Physics Advisory Committee

June 19-24, 2004
Aspen, Colorado

Comments and Recommendations

I. Introduction

At the 2004 Aspen Retreat, the Fermilab Physics Advisory Committee (PAC) heard presentations on the status and progress of many aspects of the Laboratory program and gave close consideration to four principal issues: the status of Run II experiments and accelerator performance; the impact of a possible staging of BTeV to accommodate a funding-limited schedule; the Dark Energy Survey proposal to build an astronomical camera for deep galaxy cluster surveys in the southern hemisphere; and the NOνA proposal for a long-baseline neutrino experiment to explore fundamental neutrino parameters.

Above all, the exciting improvements in Run II accelerator performance were the highlight of the meeting. The Tevatron peak luminosity has surpassed $8\times10^{31}cm^{-2}s^{-1}$ and the integrated luminosity has exceeded the *Design* projection consistently for four months. Shortly after the Aspen meeting the peak luminosity broke the $1\times10^{32}cm^{-2}s^{-1}$ mark. Mixed-source injection, an unexpected bonus of the highly successful Recycler improvements, appears to work well and may lead to yet higher luminosity in the future. The delivery of protons already exceeds the initial MINOS needs and almost meets MiniBooNE demand while both maintaining antiproton production at top priority for the collider program and decreasing proton losses in the Booster line. Careful attention to alignments and basic infrastructure throughout the complex has brought increased stability and robustness to accelerator operations. The Committee congratulates the entire Laboratory community on these accomplishments and expresses its hope for continued progress in the future.

BTeV is expected to be the flagship experiment in heavy quark and flavor physics in the next decade, and it continues to make excellent progress on all fronts towards that goal. Recently, however, revisions to its schedule mandated by a Baseline Readiness Review Committee resulted in a modified plan that calls for staged installation in the period 2009-2010. At this meeting, the Committee examined the issues to probe for possible impact that such staging might have on BTeV's physics capabilities and competitiveness.

In the area of particle astrophysics, the Laboratory continues to be involved in an excellent group of projects. The Sloan Digital Sky Survey (SDSS) has been wildly successful as a scientific project, and has made excellent use of the technical capabilities of the Computing Division. The Cold Dark Matter Search (CDMS II) recently registered a major success by publishing new limits on Weakly Interacting Massive Particle (WIMP) candidates that are currently the most stringent available. The Pierre Auger Project is making excellent progress toward completing construction, and a subset of the installed modules have begun to yield preliminary data. The Committee examined the Dark Energy Survey (DES) proposal in detail during this meeting. Although the overall scale of funding for the astrophysics efforts is modest, the projects involved are uniformly excellent and make good use of the technical capabilities where Fermilab enjoys a special advantage. In most cases there are scientific connections between the questions being addressed by these projects and those being addressed in the accelerator program.

Interest in neutrino physics has been profoundly invigorated in recent years by the discoveries of neutrino flavor oscillations and measurements of the mixing angles and Δm^2 parameters that drive these oscillations. Further advances in this area will require investigation of the mixing angle θ_{13}, the Δm^2 hierarchy, and the possibility of CP violation in the neutrino sector. The Laboratory is a natural candidate for siting such experiments, but the scale of the experiments is large, the scientific risks are substantial, and there is competition at other laboratories. The NOνA proposal, which had already been presented to this Committee for consideration at two previous meetings, has been evolving and was examined again in great detail. To understand better the world context for such experiments, a subset of the Committee, together with several outside experts, met for one and one-half days of talks, discussion, and study prior to the Aspen Retreat.

As has been the case for many years, the Laboratory continues to labor under budgets which do not keep pace with inflation. Up to some point the effective losses in operating funds can be passed on to the Laboratory infrastructure, but the losses now limit research options and threaten to impair the scientific mission of the Laboratory. In this document the Committee discusses possible future physics programs which are costly but address profound scientific issues such as the expansion of the universe, the nature of neutrinos, and the origin of matter/antimatter asymmetry. These major scientific goals cannot be realized without sufficient funding.

II. Current Activities and Laboratory Planning

Long Range Planning

The Fermilab Long Range Plan was developed in the past year and released this spring. The 15-member Long Range Planning Committee (LRPC) considered the future of Fermilab beyond the LHC, drawing on broad input from the Fermilab community, and considering the role of Fermilab in the exciting discoveries of the coming decades. Recognizing the strong current and near term programs, the LRPC, following the charge of the Director, considered two specific future scenarios for the Fermilab program. In one, the International Linear Collider is sited near Fermilab, and Fermilab serves as the host laboratory. In the second scenario, the linear collider is sited offshore, and Fermilab's future on-site accelerator-based program is advanced by the construction of a proton driver at Fermilab.

The Linear Collider has been recognized world-wide as the next collider project for the field of high-energy physics, with concurrent running with the LHC, and carried out by a global collaboration. This year the DOE Office of Science (SC) identified the Linear Collider as the top priority mid-term future facility in the SC program. Selection of Fermilab as the host laboratory for this project would be an extremely positive step for the future of the Laboratory and the U.S. high-energy physics program. The PAC supports the Laboratory's efforts to bring this about.

Fermilab is engaged in accelerator R&D for both potential technologies for the Linear Collider: that based on superconducting RF accelerating cavities, and that based on room-temperature X-band accelerating structures. The International Technology Recommendation Panel has been meeting since the beginning of this year to review the arguments for each approach, and is expected to recommend the preferred technology to the International Committee for Future Accelerators (ICFA) in 2004. Once this choice is made, the PAC encourages Fermilab to reinforce its activities on linear collider accelerator and detector R&D, and to compete aggressively to host the project. Fermilab will bid to be the host for the central project office for the global design team. The International Linear Collider Steering Committee envisions a siting decision for the linear collider itself in 2008.

A proton driver would be the focus of the on-site accelerator development in a variety of scenarios. This would be the case if the linear collider is sited offshore (the second scenario of the long range study), or if the linear collider is significantly delayed, or if accelerator developments for the linear collider and the proton driver were designed to be complementary, as is currently under study at Fermilab. A proton driver would advance the neutrino program as discussed below, and open other opportunities for the Laboratory. The complementary program that is now under study by the Laboratory in the event of selection of superconducting RF as the linear collider technology, considers a linear collider engineering test facility at Fermilab which might coincide with a linear accelerator proton driver. The Committee's view of the neutrino physics opportunity that the proton driver would create is discussed further in response to the NOνA proposal below.

