Fermilab Submission to the
Particle Physics Project Prioritization Panel

March 20, 2003

INTRODUCTION

This document contains background information for the consideration of Fermilab projects by P5. It includes responses to questions asked in the letter from Abe Seiden, Chairman of the P5 HEPAP Subpanel to Mike Witherell and the individual projects involved. This is the outline:

- In the section entitled **Overview of the Fermilab Program**, we provide the physics context within which we view the Fermilab program and the components of that program.
- In the following section we provide the **Laboratory Responses**, which address those issues which are either program wide or for which the Laboratory is in the best position to respond.
- In the separately posted sections, **Run IIB Detectors**, **BTeV**, and **CKM**, we provide the responses deemed to be within the purview of the individual projects.

In addition, we have provided a web-site:
http://www.fnal.gov/directorate/program_planning/HEPAP_P5_Subpanel03_03.html
with pointers to further information and reviews of the Tevatron operations and of the individual projects.

Overview of the Fermilab Program

Fermilab and the National Program of High Energy Physics

A recent HEPAP subpanel wrote a long-range plan for U.S. High Energy Physics that discussed the compelling scientific questions that drive the field and the experiments that will address them. More recently, the Office of Science asked HEPAP for advice in shaping a facilities plan for the next twenty years. The documents submitted to the Director of the Office of Science constitute an elaboration of the subpanel plan. Given the central role of Fermilab, it is important that the laboratory program be developed in the context of the planning process for U.S. HEP.

Before discussing the parts of the program before P5, therefore, we will discuss the broad scientific issues, starting with the Subpanel report. The Subpanel report lists the major areas of research within particle physics, and that list is repeated below. Underneath each, we give the Fermilab programs that are currently operating, those in construction, and those included in the recently completed facilities plan. In bold are those presently being considered by P5.

1. Electroweak scale physics
   - **CDF and Dzero**
   - **Run IIB Detectors for CDF and Dzero.**

2. Lepton flavor physics
   - MiniBooNE
   - MINOS
   - Off-axis Neutrino Experiment and upgrades

3. Quark flavor physics
   - CDF and Dzero
   - **BTeV, B Physics with the Tevatron**
   - **CKM , Charged Kaon Physics with the Main Injector**

4. Unification scale physics
   - (See neutrino oscillation experiments under lepton flavor physics.)

5. Cosmology and Particle Physics
   - Sloan Digital Sky Survey and similar future programs
   - Cryogenic Dark Matter Search

6. High-Energy Particle Astrophysics
   - Auger Cosmic Ray Observatory

The Experimental Program at the Fermilab Accelerator Complex

The Fermilab program includes research using the Fermilab accelerators, research at the Large Hadron Collider (LHC), and research that does not need accelerators. Since all of the experiments being considered in this session of P5 are using the Fermilab accelerators, we will focus this discussion on that part of the program. Planning that program is not done in isolation, however. With our Physics Advisory Committee we consider the entire Fermilab program, the national program, and the world program of particle physics.

Electroweak scale physics is at the center of the roadmap, and it includes the questions that are most likely to provide a revolution in our picture of nature at the smallest scale in the next decade. Appropriately, this is the field that drives our need for the largest accelerators, the LHC and the Linear Collider. An important part of the context for planning the Fermilab program is its role in these two global projects.

Over the last decade, while LEP and SLC provided precision measurements on the Z pole, the Tevatron and LEP shared the exploration of physics beyond the Z boson. The Tevatron provided the major discovery, that of the top quark, along with its mass measurement. LEP provided the most sensitive Higgs limits, while W mass measurements and limits on new physics came from both electron and proton experiments.

From now until arrival of the first LHC physics results, CDF and D0 are the only experiments able to address these central physics questions. It is especially important for the field of particle physics that we maintain the only program at the energy frontier over this period. Any discovery would clearly reshape our understanding of particle physics and in addition would help to clarify the energy requirements for the initial phase of the linear collider. Even in the absence of discovery, the Standard Model will be challenged by improved top and W mass measurements combined with expanding exclusion plots from Higgs searches at the Tevatron.

