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SANTA CRUZ
INSTITUTE FOR PARTICLE PHYSICS

SANTA CRUZ, CALIFORNIA 95064

September 8, 2003

Professor Frederick Gilman
Chairman of HEPAP
Department of Physics
Carnegie Mellon University
Pittsburgh, PA 15213

Dear Fred:

I am submitting the first report of the Particle Physics Project Prioritization Panel (P5) to HEPAP for consideration and forwarding to the DOE and the NSF. The P5 Subpanel of HEPAP was formed based on the November 6, 2002 letter to HEPAP from Dr. Raymond L. Orbach, Director of the Office of Science of the DOE and Dr. John B. Hunt, Acting Assistant Director for Mathematical and Physical Science of the National Science Foundation. I attach this letter as Appendix A. The P5 committee membership was chosen soon after, with the membership listed in Appendix B. This first report is in response to the January 21, 2003 letter to P5 from Dr. S. Peter Rosen, Associate Director for High Energy and Nuclear Physics of the DOE Office of Science and Dr. Joseph Dehmer, Director of the NSF Division of Physics. This letter is attached as Appendix C.

A major task of P5 is to serve as the guardian of the facilities roadmap that provides the agencies and the scientific community with a 20-year plan of potential world-class science projects. The roadmap includes approximate time windows needed for the R&D, construction, and facility utilization in order to reap the scientific benefits of each project. In the next section, we describe the scientific vision on which the roadmap is based and discuss recent changes that have been made to the roadmap since its original appearance in the report of the Subpanel on Long Range Planning for U.S. High-Energy Physics.

The roadmap includes projects that have already received endorsement from the appropriate peer-review advisory body and are ready to move into a construction phase. Based on an explicit request for prioritization from the agencies for a number of such projects in the $50M to $600M range, P5 is charged with providing an evaluation of the relative merits among these projects. This includes a broad evaluation of costs, schedule, and scientific potential. This report presents the first such evaluation, based on a request to review three projects ready for construction:

1. the CDF and D0 detector upgrades for Run IIB of the Tevatron Collider,
2. the BTeV experiment that would carry out very high sensitivity studies of the decays of B hadrons, and
3. the CKM experiment, which has as its primary goal the study of the very rare decay $K^+ \to \pi^+ \nu \bar{\nu}$.

In this particular case, it happens that all three of these projects would be supported primarily by the Department of Energy at Fermilab. However, it is the charge of P5 to consider them in the broader context of the overall U.S. program. Given the responsibility of P5 to all of U.S. particle physics, it is anticipated that in the future this panel, or a subsequent panel, will also be asked to consider projects whose major funding would come from the National Science Foundation. Such projects would most naturally be brought to P5 after mail and/or special committee review, but before consideration by the National Science Board.

Although this report focuses on the relative priorities of the three projects, we note that the agencies will need to use our recommendations in the context of the unfolding funding situation for the whole field of particle physics over the next few years, including the rate of progress on the international Linear Collider project. Putting aside major increases in funding tied to specific new facilities, the funding assumption we are using for the context of our report is a constant level of effort.

With regard to schedules, we assume that the first priority for running the Tevatron will be CDF and D0 until the LHC program is underway. We anticipate that the LHC schedule can be predicted with greater confidence at the end of the next fiscal year when the detectors will be nearly complete and a large number of magnets for the accelerator delivered. We have assumed that the first priority for running the Tevatron could be switched to B physics in 2009, and that the Tevatron could run for B physics at least until the end of 2012.

The context for our P5 report is provided by the two most recent HEPAP subpanel planning reports and the recent " High-Energy Physics Facilities Recommended For The DOE Office of Science Twenty-Year Roadmap" forwarded to the Office of Science by HEPAP in March 2003. The P5 membership served as part of the committee that drafted the facilities report. We look forward to the completion of the facilities plan now being formulated and to including it more centrally in our planning. Along with projects within the NSF, it should form part of a broad program of both scientific discovery within the physical sciences and training of the next generation of physical scientists.

The Particle Physics Roadmap

The report of the HEPAP Subpanel on Long Range Planning for U.S. High-Energy Physics includes a 20-year roadmap for our field to chart our steps on the frontiers of matter, energy, space and time. Any such list of future facilities is a dynamic one. With time, decisions will be made to begin construction of some facilities and not of others on the current roadmap. Still other facilities may be added in response to new scientific and technical opportunities. Indeed several new projects in the neutrino area have been added to the initial roadmap. We are part of a world community and the roadmap needs to be viewed in an international context. Especially for the very large facilities, some will be located here and others abroad. We want to participate in the most important science, wherever the facility is located, just as our colleagues from other regions of the world would want to collaborate on facilities in the U.S.

The roadmap is maintained on a public web site at
http://doe-hep.hep.net/P5/Roadmap.html
Projects on the roadmap are grouped into the primary areas they address: the energy frontier, lepton flavor physics, quark flavor physics, unification scale physics, cosmology, and particle astrophysics. Approximate decision points on whether or not to proceed with projects and the timelines for R&D, construction and operation phases of each project are indicated in the roadmap. The web site contains both the original roadmap described by the Subpanel on Long Range Planning for U.S. High-Energy Physics, and

changes to the roadmap motivated by additional scientific or technical input, or decisions on particular projects.