Computing Division

The Committee heard a report on the activities and plans of the Computing Division (CD). The CD is providing computing resources to a wide range of Fermilab programs. Computing facilities are heavily utilized but are generally keeping up with the demand. In addition to the traditional hardware and software support for the experimental program, CD has embarked on joint projects with the Accelerator Division in areas such as simulations and improved Beam Position Monitor (BPM) readout and analysis.

There is a significant move towards sharing, both within CD, where experiments are being encouraged to use common tools and resources, and worldwide. For example, CD and CERN are working together on scientific Linux tailored to HEP. CD is also a driving force in large scale grid implementations. Fermilab experiments are already using substantial resources at other sites for reconstruction and analysis, and the grid efforts should substantially aid these efforts. Fermilab is host to a CMS Tier 1 computing facility which has played a major role in the first CMS data challenge and in the development of the USCMS Physics analysis center at Fermilab.

CD is working, within funding limitations, on improvements to the computing infrastructure of the Laboratory. Improved links to the national and international networks are being made. These upgrades are absolutely necessary for the CMS Tier 1 center and also current experiments with international data analysis. Onsite, Fermilab's CPU facilities are hitting the limit of power and cooling in the Feynman Computing Center. Experimental halls at New Muon and Wide Band are being retrofitted as computer centers.

Particle Physics Division

The Committee heard a report on many of the activities of the Particle Physics Division (PPD). These include support for running experiments such as CDF, DZero, MiniBooNE, MIPP, and CDMS, as well as future experiments such as MINOS, BTeV, CMS, and new proposals such as NOνA, DES, and FLARE, as well as experiments in the Meson Test Beam Facility and detector R&D for the Linear Collider. Among the significant infrastructure components in the Particle Physics Division, the SiDet facility and the ASIC development facility are prominent and have made numerous contributions to Run II experiments, BTeV, and many other experiments, ongoing and proposed. The PPD also provides essential manpower during shutdowns for activities such as accelerator alignment and instrumentation.

Technical Division

The Committee heard a report on the activities of the Technical Division (TD). These activities include responsibility for all magnet work at the Laboratory, from maintenance of magnets in accelerators to development of new magnet technology. Accelerator component R&D falls under the aegis of the Technical Division, and includes High-Field Magnets, the LHC Accelerator Research Program, Linear Collider R&D in many forms, and R&D for a proton driver, both in its synchrotron and superconducting linac forms. The Pierre Auger Project for extreme high-energy cosmic ray detection is part of the TD portfolio. Overall management of the US-LHC Accelerator Project encompasses design and construction of LHC IR regions, for which the TD has special responsibility for the high-gradient superconducting quadrupole assemblies. Looking ahead to BTeV operations, the TD is working to establish baseline design, cost, and schedule estimates for the C0 IR magnets.

The Committee was especially pleased to hear of the intense R&D work for the Linear Collider (LC), which is being carried out in close collaboration with the Accelerator Division (AD). The Committee notes Fermilab's unique role in collaborating on both warm and cold LC accelerator technologies. In collaboration with SLAC, Fermilab fabricated five NLC X-band RF structures which successfully achieved the LC Technical Review Committee R1 demonstration requirements. In collaboration with DESY, ANL and other laboratories, Fermilab is using TESLA-type SCRF cavities in the Fermilab/NICADD Photoinjector Laboratory. The Committee notes in particular that the ongoing R&D on SCRF is important and useful not only for a future LC but also as a possibility for the 8-GeV linac of a proton driver. Both activities on warm and cold R&D technologies are good bases for an intense involvement in the international LC R&D and design effort.

SY120 Experiment and Facilities

The MIPP (E-907) experiment in the Meson Center beamline is aimed at studying particle production on various nuclear targets using the 120 GeV proton beam from the Main Injector. The experiment overcame a fire in the RICH electronics and TPC wire breakage to bring the detector close to physics data-taking.

The Meson Test Beam Facility is being actively used for BTeV and a number of other experiments. The availability of this facility is also very valuable for other future experiments. The Committee would very much like to encourage the efforts already underway to attract more users, such as the Linear Collider and LHC detector R&D projects.

III. Run II

The great strides made in Tevatron luminosity and the first wave of physics results from CDF and DZero marked a very successful year for Run II.

Run II – Accelerator

The Committee would like to congratulate the Accelerator Division and the Laboratory as a whole for the impressive progress made during the last year in improving the Run II luminosity and the accelerator operations in general. A revised set of luminosity goals were established a year ago, following a major reorganization of the Accelerator Division to keep the Run II program on track. Since that time, the peak luminosity has recently reached a record $8.3\times10^{31}cm^{-2}s^{-1}$ and total delivered luminosity for Run II has exceeded 500 pb^{-1} per experiment. The integrated luminosity for the current run is even above the *Design* luminosity projection. This is a significant accomplishment in keeping this flagship program on track for the HEP community worldwide.

The improvements described reflect efforts across the whole accelerator complex, with most performance parameters already surpassing the design goals for the current run. The efforts to address systematically the reliability issues through daily planning are an integral part of this success. Close collaboration with the experiments, e.g. in resolving the CDF IP offset which led to improvements for both the detector and the machine, will hopefully be a continuing trend in further improving the overall collider operation efficiency. There is also encouraging progress in the preparations for the next major luminosity upgrades, especially the advance in Recycler commissioning and slip stacking. The innovative alternative commissioning strategy such as the mixed-mode antiproton extraction, utilizing the antiprotons from the Recycler, is an important development for a much more flexible introduction of the luminosity upgrades.

Full commissioning in the next two years of slip stacking and, especially, electron cooling, remain challenging tasks, and these two improvements are crucial for fully realizing the Run II luminosity potential. Priority should be given to installations and machine studies that will help realize these luminosity upgrades as early as possible. In the area of diagnostics, the deployment of the improved Tevatron Beam Position Monitor system is still to take place.