The primary goal of the Laboratory for the immediate future is to keep producing important new physics results with the CDF/D0 program until the LHC has significantly eclipsed the Tevatron in its ability to discover new phenomena. One generation ago, the UA2 experiment ran until it was clear that the mass of the top quark exceeded that of the W boson and there was nothing further to gain from further running. Similarly, there is every reason to maintain an aggressive search strategy with CDF and D0 until they have reached the practical limits of what they can do or until the LHC experiments are able to publish conclusive results from their low-mass Higgs searches.

If it becomes clear that there is no discovery potential remaining for high-mass searches at the Tevatron, we will terminate the program.. We are not interested in maintaining a program when it is not central to the field’s exploration of new physics. If the low-mass Higgs becomes evident, we would need to consider whether improved Tevatron results could add enough new information to those from the LHC to warrant extension, but the threshold for doing so would be very high.

Fermilab is host for both the US-LHC accelerator and the US-CMS projects, and both projects are proceeding very well. We are preparing for the CMS research program with a computing and software project, including building a Tier-1 data center. We are also starting up a LHC accelerator research program. A growing effort on the LHC, especially by scientists on the research program, is an important part of our plans for the Fermilab program.

The linear collider has received strong support in all regions involved in particle physics as the next major facility. We are participating in accelerator and detector R&D on the linear collider, although at the small level presently possible, and we are providing a laboratory connection for involved university groups. In choosing the experimental program for the Fermilab site, we take into account the need to build up effort in this area when the government approves a substantial increase in support for linear collider R&D. In keeping with this, the experiments being considered by P5 do not require substantial accelerator R&D or improvements after early 2006. We also realize and expect that the duration of operation for any new experiments will depend on the evolution of the linear collider project.

Lepton flavor physics is advancing rapidly and has provided more surprises than any other field of particle physics over the last decade. Neutrinos provide another window on unification, since their tiny masses could well be generated at the unification scale. The most exciting new results from Kamland appear to choose the large-angle mixing solution for solar neutrinos, advancing the possibility that a new CP violation could be measured in future neutrino experiments. MiniBooNE will check the LSND evidence for a high-mass oscillation over the next couple years. MINOS, getting first neutrino beam in 2005, will be able to measure the parameters of the atmospheric neutrino oscillation accurately and to extend somewhat the limits on the fundamental parameter θ_{13} .

The goals for the future neutrino progam are:

- observation of ν_μ to ν_e transition and measurement of the mixing angle;
- determination, via matter effects, of the pattern of neutrino mass hierarchy; and
- potential discovery of the CP violation in the neutrino sector, if θ_{13} is large enough.

Proposed facilities to address these goals are a NuMI off-axis experiment, a neutrino superbeam, and perhaps in the long run a neutrino factory. Fermilab and the broader particle physics community will be discussing the roadmap for neutrino physics in the next year or so and our planning will depend on the outcome of those discussions

Quark flavor physics is a large and active field of particle physics with exciting new results. The B-factories at SLAC and KEK had spectacular success in achieving their goal of observing and measuring the large CP violation in the decay $B^0 \rightarrow J/\psi\ K^0$ predicted by the standard model. Kaon decay experiments at Fermilab and CERN have observed the predicted second mode of CP violation in kaon decays as well. The scientific interest of those working on quarks is now turning to the use of CP violation as an extraordinarily sensitive probe for many of the most plausible varieties of new physics. We have a unique opportunity to look for new physics phenomena using the amplification of such effects in quark mixing, CP violation, and rare decays.

The goal of the worldwide program in quark flavor physics is to make precise measurements of the fundamental CP-violating and non-CP-violating parameters through various quark-level processes and look for the inconsistencies expected in many models of new physics. The experiments needed to further this program require significant detector projects, but are of modest size because they do not require the construction of major new accelerator facilities. There will be about 5 experiments worldwide, including collider experiments studying bottom and charm quark decays and kaon beam experiments studying strange quark decays. Taken as a whole, this collection of important experiments will be central to our understanding of quark flavors and CP violation in the quark sector.

The next major goal in B physics is a sensitive search for non-CKM sources of CP violation in decays of B_d and B_s mesons. BTeV will be able to make the most precise measurements of such parameters in B decays and in B_S decays, which are not accessible at present or future e^+e^- B-factories. An extremely valuable asset needed for this experiment is the Fermilab Tevatron complex, which will become available for dedicated B physics near the end of the decade. BTeV will be the most important experiment in B physics from the time it starts producing results.

