Fermilab Physics Advisory Committee Meeting
December 8-10, 2011
Comments and Recommendations

Introduction

The Fermilab Physics Advisory Committee (PAC) met at Fermilab to consider three proposals and discuss several key areas of Laboratory scientific activities. Of the three proposals TAPAS andORKA are revisions of previously considered projects, and BooNE had been presented as a Letter of Intent. TAPAS addresses several physics topics associated with antiproton annihilation, ORKA is a replacement for Proposal P-996 dealing with a precision measurement of $K^+ \to \pi^+ \nu \bar{\nu}$ decay, and BooNE aims at resolving the LSND and MiniBooNE anomalies reported in short baseline neutrino studies. The Committee was charged to provide comments and recommendations regarding these proposals under the assumptions of (i) no budgetary issues and (ii) if the Fermilab budget is severely constrained over the next four years. The Committee greatly appreciates the time and effort of the proponents for preparing the detailed proposals, making the presentations, as well providing informative responses to the Committee's questions.

The Committee also heard status reports on other aspects of the Laboratory's physics program, including the major thrusts in neutrino physics, the Mu2e experiment, the Energy Frontier experiments, and the detector R&D program – as well as a report on the recent Intensity Frontier Workshop sponsored by the DOE. The currently-running neutrino program, consisting of MINOS, MiniBooNE, MINERvA, and the NOvA Near Detector on the Surface, is progressing as planned. The near future also will include MicroBooNE, MINOS+ and NOvA. These were described in presentations including MINOS and MINOS+ plans for studying sterile neutrinos and neutrino speed. The Long Baseline Neutrino Experiment (LBNE) program is facing a number of decisions at the DOE following the demise of the NSF's DUSEL initiative. A choice of far-detector-technology preference by the scientific LBNE Collaboration is imminent.

The Energy Frontier experiments are proceeding actively to determine the Higgs mass and to search for new physics. Announcements from the LHC experiments dealing with the Higgs search are imminent. In addition, the Committee met with representatives of the Tevatron experiments (CDF and DZero) and the Fermilab CMS group to discuss common scientific issues and the impact of LHC and detector upgrades. The Committee appreciates the willingness of the various experiment participants to meet on short notice, and thanks them for their comments.

The Committee also thanks the Fermilab Director for reporting on the overall status of the Laboratory programs and key issues facing the Management.

In the following, we provide comments and recommendations relating to the proposals and issues raised by the presentations.

BooNE (P-1002)

The Committee reviewed the BooNE proposal (P-1002) to build an identical second MiniBooNE detector for measurements at a distance of ≈200 m from the Booster Neutrino Beam (BNB) production target.

The MiniBoone collaboration previously reported an excess of events at low energy over the expected backgrounds, in neutrino mode at a 3 sigma confidence level (6 sigma statistical error only) and at 2.5 sigma (3 sigma statistical error only) in the antineutrino mode. It also presented muon disappearance results, limited, in the present one-detector configuration, by the knowledge of the initial neutrino flux.

Several different explanations for the MiniBooNE low-energy excess have been proposed in the literature: an underestimation of the single-photon background, new types of oscillations between active and sterile neutrino states, existence of a decaying sterile neutrino state, and even Lorentz symmetry violation. In the context of the active-sterile-active neutrino-oscillations hypothesis, the fits to world data in 3(active)+1(sterile) models indicate a mass scale for new physics characterized by $\Delta m^2 \approx 1$ eV^2. They however suffer from a strong tension between appearance and disappearance data. Indeed, a "smoking gun" prediction of this class of models is active-sterile transitions, which imply neutrino disappearance at the level of ≈10%. This kind of effect is incompatible with the observed absence of muon-neutrino and electron-neutrino disappearance from other experiment data.

Two identical BooNE detectors at 200m and 500m would allow near/far ratio measurements, thereby reducing the systematic errors on the background predictions. If the present level of background is confirmed, reducing its systematic error would enhance the significance of the result of the MiniBooNE low-energy excess.

In addition, a near/far comparison would improve the sensitivity of the muon-neutrino disappearance search. The resulting sensitivity in the Δm^2 region above 1 eV^2 would be better than that expected from the approved MINOS+ run. The MINOS+ disappearance sensitivity is expected to be far better at lower Δm^2 values, as the BooNE reach is limited by the short baseline.

The addition of the BooNE near detector would give a significant improvement in sensitivity in the neutrino mode. The presented near/far comparison sensitivity based on an exposure of 1×10^{20} protons on target would cover the entire oscillation parameter space indicated by LSND at more than 3 sigma.