Recent successes within the program have included the discovery of neutrino oscillations, definitive measurements of CP violation in B meson decays and direct CP violation in K meson decays, and the discovery of dark energy as a major force shaping the evolution of the universe. Advances in astrophysics and astronomy, which reveal the important role of dark matter, require advances in particle physics to provide a deeper understanding of these phenomena. Over the next few years we can expect that very large data samples from the B-factories will continue to probe the source of CP violation and that large data sets from the Tevatron Collider will allow for a continued search for new physics at the energy frontier. CLEO-c will map the effects of strong interactions on heavy quark decays and search for new gluonic forms of matter.

The projects on the roadmap provide a diverse and interconnected research program that aims to keep the U.S. among the world leaders in the exploration of the frontiers of mass, energy, space and time. The science-centered program provided by the primary physics areas of the roadmap collectively target three broad research themes based on the discoveries of the last few decades. These principal themes are:

> **Ultimate Unification** – Unification is the search for simplicity at the heart of matter and energy. The rich and complex phenomena we observe today appear to have emerged from a much simpler world at high energies that existed in the first moments of our universe. Experiments of the last few decades have confirmed that new fundamental particles reflecting this simpler world must exist at energy levels just beyond the reach of current accelerators. Our goal is to explore phenomena that will give us insight into the mechanism by which the disparate particles and forces of the universe merge into a single coherent picture.
>
> At energies approaching a TeV, we will begin to explore an uncharted world where we know that two of the forces, electromagnetic and weak, are unified into one Electroweak force. As part of this unification, the fundamental particles acquire the property of mass and their characteristic behavior under the weak and electromagnetic forces. We know that something fundamentally new and different

than anything we have seen before must happen. But what is it? One often-cited example is a new kind of particle, the Higgs boson, as a remnant and thereby a signal of the unification mechanism. In addition, neutrino masses may be connected to energy scales where all the forces become one.

Hidden Dimensions – The visible world appears to have three spatial dimensions. String theories, however, predict that there are more. Some of them might be observable by kicking particles with enough energy that they could disappear into the extra dimensions. Particle accelerators would allow the discovery of such dimensions, and measurement of their shapes and sizes. In the long term, string theory may provide the ultimate unification of forces. Our goal is to explore whether there are extra dimensions and to decipher their structure. Supersymmetry, which is strongly favored theoretically, predicts that additional dimensions with spin lead to a set of new fundamental particles, one partner for each known fundamental particle. We suspect that the entrance to the world of supersymmetry also lies at the TeV energy scale, where it plays an essential role in unification.

Cosmic Connections – Elementary particles that interact through a few fundamental forces shape the evolution, present state, and future of the universe. Recent astrophysics experiments indicate that most of the matter in the universe is dark, unlike any conventional matter here on Earth, and that empty space is filled with dark energy, pushing the universe to expand at an ever-increasing rate. Our goal is to explore the nature of dark matter and dark energy through experiments both on earth and in space.

A prime candidate for the dark matter is the lowest-mass particle of supersymmetry, left as a remnant of the early moments of the universe. If so, we will produce the dark matter particles and precisely study their properties and connections to unification and hidden dimensions at TeV-scale accelerators. In contrast, the next-generation experiment to study dark energy will be in space. Back on earth, by studying differences in the behavior of matter and antimatter in accelerator-based experiments, we hope to understand why our universe is now composed of matter, even though there were equal amounts of matter and antimatter in the very early universe.

In the intermediate term the roadmap includes projects either in construction or that could soon be in construction and that would provide an exciting physics program later in this decade. These include, by primary physics category in the roadmap:

1) The LHC, which will be the energy frontier program, with potential impact on all major goals of the field.
2) BTeV, potentially the best quark flavor physics experiment into the next decade. CKM and KOPIO, which will study CP violation in the kaon system. We provide our recommendations regarding BTeV and CKM later in this report.
3) SNAP, which could map the dark energy content of the universe as it evolved. Along with GLAST, Ice-Cube, Pierre Auger and dark matter searches, it would provide new capabilities for studies of the cosmos.
4) The NuMI-MINOS program, the first high statistics accelerator based neutrino experiment able to carefully measure neutrino oscillations. There are also likely to be additional opportunities in the area of neutrino physics, based on the discoveries of the last few years. In the charged lepton sector, MECO will search for lepton flavor violation.

This program is diverse, addresses the primary physics goals of the field, and has important connections to other fields. It would provide training in physics for a generation of scientists later in the decade. To fund such a program would require incremental funding, particularly for the construction of SNAP, which would be an interagency project involving NASA.

The highest priority for the U.S. program has clearly been indicated by the Long-Range Planning Subpanel based on the expectation that the Linear Collider will be the next major step forward in exploring physics at the energy frontier. Along with the LHC it will provide a sweeping view and incredible precision, with the discoveries of each accelerator used to great advantage in extracting and extending the physics results of the other. The Long-Range Planning Subpanel therefore recommended, as its highest priority, that the U.S. participate in such a project, wherever it is located in the world, and that the U.S. prepare to bid to host the facility. Since this recommendation, several suggestions contained in the subpanel report have moved ahead. This includes the formation of a U.S. steering group and a process for a technology selection, which is expected in the coming year. A clear plan for the required remaining R&D is being fleshed out.

