July 2003

The Fermilab Physics Advisory Committee met from June 14-19 in Aspen, Colorado. They spent most of their time on the evolution of Run II of the Collider. They heard reports on the status and prospects of the accelerators and the experiments. The reports of the PAC on Run II and on the future experiments BTeV and CKM continue to be a vital input to the deliberations of the Particle Physics Project Prioritization Panel.

The Committee discussed the Fermilab neutrino program, following up on preliminary discussions at their meetings in June and November, 2002. The Laboratory held a series of well-attended lectures in spring, 2003 on future neutrino options throughout particle physics. The MINOS Collaboration submitted a proposed run plan, and will make a presentation to the Committee at its next meeting based on that plan. Clearly the Committee will focus a great deal of attention on neutrinos in the coming year.

The Committee also heard about the potential of the accelerator complex to provide for the demands of the experimental physics program from Dave Finley, the chair of the Proton Committee. The final report of the Proton Committee, to be completed soon, will be an important part of evaluating the future of the neutrino program.

A new issue discussed at the March and June Meetings of the Committee is the possibility of Fermilab's participation in the new Supernova Acceleration Project (SNAP). This was part of a general discussion of the scope and role of particle astrophysics at the Laboratory.

Finally, the Committee considered the general health of the physics program and heard reports on the contributions of each of the Divisions to that program. The general report from the Committee is attached below. As usual, the quality of the work by the PAC was of extremely high standard and will inform our interactions with the elements of the program over the next months.

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Physics Advisory Committee

June 14-19, 2003
Aspen, Colorado

Comments and Recommendations

I. Introduction

Over the past few years, a number of important projects at the Laboratory have moved from being concepts or proposals to positions well on the road to realization. The Tevatron Run II detectors have been completed and are now reaching stable operation with efficient data-taking and have begun producing physics results. The civil construction for NuMI is nearly complete and the construction of the MINOS far detector has been completed. The MiniBooNE experiment is taking data with a proton flux approaching expectation. For the future, the BTeV and CKM projects have been granted Stage I approval and have carried out R&D programs that have resolved many of the important technical challenges. Over the rest of the decade, these experiments will allow the Laboratory to make major contributions to our understanding of fundamental physics. The Laboratory has constructed large-scale components for the LHC accelerator and the CMS experiment. By the end of the decade, physics analysis for the CMS experiment should be a significant part of the Laboratory's program. Finally, the Laboratory has begun its involvement in the great project of experimental high energy physics beyond the LHC, the international Linear Collider.

Thus, the Laboratory has excellent opportunities before it. However, there are serious questions as to whether the Laboratory can in fact realize these opportunities. These questions have to do with the overall level of funding of the Laboratory and the fundamental soundness of the Laboratory's accelerator infrastructure. The Committee sees a bright future for the Laboratory, but many possible obstacles in the path of reaching it. We now expand on both of these topics.

A. *Opportunities*

The Tevatron is and will be the world's highest energy collider from now until the operation of the LHC. This accelerator will allow searches for new physics in quark-antiquark scattering beyond 1 TeV in the center of mass, including searches for new particles with masses of 400 GeV and above. The facility will also dramatically improve our understanding of the top quark, establish the rate of B_s mixing if it is in the Standard Model range, explore very rare B decays, and bring the precision QCD tests at collider energies to a new level of precision.

The MINOS experiment should observe the minimum in neutrino charged-current events as a function of energy that is the characteristic sign of neutrino oscillations. This experiment should also make precise measurements of the parameters of the μ/τ neutrino oscillation. The MiniBooNE experiment should resolve the question of whether additional sterile neutrinos with larger mass differences are present. These experiments together will dramatically clarify the current picture of neutrino mass and mixing.

The BTeV experiment will be a unique facility in the US for the study of CP asymmetries in B meson decays, in that it combines the large statistics available in hadronic production, the availability of B_s as an object of study, relatively unbiased vertex-based triggering, excellent particle identification, and excellent electromagnetic calorimetry. BTeV will be an effective tool for measuring the CP angle γ from $B_s \rightarrow D_s K$ decays and the CP angle χ (expected to be small in the Standard Model) from $B_s \rightarrow \psi\eta, \eta'$, or ϕ decays. It will make a substantial contribution to the difficult experimental issues of measuring γ in B decays and measuring α in $B \rightarrow \rho\pi$. BTeV faces competition from the LHCb experiment in the measurement of B_s decays, but its qualitative advantages will be evident if BTeV can begin within about a year of the start of LHCb and with a luminosity comparable to that in the BTeV proposal.

The CKM experiment will be the first experiment on K physics that will provide a precise and theoretically clean determination of underlying quark mixing parameters. To achieve this, the CKM experiment will focus on an extremely rare decay of the K^+ and apply to it an elegant experimental method.

The Laboratory has been deeply engaged in the LHC physics program as the US host of the CMS experiment. The Laboratory has done an excellent job of building technical components for the CMS experiment and the LHC accelerator. The next step is to host a physics analysis center at the Laboratory that will create a community working there with sufficient size and knowledge to make discoveries from the LHC data. The Computing Division is already tackling the wide-area networking issues and GRID technology needed to make this a success. Realization of such a center would be an important step, perhaps an essential one, for the US high-energy physics community.