However, the sensitivity in the antineutrino mode would be considerably weaker, due to the lower event rate in that mode. Even an optimistic integrated exposure of 10×10^{20} protons on target would not conclusively elucidate the LSND oscillation parameter space. The Committee notes that this represents a serious limitation of the BooNE proposal.

In parallel, the MicroBooNE experiment currently under construction will provide a stringent test and possibly an elucidation of the low energy excess in the neutrino mode observed by MiniBooNE.

In conclusion, the Committee did not see a strong argument in favor of simultaneously operating two identical BooNE detectors in the BNB. The Committee also evaluated the alternative cost-saving scenario of moving the existing detector, and it did not find the scientific case compelling enough to recommend it even in a scenario with no budgetary issues.

The Committee recommends that the Laboratory and the community consider new-generation detectors and/or new types of neutrino sources that would lead to a definitive resolution of the existing anomalies. Workshops could be an effective way to foster progress in this direction.

TAPAS

The TAPAS Collaboration proposes to perform four different measurements:

- CP violation in D^0 mesons
- CP violation in hyperons
- Width of the new resonances recently discovered at the B factories, such as X(3872)
- Drell-Yan production at large x.

This relatively broad program, with the potential for many PhD theses, is based on the Fermilab antiproton source, which is currently the most powerful in the world, and on the reuse of various existing detector components. However, implementing TAPAS would require a significant reconfiguration of the approved Fermilab program. In the current plan, components of the antiproton source are used for two flagship experiments, g-2 and Mu2e. The questions in front of the Committee are therefore whether the scientific case and the implementation plans of TAPAS are strong enough to justify the substantial change of plan and associated expense.

Among the physics topics, the most immediately interesting is the measurement of D^0 (charm sector) CP violation, for which LHCb has recently reported evidence at the 3.5 sigma level. TAPAS proposes a measurement in pbar p with a fixed target, and therefore with very different, and potentially smaller, systematic uncertainties. However, even in the most optimistic schedule for a start in 2016, the TAPAS experiment is unlikely to be a major player in the confirmation of the signal. By that time, LHCb will have accumulated twenty times the statistics used to date and analyzed additional decay channels with different systematic uncertainties. The KEK B-Factory, which is scheduled to start data taking in late 2015, is also likely to have integrated enough luminosity to have made a significant measurement.

The Committee has already commented about the search for CP violation in hyperons. While a broad exploration of CP violation is interesting, the hyperon channels suffer from the difficulties in the theoretical interpretation of a possible effect. Moreover, as emphasized in our previous reports, the statistical and systematic uncertainty improvements of TAPAS with respect to HyperCP (a factor 2 in both) is not compelling.

The unique measurement that TAPAS offers is the systematic mapping of the X states and of their widths. While this is an interesting measurement, which could help identify exotic states in QCD, it does not rise to the level to justify the required major changes in the Laboratory plans.

The Drell-Yan measurement at large x is useful to map better parton distributions, but is unlikely to provide major surprises. The Committee did not find that the Collaboration had been strengthened sufficiently. While we recognize this may be difficult before Stage I approval, the experiment appears still to be planned by a small core of enthusiastic proponents. At present, the manpower does not appear to be adequate to complete the task in a timely manner.

The Committee was not convinced that the proposed plans are realistic. The time and effort needed to retrofit a disparate ensemble of hardware components and build a fully integrated detector seem underestimated. As a consequence, the presented cost estimate for the detector appears to be too low. The cost of the infrastructure needed from the Laboratory and the potential increase in the cost of the g-2 and Mu2e will, of course, have to be added.

Although the proponents claim that there are ways to insert this proposed experiment in the already approved program of g-2 and Mu2e, this would be expensive and would likely results in delays. Orders-of-magnitude estimates were presented to the Committee, but additional engineering would be needed to get better numbers. The cost of such studies is not justified by the promise of the proposal.

The Committee was intrigued by the proposed broad physics program and by the use of the antiproton source - a unique asset of the Laboratory. While the physics is interesting, the schedule, the implementation plan, and the integrated cost estimate lack credibility. Overall, the case is insufficiently compelling to justify the substantial changes in the Laboratory plan that TAPAS would necessitate. This assessment applies in both budget scenarios.