The other pillar of discovery mentioned above is the LHC. Thanks to a cooperative DOE-NSF construction program, U.S. groups have managed to

play a leading role in the two LHC detector projects as well as elements of the accelerator. A smooth and timely transition of the people and resources to the LHC is needed if U.S. groups are to continue to play a leading role at the energy frontier when it moves to the LHC during the second half of this decade.

A dynamic area of recent discovery has been the neutrino sector. Successes in understanding neutrinos, including oscillations of neutrinos from the sun, present a number of important research opportunities. Work to define an optimum program in the areas of neutrino mixing and mass measurement, to follow NuMI-MINOS, is underway. This effort would be complementary to the NSF supported initiative, Ice-Cube, which will study neutrinos from cosmological sources. Parts of the mixing program may well use the Fermilab accelerator as a copious source of neutrinos. Planning for such a program will have to be integrated with the rest of the Fermilab program, which we discuss below, as well as the other elements of the roadmap.

Projects for Prioritization

P5 is currently charged with prioritizing three projects at Fermilab:

1. the CDF and D0 detector upgrades for Run IIB of the Tevatron Collider,
2. the BTeV experiment that would carry out very high sensitivity studies of the decays of B hadrons, and
3. the CKM experiment, which has as its primary goal the study of the very rare decay $K^+ \to \pi^+ \nu \bar{\nu}$.

P5 is very impressed by the depth of the preparation that has been made by all three experiments prior to the decision to begin construction.

Each of these projects has undergone extensive scientific and technical reviews. The Fermilab Physics Advisory Committee carried out in-depth studies of the proposals, recommending approval at an annual week-long retreat. The PAC placed these experiments in the international context of the world high energy physics program and concluded that each would produce important results that address the major goals of elementary particle physics, providing one to two orders of magnitude more sensitivity than experiments performed to date.

Fermilab also carried out internal Lehman-style reviews of cost and schedule. The cost estimates are now believed to be quite reliable. In the

case of the CDF and D0 upgrades, there have also been both Lehman reviews and a DOE External Independent Review. Each project has major foreign collaborating institutions that would contribute significantly to the construction and operation of the detector.

The time schedules of the three projects are quite different. The CDF and D0 upgrades could be mostly completed in the next two years. The construction activities for BTeV and CKM are proposed to ramp up after completion of the upgrades. Both the upgrades and BTeV have significant time pressure because of projects at CERN. Their competitiveness would be seriously compromised by delays in scheduled completion of construction or startup in data collection.

P5 conducted its review of these projects at a two-day meeting at Fermilab on March 26 and 27, 2003. The meeting included presentations from the projects and the laboratory as well as follow-up questions and committee discussion. The meeting was preceded by examination of a large number of documents that record the history of previous reviews of the projects. The proponents also were requested to answer a number of questions contained in a letter to Fermilab Director Witherell and included as Appendix D. We completed our evaluations at a two day meeting on July 17 and 18, 2003, where we were also able to include additional input from Fermilab, including the June 2003 Fermilab PAC report, and documents submitted in June and July by the experiments under consideration by P5. Subsequent to our P5 meeting, the Fermilab management has carefully analyzed the luminosity prospects and funding and manpower constraints expected over the next few years for the Run II program. They have concluded that the maximum physics from the Run II program over the next five years could be gotten by implementing the trigger upgrades for Run II but not the silicon tracking upgrades. P5 strongly supports this difficult decision.

BTeV and CKM would take place at a time that the LHC has launched its program of discovery at the energy frontier and we have examined these experiments as contributors to this discovery program. As such, it is critical that the experiments make definitive measurements that can be examined for contributions from physics found at the LHC. The possibility to adjust to new directions of inquiry is also potentially important. P5 concurs with the Fermilab PAC on the quality of the proposed experiments. In the next sections we will discuss each experiment in turn as well as give our recommendations regarding these projects.

CDF and D0 Upgrades

Description – The Tevatron is the world's highest energy accelerator and, until the LHC produces physics, it will have an unparalleled opportunity to address the major questions in elementary particle physics. The proposed CDF and D0 detector upgrades would replace the silicon vertex detectors in order to recover performance that might be lost due to radiation damage after the Tevatron has delivered 3-4 fb^{-1} of data. The collaborations would also upgrade the trigger, data acquisition and online computing systems in order to handle the high instantaneous data rates associated with an instantaneous luminosity of 1×10^{32} $cm^{-2}s^{-1}$ or more. In addition, CDF proposes to make several changes to the calorimeter to improve its performance, particularly at high rates.

Science – The Run II program attacks the most fundamental questions facing particle physics. While we suspect that the direct search for the Standard Model Higgs will not succeed due to difficult-to-understand background distributions and other challenges, the measurements of the top quark and W boson properties are essential to Ultimate Unification. Discovery of new particles or forces might well herald Hidden Dimensions, or other facets of nature that are now completely unknown.