In the past two years, an energetic community has grown up around the idea of an off-axis neutrino experiment based on the NuMI beam. A major neutrino experiment using this strategy could be an important step toward the discovery of leptonic CP violation. Here there is competition from the Japanese neutrino program based on the J-PARC neutrino beam. However, the challenging nature of the experiments suggests an opportunity for complementary programs in the US and Japan.

Over the past two years, the principal governing bodies of high-energy physics in all regions of the world have put the next-generation linear collider as the first priority for the next major high-energy physics facility after the LHC. This is a tremendous opportunity for the Laboratory, which the Committee sees as an optimal site for this facility. If the Laboratory could win the role of host of the Linear Collider facility, that would ensure for it a bright future of physics discoveries.

B. *Challenges*

There are two major considerations that may block the path to this bright future. The first is the decline in the Laboratory budget in terms of purchasing power. The maintenance of the Laboratory at a budget roughly fixed in dollars and thus declining in real terms has been in place for almost ten years. In the 1990's, it was possible to compensate, to some extent, by streamlining the Laboratory's administration and support services. However, over the past four years, the cuts have come from the Laboratory's physics program. The Laboratory would have great difficulty surviving another five years in which its budget is constant in dollars. Such a budget would threaten the BTeV and CKM experiments and any future neutrino experiments. The Laboratory's ability to compete to host the Linear Collider is already being compromised by lack of funds to devote to Linear Collider R&D.

The second obstacle comes from serious questions that have been raised about the soundness of the Laboratory's aging accelerator complex. The Linac is working well, but it is not clear that replacement RF tubes for the front half of the Linac will be available once the current stores are exhausted. The Booster is now running at intensities ten times higher than the historical level. Its performance is limited by radiation from significant proton losses. The future plans of the Laboratory require running the Main Injector with currents in protons/sec 7-10 times greater than the current level. Technical improvements required for this, for example, slip-stacking, have not yet been demonstrated. The luminosity of Tevatron Run II has been limited by the unexpectedly poor performance of the accelerator complex. Part of the problem comes from the lack of basic understanding of beam dynamics in these devices, part from the unreliability of the Tevatron magnets and other components. These problems raise the possibility that the future program of the Laboratory could be compromised by its stewardship of the accelerator complex.

The Committee feels that the leaders of the Laboratory's divisions have a clear understanding of what the problems are in the accelerator complex and a strategy for addressing them. The recent modifications of the Booster have followed from improvements motivated by modelling of this device, and these have led to the expected performance gains. The luminosity increases of the Tevatron over the past year have followed from modifications of the Accumulator and removal of the C0 Lambertson magnet, both motivated by modelling. The Beams Division recognizes that the next steps in improving the performance of the Tevatron require a major increase in the level of instrumentation of the complex and the integration of the information from these instruments into a more sophisticated model of the beam dynamics. However, it is clear to the Committee that the execution of the plan to put the accelerator complex on a firmer basis requires additional resources – both money and engineering and computing talent. It is essential that these resources be applied to this fundamental problem. The Committee hopes also that the Fermilab user community will recognize the breadth of the concerns here and the importance of its help on the basic issues of the accelerators.

Despite the uncertainties about the future of the Laboratory, there is much reason for optimism. Indeed, the Laboratory could well find itself ten years from now with major achievements from the current program and stewardship of a great international project. The Committee encourages the management and staff of the Laboratory to keep their attention on the prizes that the future offers and the renown that will come from the hard work needed to clear the path to them.

II. Current Activities and Laboratory Planning

A. *Division Reports*

The Divisions form the backbone of the Laboratory and their high quality performance is essential to success in every Laboratory mission. The Committee heard comprehensive reports from the heads of the Beams Division, Computing Division, Technical Division, and Particle Physics Division. All reported excellent progress, extensive portfolios of challenging projects, and good focus on both near-term and longer-term issues confronting the Laboratory.

Beams Division efforts are covered elsewhere in this summary.

The Computing Division (CD) recognizes its triple role in serving the basic infrastructure needs of the Laboratory, participating in scientific endeavors, and keeping the Laboratory's computing capabilities at the frontier of current technology. The Committee notes in particular the efforts of the CD to aid the Beams Division in Run II projects such as the Shot Data Analysis, Recycler software, and instrumentation in the Main Injector and the Recycler, Tevatron emittance, orbit, and tune studies, as well as many other efforts involving instrumentation and software throughout the accelerator complex. The Committee applauds the cross-division cooperation evident in this effort. Support for CDF and D0, and the handling of extremely high volume data storage, data processing, and I/O is a notable and continuing effort of the Division. The Division is also active in pushing the envelope of wide-area networking and is deeply involved in the GRID technology that will support future Fermilab leadership in CMS data processing and analysis. The Committee further notes the Division’s essential participation in supporting astrophysics and computationally intensive theory projects in the Laboratory.