ORKA (P1021)

ORKA (P-1021) is a proposal for an experiment to measure the rare $K^+ \rightarrow \pi^+\nu\nu$ decay, with the sensitivity to record of order 1000 signal events if the branching ratio is that expected in the Standard Model (SM). The experimental technique is to use a stopped kaon beam, as was successfully employed by the Brookhaven experiments E787/E949 that reported seven signal events, the first observation of this decay mode. The increase in sensitivity by two orders of magnitude is made possible by the combination of the higher intensity beam available at Fermilab and incremental improvements in the experimental acceptance. A closely related proposal (P-996) was considered by the Committee in October 2009, and was judged to be of high scientific quality, but involved the continued use of the Tevatron ring and would have consequently required substantial operational resources. The proponents have now modified the proposal to use beam from the Main Injector, which significantly reduces the operating costs to the Laboratory. The Committee judges this to be a compelling scientific opportunity, and recommends ORKA for Stage I approval.

The $K^+ \rightarrow \pi^+\nu\nu$ decay is considered to be a "golden mode" in flavor physics, as it is a strongly-suppressed flavor-changing neutral-current process in the Standard Model, with a branching ratio that can be predicted precisely, $(0.78 \pm 0.08)\times10^{-10}$. It is sensitive to non-SM physics contributions that could strongly modify that value. The final result from E787 and E949 was $(1.73\,^{+1.15}_{-1.05})\times10^{-10}$, consistent with the SM, but leaving room for new physics. The precision of the SM prediction is expected to improve further on the timescale of the proposed experiment, making a 1000-event sample an extremely sensitive test. This is a key opportunity for discovering new physics, and/or constraining its flavor structure.

There is strong competition for the measurement of this channel from the NA62 experiment at CERN, which is currently under construction, and is aiming to record of order 100 signal events on the timescale of the next five years. NA62 will use a complementary technique of in-flight decay with an intense unseparated beam, and involves a much larger extrapolation from previous studies of that technique, with associated technical challenges. Even if NA62 achieves a measurement of the BR before ORKA, the Committee considers that the higher precision that could be achieved by ORKA would give it high impact, either to confirm a new-physics signal with higher precision or to push the sensitivity further. In the longer term, ORKA will provide a link to Project X at Fermilab, where the beam intensity (and sensitivity) would be increased further, and where the closely related neutral mode $K^+ \rightarrow \pi^+\nu\nu$ could also be studied. A measurement of the ratio of branching fractions for charged and neutral modes would be of the highest importance, and would profit from the best precision possible on BR($K^+ \rightarrow \pi^+\nu\nu$). Implementing the ORKA experiment would help to build the team and develop the expertise necessary for this longer-term program. The experiment can also perform a number of other interesting measurements using the most intense source of stopped kaons in the world, which would lead to a significant number of publications and PhD theses.

The Committee feels that even in a constrained budget, the Laboratory should direct resources to this activity over the next several years in order to (1) assess the impact that ORKA might have on the current physics program (NOvA, Mu2e, g-2, etc.) and the planned LBNE effort, and (2) determine in more detail the resources that are needed from the Laboratory to make this a successful experiment. In particular, the ORKA collaboration brought up several matters in its presentation that may need immediate attention, including for example:

- The options for siting the experiment in the CDF hall at B0 or elsewhere at the Laboratory need to be resolved.
- Slow extraction from the Main Injector appears to be a solvable problem, but this needs to be verified.

Support from the Laboratory should be directed to fleshing out solutions to these issues. The ORKA collaboration presented a rather aggressive schedule to the Committee. This appears to be manageable given that the experimental technique is proven and is relatively low-risk. The collaboration appears to be strong presently, but may need to be strengthened, as it proposed already.

The Committee is eager to learn at the June meeting about the following issues:

- Attraction of substantive resources/in-kind contributions from other nations/laboratories/institutions.
- Progress on the dogleg design described in the ORKA presentation.
- Progress in more precisely determining the contingencies.

In summary, the Committee feels that even in the constrained budget scenario, the Laboratory should explore how ORKA could be included in the full complement of Intensity-Frontier experiments.

Neutrinos

The field of neutrino physics continues to produce tantalizing results. Fermilab, with its varied and vital neutrino-physics program, can address many crucial issues in this field, including confirming or refuting OPERA's result on superluminal neutrinos. The status and plans for the Fermilab neutrino program were put into perspective in presentations. Below we report our findings about this rich program.

LBNE

The 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 starting to be constrained by running experiments. The recent data from T2K, MINOS, and Double Chooz indicate that the value of $\sin^2 2\theta_{13}$ could be of order 0.1, which reinforces the case for a long baseline superbeam experiment.

The Committee congratulates the LBNE collaboration for the progress since last June, in particular for the decision taken for the beam layout and for continuing the process that should lead to a rapid decision on the detector technology.