The latest round of luminosity improvements points to the importance of systematically tackling all performance parameters. It is very commendable that continued efforts are focused on remaining issues that prevent performance from reaching the projected design levels, such as the antiproton stacking rate. This persistent focus on achieving each performance goal may hold the key to maximizing the total integrated luminosity. The revised version 3 of the luminosity projection, with the adjusted electron cooling commissioning schedule and allowance for gradual ramp-up of luminosity upgrade benefits, now has a more realistic base. The Committee would very much like to encourage the pursuit of this plan to ultimately reach or exceed the design goal of a total integrated luminosity of 8.5 fb^{-1} by 2009, which could pay significant physics dividends.

As Run II continues, the Committee very much shares the concerns of management regarding various accelerator maintenance issues, such as Linac RF tube replacements and the shortage of technical expertise on older parts of the accelerator complex.

Run II – E-830/924 CDF (Young-Kee Kim / Luciano Ristori) and E-823/925 DZero (Gerald Blazey / John Womersley)

The Committee heard summary presentations about both the present detector operations and the status of the upgrade projects. Data-taking over the last year has been very successful and the experiments effectively exploited the excellent Tevatron luminosity performance. CDF and DZero have each collected more than 350 pb^{-1} of good-quality data on tape.

CDF / DZero Physics

CDF and DZero are starting to reap rewards from the greatly increased integrated luminosity collected in the past year. Very many analyses on Run II data are underway, including B physics, charm physics, top physics, Higgs searches, electroweak, QCD, SUSY searches, and searches for extra dimensions, with corresponding publications in preparation.

New measurements on the top quark mass are reported by both experiments. DZero has applied their Dynamic Likelihood Method to their Run I data. The measurement has already made an impact on precision electroweak analyses, nudging the expected central value of Higgs mass upward to 117 GeV. This improved technique, which is similarly applied in the CDF Run II analysis, represents the general effort of maximizing the sensitivity of precision measurements. The wide range of heavy flavor physics analyses, such as the first observation of $B_s \rightarrow \phi\phi$ by CDF, is starting to demonstrate the unique capabilities of the Tevatron in this area. A number of new particle search results remind Committee members of the exciting exploration of the energy frontier in the Run II program ahead. Many more forthcoming crucial measurements, such as Higgs searches, W mass, single-top cross section, and B_s mixing are eagerly awaited.

CDF / DZero Detector Operations

The Committee would like to congratulate the collaborations for successfully bringing the experiments into smooth physics operation, with data-taking efficiencies of 80-90%. The effort to reduce remaining inefficiencies will hopefully continue and further increase the fraction of data with all detector components fully functional for all analyses.

One major issue in CDF operations is the aging of the Central Outer Tracker chamber (COT). It is encouraging that a great deal of progress has been made in understanding the nature of the problem, and an active taskforce is working vigorously on solutions to stop and even recover from the degradation. One area of concern in DZero operations is the continuing issue of mortality of the electronics of the DZero silicon tracker. The Committee would like to encourage more aggressive research into the origin of the problem, which will hopefully lead to recovery of efficiency and long-term stability.

CDF / DZero Detector Upgrades

The CDF/DZero detector upgrades are generally progressing well and on schedule for implementation in summer/fall 2005, thereby matching the Tevatron luminosity increase expected. A number of upgrade installations are already planned for summer 2004.

It is particularly encouraging to see that the DZero Layer 0 silicon upgrade is progressing even slightly ahead of schedule, as this not only provides the protection for the overall tracking performance against radiation damage to other parts of the silicon tracker, but also brings valuable improvements to the tracking resolution to benefit many key measurements. The trigger and DAQ upgrades for both CDF and DZero are important steps to maximize the physics output as the Tevatron luminosity continues to increase. Among these upgrades, the CDF XFT/SVT upgrade appears to be particularly urgent, in part due to the COT radiation damage problem. This upgrade may involve major redesign efforts. A careful examination of the schedule for all these projects should be an important part of the CDF/D0 Director's Reviews scheduled for July.

IV. E-918 BTeV (Joel Butler / Sheldon Stone)

Introduction

The DOE Office of Science conducted a technical, cost and schedule review of the BTeV experiment in April, 2004. While the outcome of that review was positive with respect to technical and cost issues, the proposed schedule was judged to have inadequate float. In light of this, the BTeV collaboration has proposed a new schedule with a staged installation. The bulk of BTeV will still be installed in the summer of 2009; however, a fraction of the electromagnetic calorimeter and elements of the straw tracker and RICH are scheduled to be installed in the summer of 2010. The Committee has been charged with reviewing the initial and ultimate physics potential of BTeV in the context of the proposed staged schedule.

The completed BTeV detector will enjoy significant advantages over LHCb, including a superior, inclusive triggering scheme, a three-dimensional high-granularity pixel tracker, a higher resolution electromagnetic calorimeter, and a DAQ system that allows for high event rate to mass storage. Thus, despite the higher b-quark cross section at the LHC, each fb^{-1} of data is expected to yield a superior physics payoff for BTeV. The Committee found nothing in the staging scheme that will diminish these ultimate physics capabilities of BTeV.

The proposed staging preferentially maintains charged-mode capabilities in order to remain competitive in areas of LHCb's relative strength. Even so, BTeV will have significant physics capabilities in neutral modes, where LHCb is less capable, during Stage 1. By the summer of 2010, BTeV could have acquired about 1.0 fb^{-1} with their Stage 1 detector, and LHCb could have about 1.8fb^{-1}. The Committee reviewed a series of physics studies that compare initial results from a staged BTeV to LHCb. The results of these studies show that BTeV will become the superior experiment essentially as soon as BTeV data are available. Since LHCb will be online 1-2 years before BTeV, LHCb will have some opportunities for new physics discoveries. However, this statement is true even if BTeV is not staged.

The Committee finds the studies presented to be sound. The Committee expects BTeV to be competitive with LHCb as soon as BTeV starts analyzing data, giving it a good chance to participate in the initial measurements, which should have significant discovery potential. The Committee reiterates that nothing in the staged schedule will affect the expected superiority of BTeV on a wide range of compelling heavy flavor physics topics. In light of these findings, the Committee unanimously endorses the staging plan for BTeV.