The Technical Division (TD) provides technical infrastructure and support for ongoing accelerator operations, particularly Run II, construction projects such as LHC magnets and detector components, and future-oriented tasks such as accelerator R&D projects. Work on magnets includes building, measuring, repairing, and maintaining magnets throughout the Laboratory complex, and recently has been extended to cover detailed studies of the Tevatron magnet field errors, aging problems, and alignment issues. LHC magnet development is proceeding very well, as are detector construction tasks for CMS and Auger. Accelerator R&D includes high-field magnet development and exploration of novel superconductor materials and construction technologies for future accelerators such as the VLHC, as well as LHC upgrades under the LARP program. The Committee was pleased to see the range of activities, the willingness of the Division to take on tasks outside its traditional range, and the scope of future-oriented R&D being conducted within the Division.

The Particle Physics Division (PPD) provides extensive operational, technical, and R&D support for all experimental activities, ongoing or planned, at the Laboratory: CDF, D0, NuMI/MINOS, MiniBooNE, CMS, CDMS, E-907, BTeV, and CKM. This includes operation of the SiDet Facility, essential engineering for electronics developments such as the SVX4 chip, BTeV's FPIX readout chip, the QIE chip for CMS, and the FSSR chip for BTeV strip detectors. In addition, mechanical, construction, and R&D efforts for detector work are devoted to the BTeV pixel detectors, the CKM vacuum straw chambers, instrumentation for the new test beam facilities, and many other projects. The Committee recognizes the crucial role of the PPD throughout all the Laboratory experimental activities and the significant contributions to several accelerator-related instrumentation projects and studies.

B. ***Long-Range Planning***

The Committee commends the Laboratory on its recent initiative to engage both Laboratory staff and outside physicists from the HEP community in a broad review of the future of the Laboratory, and looks forward to the report of the newly formed Long Range Planning Committee at its next meeting.

The Committee urges the Laboratory in its planning exercises to ensure that accelerator-based physics continues to be the fundamental focus and mission of the Laboratory.

C. ***Short- and Long-Range Budget and Schedule***

The Committee was shown the Laboratory budget projections for the next ten years. Given the roughly fixed dollar levels of the overall Laboratory budget, real purchasing power has been steadily eroded over the previous ten years and the Laboratory has responded by steadily streamlining its administrative and support services. This strategy has reached its limit however, and the physics program of the coming decade will be threatened by any further erosion. The projection based on constant purchasing power on the other hand, as shown to the Committee, allows completion of Run II obligations, BTeV and CKM funding, and the opening of a significant opportunity for new initiatives beyond 2007. Future budget allocations are unpredictable, but the Committee welcomes the prospect of new initiatives for that period.

The Committee was also shown draft schedules for the coming ten years which cover all aspects of the approved activity based on the Fermilab accelerators. A strong neutrino program is evident, BTeV and CKM both appear in the outyears, and the high-p_t Collider program runs to the latter part of 2009 as will be discussed in further detail in the next section. There are significant "OPEN" periods later in the decade, and the details are likely to evolve as the Laboratory program and budget evolve. The Committee appreciates seeing the long-range scheduling planning and looks forward to seeing such presentations each summer at Aspen.

III. Run II

A. *Introduction*

Run II is the centerpiece of the Laboratory's physics program, and significant time and discussion was devoted to it at Aspen. The luminosity is still below expectations, resulting in considerable frustration and uncertainty. The Committee received a number of reports, including the final report from a recent Director's accelerator review, and heard presentations from the head of the Beams Division, and from the Beams Division Assistant Head, who presented a new Run II Upgrade Project Plan. This plan is resource-limited, despite an accelerator share of the research budget growing from \$60M in FY01 to \$90M in FY04, with the overall Laboratory budget lagging inflation.

The Committee also heard updates from CDF and D0 about the performance of their detectors, the status of the planned detector upgrades, and the collaborations' responses to the most recent luminosity projections. Both detectors are performing well now, having completed their commissioning periods, and are achieving operational efficiencies of 85-90%. It was unfortunate that the results of a new study, undertaken jointly by the collaborations to re-evaluate the Higgs reach as a function of integrated luminosity, were not yet available to inform the PAC discussions. A large number of new physics results will be presented at the summer conferences based on 100-140 pb^{-1}, a data set larger than all of Run I, recorded at higher energy, and with improved detectors. This will be the first trickle of the torrent of physics the Committee expects to flow from Run II.

B. *Run II Accelerator*

The Committee heard reports on the current status of the accelerator complex and on the program of luminosity upgrades for Collider Run II. While there has been progress, the Tevatron luminosity continues to fall short of expectations and of the needs of the physics program. In recent months this has led to a heightened level of scrutiny, with numerous internal and external reviews. Last fall's DOE review of Run II luminosity performance provided guidance for the formulation of the project plan. The Laboratory and the Committee have also benefitted from the recent report of the Director's Review Committee for Tevatron Run II. The upcoming DOE review in July will be important in establishing the viability of the project as presently formulated.

The head of the Beams Division described the significant reorganization he has undertaken of the Beams Division and the luminosity upgrade effort. The Committee applauds these actions, and agrees with his assessment that much work remains to maximize the effectiveness of the intellectual and technical resources of Beams Division. The Assistant Head for the Run II Upgrade presented a comprehensive report on the upgrade program, corresponding to the "Run II Luminosity Upgrade Project Plan and Resource-Loaded Schedule" submitted to DOE on June 15. This plan reflects an across-the-board reassessment and puts the upgrade program on a firm project footing. Some components have been eliminated as not providing sufficient potential return on investment, such as recycling antiprotons at the end of a store and the option for 132-ns bunch spacing. Other components, such as electron cooling in the Recycler, have been reaffirmed as critical, and now have more detailed plans and milestones. Other items have moved into higher-priority positions, including refinement of simulations and beam-position instrumentation, systematic addressing of maintenance challenges, and the development of improved operational procedures.