The Committee is impressed also by important contributions of the Laboratory to liquid-argon R&D. We are very pleased that the Liquid Argon Purity Demonstrator has started operation. Having reached an oxygen contamination of less than 1 ppb without prior evacuation of the vessel achieves an important milestone towards building a large liquid-argon system. Given the promise of liquid-argon technology, the Committee reiterates its recommendation that this R&D should be vigorously pursued independently of the LBNE technology choice.

NOvA

The Committee was impressed by the progress achieved by NOvA. The Near Detector on the Surface (NDOS) is operational and has provided critical insight on many installation and integration issues, including fiber-manifold improvements and APD and APD-board coating. The Collaboration is now developing the calibration and analysis software using cosmic-ray muons and neutrino data. The far detector building has been completed and was ready for occupancy as of April 2011. The installation of the block pivoter is underway and the collaboration expects to complete the installation of half of the detector before the return of the beam in 2013. The Committee notes that the "*Proton Improvement Plan*" is essential to the success of the overall program. The Laboratory must carefully monitor its implementation. In view of the international competition, it is critical that beam is given as soon as possible to NOvA since the sensitivity to θ_{13} turns on very rapidly with early protons on target. The Committee would like to hear an update on the status of NOvA, including SciNOvA, at the next meeting, as well as possible strategies for running neutrinos versus antineutrinos.

MINERvA

MINERvA is on track to provide cross-section measurements useful for many of the existing and planned oscillation experiments. The Committee is pleased to see that the known problems related to the limited NuMI target lifetime appear to have been resolved. The Committee is pleased that the first quasi-elastic and nuclear-target distributions were shown at NuINT 2011. The water target was successfully installed in November 2011, and the cryogenic helium target was filled in time for the October 2011 neutrino run. The first steps have been taken to understand solutions for the safety issues associated with operating a deuterium target underground. The experiment has proposed two designs that are currently being evaluated. A Field Task Proposal will be written and sent to the DOE to cover conceptual-design costs.

MINOS+

During the June 2011 PAC meeting, the MINOS+ Collaboration presented a proposal for continued running of the MINOS detector during the first three years of NOvA running. The Committee recommended and the Laboratory gave Stage I approval of this proposal, but the Committee had questions about the MINOS+ sensitivity to sterile neutrinos. At this meeting the MINOS+ Collaboration presented an updated analysis of their sterile-neutrino sensitivity which the Committee found interesting and illuminating. The Committee now considers this question answered and looks forward to results from this analysis in the future.

In 2007, MINOS published an intriguing neutrino time-of-flight measurement, which indicated a shift of –126±32(stat.)±64(syst.) ns, limited by the systematic error. The MINOS/MINOS+ Collaboration presented plans to update this previous measurement of the speed of neutrinos in view of the recent OPERA result, which is compatible with superluminal neutrinos within the quoted systematic error below 10 ns. The MINOS plans involve a three-phase approach which will allow them to present some new results relatively quickly, as well as to have a measurement with much smaller uncertainties after data taking with new timing equipment installed for

MINOS+ running. The Committee found these plans to be very reasonable, and was pleased to see that MINOS/MINOS+ and the Laboratory had rapidly responded to the urgency to update this measurement.

Booster-Based Neutrino Experiments

An update of the status of the Booster-based neutrino experiments was reported in the meeting. MiniBooNE is collecting more antineutrino data, and has noted that the high-energy anomaly for antineutrino remains; but its significance is somewhat reduced. At the same time, MiniBooNE produced many results on cross sections for various neutrino interactions, which are important for neutrino physics. SciBooNE still continues its analysis activities, producing papers. The Committee congratulates MicroBooNE for obtaining CD 2/3a approval, and looks forward to a timely start of the experiment. The Committee encourages the Laboratory to support MicroBooNE in order to achieve its goals.

Mu2e

The Committee heard a presentation on the status of the Mu2e experiment, which aims for an unprecedented improvement of four orders of magnitude in sensitivity for muon-to-electron conversion. In order to contain project costs within the anticipated budget, the project group worked effectively with the Laboratory to make substantial revisions of the plans for the accelerator and experimental configurations. The requirements were scrutinized, the proton delivery scheme was re-evaluated by an accelerator task force, and major costs were reviewed. The removal of the Accumulator from the proton delivery system resulted in substantial savings at the expense of about a factor of three in intensity which can be compensated by a longer run time. The Committee commends the Collaboration and the Laboratory for this timely work.

