Physics Advisory Committee Meeting

November 1-3, 2007

Comments and Recommendations

Project X

The Committee commends the Laboratory and the Steering Group on having carried out a thoughtful and comprehensive planning exercise. The Steering Group's report offers a strategic plan for the most desirable scenario, wherein the ILC proceeds according to a technically driven schedule, as well as scenarios where progress on the ILC is slower than one might like as a result of the challenges surrounding the funding of a large international project. Developing a plan that provides for an exciting interim physics program, while keeping the Laboratory on a technological path that is aligned with that of the ILC is clearly prudent. Moreover, the Steering Group's plan provides a way forward even in scenarios where the ILC is delayed indefinitely by incorporating R&D on advanced accelerator concepts such as a neutrino factory and a muon collider.

The Committee notes that the Laboratory's current and near-term allocation of resources for accelerator R&D is predominantly directed towards the ILC. This is appropriate given the strategic importance of that facility to particle physics in general and Fermilab in particular.

The origin of flavor, both in the lepton and quark sectors, is one of the greatest mysteries in particle physics. By supplying intense beams of protons ranging in energy from 8 GeV to 800 GeV, Project X creates a wide range of opportunities to address this puzzle. Windows for discovery in neutrino physics, charged lepton physics, and strange quark physics are opened, providing a view of the Terascale and beyond different from that provided by the LHC and non-accelerator based programs.

The Project X program outline is complete in the sense that no Project X accessible area of important flavor physics is omitted. The program as described in the Steering Group Report is, however, only an outline, and the physics case now needs to be developed in greater detail. It is expected that the Project X Physics Workshops at Fermilab in November and January will play an important role in this.

Neutrinos, which have already yielded important evidence for physics beyond the standard model, are likely to be the centerpiece of the Project X program. The accelerator hardware that is envisaged can provide intense neutrino beams (>2MW) over a range of energies (50-120 GeV), which will greatly extend the sensitivity of experiments such as NOνA to θ_{13}, the neutrino mass ordering, and CP violation. In the longer term, a large neutrino detector, located at DUSEL or some other distant site, could further extend this reach. Such a detector might also be used to probe grand unification through searches for proton decay, and be sensitive to

astrophysical sources of neutrinos. Such a long-baseline experiment could be the flagship of the Project X program and should be emphasized. Project X could simultaneously provide excess 8 GeV protons to continue a low-energy neutrino program and could feed the Tevatron for precision neutrino electroweak measurements.

Kaons are a marvelous laboratory for discovery; neutral meson mixing and direct and indirect CP violation were all discovered in the kaon system. The rare decays $K^0_L \to \pi^0\nu\nu$ $\sim 3\times10^{-11}$ and $K^+\to\pi^+\nu\nu$ $\sim 8\times10^{-11}$ are theoretically clean and sensitive probes of the Terascale. Past proposals [KAMI (Fermilab), KOPIO (BNL) and CKM (Fermilab)] developed a strong physics case for studying these decays. The Project X 8 GeV proton beam would allow ~800 SM $K^0_L \to \pi^0\nu\nu$ to be recorded which would significantly exceed the estimated yield at the KEK–J-PARC II experiment. Project X with the Tevatron could provide 300-600 SM $K^+\to \pi^+\nu\nu$/year, exceeding the reach of NA48 at CERN and providing a rich physics program in other rare kaon decays as well.

Studies are needed to better understand the roles of the two $K\to\pi\nu\nu$ modes in probing non-standard model physics. In particular, it would be helpful to quantify their relative sensitivities and the extent to which they are complementary to one another. Moreover, comparisons to analogous studies using rare B decays are also needed. On the experimental side, the projected sensitivities go well beyond what has been achieved thus far. A considerable amount of work is needed to establish that the detectors can accommodate the rates, and that backgrounds can be reduced to levels commensurate with the anticipated statistical sensitivities.

The Mu2e proposal, discussed elsewhere in this report, would gain in sensitivity by using excess 8 GeV protons as part of Project X.

The charm quark provides a sensitive probe of physics beyond the standard model. Project X allows a Tevatron charm fixed-target program. A study is needed of the relative sensitivity of this program compared to other programs.