The most disappointing aspect of the newly formulated project plan is a considerable lowering of luminosity expectations, calling into question the potential for Run II to achieve all its physics goals. It is absolutely clear that achieving or exceeding the *Design* goals described in the project plan must be the Laboratory's highest priority. Organizational barriers to success must be removed, and resources must be provided so that the integrated luminosity ultimately achieved represents the technical limitation of the Tevatron.

The Committee would like to highlight some special concerns. Electron cooling in the Recycler has been singled out by reviewers as posing significant risks. The Recycler is needed for electron cooling by October 2004, but its commissioning has been delayed by technical problems, especially vacuum, exacerbated by limited tunnel access. A decision on how to proceed is planned in December 2003. Electron cooling is essential to the plan as currently formulated: a limit of ~1 fb^{-1} per year is envisioned without it. The risks associated with the other components of the luminosity upgrades arise from factors such as the application of established concepts under unprecedented conditions. For example, slip stacking requires beam manipulations at currents exceeding previous values, and the much higher antiproton intensities could lead to a transition from a strong-weak to a strong-strong regime in beam-beam interactions.

The Committee is alarmed by the numerous potential maintenance and operational vulnerabilities that have been identified by the Beams Division and reviewers. While it appears that appropriate steps are being taken, a lack of critical spare parts such as linac power tubes represents a single point of failure for the entire accelerator complex. Other potential vulnerabilities arise from an aging infrastructure and require a continuing program of inspection, repair and replacement of components. Another area where Beams Division leadership must be forceful in establishing priorities and identifying the required expertise is the enhancement of accelerator instrumentation and control systems. A more effective translation of instantaneous luminosity into integrated luminosity could be the deciding factor in reaching the Run II goals.

The Committee commends the Computing, Particle Physics and Technical Divisions on their many valuable contributions to Run II, especially the Shot Data Analysis (CD) and analysis of the Tevatron magnet alignment issues (TD). Other national labs have also begun to contribute important expertise. Addressing the technical challenges of the program will require continued aggressive recruitment of talent from within Fermilab, from other labs, and from the community of interested and highly motivated experimenters. Project-specific panels of consultants, in addition to more traditional advisory committees, could be useful. The "task-force" approach, with daily meetings of principals, has been very effective at other labs facing similar challenges. The Committee applauds the steps that have already been taken in these directions, and encourages that efforts be redoubled.

C. ***Physics with 2 fb^{-1}***

An integrated luminosity of 2 fb^{-1} corresponds to almost 20 times as much data as was collected in Run I. This will significantly sharpen our understanding of the Standard Model, and provide new possibilities in the search for new physics.

The top quark, discovered at the Tevatron in 1995, occupies a central role in Run II physics. The properties of the top quark will be more precisely measured, looking for any deviations from the predictions of the Standard Model. This includes measurements of the top-quark production cross section and the angular distribution of its decay products. An improved measurement of the top-quark mass is particularly interesting in the context of precision electroweak analyses (see below). The electroweak production of single top quarks via *t*-channel *W* exchange, $qb \rightarrow q't$, will be observed for the first time. This yields a direct measurement of $|V_{tb}|$ with an accuracy of 10%.

The top-quark mass, combined with a precision measurement of the *W* mass, yields a very sensitive test of the Standard Model. These measurements currently indicate that the Higgs boson is relatively light. However, the internal consistency of the precision electroweak data is marginal. The anticipated measurements, with an accuracy of $\delta M_W \sim 30$ MeV and $\delta m_t \sim 3$ GeV per experiment, might alleviate or exacerbate this tension, and will tighten the allowed range of the SM Higgs boson mass to an accuracy of about 45%.

An integrated luminosity of 2 fb^{-1} will yield many interesting results in *B* physics. Among the most important measurements is likely to be the rate of B_s mixing. This will allow the extraction of $|V_{td}|$ with about half its current uncertainty which, when combined with measured B_d mixing, provides a sensitive test of the CKM model of quark mixing. Even more interesting would be a limit on B_s mixing that is in conflict with the CKM model.

There will also be interesting measurements of basic QCD processes. The production of *B* and J/ψ mesons in Run I did not agree well with theoretical expectations. We hope to understand these discrepancies with much larger data sets, which will allow more detailed comparisons of theory and experiment. The production of jets will probe QCD at the highest energies available, and will test our understanding of parton distribution functions at large values of x_{BJ}.

In addition, we hope to find physics beyond the Standard Model. The Tevatron will be sensitive to new physics in the 100 - 1000 GeV range, a region that has not yet been explored. It is possible that extensions of the Standard-Model gauge group may be found, manifested in heavy *Z'* or *W'* gauge bosons. Rare *B* decays offer another avenue to search for new physics. Evidence could be found supporting the theory that our three-dimensional space is embedded in a space of larger dimensions. An attractive possibility is that nature is supersymmetric at the weak scale; Run II could reveal the superpartners of the known particles, including the particle responsible for the dark matter content of the universe. Any new physics that we uncover in Run II will be a major discovery that will change the landscape of particle physics and could have major implications for our understanding of the cosmos.