The CDF and D0 detectors will allow physicists to make incisive measurements of the properties of the top quark and the W boson and signatures of new physics. Depending on the integrated luminosity delivered by the Tevatron, they will reduce the uncertainty on the W mass by about a factor of two, measure the top quark mass with a precision of ±2.0 GeV, and measure its coupling to the W and bottom quark (V_{tb}) with a precision of 7% or better. The experiments will also search for light supersymmetric particles, heavy versions of the W and Z bosons, and extra spatial dimensions.

Collaborations – The D0 collaboration numbers 664 physicists from 78 institutions and 18 countries. The CDF collaboration numbers 706 physicists from 59 institutions and 12 countries. More than half of the participants are from outside the US. Even considering the expected transfer of manpower to the upcoming LHC experiments, these collaborations are of adequate strength to carry out the anticipated physics program with the CDF and D0 detectors.

Cost – The costs of the upgrades are $30.4 M (CDF) and $28.6 M (D0), including G&A and contingency. The costs are summarized in Table 1. The cost estimates have been validated by extensive reviews, including a Lehman review in September 2002. The DOE will provide $27.1 M (CDF) and $24.5 M (D0) in R&D and equipment funds. The NSF will provide $3.1 M in Major Research Instrumentation funds, NSF- and DOE-funded University groups will provide $0.8 M and foreign countries will provide the remaining $3.5 M. The DOE Office of Science has granted the upgrades CD-3(a) approval, and has provided equipment funds for Fiscal Year 2003.

Table 1. The cost of the CDF and D0 upgrades, including G&A and contingency. All costs are in millions of dollars.

CDF		*D0*	
Silicon vertex det.	$18.13	Silicon vertex det.	$17.04
DAQ/Trigger	5.79	Trigger	4.46
Calorimeter	1.32	Online computing	1.39
Administration	1.68	Administration	1.83
R&D Cost	3.46	R&D Cost	3.88
TOTAL	$30.38	TOTAL	$28.60

Evaluation – The Tevatron is the forefront facility in high-energy physics, not just nationally, but worldwide, and it will continue to be the forefront facility for most of this decade. We believe that the surest road to maximizing the potential of Run II will be to maximize the integrated luminosity without a major shutdown to replace the silicon vertex detectors. The combined CDF and D0 double-B tag weighted integrated luminosities through 2008 appear to be comparable with and without the upgrade, and physics that does not rely on double-B tags will benefit even more from running flat-out. Furthermore, any long shutdown carries the risk that the accelerator will take many months to recover its pre-shutdown performance and time will be required to commission and integrate the new detectors into the physics analysis. The overriding priority of the Tevatron physics program should be to maximize the physics-quality data recorded by each experiment by the end of 2008. This will be accomplished with the highest probability by running with well-understood detectors in a factory-like mode, with minimal down-time. It will require the planned improvement to

the accelerator complex and the modernization of aging accelerator components that might fail, with few interruptions for changes to the detector. *P5 strongly supports the Fermilab management's decision that the silicon detector upgrades not be constructed.*

If the nation is to benefit fully from the Tevatron physics program, CDF and D0 must maintain efficient event triggering, and adequate through-put in their data acquisition and online computing systems. The trigger, DAQ and offline system upgrades guarantee these capabilities through the end of Run II. *P5 strongly endorses the upgrades of the trigger, DAQ and offline systems.*

BTeV

Physics – By exploring CP violation in promising B meson decay modes, and by studying rare B decays, BTeV will seek evidence of the physics that lies beyond that of the well-established Standard Model of elementary particle physics. If new, non-Standard Model particles will already have been discovered when BTeV runs, BTeV will help us determine the nature of these new particles and of the physics they represent. The new physics that could be revealed or elucidated through BTeV measurements encompasses Hidden Dimensions, notably including those of supersymmetry, physics with Cosmic Connections, and the physics of Ultimate Unification.

To search for new physics in B decays, one must explore a large number of decay modes, including decays of the B_s mesons, which can be studied with high statistics only at a hadron collider, such as the Tevatron, where BTeV will run. One must see if the mixings, decay rates, daughter-particle kinematical distributions, and CP-violating asymmetries found in the many decay modes can all be described by the Standard Model, or if there are inconsistencies pointing to physics beyond that model. If evidence of new physics is found, one must study a variety of decay modes to illuminate its nature. If new particles have already been observed at the Tevatron or the LHC, one must study a variety of B decay modes to help determine their couplings to each other and to the Standard Model particles.

At the Tevatron, BTeV will record very large samples of B mesons, making possible precision studies of CP asymmetries in relatively rare decays, and of the rates for even rarer decays. Thanks to a sophisticated detached-vertex

trigger, electromagnetic calorimetry for the detection of neutral particles, and good K/π separation, BTeV will have access to a broad range of important decay channels. These channels include $B^0 \rightarrow \rho\, \pi \rightarrow \pi^+ \pi^- \pi^0$, $B_s \rightarrow D_s\, K$, and $B_s \rightarrow J/\psi\, \eta'$. In the Standard Model, the CP asymmetries in these channels are described, respectively, by the elusive CP-violating phase angles α, γ, and χ. However, the actual asymmetry in B^0 decays may be influenced by new physics in $B^o - \overline{B^o}$ mixing, while that in the B_s decays may reflect new physics in $B_s - \overline{B_s}$ mixing. As this illustrates, BTeV's broad capabilities may well allow it to uncover and study new physics, and to discriminate between different kinds of new physics, such as different realizations of supersymmetry. Thus, it will form an important component of our exploration of the physics beyond the Standard Model.

