Physics Advisory Committee

June 16-21, 2001

General Comments and Recommenations

At its 2001 meeting in Aspen, the Committee considered and made recommendations on two proposed rare kaon experiments. The Committee had extensive discussions, organized around the Director's presentation to the HEPAP Long-Range Subpanel, on the long-term future of the Laboratory. The Committee also considered the status and direction of the Run IIb upgrades proposed by CDF and D0.

The Committee heard a presentation from the Fermilab Directorate on the status and future plans for Fermilab R&D related to a linear electron-positron collider. The Committee was pleased to hear detailed plans for an NLC test facility (ETF), and continued studies of Fermilab site options. The Committee was also pleased to hear that Fermilab is engaging with the TESLA collaboration to examine various aspects of the TESLA TDR and R&D program, including cost estimates.

The Committee heard a presentation summarizing the recently completed VLHC Design Study, which was commissioned by the Fermilab Director. This is an excellent study that greatly adds to our understanding of the possibilities for a staged hadron collider facility in the energy range 40 – 200 TeV. The Committee strongly encourages continuation of these engineering and design efforts, accompanied by appropriate magnet R&D, and efforts to broaden and internationalize the VLHC collaboration.

Beams Division presented an overview of the challenges of "proton economics," and of possible paths toward satisfying the proton requirements of the experiments in the Fermilab program. Such improvements are absolutely necessary if the currently approved experiments are to meet their physics goals. The Committee was pleased to hear about plans for upgrading the performance of the Booster and for study of a possible Linac energy upgrade.

The Committee heard with interest a presentation on the recent physics study related to a possible new Proton Driver facility at Fermilab. While the broader physics case for the Proton Driver is still unclear, such a facility could provide essential capabilities for next-generation neutrino experiments, and for the Fermilab program beyond the Tevatron era.

Proposals for Rare K Decay Experiments: P-921 (CKM) and P-922 (KAMI)

The rare decays $K^+ \to p^+ n\bar{n}$ and $K_L^0 \to p^0 n\bar{n}$ offer a unique window into the flavor-changing transitions of the weak interactions. Both decays use the quark-level transition $s \to dnn$. This process is forbidden in the Standard Model at tree level but proceeds through one-loop diagrams. The evaluation of these diagrams in the Standard Model gives a (complexvalued) coefficient of short-distance origin, involving the W and top quark masses and the Cabibbo-Kobayashi-Maskawa (CKM) mixing angles, multiplying a simple current-current operator. For the rare kaon decays, the hadronic matrix element of this operator can be evaluated very simply, since it is related by isospin to the operator that mediates $K \rightarrow pln$. Thus, the measurements of the rates of these rare processes give direct information on the CKM parameters with a cleanliness comparable to the best B-meson time-dependent asymmetries. Within the Standard Model, $K^{+} \rightarrow p^{+}n\bar{n}$ directly measures a CKM parameter that approximately coincides with V_{td}, and $K_{L}^{0} \rightarrow p^{0}n\bar{n}$ directly measures the CP-violating amplitude η. The combination of the two measurements picks out a unique vertex for the "unitarity triangle" that determines the magnitude of all CP-violating processes.

In models that go beyond the Standard Model, these decays are equally significant. New physics that contributes to loop diagrams (for example, an extended Higgs sector or supersymmetry) can potentially give contributions of the same magnitude as that of the Standard Model. These contributions have the structure of new complex amplitudes multiplying the same current-current $s \rightarrow dn\bar{n}$ operator that appeared in the Standard Model case. Such new physics will give a comparably large but different contribution to other flavor-changing loop diagrams, in particular, the $B_{d} - \bar{B}_{d}$ and $B_{s} - \bar{B}_{s}$ mixing amplitudes. Comparison of the unitarity triangles picked out by measurements of B asymmetries, B_{s} asymmetries, and rare K decays can reveal that the CKM model is incomplete and that new heavy particles must be present to have a consistent picture of flavor physics. The simple operator structure and consequent theoretical cleanliness of the rare K decay amplitudes are crucial for giving this light-quark window into flavor physics the same clarity as windows from the best heavy-quark processes.

Though the information from rare K decays has an important role to play in the study of the weak interactions, it is not at all straightforward to obtain this information. The two $K \rightarrow pn\bar{n}$ processes have Standard Model branching ratios smaller than one in ten billion. The experimental signatures are not particularly characteristic; in fact, two of the three final state particles cannot be observed. These rare decays can easily be mimicked by $K \rightarrow pp$ decays in which one pion is lost due to some inefficiency, and by many other background sources. Thus, even the detection of these processes requires a heroic effort. The Brookhaven experiment E787 has observed one event of $K^{+} \rightarrow p^{+}n\bar{n}$. For $K_{L}^{0} \rightarrow p^{0}n\bar{n}$, the best experimental upper limit comes from the KTeV experiment, with a sensitivity four orders of magnitude above the Standard Model expectation. The importance of these processes in the whole picture of flavor physics is such that the Committee finds justification for a major campaign to measure the rates of these decays. But our enthusiasm must be tempered by the difficulty and costs of the experiments required.