Detailed Discussion

The Committee was presented with two important estimates for predicting the degradation in performance of the staged BTeV experiment. For analyses using charged modes that require a flavor tag, the staged configuration has approximately 75% the efficiency of the full BTeV, and for analyses that use decay modes with one π^0 or η and need a flavor tag, the staged configuration is approximately 45% as efficient as the full BTeV. Using these estimates, and further assuming the worst-case scenario, namely a physics analysis completely dominated by statistical errors, we can readily predict the increase in the uncertainty that a staged BTeV would have over the full BTeV for the same integrated luminosity. For a tagged charged mode, the uncertainty is increased by about 13%, and for a tagged neutral mode, the uncertainty is inflated by 49%. This gives a quantitative summary of the relative performance of a staged and full BTeV.

Additional factors should be considered when trying to compare initial running of a staged BTeV and LHCb. First we consider the expected luminosity for each detector. BTeV asserts that beyond 2008, the Tevatron will deliver 1.6 fb^{-1} to CZero in each ten-month running period per year. This assumption is consistent with the Tevatron Upgrade Project plan (which has a *Design* goal of 2.1 fb^{-1}/year for 2008), and is within a factor of three of the present Tevatron performance. The Committee notes that the *Base* performance goal, judged by the recent Lehman review to be "highly achievable," would achieve 1.2 fb^{-1} per year, or 75% of BTeV's assumed luminosity. In fact, given the recent decision to build an optimized interaction region at CZero, the anticipated ~1.6 fb^{-1} per year is even more likely now than it was when BTeV was previously recommended for approval by the PAC and P5, and there could yet be further gains - not assumed here - from running the Tevatron with only one interaction region. Thus the Committee finds it reasonable to assume that the Tevatron will deliver ~1.6 fb^{-1} per annual run period beyond 2008, and that during the period when BTeV expects to run for six months with their Stage 1 detector, the experiment should integrate approximately 1.0 fb^{-1}.

Luminosity to LHCb is estimated using the note by Collier, which indicates that they will obtain 0.1, 0.6, and 0.8 fb-1 in the years 2007, 2008, and 2009, and 0.8fb^{-1} in each subsequent year. The 0.1 in 2007 is not likely to be physics-quality data. Assuming half of the 2010 luminosity is integrated before the summer, this leads to a best-guess estimate of 1.8 fb^{-1} for LHCb by mid-summer of 2010. Note that after accelerator commissioning, BTeV should enjoy a factor of two advantage in integrated luminosity per year. This arises from the difference in planned yearly run periods, 10 months for the Tevatron and about 160 days for the LHC.

BTeV is commissioning during 2009 and 2010, while LHCb will be commissioning more slowly during low luminosity running in 2007 and 2008. The impact of commissioning is impossible to quantify reliably. BTeV argues that "both experiments have to commission so the losses cancel out". We hold some doubt about this statement. In all likelihood, BTeV will lose more useful luminosity to commissioning and other start-up factors, somewhat eroding their physics advantage to LHCb. As for the relative accelerator performance, the Tevatron turn-on in 2009 is more predictable at this time than early LHC performance, leaving a bigger downside uncertainty on LHCb luminosity. This could result in an advantage to BTeV during early running.

Finally, BTeV claims that it will write 1 kHz of B decays to tape and that LHCb will write 200 Hz, a factor of five in BTeV's favor. There is reasonable evidence obtained by private communication that LHCb can do better than 200 Hz - perhaps by a factor of two. However, 200 Hz is the figure that has been documented and subjected to internal and external review.

The above discussion leads us to believe that for running before the summer of 2010, BTeV's statistical uncertainties will be degraded by about 13% compared to an unstaged BTeV, for physics analyses for which we would expect LHCb to be competitive. The Committee does not believe the performance difference between a staged and an unstaged BTeV is significant. The luminosity disadvantage for BTeV before the summer of 2010 is not overwhelming, and once BTeV starts taking data, that disadvantage should be compensated for rather quickly.

The above discussion is simplified, but complements the more detailed studies presented by the BTeV collaboration. BTeV has conducted Monte Carlo studies to evaluate the impact of staging on measurements of four key physics modes, and compared the results to LHCb projections. The studies give expected uncertainties vs. year or for a given integrated luminosity. We summarize as follows.

1. γ, the phase of V_{ub}, from $B_s \to D_s K$.

   This analysis uses an all-charged decay mode and requires flavor tagging, for which the staged configuration will be 75% as efficient as the full BTeV. At the end of 2010, BTeV should measure γ with an uncertainty of approximately 11°, while for LHCb the uncertainty at the same point in time is expected to be about 14°. If BTeV runs in full configuration from the start, their uncertainty would be approximately 10° by the end of 2010. We consider the difference between staged and unstaged BTeV to be unimportant in the first year, and it is also unimportant in the long run, where BTeV is expected to measure γ in this mode with twice the precision of LHCb.

2. Determination of α from $B \to \rho\pi$.

   This mode involves reconstructing a π^0, something BTeV – staged or full – should do with significantly better efficiency and mass resolution than LHCb, due to its finer-grained, higher-resolution calorimeter. BTeV estimates that with 2 fb^{-1} of data and their Stage 1 detector, an uncertainty on α of 6.3° can be obtained. With the full BTeV, 2 fb^{-1} yields an uncertainty of 4.2°. LHCb does not provide an uncertainty on α in its TDR. However, the BTeV collaboration estimates that with 2.0 fb^{-1}, the LHCb uncertainty will be approximately 12°. The Committee agrees that BTeV's performance should be superior in either configuration.

3. Determination of χ.

   A measurement unique to hadron collider B experiments is the measurement of the B_s mixing phase, χ. Here BTeV can take advantage of its better capability on neutral decay modes. Using 2 fb^{-1} they estimate their uncertainty in χ will be 1.1° for a Stage 1 detector, and 0.7° using the full detector; the corresponding resolution for LHCb, which uses an all-charged mode, is 1.8°. Ultimately, BTeV should achieve a precision of about 0.5°, which is small enough to measure the expected Standard Model value; for LHCb the corresponding resolution is about 1°. The Committee judges BTeV to be superior in both the short term and the long term, independent of staging.

4. Measurement of $K^*\mu^+\mu^-$ branching ratio and decay kinematics.

   This electroweak penguin decay has rich potential for exploring new physics, and is one for which LHCb, with its muon trigger, is well suited. In this case BTeV and LHCb estimate their quality factor *QF* (roughly the square root of the normalized event yield weighted by the signal purity) to be comparable in the short term and long term. This is true whether BTeV is staged or unstaged.