The next major goal in K physics is to measure V_{td} precisely in kaon decay channels that are well understood theoretically. The best opportunity for obtaining this precise (<10% error) measurement of $|V_{td}|$ will come from the CKM experiment, using the already observed decay $K^+ \rightarrow \pi^+ \nu\nu$. This experiment can be done with relatively modest investment because of the existence of the Fermilab proton source and Main Injector. CKM will be the most important experiment in K physics from the time it starts producing results.

Planning the Fermilab Program and Reviewing the Experimental Proposals

The experimental program outlined above was the result of a lengthy and deliberate process of considering how to contribute the most to the advance of the major research areas of particle physics using the accelerator complex at Fermilab, within the constraints on resources for the field. The criteria for approving a major experiment are that it address one of the central issues in particle physics, that it be the best of its type in the world at the time that it is operating, and that the physics return be great for the investment required. In addition, at every stage, we decide whether a new experiment could be afforded within reasonable expectations for available resources. Of course this judgment depends on predicting budgets for the High Energy Physics program at the Department of Energy rather precisely, since few percent changes in that budget can equal the costs of the experiments.

The Laboratory asked the Fermilab Physics Advisory Committee for general advice and specific recommendations throughout the process of shaping the future program. The Fermilab PAC is a group of exceptional particle physicists with broad background and experience. The Committee does the most thorough review of experimental proposals of any review or advisory committee in US HEP, deciding on major recommendations during the annual 5-6 day retreat in Aspen. These decisions are usually taken after a period of regular interaction and feedback between the collaboration and the PAC, which for major experiments lasts for a few years years. Importantly, the PAC produces carefully written reports that document the considerations they considered the most important. They are asked to judge the standing of an experiment in the context of particle physics worldwide as well as they can, using their experience and judgment.

CDF and D0

The primary goal in planning the Fermilab program for the period 2002 to 2008-9, has been to maximize the opportunities for advancing our understanding of particle physics at CDF and D0, while maintaining expected progress on the large projects preparing for LHC science and on the NuMI project.

Exciting new scientific opportunities in Run II have been identified for every new factor of 2 or so in integrated luminosity. Accelerator experts could foresee a modest number of accelerator improvements in addition to those already in the original Run II plan, leading to an additional factor of about 2-3 in luminosity. Without these improvements, the total integrated luminosity throughout Run II, limited both by accelerator performance and detector capability, would be around 3-4 fb^{-1}. For an additional investment that is modest relative to the accumulated investment already in place, the integrated luminosity for all of Run II can be increased to 7-15 fb^{-1}.

The experiments can and should run profitably for a period of roughly three years after the final luminosity improvements are in place, when the amount added to the data sample drops to about 25-30% per year. For reasons detailed below, the planned date for ending the CDF/D0 program is around 2009, based on the LHC schedule. It is important to complete the accelerator upgrades a few years before this time so that the improvement can be converted to integrated luminosity. The detectors are making only those upgrades needed to assure that they will keep running at high efficiency through the end of the program.

The Fermilab PAC reviewed the progress of the CDF and D0 upgrades continuously from 2000-2002. In the report of the June, 2002 meeting, they wrote the following: "Maintaining the capabilities of the CDF and D0 detectors throughout the run is also essential for the success of Run II...The Committee enthusiastically endorses the physics goals of Run IIb and acknowledges the necessity of maintaining the capabilities of all the detecdtor subsysstem included in the upgrade projects." They emphasized that the detailed technical, cost, schedule and management reviews would be completed by the Temple/Pilcher review later that year, and gave advice about what more needed to be addressed at that review.

About the physics, the PAC said: "Run IIb offers the extraordinary opportunity to discover the Higgs boson predicted by the Standard Model or its minimal supersymmetric extensions (MSSM)...Even non-observation of the Higgs in Run IIb would be a result of extreme importance. If the Higgs is not observed, 95% CL exclusion over the mass range required by the electroweak precision data would put the Standard Model in crisis. This is especially so since the Run II measurements of the *W* and top masses may tighten the precision electroweak constraints. If the Higgs is not observed, supersymmetry in the form of the MSSM will be excluded at the 95% CL or better over all but a tiny sliver of its parameter space."