To understand how much effort toward the rare K decays might be justified, we should put them into the context of other measurements of CKM parameters. The experiment E787 that discovered $K^{+} \rightarrow p^{+}n\bar{n}$ was planned in an era when we were almost totally ignorant of the values of the parameters of CP violation. In this context, the idea that the two rare K decay experiments gave a complete, independent determination of the unitarity triangle was very important, and the fact that $K_{L}^{0} \rightarrow p^{0}n\bar{n}$ measures the fundamental parameter of CP violation gave it special significance. The situation is very different today, and it is rapidly evolving. The discovery that ε'/ε has a non-zero value demonstrates that there is CP violation at the weak interaction scale and not just at a much higher "superweak" scale, giving strong support to the general CKM picture. This conclusion is likely to be confirmed in the next few years by the measurement of a non-zero value of sin2β in the B meson system and in the measurement of the ratio of the B_s and B_d mixing parameters. In fact, we expect that, within a few years, we will have an accurate determination of the parameters of CP violation in the B-meson system under the assumption of the Standard Model. The question for the rare K decay experiments is then: Do the amplitudes for these processes agree with the CKM parameter values picked out by B physics measurements? If so, this would be a dramatic confirmation of the CKM model. If not, it would require new physics. Effects of new physics would generically appear in both $K \to p n \bar{n}$ processes.

This new situation has changed the context for rare decay experiments and has considerably raised the standards that such experiments must meet. It is no longer enough to simply observe the $K \to p n \bar{n}$ decays. What is needed is to measure the rates for these decays with a precision that can confront the current physics issues. This led the Committee to require that these experiments plan to collect at least 100 events, with systematic errors controlled to the level that would allow a determination of the underlying CKM parameters to better than 10% precision. That statement stretched the proton requirements for these experiments, putting them necessarily in the post-Run II era. It also raised the cost of these experiments to the $50-100M class.

The increase in cost and scope of these experiments raises the issue that both experiments put a significant burden on Laboratory resources. The cost in dollars and Laboratory personnel and the cost in protons are both relevant. The Committee notes that the CKM proposal requests 2.8×10^{19} protons/year and that the KAMI proposal requests 2.0×10^{20} protons/year.

The basic physics criterion that the Committee imposed on these experiments is then the following: Can the experiment, considered on its own, credibly establish a discrepancy with the values of the CKM parameters observed in B physics? This criterion includes the capability to acquire a large enough event sample and an apparatus that can likely reduce the background to a small fraction of the signal. But it also includes the capability to use the experimental data to prove that an observed discrepancy with the Standard Model prediction is not the result of an unforeseen background reaction and is thus directly attributable to new physics.

After extensive review, the Committee has concluded that the CKM experiment could meet this criterion, despite the difficulty of the measurement. The experiment is based on an innovative technique that will provide redundant measurements of the momentum vectors of the initial K^+ and the final π^+. This and other features of the experiment give us confidence that the backgrounds can be understood from data, and thus that the substantial resources required by this experiment will be productively spent. The Committee recommends that this experiment be given Stage I approval.

At the same time, the Committee has concluded that the KAMI experiment does not meet this criterion. The Committee believes that the approach taken by this proposal does not provide enough constraints to measure the branching ratio reliably at the stated level and, in particular, to distinguish signal from an unanticipated source of background. The Committee acknowledges the years of effort that the KAMI collaboration has invested in designing an experiment to measure this very difficult and important process. Nevertheless, it recommends rejection of the KAMI proposal for the reasons above.

The Committee warmly thanks several other committees that provided valuable assistance in evaluating the rare K decay proposals: the Technical Review Committee for KAMI/CKM; the CKM Beam Review Committee; and the Superconducting Separator Project Review Committee.

Options for the Long-term Future of the Laboratory

Over the past few years, the Laboratory has been engaged in an extensive process to evaluate the potential of the various accelerators which individually or together could form the basis for the long-term future of high-energy physics. This process has included physics and design studies, workshops, and seminar series, and has involved an extremely broad group of scientists both at the Laboratory and across the country. By all accounts, it has been a resounding success in informing the Fermilab community about the various options which are available and in engaging them in the process of deciding which path to pursue. The Committee has also reviewed and discussed these options at its meetings over the past several years.

This Fermilab process is part of and coincides with a larger effort in U.S. high-energy physics to develop a long-term plan for our field, an effort that is culminating in the Snowmass Summer Study and the HEPAP Long-Range Subpanel. In the context of the Subpanel's deliberations, and informed by the extensive and ongoing Fermilab studies, the Director presented the Subpanel with his long-range vision of the future of high-energy physics and his plan for the future of the Laboratory.