These studies lead to the conclusion that the staging scheme has little impact on the BTeV performance in the first year of its running, and has no impact at all on the expectation of superior long term performance by the BTeV detector in most analyses.

Nevertheless, the Committee notes that LHCb will start one to two years earlier than BTeV, and in any analysis this is certainly an advantage for making early discoveries. If γ measured in $B_s \rightarrow D_s K$ turned out to be large, say 125°, LHCb will discover it first. If χ turns out to be large, say 5° or 6°, LHCb will discover it first. This is simply the advantage of being online first, and is largely unaffected by any question of BTeV staging. The Committee notes that any discovery made by LHCb in this period will feed the physics program of BTeV for the long run, and the ultimate superiority of the BTeV detector ensures that BTeV will write the final chapter in such cases.

General Observations

We conclude with several observations relevant to maximizing BTeV's discovery potential:

1. The BTeV collaboration has indicated that they will attempt to avoid the necessity of staging altogether, through a combination of additional non-DOE funding, additional forward funding, and minimizing the necessity of using the float in their schedule. The Committee encourages these efforts.

2. It is important to minimize the time needed for commissioning of BTeV after the detector goes on-line. The BTeV collaboration has a stated goal of completing their trigger and DAQ commissioning in the first month of running, which is quite aggressive. In recognition of the importance of rapid turn-on, the collaboration is making plans for extensive pre-commissioning of individual subsystems and of horizontal and vertical slices of the full detector. These efforts should be continued, extended where possible, and developed into an overall detailed commissioning plan.

3. Timely completion of BTeV will require augmenting the collaboration. In addition, rapid data analysis after BTeV turn-on will depend upon having a sufficient pool of physicists devoted to analysis, even as the staging activities are being completed. Rapid analysis will also require that offline software is ready near the time that BTeV turns on.

In conclusion, the Committee notes that the compelling physics reach of BTeV is largely independent of detailed assumptions about their modest staging scheme. The BTeV experiment will have unique sensitivity to effects from new physics at multi-TeV scales. The Committee expects that BTeV physics results will play an exciting and essential role in understanding new physics in the LHC era.

V. The Neutrino Program

Introduction

The Committee heard several presentations on the status of the present neutrino program, proton economics, and a summary of a Director's Review which covered neutrino projects worldwide. There were also oral presentations and substantial new materials submitted by the

NOνA collaboration. This section summarizes this information, gives the Committee's findings about the NOνA proposal, and concludes with some recommendations.

The physics case for NOνA must be assessed within the context of the international program in neutrino physics. It is both fortunate and timely that the American Physical Society is conducting a multi-divisional study of the neutrino physics program in the United States and its international context. The Committee heard an oral presentation on the current status of the APS study. The Committee hopes that its assessment of the NOνA proposal will provide useful input to the APS study, and looks forward to the recommendations from the APS study, which are expected to be available in August 2004.

The discovery of neutrino mass and mixing has raised very interesting questions about neutrino physics: How many different types of neutrinos are there? What are the masses of the neutrinos, and to what new, very high mass scale do they point? What is the pattern of mixing among the different neutrino species? Is there a CP-violating difference between neutrino and antineutrino oscillation? If there is, are neutrinos the key to understanding the matter-antimatter asymmetry of the universe?

The question of whether there are more than three types of neutrinos is currently being addressed. If there are only three, then the neutrino spectrum consists of two mass eigenstates, ν_1 and ν_2, that participate in the evolution of solar neutrinos, plus a third one, ν_3, that is a key player in the evolution of atmospheric neutrinos. The spacing between ν_1 and ν_2 is much smaller than that between them and ν_3. The coupling of ν_3 to electrons, described by a mixing angle θ_{13}, is known to be smaller than the other couplings, but it is not known *how* small. Given that two other measured angles, θ_{12} and θ_{23}, are large, most models predict sizable θ_{13}.

We do not know whether the ν_1 - ν_2 "solar pair" is lighter than the isolated neutrino ν_3 (a normal hierarchy), or heavier than it (an inverted hierarchy). Quarks and charged leptons have two lighter states closer in mass, hence this configuration is referred to as the normal hierarchy. Grand unified theories generically predict a normal hierarchy, while an inverted spectrum would suggest a new underlying symmetry. Thus, whether the neutrino mass spectrum is normal or inverted is a very interesting question.

The physical effects that can establish that neutrino oscillation does violate CP, and those that can discriminate between a normal and an inverted spectrum, both depend on the size of θ_{13}. Thus, determining the rough value of this angle is an important step on the road to the exploration of CP violation and of the nature of the spectrum.

Taken together, the measurement of θ_{13}, the determination of the normal or inverted character of the neutrino mass spectrum, and observation of CP violation in neutrino oscillation will form a very fundamental and important program of exploring physics beyond the Standard Model. Fermilab can play a leading role in this program.

Current Status and Future of Neutrino Beams at Fermilab

At present, the Laboratory is running one neutrino experiment, MiniBooNE, in the Booster neutrino beamline and is preparing to commission the NuMI beamline for the MINOS experiment. After major improvements in the performance of the Booster, including substantial reduction in the losses per protons delivered, the 8 GeV Booster beamline is now running at a rate of 10^{19} protons on target/week. A total of 3×10^{20} protons have been delivered to MiniBooNE, and the experiment should receive a total of 5×10^{20} protons by early 2005. The Committee congratulates the Accelerator Division on these improvements, and also recognizes the efforts of the MiniBooNE and MINOS collaborations.

NuMI commissioning is proceeding on schedule, with first beam extraction expected in December 2004 and substantial neutrino production starting in the first quarter of 2005. The Booster has already demonstrated the ability to produce enough protons for both NuMI running and antiproton production. However, once NuMI turns on, the number of protons delivered to the 8 GeV beamline will be substantially reduced. A program of improvements to the Booster complex was outlined. The improvements could provide the capability for some beam to Booster neutrino experiments during the NuMI era.