D. ***Physics Beyond 2 fb^{-1}***

Integrated luminosities beyond 2 fb^{-1} will allow us to further sharpen our understanding of the Standard Model, and to increase the sensitivity with which we probe for new physics. We do not know at exactly what energy this new physics might appear, so each additional fb^{-1} represents a new opportunity for a major discovery. Because the Committee is reconsidering the need for silicon vertex detector upgrades, the following discussion will be presented in terms of the *b*-tagging requirements of various physics analyses. When specific numbers are quoted, they are taken from the D0 studies summarized at this meeting.

Many Standard Model measurements do not require *b*-tagging. With 10 fb^{-1} of integrated luminosity, the *W* mass can be measured with an accuracy of $\delta M_W \sim 20$ MeV, and the top-quark mass can be measured in the dilepton mode with an accuracy of $\delta m_t \sim 2$ GeV per experiment. These two measurements combined will provide an indirect determination of the SM Higgs mass with an accuracy of about 30% per experiment. This approaches the accuracy with which the top-quark mass was indirectly determined by precision electroweak analyses prior to its discovery at the Tevatron.

In addition, many searches for new physics do not require *b*-tagging. The search for a *Z'* boson via its leptonic decay reaches 1 TeV per experiment with 10 fb^{-1} of integrated luminosity. The search for the supersymmetric partners of electroweak bosons via a trilepton signal can be improved significantly with increasing luminosity if these particles are relatively light. The supersymmetric partners of gluons and quarks may be sought via their decays to jets and leptons accompanied by large missing transverse energy.

With the exception of the dilepton signal discussed above, most top physics requires at least one *b*-tag in order to separate the signal from backgrounds. The top-quark mass will be measured with an accuracy comparable to or greater than that of the dilepton mode using the *W*+4 jet signal with one or two *b*-tags. Single-top production via *t*-channel *W* exchange requires a single *b*-tag, and yields an accuracy on $|V_{tb}|$ of 8% per experiment with 10 fb^{-1} of integrated luminosity. Supersymmetric models include particles whose decays result in final-state *b*-jets.

With more than 2 fb^{-1} it will also be possible to observe *s*-channel single-top production, $\overline{qq'} \to t\bar{b}$, which relies on two *b*-tags. This yields the most accurate measurement of $|V_{tb}|$, with an uncertainty of 6% per experiment with 10 fb^{-1}. Double *b*-tagging also yields the purest sample of $t\bar{t}$ events, with almost no background and the minimal combinatoric ambiguity.

The most prominent physics goal that requires two *b*-tags is the search for the Standard-Model Higgs boson via *Wh* or *Zh* with $h \to b\bar{b}$. As mentioned above, precision electroweak analyses indicate that the Higgs boson is relatively light; in addition, the minimal supersymmetric model requires $m_h < 135$ GeV. At the current lower bound on the Higgs mass, $m_h > 114.4$ GeV, and combining both experiments, a 3σ (5σ) signal requires 3.5 (10) fb^{-1} of integrated luminosity; $m_h = 130$ GeV requires 10 (30) fb^{-1}. (These figures are presently under study by the collaborations.)

The minimal supersymmetric model has two Higgs doublets (with vacuum-expectation values v_1, v_2), which results in several Higgs bosons (h^0, H^0, A^0, $H^\pm$), some of which have enhanced coupling to the *b*-quark for large values of tanβ = v_2/v_1. If they are sufficiently light and tanβ is sufficiently large ($4 < \tan\beta \leq 50$ is the most plausible range), these Higgs bosons may be observed using $gb \rightarrow hb$ with $h \rightarrow b\bar{b}$, which requires three *b*-tags. This search reached the LEP limit with tanβ = 50 in Run I, so Run II is entering unexplored territory. A supersymmetric Higgs boson (A^0) of mass 150 GeV can be excluded for tanβ > 27 with 10 fb^{-1} of integrated luminosity.

The search channels for the Standard-Model Higgs and the supersymmetric Higgs described above have much higher backgrounds at the LHC than at the Tevatron. Thus the Tevatron, with sufficient luminosity, could prove to be complementary to the LHC for low-mass Higgs sensitivity.

E. ***Run II Detector Silicon Upgrades***

Brief status reports were presented by the collaborations on their silicon upgrade projects. They have received approval from DOE for a construction start (CD-3a) and have funds to support the projects through the end of the calendar year. With this funding they have made good technical progress. The SVX4 readout chip, which will be used by both projects, is a notable success, having achieved a working chip that meets all requirements in the pre-production run. Early estimates of the yield are good and suggest that a large fraction of the required integrated circuits are already in hand. Sensor orders are also well advanced; CDF has ordered all of its sensors, financed by its Japanese collaborators, and D0 has ordered sensors for Layers 2 – 5 and expects to place the order for the Layers 0 and 1 sensors on July 14. Both projects are holding to their baseline schedules. D0 has spent or obligated $7.3M of the $20.9M total silicon project cost; $1.9M of the current obligations is non-DOE funds. CDF has obligated $3.9M of the $20.5M total (including contingency) silicon project cost; $1.0M of the obligations is non-DOE funds. These figures include contingency (in the totals), G&A, and both Equipment and R&D as defined for the Projects.