Description – The BTeV detector is a single-arm spectrometer designed for installation in the C0 interaction region of the Fermilab Tevatron Collider. BTeV covers the angular region from 10 mr to 300 mr with respect to the proton beam and therefore is similar in appearance to fixed target detectors, although it will operate at a collider. Here we list the components of the experiment:

- A dipole magnet centered on the interaction region.
- A precision vertex detector consisting of planes of pixel arrays. This pixel detector is located in the magnetic field.
- Forward tracking detectors constructed from silicon microstrip and straw tube elements.
- A Ring Imaging Cherenkov Detector that provides hadron identification in the momentum range 3-70 GeV and lepton identification from 3-20 GeV.
- A $PbW0_4$ electromagnetic calorimeter with excellent energy resolution.
- A steel toroid muon detector with proportional tube instrumentation.

A central feature of the BTeV design is the high rate, high throughput data acquisition system and sophisticated three-level trigger system. BTeV will operate at a Tevatron luminosity of $2x10^{32}$ cm^{-2} s^{-1}, corresponding to an average of about 5 interactions per crossing. The BTeV Level 1 trigger is designed to reject 97.5% of the incoming events, reducing the overall data rate to ~20 GB/s. This reduction is achieved using the Level 1 Vertex

Trigger. This trigger will use FPGA processors to perform pattern recognition using the pixel hits, reconstruct the primary vertices from these tracks and search for tracks inconsistent with these primary vertices (secondary vertex candidates). The Level 2 and 3 triggers will be implemented using a farm of commercial processors.

The strength of the BTeV experiment comes from the combination of its vertex trigger with precision mass measurements for both charged and neutral decay modes and excellent particle identification capabilities.

Costs and Schedule – The most recent cost estimate of BTeV occurred as part of the Director's Status Review held by the Laboratory in October 2002 ("Temple Review"). This review followed the descoping from a double-arm to a single-arm spectrometer and included some technical changes in the design of subsystems. The descoping was in response to previous reviews by the Laboratory.

The Temple Review recommended increasing the estimated cost by $18M to $122M (FY02 $'s). Assuming a construction start in FY05, the cost in "as spent" dollars would increase to $137M. This cost does not include funds for outfitting the C0 interaction region to accommodate the BTeV detector, but does include $4M to complete the BTeV R&D program.

To outfit C0 for BTeV, there is a baseline plan to move and reuse magnets from the B0 and D0 IR regions, requiring about 3 months to install, at a cost of approximately $10M. Under this plan, installation would therefore come at the end of CDF and D0 running.

An alternative and preferred plan, which increases the luminosity by a factor of approximately two, involves constructing new magnets for C0, installing them in 2006, and commencing with the commissioning of BTeV while running CDF and D0. The alternate plan, whose importance depends on the luminosity achieved by the Tevatron, adds an estimated incremental $22-23M to the cost for preparing the C0 interaction region.

The cost of operating BTeV (but not including the cost of running the Tevatron Collider) is estimated at $4M per year. The cost of operating the Tevatron for BTeV is estimated to be $25M per year, with an uncertainty of approximately 20%. Beyond 2006, no accelerator upgrades are needed for BTeV (except for the C0 interaction region).

BTeV is presently preparing a resource-loaded schedule as requested by the Temple Review. With this schedule, BTeV would be ready for a Lehman Review by the end of 2003. A successful outcome of a Lehman Review would then enable the start of construction in 2005. Commissioning of a portion of the silicon pixel system could commence in 2006 on a parasitic basis. This schedule would have BTeV completed and installed by early FY2009.

Evaluation – The BTeV Collaboration responded well to the descoping requested in early 2002. The descoped BTeV was reviewed by the Fermilab PAC, which recommended Stage I approval (again).

As discussed above, BTeV has particular strengths in measuring the CKM angles α from $B^0 \rightarrow \rho\ \pi$, γ from $B \rightarrow D\ K$ and other decays, and χ from measurements of particular channels in B_s decays. The BTeV program is very broad in quark flavor physics. Its focus is on CP violation in B decays, but includes other areas such as charm physics. BTeV will extend the studies of B_d mesons beyond what will be done by BaBar and BELLE. BTeV will open up the subject of B_s mesons, which will not be done by the e^+e^- B Factories.

BTeV will operate during the early phases of the LHC, and any new physics discoveries, such as SUSY states, will immediately be translated into possible effects in B physics processes. Studies of the effects in B decays could well illuminate the nature and details of any newly discovered physics at LHC.

The competition with LHCb will be keen. While the statistics resulting from the two detectors should be comparable in most decay channels, BTeV's calorimetry and open trigger should give it a broader physics reach than LHCb.