Since Mu2e is a challenging experiment, requiring considerable resources, its estimates of sensitivity and expected background rates deserve careful scrutiny. The Committee was pleased to hear of the progress being made on detailed simulations and on the initial assessments of uncertainties in the background estimates. Recent investigation of backgrounds has prompted the requirement for an order-of-magnitude improvement in the proton-extinction level. The Committee recommends that the demonstration of the proton-extinction capabilities be carried out in a timely manner. The Committee encourages the experiment to continue to quantify the risks associated with uncertainties in sensitivity and background projections, making measurements where possible, and to put mitigation plans into place as early as possible to avoid erosion of discovery capability. The Committee also recommends that the Laboratory pay particular attention to ensure that the project is ready for effective reviews.

Tevatron

On September 30, 2011, the Tevatron's spectacular running was completed as planned. The Committee congratulates the entire Laboratory and the collaborations for the enormous success of this program, which produced great discoveries and shaped more than a generation of physicists in the US and around the world.

The end of data taking does not mean the end of the physics program. The CDF and DZero collaborations are working on a large number of results utilizing the full data set of 10 fb^{-1} per experiment, and important analysis improvements that enhance the physics reach are being implemented. The Committee was pleased to learn that the collaborations retain significant strength and are prioritizing the physics analyses to ensure timely coverage of critical topics. It is important to complete the Tevatron "legacy" with, *e.g.*, the final top and W mass measurements.

Although the Energy Frontier has moved to the LHC, Tevatron data still offer the best opportunities to study some processes. Most notably, the LHC and the Tevatron experiments explore complementary channels in the search for a low-mass Higgs boson. This is important, not only for the discovery or exclusion of this crucial piece of the Standard Model, but to probe Higgs couplings. The LHC and the Tevatron collaborations have started planning to combine Higgs search results. It is important to perform the combination while the Tevatron experts are still actively engaged in the data analysis.

The Committee was told of the work of the task force to address the W+2jets anomaly observed by the CDF experiment. The task force reported that a 2.5-sigma discrepancy between the event spectra of the two experiments remains. Work is continuing within CDF and DZero to update the results with the full data set and to address possible sources of systematic uncertainties. The Committee looks forward to seeing the progress in this situation from both experiments at the next meeting.

The Laboratory is urged to continue to provide necessary resources to ensure a timely completion of Tevatron physics analyses, including sufficient computing and support for visitors.

LHC upgrade

With the LHC delivering high luminosity to experiments and anticipating further luminosity increases, it is important to move forward on the upgrades to the detectors. As the host laboratory to the CMS experiment in the US, Fermilab is taking a lead role in the upgrades to several critical systems, including the pixel detector and the hadron calorimeter. The Committee encourages the Laboratory to continue its support to this important activity.

Detector R&D

The Committee received a comprehensive overview of the Detector R&D program at the Laboratory. This effort constitutes an investment of more than 30 full-time-equivalent engineers and technicians. It is an extremely broad program, with several projects presented (and even more referenced), and facilities that benefit the entire community. But Fermilab detector R&D is particularly strong in a few key areas. Notable are innovative efforts in:

- 3D ASIC technologies, which has spawned an international consortium of 17 members, including possible industrialization;
- Liquid-argon TPC development for massive detectors, including large-scale studies of how to purify liquid argon in an industrial-sized vessel without evacuation;

In addition to its numerous internal projects, Fermilab plays a leading role in collaboration with other laboratory and university detector R&D groups, including support of the LAPPD program; Digital Hadron Calorimetry, xTCA architecture program, Parallel Multiwavelength Optical DAQ devices, and others.

While a vigorous detector R&D effort supports the entire Fermilab physics program (Intensity, Energy, and Cosmic Frontiers), it also serves a national need, and one of particular importance to high-energy physics. To that end the Laboratory's long-standing collaboration with university groups helps to continue the training effort that has otherwise weakened on university campuses. The unique user-oriented test beam facility – currently expanding to an additional beamline – is oversubscribed and is an important resource for the community. The EDIT school scheduled for February 2012 is another example of Fermilab's program in support of the training of graduate students in hardware techniques.

Management of the detector R&D program has undergone structuring with an active internal Detector Advisory Group. There are plans to broaden the advisory structure beyond the Laboratory, which would bring a welcome additional spectrum of ideas. Enthusiastic participation in the newly forming APS/DPF standing Detector R&D Coordinating Panel is encouraged.

The Committee is pleased to see such vigorous efforts, and encourages the Laboratory to continue to maintain and support its high-profile, unique flagship programs in detector R&D.