As the NuMI beamline turns on, the MINOS experiment should begin accumulating neutrino events. The far detector has been operational since 2003 and is taking data with cosmic rays, while the near detector is more than 50% assembled and should be ready for first beam at the start of 2005. The MINOS program will concentrate on ν_μ disappearance and will also have some sensitivity to $\nu_\mu\rightarrow\nu_e$ if $\sin^2 2\theta_{13}$ is close to 0.1.

The report of the Fermilab Long Range Planning Committee presents a vision of a future neutrino program. This vision includes further oscillation measurements with the Booster, if MiniBooNE results lead in that direction, and a program of low-energy neutrino cross section measurements useful to oscillation measurements and interesting in their own right. The vision also outlines a series of steps in a long-term oscillation program following MINOS. These steps include:

1. An experiment designed to measure θ_{13} with a sensitivity to $\sin^2 2\theta_{13} \sim 0.01$ and to determine the mass hierarchy if θ_{13} is not too small;

2. A proton driver that would enable the sensitivity to $\sin^2 2\theta_{13} \sim 0.005$, and enable determination of the mass hierarchy and the search for CP violation;

3. A possible second detector on the second oscillation maximum; and

4. A possible future Neutrino Factory.

In this approach, each step would be guided by the results of earlier steps. The Committee endorses this long-range vision and its implementation via a step-wise campaign to discover non-zero θ_{13}, followed by more precise measurement of $\sin^2 2\theta_{13}$, determination of the mass hierarchy, and search for CP violation.

A proton driver would raise the beam power available from the Main Injector by a factor of five, to 2 MW. Two technologies are being considered for the proton driver. One approach is similar to the existing linac and synchrotron, while the other is an 8 GeV superconducting linac. Either undertaking would greatly expand the physics capabilities of the Fermilab complex.

World-wide Context

In Europe, the experiments ICARUS and OPERA of the ongoing long-baseline (730 km) CNGS (CERN to Gran Sasso) neutrino oscillation program will soon search for ν_τ appearance in a ν_μ beam. Although not optimized for the measurement of $\sin^2 2\theta_{13}$[1], OPERA will be capable of establishing an upper limit on $\sin^2 2\theta_{13}<0.06$[2] on a timescale of ~2011. ICARUS, if funded for its complete detector, will be able to set a comparable limit. Proposed 50% improvements to proton intensity would lower this upper limit by ~20%. In Europe, a Superconducting Proton Linac (SPL), beta beams, and a megaton-scale detector are being studied for a long-range oscillation program starting some time after 2015.

The approved and funded T2K (Tokai to Kamiokande) experiment in Japan, after five running years in its Phase 1, could have the capability to discover ν_e appearance for values of $\sin^2 2\theta_{13}>0.018$[3] by ~2014 if the full intensity is obtained quickly after the turn-on. If $\sin^2 2\theta_{13}$ is smaller, then T2K may be able to set an upper limit as low as $\sin^2 2\theta_{13}<0.006$[4] on the same timescale. In Phase 2 (T2K-II), which involves substantially increased neutrino intensity and a megaton-scale water Cherenkov detector dubbed Hyper-Kamiokande, T2K's $\sin^2 2\theta_{13}$ discovery potential will extend down to 0.002[5], and in the absence of a ν_e signal its upper limit will be pushed down to $\sin^2 2\theta_{13}>0.001$[6]. Because of its relatively short baseline (295 km), T2K is not very sensitive to matter effects, and therefore cannot determine the mass hierarchy on its own.

There is a conceptual study of a wideband on-axis beam and 500-kton water Cherenkov detector with a ~2000 km baseline from Brookhaven. In this approach, a single experiment would observe multiple oscillation peaks and resolve θ_{13}, the mass hierarchy, and CP violation. It is not clear, however, if the neutral current background can be suppressed to an adequate level. This experiment may require a different detector technology, such as a liquid argon TPC, and/or a slightly off-axis beam.

Reactor oscillation experiments, which measure anti-ν_e disappearance, are capable of directly measuring $\sin^2 2\theta_{13}$ without the ambiguities imposed by the mass hierarchy uncertainty or by CP effects. Future experiments at reactors will aim at limits on $\sin^2 2\theta_{13}$ comparable to that

---

1 The neutrino beam has E=17GeV in order to operate above tau threshold; consequently, the L/E of CNGS is not at the ν_μ oscillation maximum.

2 At 90% confidence level assuming $\Delta m^2 = 2.5\text{x}10^{-3}$ ev^2.

3 Discovery at $\geq 3\sigma$, assuming $\Delta m^2 = 3.0\text{x}10^{-3}$ ev^2.

4 At 90% confidence level assuming $\Delta m^2 = 3.0\text{x}10^{-3}$ ev^2.

5 Discovery at $\geq 3\sigma$, assuming $\Delta m^2 = 3.0\text{x}10^{-3}$ ev^2.

6 At 90% confidence level assuming $\Delta m^2 = 3.0\text{x}10^{-3}$ ev^2.

of T2K, and are likely to be systematics-limited within a few years of running. A disappearance signal, if detected, may be difficult to establish without confirmation. Numerous reactor experiments are in the planning stages worldwide. The Double-CHOOZ experiment in France, whose Letter of Intent has been accepted, will be capable of an upper limit of $\sin^2 2\theta_{13}<0.02$[7]-0.03[8] on a timescale of ~2013, with first results of $\sin^2 2\theta_{13}<0.04$[9] in ~2009. Other possible reactor experiments may reach upper limits on $\sin^2 2\theta_{13}$ of 0.01-0.02. Once approved and funded, the timescales for construction and running of reactor experiments can be somewhat faster than long-baseline experiments. Future experiments at reactors may have results available as soon as 2011-2012. Thus, reactor oscillation experiments are roughly competitive in sensitivity and timescale with measurements from T2K. Limits on $\sin^2 2\theta_{13}$ from the reactor experiments depend only on Δm^2, while long-baseline experiments probe combinations of $\sin^2\theta_{23} \times \sin^2 2\theta_{13}$, matter effects, and CP effects. Therefore, reactor measurements are complementary to long-baseline measurements.