To best capture the physics opportunities possible with BTeV, two conditions must be met. First, the construction should start soon so that it can be completed before 2009. This requires advancing the funding profile presently being planned at Fermilab. Second, there needs to be provided in the C0 interaction region special optics to enhance the luminosity delivered to BTeV. *P5 supports the construction of BTeV as an important project in the world-wide quark flavor physics area. Subject to constraints within the*

HEP budget, we strongly recommend an earlier BTeV construction profile and enhanced C0 optics.

CKM

Physics – CKM (Charged Kaons at the Main injector) is an approved Fermilab experiment to measure the branching ratio of the ultra-rare decay of a charged kaon into a charged pion and a neutrino-antineutrino pair ($K^+ \to \pi^+ \nu\bar{\nu}$). The theoretically well-understood Standard Model branching ratio for $K^+ \to \pi^+ \nu\bar{\nu}$ is about 10^{-10} and the experiment proposes to measure 100 such events in order to produce a 10% measurement. Thus, the experiment must be able to reject background kaon decays at the level of a few in a trillion. CKM proposes to do this by utilizing redundant momentum measurements of both the charged kaon and pion and highly efficient photon, electron, and muon vetoes. A previous experiment at BNL, E-787, measured two events consistent with the theoretical predictions, demonstrating that background can be controlled to the required level. To achieve the necessary high event rates, CKM will measure kaon decay in flight from an RF-separated kaon beam. Previous experiments have studied kaon decays at rest.

The measurement of $K^+ \to \pi^+ \nu\bar{\nu}$ determines the magnitude of the V_{td} element of the CKM matrix, a measure of the probability for a virtual W boson to decay into a top quark and a down antiquark. When combined with similar measurements from the study of B meson decays, it is sensitive to new physics at high-energy scales. In addition to its primary goal, CKM proposes to extend the sensitivity of other measurements of charged kaon decay including the study of twelve rare and ultra-rare decay modes.

Cost and Schedule – CKM is largely a collaboration between American and Russian groups. The present collaboration has about 50 physicists from ten institutions, but expects to double its size by the time of data taking. There is no approved experiment in the world with comparable goals to those of CKM. However, recently a letter of intent was submitted to J-PARC for a stopped kaon decay experiment with the goal of measuring 50 $K^+ \to \pi^+ \nu\bar{\nu}$ decays. An experiment to measure the related $K^o \to \pi^o \nu\bar{\nu}$ decay, KOPIO, has been approved at BNL and is expected to be funded through the NSF

MRE program. The measurement of $K^o \rightarrow \pi^o \nu\bar{\nu}$ provides complementary information to that from the measurement of $K^+ \rightarrow \pi^+ \nu\bar{\nu}$.

A recent Fermilab internal review (Temple review) estimated the cost to be 101.4 M$. The cost in "as spent" dollars would be about 10% higher. Items included in this sum were design and fabrication ($91M), management ($7M), and installation ($3M). R&D is largely completed, so there is no appreciable cost for future R&D. Items not included in the review, and their costs, were estimated by the proponents to be off-line analysis ($2M), pre-operations ($2M), and operations ($6M per year).

Evaluation – The subpanel was impressed with the excellent work of the proponents on the design of the experiment and their successful prototyping results. CKM is an elegant world-class experiment, which would be able to produce important physics results. However, the committee assigns it a lower priority than the BTeV experiment. The main reason is that BTeV has a much broader physics program at a comparable cost.

Suggestions Based on Prioritization – The present Fermilab plan calls for a similar funding profile and time-line for BTeV and CKM construction, with both starting to take data around 2009. The P5 Subpanel believes that this plan is likely to be too ambitious given the need to optimize the physics from the Tevatron Collider, as well as the desire to have BTeV completed promptly. *Based on current budgetary models, P5 does not recommend proceeding with CKM.*

Sincerely,

Abraham Seiden, Chair
P5 Committee

APPENDICES

Appendix A	Charge letter, November 6, 2002, signed by Dr. Raymond Orbach, DOE and Dr. John Hunt, NSF
Appendix B	P5 Membership
Appendix C	Charge letter, January 21, 2003, signed by Dr. Peter Rosen, DOE and Dr. Joseph Dehmer, NSF
Appendix D	Letter to Fermilab Director Witherell

U.S. Department of Energy
and the
National Science Foundation

NOV - 6 2002

Professor Fred Gilman
Physics Department
Carnegie-Mellon University
5000 Forbes Avenue
Pittsburgh, PA 15213

Dear Professor Gilman:

In January 2002 the High Energy Physics Advisory Panel (HEPAP) unanimously endorsed the report of the Long-Range Planning Subpanel chaired by Jonathan Bagger and Barry Barish, which created a twenty-year vision for the field of particle physics. One of the central recommendations of the Subpanel was the creation of a Particle Physics Project Prioritization Panel (P5). The Subpanel felt that the U.S. particle physics program would greatly benefit from this new mechanism to assess and prioritize mid-scale initiatives. We agree that, given the significant number of such proposals for exciting new science now on the table, and the overall constraints on financial and human resources, P5 can perform an important function. Thus we are writing to ask you to implement this recommendation.

