JDEM team.