P-929 Proposal to Build an Off-Axis Detector to Study $\nu_\mu \rightarrow \nu_e$ Oscillations in the NuMI Beamline – NOνA (John Cooper / Gary Feldman)

The NOνA (NuMI Off-axis Electron ν Appearance Experiment) collaboration submitted a proposal to the Laboratory for consideration at the April, 2004 PAC meeting. The NOνA collaboration is a strong team consisting of over 150 physicists from 34 institutions. There is significant overlap with the MINOS collaboration. They propose the construction of a 50 kton, sampling detector built from particleboard and liquid scintillator with APD readout. The detector would be located above ground, with a long baseline of ~800 km and an off-axis displacement of ~12 km from the main NuMI beamline.

Additional written and presented materials were submitted at the June, 2004 PAC meeting to address questions raised by the PAC, to further quantify and refine the physics case, and to describe the ongoing R&D program. The collaboration also presented the preliminary design of an attractive alternative detector based on a totally active liquid scintillator design (TASD). Simulations of this option show an improvement in efficiency of almost a factor of two, and a cost per mass that is roughly double that of the sampling calorimeter. Better background rejection capability and improved energy resolution may give this option better overall sensitivity.

Physics Case for NOνA

To establish a compelling physics case, NOνA must meet the following criteria:

---

[7] At 90% confidence level assuming relative error of σ=0.2% between near and far detectors, $\Delta m^2 = 2.0\text{x}10^{-3}$ ev^2.
[8] At 90% confidence level assuming relative error of σ=0.6% between near and far detectors, $\Delta m^2 = 2.0\text{x}10^{-3}$ ev^2.
[9] At 90% confidence level assuming $\Delta m^2 = 2.0\text{x}10^{-3}$ ev^2.

1. Uniqueness.

   NOνA must have a unique physics capability not achieved by any other experiments worldwide.

2. Competitiveness with T2K.

   NOνA must compete with T2K, the Japanese program discussed above, within a similar time frame.

3. Competitiveness and/or complementarity with future experiments at reactors.

   NOνA must compete in sensitivity with reactor experiments, or provide information not obtainable by reactor experiments.

4. Capability for evolution with a future neutrino program.

   NOνA must allow a natural progression to CP violation studies with a future proton driver with the currently proposed detector at the same location.

In the near future, NOνA is the only experiment in the world that can potentially determine the mass hierarchy, albeit for a limited range of parameters. Its performance is competitive with T2K in other areas, namely the search for electron appearance for $\sin^2 2\theta_{13} \geq 0.01$ and precision measurements of $\sin^2 2\theta_{23}$ and Δm^2_{23}. A measurement of $\sin^2 2\theta_{13}$ in Europe that is competitive with those of NOνA and T2K on the timescale of their running without proton drivers is not foreseen. NOνA's electron appearance signature, which will be statistically limited, is complementary to the disappearance signature from the reactor experiments, which will be systematically limited and insensitive to matter effects and CP violation. Once electron appearance is found, it will make a strong case for a proton driver and possibly a second detector. The Committee finds the proposal meets the above four criteria if the detector can be built in a timely manner.

Following construction of a proton driver, the NOνA detector, possibly augmented with a second off-axis detector, would achieve its full physics reach, able to determine the mass hierarchy for any value of the CP-violating phase δ provided $\sin^2 2\theta_{13} \geq 0.02$. Such a determination would in turn allow 3 sigma discovery of CP violation for a large range of δ. In combination with the data from T2K-II, it would extend the reach in CP violation to much smaller $\sin^2 2\theta_{13}$.

How soon must NOνA start taking data in order to be timely? T2K will come online in 2009 using the existing SuperKamiokande detector and ramping up the new beamline over a four-year period. The situation is reversed for NOνA, since the NuMI beam will be in routine operation, whereas the detector must be built. NOνA can start data-taking with a near detector and a partial far detector (~15%), then increase the detector volume continuously thanks to its modular structure. The Committee concludes that NOνA must start data-taking in the same time frame as T2K, and complete the far detector within four years to meet this criterion. An early start for data-taking is essential because the sensitivity improves most rapidly in the first year or two of operation. That is, the most critical aspect of timeliness is when the data-taking starts, not when detector construction finishes. The Committee notes that the timely construction of NOνA is inconsistent with the present budget projection of the Laboratory.

In the context of a coherent long-range neutrino program, the Committee finds the case for NOνA compelling. The physics goals are to first measure $\sin^2 2\theta_{13}$, then to resolve the mass hierarchy and possibly discover CP violation in neutrino oscillations. This approach is attractive, proceeding in incremental steps that allow for decisions based on outcomes at each stage of the program, taking into account new results from other experiments, as well as funding constraints. A coherent vision for the long-term program, together with clear decision points, strengthens the case for NOνA.

Recommendations

The Committee strongly endorses the physics case for the NOνA detector, and would like to see NOνA proceed on a fast track that maximizes its physics impact. Both the physics case and the detector design have undergone rapid evolution since the PAC first received the NOνA proposal. While the Committee applauds this progress, it concludes that Stage I approval at this time is premature. The collaboration should first complete the following steps:

1. Finalize the choice of detector design, mass, and location.

   The totally active scintillator design looks very promising. If it is chosen, a revised proposal will be required for the Committee to recommend approval, as well as for the subsequent levels of approval the experiment must secure.

2. Complete the proposed R&D program.

   A demonstration of the photo-electron yield for a full-length cell of the chosen detector design is necessary; this is a key parameter underlying all of the physics simulations. Measurements of APD performance and detailed noise studies are important, and further engineering studies for the TASD option are also needed, if this option is selected. Evaluation of the cosmic ray background should also be done. The Committee is also interested to know how the construction schedule could be optimized for rapid initial start with a partial detector.

3. Update the proposal to reflect the complete science case.

   The revised proposal should include all the new information presented at the June, 2004 PAC meeting. A more complete discussion of possible neutrino measurements beyond $\sin^2 2\theta_{13}$ is also desirable (e.g., improved determination of $\sin^2 2\theta_{23}$, neutrino scattering cross section measurements with the near detector, etc.).