The Committee commends the leadership of Fermilab for the broad input they are seeking in developing the roadmap that incorporates Project X. In particular the Steering Group includes members from Fermilab and the national particle and accelerator physics communities. The Steering Group has consulted widely and has received numerous proposals and pre-proposal documents. The possibilities for discovery are sufficiently compelling that the physics program will likely attract significant international interest, which the Laboratory should encourage.

The range of opportunities afforded by Project X is so broad that it is inevitable that the field will need to be narrowed to fit the program within practical constraints. The Committee encourages the Laboratory to maintain the high level of community input that has characterized the planning effort thus far—e.g., by maintaining a proposal-driven process—while simultaneously taking steps to ensure that the overall program makes efficient use of the available facilities.

P-973 Mu2e (Prebys/Miller)

The Mu2e collaboration has expressed the intent to submit a proposal for an experiment to extend the search for muon-electron conversion at Fermilab. The physics justification for such a search reaching the level of $R_{\mu e}=10^{-17}$ has been well articulated by the MECO experiment at BNL, and a strong endorsement of the physics case was made by the RSVP HEPAP subpanel in June, 2005. The collaboration should participate actively in the upcoming workshops at Fermilab in order to further develop the physics case.

The Committee encourages the collaboration to submit a formal proposal to the Laboratory and to explore how the experimental setup could be optimized at Fermilab for a Mu2e experiment. In addition, the collaboration needs to be strengthened with additional members. A proposal from the collaboration must be evaluated in the context of the broader Fermilab program.

P-974 MicroBooNE (Fleming)

The MicroBooNE proposal involves the construction and operation of a new liquid argon TPC on the Booster Neutrino Beamline. The physics goals are to investigate the low-energy excess observed by MiniBooNE and to study low-energy neutrino cross-sections relevant to interpretation of future neutrino oscillation data. In addition, the construction and operation of the TPC are envisioned as steps in the development of future large (100 kTon scale) liquid argon TPC's for long-baseline neutrino studies and proton decay searches.

The proposed plan for implementation of this detector as a physics experiment was not adequate for Stage I approval at this time. The construction schedule and project plan were judged to be unrealistic. In addition, the question of how the project would address the physics goals, given a realistic beam delivery schedule, was not fully developed. Additional information from MiniBooNE regarding the significance and nature of the low-energy excess should be helpful in further developing the physics case.

The Committee considers R&D towards large scale liquid argon TPC's to be a very important activity. However, the Committee was not convinced that constructing the MicroBooNE detector was the optimal approach for R&D towards much larger devices. For example, individual issues such as contamination of the liquid argon by electronics components could be studied with smaller test setups.

E-944 MiniBooNE Extension (Brice/Van de Water)

The MiniBooNE collaboration presented a proposal to extend their running with antineutrinos beyond the present 2×10^{20} protons on target (POT), corresponding to a total of 5×10^{20} POT for antineutrino data. As presently envisioned, the additional running will not affect accelerator operations at the Laboratory. These additional data would improve the sensitivity for appearance of electron antineutrinos to cover most of the region allowed by the existing LSND,

KARMEN and Bugey data. While this will not lead to conclusive results regarding this appearance channel, it does represent an improvement over the present situation.

The Committee recommends that the run extension be approved. However, if significant maintenance issues arise, the continuation of the run should be re-evaluated.

P-975 NuSOnG (Conrad/Fisher)

The NuSOnG collaboration has submitted an Expression of Interest to develop a proposal for a high-statistics study of high-energy neutrino scattering. The main physics goals are to obtain additional data on neutrino electron scattering and deep inelastic scattering to provide higher precision measurements of $\sin^2\theta_W$. These measurements are sensitive to physics beyond the standard model. The neutrino electron scattering measurement would represent a major improvement over previous data from CHARM-II. The deep-inelastic scattering data would provide uncertainties about a factor of two smaller than NuTeV.

The detector design would involve glass target planes (3.5 kTons) interspersed with active tracking detectors, followed by a muon spectrometer. Although based on previous detectors, the design is still only conceptual.

The physics case for NuSOnG needs to be further developed to enable full consideration of the physics impact. For example, the feasibility of using measurements of $F_3(x)$ in order to remove the effects of isospin violation would need to be quantitatively demonstrated. In addition, the relationships to other measurements need to be more fully discussed.

NuSOnG needs to be judged in the broader context of other potential experiments at Fermilab and elsewhere, including the impact of continued running of the Tevatron. It would be premature to provide Laboratory resources towards detailed design of this project at this time.