The Director emphasized that the United States has laid the groundwork for a compelling high-energy physics program over the next decade and that, in the near term, we must realize this program and reap the benefits of the investments we have made. The Director then articulated a clear vision for the long-range future. He stressed that:

- To remain a world leader in the field beyond this decade, the U.S. must make a twenty-year commitment to supporting the advance of high-energy physics. The construction of more powerful and luminous accelerators has always led to important and exciting progress in high-energy physics, and we must continue to design and build these remarkable scientific tools.

- The next large accelerator project should be a linear electron-positron collider (LC) with initial center-of-mass energy of 500 GeV. The Director stated that the physics case was "very strong" and that "we know enough to make the choice now." There should be only one Linear Collider in the world, built as a truly international collaboration, with the final design chosen by technical considerations only. Regardless of where this collider is sited, the United States must be a major player.

- The construction of a future hadron collider (VLHC) operating at an energy of the scale of 100 TeV, perhaps developed in stages, is a promising path for the field after the LHC era. While construction of such a machine is unlikely before the start of the next decade, it is vitally important that we continue a strong R&D effort on critical technologies, in the context of an international VLHC collaboration. The physics studies that will address key issues such as the balance between energy, luminosity and cost should start now, and they should be carried out as an international effort.

The Committee found this long-term vision of the future of the field to be coherent and compelling, and it strongly endorses the priorities articulated by the Director.

By the end of this decade, the twenty-year career of the Fermilab Tevatron Collider as the world's flagship accelerator at the energy frontier will come to an end. The time will be right to start construction of a new major facility at the Laboratory. The Director proposed to the Subpanel that this new facility be the Linear Collider, and that it be located at Fermilab. While he argued unambiguously that Fermilab would be the ideal site for the LC, he stressed that the accelerator project must be organized as a true collaboration of the world's high-energy physics laboratories.

The Committee agrees that a bid for the Linear Collider should be the next step for Fermilab. The physics is compelling and complementary to that of the LHC. In particular, this machine will play a crucial role in uncovering the properties of the Higgs boson and other new particles associated with electroweak symmetry breaking. The technology is at hand, and the world HEP community is eager to move forward. It is in the interest of the international community to maintain geographic balance in future major accelerator projects. Fermilab has the right culture, expertise, infrastructure and location to host a world machine. It is important both for the strength of high-energy physics in the United States and for the world program that the next major HEP facility be built at Fermilab. Furthermore, the Laboratory is well positioned to play a constructive role in the coming comparison of warm and superconducting LC technologies. For all of these reasons, the Laboratory should strengthen its program of LC R&D, with a view to hosting the Linear Collider project.

Although the time is not yet right to propose construction of a VLHC, a hadron collider beyond LHC is likely to be a key component of the twenty-year future of the field. Fermilab has an indispensable role to play in continuing to develop and establish the technologies that will make this project possible. In his remarks to the Subpanel, the Director emphasized that even if the Laboratory bids to construct a LC as its next major project, VLHC R&D will remain a high priority of the Laboratory. The Committee strongly endorses this view. On the other hand, like the LC, a VLHC can only be viable if its physics case and parameter choices emerge from international consensus. International coordination and collaboration both on accelerator technologies and on developing the physics case for a VLHC should start as soon as possible.

Neutrino physics may also remain a major focus of the Laboratory after the current generation of experiments. Anticipated results on solar, atmospheric and accelerator neutrinos will determine the interesting parameters for the next generation of neutrino experiments. These physics results may eventually motivate more intense conventional neutrino beams that make use of an enhanced proton source, or perhaps a neutrino factory based on a muon storage ring. In the meantime, the Laboratory should pursue the accelerator R&D necessary to establish the technologies for these facilities.

Run IIb Upgrades to CDF and D0

The CDF and D0 collaborations are assessing modifications and upgrades to their detectors that will ensure effective operation through the end of Run IIb. The detectors' silicon trackers are the principal concern, but other needs for high-luminosity running are also being considered. Proposals and detailed design studies are expected later this year.

The replacement of the CDF and D0 inner silicon trackers is scheduled for a six-month shutdown starting in June 2004, three years from now. By this time, the present (Run IIa) inner silicon tracking systems are likely to have suffered significant radiation damage, so their replacement is essential to the success of Run IIb. The silicon upgrades are on the critical path for Run IIb, and they must be installed during the 2004 shutdown planned for major accelerator upgrades. Any delay in the installation of Run IIb detector upgrades will be very costly in terms of physics results, because the luminosity will be highest after the accelerator upgrades in the years prior to LHC turn-on. The Committee, the collaborations, and the Laboratory all recognize the urgency of this work and the need to build these complex detectors in the short available time.