The Committee devoted some time to a re-examination of the failure mechanisms of the existing silicon detectors, in light of the reduced luminosity estimates. The sensitivity to radiation damage and detector failure mechanisms are different for CDF and D0, and were discussed separately. The failure mechanisms were then mapped, based on information provided by the collaborations, into degradation of performance of the currently-installed detectors. In addition, the non-silicon upgrades were discussed, albeit less rigorously.

F. ***Run II Detector Upgrades – Non-silicon***

Due to focus on the dominant issue of the silicon upgrades at this meeting, there was only very brief mention of other upgrade projects in the presentations, and the Committee did not discuss them extensively. Most of the non-silicon detector upgrades are driven by instantaneous luminosity and were generally designed for peak luminosities of 4×10^{32} at 396ns bunch separation. However, some of these upgrades are already desirable at considerably lower luminosities. The total cost of the "non-silicon" upgrades is $7.3M for CDF and $7.7M for D0.

These costs include the full costs of project management, both for silicon and non-silicon components, since both CDF and D0 projects have management as a unified WBS element.

The upgrades of trigger and DAQ (including COT TDC for CDF) for both CDF and D0 are well-motivated, enhancing the capabilities of the detectors in physics acceptance and background rejection. These upgrades are cost-effective measures to maximize the physics output and could presumably be installed during the annual shutdowns. Given the current reconsideration of the silicon detector upgrades in light of the revised Tevatron luminosity projections, the continuation of the CDF SVT upgrade and the D0 L2 Si trigger upgrade, both aimed at the Run IIb upgrade Si vertex detectors, will depend on the Si upgrade decision. In addition, without a long shutdown in FY06 the CDF EM calorimeter pre-radiator might not be installed, and CDF could suffer a somewhat reduced performance for electron identification and jet-energy resolution as the instantaneous luminosity increases.

G. ***Run II Conclusions & Recommendations***

The Committee is disappointed by the luminosity performance of the Tevatron to date. Organizational changes within Beams Division have already occurred, and more may be necessary in order to accelerate the rate of technical progress. The Computing, Particle Physics and Technical Divisions have made important contributions, and such contributions are to be strongly encouraged; likewise the help of other accelerator laboratories must be enlisted. A careful re-evaluation of delivered integrated luminosity for Run II is required to validate the recently released projections. This re-evaluation will occur during the Director's and DOE accelerator reviews scheduled in July. Another important milestone in understanding future luminosity prospects will be the tests of the Recycler after the summer shutdown.

The Committee believes that the following considerations are the most important in deciding whether or not it is advisable for CDF and D0 to continue along their current paths of constructing upgraded silicon detectors for Run IIb. In the paragraphs to follow “upgrades” should be understood to refer only to the upgrades of the silicon trackers.

Factors in favor of continuing with the silicon upgrades include:

- Abandoning the upgrades at this point risks capping the upside potential for success of Run II. For example, in a scenario where the performance of the Tevatron exceeds the *Design* projection and/or the LHC or its detectors encounter unexpected delays, the Laboratory could find itself in a position of having detectors unable to exploit fully the luminosity delivered by the machine.

- Upgraded detectors will mitigate the risk associated with tracker failures from a variety of mechanisms in addition to radiation damage. Successful operation through to the end of Run II will require detector lifetimes in excess of what has been explored by collider silicon detectors to date.

- Canceling the upgrades at this juncture would forfeit the efforts of the highly dedicated and talented teams working on the detectors and the financial contributions of the US and international funding agencies.

Factors in favor of discontinuing the silicon upgrades include:

- In the simple model considered by the Committee, estimates of effective luminosity weighted by double *b*-tag efficiencies achieved with and without the upgrades show, at best, modest gains in sensitivity over a broad range of parameters. Circumstances in which the upgrades may be justified in terms of integrated luminosity appear to require that the Tevatron performance meet or exceed that of the *Design* scenario.

- The resources that would be freed up by canceling the upgrades are urgently needed elsewhere, and could, for example, be put to use in improving the performance of the Tevatron and/or attending to pressing maintenance issues that are important to efficient Tevatron operation and timely accumulation of 2 fb^{-1}.

- The transition from functioning and well-understood detectors to their improved, but unproven, replacements will introduce a new, and real, element of risk.

- Even in scenarios where the effective integrated luminosity is greater with the replacement detectors, the gains tend to come late in the Run II program, when timely start-up of the LHC could undercut any advantage of the upgrades and potentially divert manpower away from Run II.

The Committee reaffirms its conviction that the physics motivations driving the Run II program are compelling. The investment that the Laboratory and the HEP community have made in the Tevatron and in Run II over the years is fully justified based on the exciting discovery potential of this program. Realizing the full breadth of this physics program depends on optimizing the combined performance of the entire accelerator complex and the CDF and D0 detectors. The Committee finds that the improvements to trigger and DAQ components in the detector upgrade plans are cost-effective measures that will help maximize the physics output under any of the foreseen luminosity scenarios. As for the silicon upgrades, the decision on their future must be taken in a measured, deliberate fashion, fully informed by a careful evaluation of all relevant factors.