We request that HEPAP form a Subpanel that will be the Particle Physics Project Prioritization Panel. The membership of the Subpanel should represent those communities in particle physics and related fields that can give independent advice on the relative merits of the various projects considered. P5 should evaluate for HEPAP the merits of specific proposals, and recommendations concerning their priority standing in the context of the national high-energy physics program. In particular, this Subpanel should recommend priorities for mid-size (approximately $50M to $600M in total project cost) particle physics projects. These projects should have already received endorsement from their respective laboratories' Program Advisory Committee(s) (if based at a national lab), or an equivalent external peer-review process that can assess the scientific merit of the proposals, such as the Scientific Assessment Group for Experiments in Non-Accelerator Physics.

The funding agencies will convey to you an initial set of proposals for P5 consideration in a separate communication. Projects that may require consideration during the timeframe of the Subpanel will be referred to P5 by the funding agencies as they arise.

The proposals referred to P5 will typically have already developed fairly detailed cost estimates. While we do not expect P5 to do an extensive review of costs, to be most helpful, in their report to HEPAP, P5 should comment on the appropriateness of existing cost estimates; indicate what funding levels are expected to be required by these new projects if they are approved (including R&D, engineering design, pre-

operations, operations, and possibly construction of new facilities); and evaluate what the scientific impacts would be if sufficient funding is not available during the timeframe of the projects under consideration. As part of its work, the Subpanel will naturally be gathering information about proposed and possible future opportunities. It will use this knowledge, together with its recommendations on projects, to update the project "roadmap" for the field created by the Long-Range Planning Subpanel. That roadmap identified decision points on a given project's path from research and development, to construction, and then to operation.

In assessing physics priorities, the Subpanel should weigh physics importance and the overall balance of the field within the context of available resources, including available funding and manpower, timescales, and other programmatic concerns. It will consider projects across particle physics, broadly defined, and across funding sources. Where relevant, the Subpanel should consider the international context of proposals, their relation to the programs of related fields such as nuclear physics and astrophysics, and their broader impacts on science and society. While understanding the broad physics program context in which these projects exist is vital for properly evaluating and prioritizing the individual projects, that context itself is outside the purview of P5. Advice on the general direction and overall priorities for the U.S. particle physics program is properly the responsibility of HEPAP itself, and any advice provided to the Department of Energy and the National Science Foundation should reflect HEPAP's views.

We look forward to the creation of the P5 Subpanel in the near future. We would like to have periodic status reports to HEPAP on the work of the Subpanel beginning in 2003, with a final report by the end of 2004.

We wish you success in this challenging and important endeavor.

Sincerely,

Raymond L. Orbach

Dr. Raymond L. Orbach
Director
Office of Science

John B. Hunt

Dr. John B. Hunt
Acting Assistant Director
for Mathematical and Physical Science
National Science Foundation

cc: Peter Rosen, SC-20
John O'Fallon, SC-22
Glen Crawford, SC-222
Marsha Marsden, SC-222

Joseph Dehmer, NSF
John Lightbody, NSF
Marvin Goldberg, NSF

Membership

DOE/NSF HEPAP Subpanel on Particle Physics Project Prioritization Panel (P5)

Professor Abraham Seiden - Chair
Institute for Particle Physics
University of California at Santa Cruz
Santa Cruz, CA 95064
abs@scipp.ucsc.edu

Professor Eugene W. Beier
Department of Physics & Astronomy
University of Pennsylvania
Philadelphia, PA 19104
geneb@dept.physics.upenn.edu

Professor Patricia Burchat
Department of Physics
Stanford University
Stanford, CA 94305
burchat@stanford.edu

Professor Gary J. Feldman
Department of Physics
Harvard University
Boston, MA 02138
feldman@physics.harvard.edu

Dr. Dan Green
Fermi National Accelerator Laboratory
Batavia, IL 60510
dgreen@fnal.gov

Professor Marc Kamionkowski
Division of Physics, Mathematics & Astronomy
California Institute of Technology
Pasadena, CA 91125
kamion@tapir.caltech.edu

Professor J. Ritchie Patterson
Newman Laboratory
Cornell University
Ithaca, NY 14853
ritchie@lns.cornell.edu

Dr. Charles Prescott
Stanford Linear Accelerator Center
Menlo Park, CA 94025
prescott@slac.stanford.edu

Dr. Tor Raubenheimer
Stanford Linear Accelerator Center
Menlo Park, CA 94025
tor@slac.stanford.edu

Professor Marjorie Shapiro
Physics Department
University of California at Berkeley
Berkeley, CA 94729
mdshapiro@lbl.gov

Professor Melvyn Shochet
Enrico Fermi Institute
Department of Physics
University of Chicago
Chicago, IL 60637
shochet@hep.uchicago.edu

Dr. Boris Kayser
Fermi National Accelerator Laboratory
Batavia, IL 60510
boris@fnal.gov

Professor Elizabeth Simmons
Physics Department
Boston University
Boston, MA 02215
simmons@bu.edu

Dr. William Marciano
Brookhaven National Laboratory
Physics Department
Upton, NY 11973
marciano@bnl.gov

Professor Fred Gilman (Ex-Officio)
Department of Physics
Carnegie Mellon University
Pittsburgh, PA 15213
Gilman@cmuhep2.phys.cmu.edu

Dr. Jay Marx
Lawrence Berkeley National Laboratory
Berkeley, CA 94720
JNMarx@lbl.gov

U.S. Department of Energy
and the
National Science Foundation

January 21, 2003

Professor Abraham Seiden
Institute for Particle Physics
University of California at Santa Cruz
Santa Cruz, CA 95064

Dear Professor Seiden:

On November 6, 2002, the Department of Energy (DOE) and National Science Foundation jointly implemented the Particle Physics Project Prioritization Panel (P5). We are writing this letter now to ask you, in the context of the P5 charge, to study and prioritize the following three projects within the framework of the national high energy physics program.