CMS Upgrade

The LHC will soon become the accelerator at the energy frontier, and ATLAS and CMS the flagship experiments in our field. A machine luminosity upgrade to $L=10^{35}$ cm^{-2} s^{-1} (known as SLHC) would further extend the LHC physics potential by increasing its discovery reach by typically 20-30% on the masses of new particles, allowing the detection of rare channels, and improving precision measurements.

Machine upgrades needed to deliver this luminosity are currently under consideration, along with the detector upgrades needed to optimize sensitivity under the new operating conditions. The Committee appreciates that Fermilab and US CMS have already started detector upgrade R&D activities.

Fermilab and US CMS played a major role in the construction of the current CMS detector, including HCAL, ECAL, muon system, tracker (outer barrel and forward pixel

detectors) and trigger. Tracking and trigger are the systems that will be most stressed by upgraded luminosity. The Committee strongly supports continuing and increasing CMS detector upgrade R&D. It believes that the effort fits the expected needs of the CMS experiment, and that the emphases on tracking and triggering are appropriate.

These R&D activities, and, in the longer-term, the LHC upgrade program if approved, are expected to play an important role in the present and future Laboratory program, and complement US CMS's strong presence on the commissioning work of the current detector. Care should be taken to balance upgrade activities with detector commissioning and data analysis efforts.

The Committee encourages the Laboratory to foster as much as possible synergies between ILC and SLHC in detector R&D (such as the technology for pixel detectors and the HCAL readout), as well as in the development and maintenance of common elements (e.g., test beam facilities and irradiation tests). Continued cooperation and collaboration between US ATLAS and US CMS on common R&D projects should also be encouraged.

Fermilab should also continue to play a leadership role in US CMS project management, as well as in promoting and coordinating the involvement of universities.

Tevatron Collider Program

The Committee heard presentations on behalf of the DZero and CDF collaborations framing an expression of their strong interest, seconded by both International Finance Committees, in extending their data-taking through 2010. The Committee is impressed by the outstanding performance of the machine in terms of peak luminosity and reliability of operation, by the smooth and efficient operation of the detectors, by the huge amount of new physics results produced by the two experiments in a timely way, and by the continuous progress and advances in analysis techniques. The Committee commends the Laboratory, in particular for the accelerator performance, and the experiment collaborations for such spectacular achievements.

Running the Tevatron in 2010 would further extend the physics reach of the experiments, in particular improving the possibility of probing the best-motivated Higgs mass region at the 2–3 sigma level. The luminosity projections (an expected increase of at least 1.5 fb^{-1} between 2009 and 2010) are solid, the detectors are taking data with efficiency in excess of 85%, aging has been shown not to be a concern, and the analysis potential and reach are still being continuously improved. The projected human resources of the collaborations are expected to be adequate to cover the needs of a 2010 run. The Committee believes it would be prudent to plan for a run in 2010.

Computational Cosmology

The Committee was pleased to hear the report of the Task Force on the Computational Cosmology Initiative. The Task Force did an excellent job in addressing the key issues

associated with this initiative, from its inception as primarily a local effort at Fermilab and Chicago through its evolution into a full-fledged national coordinated program comparable in scale to the existing Lattice QCD program. It is now clear that large cosmological simulations are essential for further understanding of the growth of structure in the universe, and for the interpretation of upcoming survey experiments aimed at addressing fundamental physics issues like the nature of dark matter and dark energy. The Committee thus strongly supports further investment in this area by the Laboratory.

In FY08, the Task Force recommends provision of limited funding to continue to build up a local cluster at Fermilab, comparable in size to that currently available to numerical cosmology groups at other institutions, as well as personnel support from the Computing Division to maintain this cluster and to provide help with the porting of the ART code. In later years, the Task Force recommends an expansion to much larger scale, approaching 10,000 processors by 2012. The Committee believes that the limited near-term support requested is essential to get this effort going. The growth to larger scale must be in the context of a national initiative with strong participation by outside groups. In addition, it will be important to get a better understanding for the need to build up large scale dedicated hardware resources at Fermilab, rather than to try to couple more directly to the petaflop facilities that are currently being developed at other DOE laboratories.

The Committee strongly encourages the computational cosmology team to assemble a national collaboration to compete for SciDAC funding in the next competition. Support from SciDAC enhanced the LQCD effort, and is likely to be important for the computational cosmology initiative as well.