Four significant factors are either not well known at this time or were not available to the Committee. They are: (a) the probability that the Tevatron luminosity will meet or exceed the *Design* profile;, (b) the life expectancy of the current silicon detectors; (c) the details of how the *b*-tagging efficiency degrades as sensors deteriorate; and (d) the benefit of re-directing resources from the silicon upgrades to the luminosity performance. Careful assessments of these factors, using the best available information from appropriate accelerator and detector experts, is required to arrive at a well-informed decision on the silicon upgrades. The Committee recommends that the Director make this decision on the timescale of the next few months.

IV. Other Initiatives

A. *Introduction*

A number of possibilities are competing to be a part of the Laboratory's future. Prominent among these are opportunities in neutrino physics, quark flavor physics, the Linear Collider and its physics, and astrophysics. At this meeting, the Committee heard discussions of future neutrino physics opportunities, from both a theoretical and experimental perspective, as well as a report on the proton economics which form the foundation of a proton-intensive neutrino program. Linear Collider R&D activities both inside and outside the Laboratory were also reviewed, and the role of Astrophysics and a specific proposal for R&D for the SNAP project were discussed.

B. *Neutrino Opportunities*

1. Health of the Near-Term Program

Neutrino physics has produced a great amount of excitement in the field during the past five years, first with the evidence for oscillation in atmospheric neutrinos from SuperKamiokande, next with the resolution of the solar neutrino problem by SNO, and most recently with the reactor anti-neutrino experiment KamLAND. An accelerator-based neutrino oscillation experiment, K2K, has also been collecting data that is consistent with the neutrino oscillation hypothesis, albeit with limited statistics. In view of the rich physics opportunities opened up by these recent discoveries and the existing neutrino program at the Laboratory, the Laboratory is in a position to address important questions.

2. MiniBooNE

MiniBooNE is an important neutrino experiment that should be in a position to definitively verify or refute the neutrino oscillation signal suggested by the LSND experiment.

The Committee commends the collaboration for timely construction of the detector, and for working closely with the Laboratory and its Booster team to successfully bring the proton intensity up quickly during the last eight months. The collaboration is poised to produce first physics results. The installation of the collimator and other improvements this summer is expected to reduce effects of the beam losses and will allow even higher intensity, potentially up to 5×10^{20} pot/year. The Committee urges the Directorate to continue to work on improvement in the performance and reliability in the Booster that provides beams to the entire accelerator program at the Laboratory. The Committee also anticipates a further discussion in the fall of progress and plans from the experiment.

3. MINOS

MINOS is a crucial neutrino experiment that will put the evidence for oscillation in atmospheric neutrinos on completely solid ground. Its goals are:

a) Verify oscillation in muon neutrinos, by observing the "dip," excluding decay or decoherence hypotheses.
b) Measure Δm^2 accurately. This is a fundamental physics parameter, and in addition it is potentially important for optimizing the location/energy of an off-axis neutrino experiment.
c) Search for ν_e appearance to a level of 1/2 to 1/3 of the CHOOZ limit.
d) Test possible CPT violation between neutrino and anti-neutrino oscillations in atmospheric neutrinos.

The Committee congratulates the collaboration for completing the construction of the far detector and for its successful initial study of atmospheric neutrino-induced events. The Committee also notes that the MINOS collaboration has worked closely with the Beams Division to ensure the success of NuMI.

4. Proton Economics

The Committee is grateful for the presentation at this meeting by the Chairman of the Proton Committee. Proton economics is a very serious issue for the Laboratory. The Committee anxiously awaits the written report.

The Committee understands from the presentation that

a) The current performance of the Booster provides 3×10^{20} pot/year for MiniBooNE and 4.2×10^{19} protons/year needed for slip stacking in the Main Injector.
b) The improvements in the Booster that are now being implemented can provide beams to MiniBooNE at 5×10^{20} pot/year level until NuMI comes online.
c) The Booster will be able to provide a baseline of 2.5×10^{20}pot/year to NuMI concurrent with the slip stacking, while leaving an option for additional protons up to 4.5×10^{20}/year coming directly from the Booster. With further improvements of the Main Injector, these additional protons might alternatively be sent to NuMI.
d) Beyond the scheduled four years running of NuMI, the requested number of protons (mostly for BTeV and CKM) drops off. However, the Committee anticipates a further request for protons for NuMI, at an even higher level.

The Committee urges the Laboratory to follow up to make sure that MiniBooNE will get at least 5×10^{20} protons in total, and NuMI will be a success, in addition to Run II as the highest priority project. An off-axis NuMI experiment may require still higher proton intensity. The plans to extend the capability of the accelerator complex beyond 2005 need further study and the costs associated with the improvements should be clarified.

5. Future

As discussed at the June 2002 PAC meeting, the important targets of the accelerator-based neutrino physics are the measurement of θ_{13}, the determination of the mass hierarchy, and eventually the observation of any CP violation. The Committee is pleased to learn of the process by which the user community is exploring detector options for an off-axis NuMI experiment, and to see a sample of the progress that is being made. The Committee looks forward to seeing the results of this work, including the response to the questions the Committee posed at the June 2002 PAC meeting.