The Committee strongly endorses the proposed R&D plan and urges the Laboratory to provide adequate support for timely completion of this program. The NOνA collaboration should be encouraged to report back as soon as the above items are addressed. This would be the time for consideration of Stage I approval. In addition, the Committee recommends that the collaboration work together with the Fermilab directorate and the larger neutrino community to:

4. Develop a coherent vision for a future proton driver-based neutrino program, with NOνA as the first step.

   Such a vision would be consistent with the report of the Fermilab Long Range Planning Committee report. The APS neutrino study will be released in a few months, and should provide the context for a coherent national program of neutrino physics. The next step is to establish clear priorities and to work with the funding agencies to make this program a reality. The Committee believes that both NOνA and a proton driver should play an important role in this future program.

5. Explore accelerated funding mechanisms.

   The window of opportunity that achieves NOνA's scientific impact in a timely fashion is inconsistent with the availability of new construction funding in the Fermilab budget projection. In this projection, significant money for new initiatives is not available until FY 2010 at the earliest. The Committee encourages the Laboratory to work together with the funding agencies to put the necessary funding profile in place for a construction start in FY 2007, or in FY 2008 at the latest.

P-942 Letter of Intent: FLARE – Fermilab Liquid Argon Experiments (Adam Para)

The Laboratory received a Letter of Intent from the FLARE (Fermilab Liquid Argon Experiments) collaboration. The LOI describes several possible future LAr TPC neutrino experiments, ranging from a 40-ton neutrino scattering experiment located underground in the NuMI beamline, to a 400-ton $0\nu\beta\beta$ experiment using ^{100}Mo cathode foils, to a very large 50-kton long-baseline LAr detector located on the surface in the off-axis NuMI neutrino beam. The LAr TPC detector technology has been pioneered by ICARUS, a 600-ton LAr detector that has been successfully tested and will take data in the CNGS (CERN to Gran Sasso) beamline. There are plans for an eventual upgrade to 2 kton. Several members of the ICARUS collaboration are also members of the FLARE collaboration.

The advantages of LAr technology for neutrino detection include high efficiency, fine-grained imaging, good energy resolution, and particle identification capabilities. The very high efficiency for electrons from ν_e appearance (~90%) combined with very good π^0 rejection would make this an excellent long-baseline neutrino detector. The main issues are the technical feasibility of such a big extrapolation over the existing ICARUS detector, and cost. An optimal neutrino detector will maximize the product of mass and efficiency divided by total cost.

The LOI requests a modest level of support for an R&D program with the goal of building a small, 100-liter, 80-cm drift LAr detector. The goals of the R&D program are to effect a technology transfer of LAr technology from ICARUS, to study materials and purification methods consistent with the proposed commercial liquefied gas cryogenic container, and to carry out further engineering studies leading to better cost estimates.

The Committee believes that the LAr TPC technology has great promise for future neutrino experiments, especially on the time scale of a future proton driver. However, there are significant technical issues associated with the construction, assembly, and operation of such a large volume of ultra-pure cryogenic liquefied gas. The Committee believes that a significant engineering investment is required in order to arrive at a more realistic design that can serve as the basis of a more reliable cost estimate that that presented in the LOI. The proposed modest program of R&D is a step in the right direction, but will not convincingly establish the feasibility of the proposed approach.

VI. P-939 Dark Energy Survey (James Annis / Brenna Flaugher)

The Dark Energy Survey (DES) project involves the development of a large, wide-field, optical camera and wide-field optical corrector to be mounted at the prime focus of the National Optical Astronomy Observatory (NOAO) 4m Blanco telescope at the Cerro Tololo Interamerican Observatory (CTIO) in Chile. The new camera would be used to conduct a survey of over 5000 square degrees of sky in four optical passbands, providing a catalog of photometric redshifts for over 300 million galaxies out to $z \sim 1$. The database from such a survey would be used to constrain dark energy parameters via four complementary techniques: (1) through the abundance of clusters of galaxies as a function of mass and redshift; (2) through measurement of the weak lensing power spectrum on large angular scales; (3) through the evolution in the spatial distribution of galaxies; and (4) through the luminosity distances of a large sample of Type 1a supernovae. Of particular importance, the DES could provide photometric redshifts for the large sample of clusters of galaxies that will be detected using the Sunyaev-Zeldovitch effect by the South Pole Telescope (SPT), which will begin survey operations in 2007. As originally proposed, the DES would see “first light” at CTIO in the fall of 2008. In this context, it would also serve as both a technical and scientific precursor project to two larger programs envisioned for the next decade: the ground-based Large Synoptic Survey Telescope (LSST) and the space-based Joint Dark Energy Mission (JDEM).

The DES would be proposed in response to an Announcement of Opportunity issued by NOAO for partnerships leading to the development of new instrumentation for the Blanco telescope at CTIO. By the deadline established for the submission of letters of intent, the DES team was the only respondent to this announcement. A formal letter proposal is due in August 2004. If the DES proposal is selected, NOAO would commit internal resources to the implementation of the camera on the telescope, and once completed, would award one third of the observing time on the Blanco telescope to the DES team for a period of five years.

The Committee first heard an informational presentation on the DES at its April 2004 meeting. Having found the science opportunities presented by this project to be “very exciting and important", the team was encouraged to further develop its proposal in preparation for Stage I approval. In the interim, the Director convened a preliminary technical, cost, and schedule review of the DES, which occurred in early June. The Director's preliminary review team found the DES to be a scientifically exciting and technically feasible program, but raised several areas of concern about the project. Among the concerns was the aggressive schedule that had been adopted, particularly given uncertainties in the funding plan at the present time. In response, the DES team has presented a revised plan with a more relaxed funding profile, which leads to "first light" in the fall of 2009. The proposed funding outside Fermilab remains unsecured.

The PAC would like to see the issues raised by the preliminary review committee addressed by the DES team. In general, however, the Committee finds the science provided by the DES to be exciting and well worth the estimated costs of the project, assuming it can be carried out on the timescale of the new proposed schedule. The Committee therefore encourages the Director to approve the submission of the letter proposal to NOAO and to grant Stage I approval for the beginning of work on the DES at Fermilab. Given the set of technical, cost, and schedule concerns that have been identified, the Committee believes that, before proceeding to Stage II approval, it is essential that this program be reconsidered when a more mature understanding of the project, technically and otherwise, has been achieved, including cost estimates and the funding plan. If the projected costs grow, or the schedule slips further, the Committee would want to see the DES team consider trade-offs among cost, schedule, and scope in this context. The proposal submitted to NOAO should indicate a conditional commitment pending this subsequent review.