- BTeV (Fermilab)
- Charged Kaons at the Main Injector (CKM) (Fermilab)
- The Run IIb CDF and D-Zero detector projects (these projects currently have been approved through CD-3a [partial construction]; awaiting next phase approval, CD-3b [full construction start]) (Fermilab)

As required by P5's charge, all of these projects have been approved by the Fermilab Physics Advisory Committee. The third item, the Run IIb CDF and D-Zero detector projects, is a special case since DOE has already given partial approval, a limited construction start through FY 2003 only. The issue before P5 for this project is a recommendation for full construction which would start with FY 2004.

While we do not expect P5 to do an extensive cost review, to be most helpful, P5 should comment on the appropriateness of existing cost estimates, indicate what funding levels are expected to be required by these new projects if they are approved, and evaluate what the scientific impacts would be if sufficient funding is not available during the timeframe of the projects under consideration.

We appreciate your performing this important function. We would like to have your response on these particular projects by June 2003.

Sincerely,

S. Peter Rosen
Associate Director for High Energy
And Nuclear Physics
Office of Science
Department of Energy

Joseph L. Dehmer
Director
Division of Physics
National Science Foundation

UNIVERSITY OF CALIFORNIA, SANTA CRUZ

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SANTA CRUZ INSTITUTE FOR PARTICLE PHYSICS SANTA CRUZ, CALIFORNIA 95064

February 11, 2003

Dr. Michael Witherell
Director
Fermilab
P.O. Box 500
Batavia, Illinois 60510

Dear Mike,

On January 21, 2003 the newly formed P5 subpanel received the request from the DOE and the NSF to study and prioritize three projects that are each ready to enter into construction. These three are: BTeV, CKM, and the Run IIb detector upgrades for CDF and D-Zero. We will be having our first meeting to work on the prioritization at Fermilab on March 26 and 27. We appreciate the willingness of the laboratory to host our meeting and look forward to working with the Fermilab staff and leaders of the projects. The charge to our subpanel from the agencies can be found in the letter of November 6, 2002 to Professor Fred Gilman.

I will be forming a subcommittee of P5 for each of the three projects to facilitate information gathering. Each subcommittee will collect for the full subpanel the cost and schedule information that is available. The costs required are the estimates for R&D, engineering design, full construction, preoperations, and operations. The quality of the information should be indicated (e.g., Lehman review, lab equivalent of such a review, or less rigorous review). It would also be very helpful if we had available in electronic format the latest presentations and responses from the PAC, any cost reviews, and any material the proponents feel that we should be reading.

At the March meeting the project proponents should address:

1. Physics goals, including measurements to be made. For each measurement, what is the expected precision for measuring Standard Model Parameters and/or the expected sensitivity to new physics? How does this sensitivity compare to other existing or proposed experiments (for BTeV compare explicitly to what can be expected from the B-factories, CDF and D-Zero, as well as LHCb)? For each measurement, what are the uncertainties stemming from hadronic physics or other physics? Are there physics topics for which one of CDF or D-Zero will provide a significantly better measurement than the other detector?
2. The international setting surrounding the project. What is the schedule for other competing experiments? By what date must the project start? What is the minimum number of years of running that the proponents would consider adequate? What is the projection for available manpower for construction and then detector operation and physics exploitation?
3. Detector related questions. For BTeV and CKM are there any detector components whose design and construction have significant risks? For each of the upgrades, what are the conditions under which silicon replacement is necessary (please be quantitative regarding the physics impact of no replacement for various luminosity choices)? What would the detector performance and schedule impacts be if the innermost silicon layer near the beam pipe were replaced with the remainder of the detector unchanged?

It would be most useful if we could receive a written response, in electronic format, to the questions above at least one week before the meeting, to prepare us for the talks.

I plan to complete an agenda for the March meeting by the middle of February. The agenda will include time for the laboratory to present its vision for the future and talks on the projects during the first day. The second day will provide an opportunity for us to ask questions regarding issues that we were concerned about. I would recommend that the cost numbers be settled with the subcommittees prior to the meeting and therefore they should not have to be covered in the first day talks. We may, however, have some questions regarding costs on the second day.

I greatly appreciate your help with our work. Please feel free to contact me if you have any questions.

Sincerely,

Abe Seiden
Chair, P5 Subpanel

cc: Peter Rosen
John O'Fallon
Bruce Strauss
Joe Dehmer
Marvin Goldberg
James Whitmore