Given that the J-PARC based neutrino beam is likely to be built, possibly in 2008, there will be competition. From the presentation by the Deputy Director based on the constant level of effort, there will not be room for new initiatives from Fermilab base funding until FY2008.

The relationship between the developing program in Japan based on J-PARC and that associated with the NuMI beam will depend on the pace of development of the off-axis experiment within the limited resources. It is anticipated that the two initiatives could be complementary because the sensitivities to the matter effect in the ν_e appearance probabilities are different between the two initiatives. The extent to which the comparison of the ν_e appearance probabilities from the two experiments can yield a determination of the mass hierarchy may well be limited by statistics.

Both from the point of view of the competition with J-PARC and the discrimination of the mass hierarchy, a high proton intensity is called for. The current discussions on the off-axis experiment assume 4×10^{20}/year for five years on a 50kT target, which is already beyond the NuMI baseline of 2.5×10^{20}/year. Extending the capability of the existing accelerator complex is highly desirable in this respect. The Committee urges the Directorate to keep working on proton economics and to identify paths to incrementally increase the available number of protons, possibly beyond 10×10^{20}/year.

C. ***Astrophysics / SNAP***

A group at the Laboratory has written a Letter of Intent to join the SNAP collaboration. This group has proposed an R&D program in support of their interest in the scientific program of SNAP. The Laboratory has asked the PAC to comment on this R&D program and on the desirable level of Laboratory involvement in the SNAP experiment.

The Committee believes that the SNAP experiment has exceptional scientific interest. By surveying supernovae out to high redshift, and by carrying out surveys of the large-scale mass distribution in the universe, SNAP will be an excellent tool for determining the equation of state of the dark energy and the balance of matter and energy in cosmology. The siting of SNAP in space allows one to specifically address the dominant systematic errors in the determination of cosmological matter and energy from supernova observations and from surveys of the large-scale mass distribution.

The Committee observes that the existing Laboratory experimental and theoretical astrophysics groups have considerable experience and knowledge that will be useful for the SNAP program. The members of the Fermilab Experimental Astrophysics Group contributing to the Sloan Digital Sky Survey have managed automated surveys and have wrestled with the data compilation and archiving problems that they entail. This group has also confronted issues of maintaining photometric calibrations such as will be crucial to the success of SNAP. The theoretical astrophysics group includes some of the world experts in gravitational lensing surveys of the cosmic matter distribution.

Other groups at Fermilab have experience in data transfer and data compression, the design of radiation-hard electronics, and the calculation of radiation hazards and design of radiation-protection systems that will be useful to the SNAP group. However, it is much less clear to the Committee how the capabilities of the Fermilab staff, applied to the specific problems met in astrophysics experiments, compare to skills that are already available in the astronomy and space science community.

The Committee believes that it makes sense for members of the Fermilab Experimental Astrophysics Group to transition to work on SNAP as their responsibilities to the Sloan Digital Sky Survey decrease. In addition, the Committee encourages members of the Fermilab theoretical astrophysics group to contribute to SNAP. The Committee is impressed by the plan that was received for an innovative wide area rich cluster survey using SNAP. This corresponds to participation in the first and last topics of the proposed R&D program—photometric calibrations and science simulations.

The Laboratory has asked the Committee to recommend criteria that might guide an eventual larger involvement in astrophysics experiments over the long term. The Committee would like to put forward four criteria. First, the scientific motivation for any new effort should be very strong. Second, the new efforts should build on special strengths of the Fermilab staff. Third, the new efforts should involve or support a community of physicists outside the Fermilab staff, in the ideal case, physicists from the community of traditional high energy physics university user groups. Finally, the new efforts should not compromise Fermilab's commitment to provide accelerator facilities for experiments in high-energy physics.

D. ***Linear Collider***

The Linear Collider is the number one new-facility priority worldwide for the future development of high-energy physics beyond the LHC. The Committee heard a number of presentations on current and planned activity related to a future Linear Collider. This included international coordination by the International Linear Collider Steering Committee, ongoing technical evaluations aimed at a technology decision next year, as well as US-based and Fermilab-based R&D on Linear Collider accelerator and detector design.

The Committee is pleased to note the progress on LC R&D given the limited budget. Recent progress in the Laboratory LC program includes substantial progress in the development and fabrication of NLC accelerating structures. Fermilab is also a major participant in the US Linear Collider Steering Group Warm/Cold Evaluation program, and in characterization of sites.

However, a successful Linear Collider effort in the US will require increased Fermilab leadership and very substantial growth in LC activities at Fermilab. Unfortunately, the scope of the current Fermilab Linear Collider effort is, in the short term, severely constrained by the redirection of effort into Run II.

The Committee was very pleased by a report on the progress of 'Grass Roots' Linear Collider R&D organization by the university community. This is a great start. It has resulted in the recent funding of a number of proposals on both Accelerator and Detector R&D by the DOE, and the possibility of additional funding by the NSF. The Committee encourages Fermilab's participation in these efforts. The Committee notes that involvement by Fermilab physicists in this direction is still subcritical. The revival of a test beam at Fermilab, however, will represent a substantial additional contribution to worldwide LC R&D efforts.