We accepted the recommendation for Stage I approval of the projects. A combined technical review and cost, schedule and management review occurred in August, 2002. This led to a successful baseline review chaired by Dan Lehman in September. There were no action items, and they recommended the cost estimate should be reduced slightly. They also recommended, "the DOE should move forward expeditiously with CD-1, CD-2, and CD-3A."

BTeV and CKM
The world of particle physics will change when the LHC starts regular operation. The energy and luminosity of the LHC will make it possible to look for new particles with masses well beyond the reach of the Tevatron. This is why the U.S. is taking a large role in the LHC program, and why Fermilab is host laboratory for the US-LHC and US-CMS projects.

The most important question for the LHC era is what the rest of the U.S. program should be. There will be a large effort on developing accelerators for the future, most notably the linear collider, which is the next global facility that addresses the physics accessible at the highest energies. One particularly important question for the U.S. program is to identify experiments in the other areas of particle physics that can be done at U.S. accelerators and that will be the best in the world around 2010.

The HEPAP subpanel and the facilities plan exercise have identified a small number of experiments that could fit this criterion. In neutrino physics, an extension of MINOS is likely to be operating, and with time, perhaps, an off-axis NuMI experiment. In quark physics, the upgraded Belle and BaBar experiments will be producing very good physics throughout this decade, but operation as long as 2010 will depend on major upgrades. The identified possibilities for new experiments are BTeV and CKM at Fermilab, and KOPIO and MECO at Brookhaven. All four are approved but not yet funded.

Given the limited number of opportunities for such world-leading experiments at U.S. accelerators, it has been particularly important for Fermilab to examine with some care and rigor how BTeV and CKM would fit in the context of the world program in quark flavor physics.

We asked the Fermilab PAC to evaluate the importance of BTeV physics and to assess where the experiment would stand in the world of quark flavor physics in the LHC era. In the June, 1999, the PAC wrote a long, detailed report on BTeV that spelled out what they would need to see in the proposal for BTeV to be considered favorably. In the report of the June, 2000 meeting, the PAC completed their extended review of BTeV and drew the following conclusions:

- "The Committee believes that BTeV has the potential to be a central part of an excellent Fermilab physics program in the era of the LHC. With excitement about the science and enthusiasm for the elegant and challenging detector, the Committee unanimously recommends Stage I approval for BTeV."
- "New experiments will be needed at the end of this decade to provide crucial pieces of information. BTeV has the potential to supply these missing pieces of information

and could in fact be the definitive experiment that finally clarifies the picture of CP violation."

- "The Committee also concludes that BTeV will have a physics reach for CP violation studies that extends significantly beyond that of current experiments and those that will exist when BTeV runs."

The BTeV experiment was evaluated to be the most important B physics experiment in the world once it started operating. The laboratory accepted the recommendation and gave Stage I approval. Because budgets did not keep inflation and because of additional resources needed for the NuMI project, BTeV did not advance toward an approval of funding. The Laboratory gave the collaboration the problem of descoping to reduce cost while maintaining physics capability that could surpass other experiments. The Fermilab PAC wrote in April, 2002:

- "After reviewing the revised proposal and re-evaluating the experiment in light of additional information that has emerged in the last two years, the Committee once again recommends Stage I approval for BTeV. Although the composition of the committee has changed substantially since 2000, this recommendation is again unanimous."

BTeV has continued to make good progress in doing the needed R&D and in preparing for a DOE baseline review, despite little support in the last two years from Fermilab.
In September, 2002, Fermilab conducted a Director's review of the cost, schedule, and management of the BTeV project. There will be a follow-up Director's review to make sure that the project is ready for a DOE baseline (Lehman) review.

In 2001 we asked the Fermilab PAC to consider two proposals, CKM and KAMI, each of which had been in development for some years. For each experiment we asked them to evaluate the importance of the physics, the likelihood of completing a successful measurement, and the standing of each experiment in the world of quark flavor physics during the LHC era. In preparation for the PAC meeting, there was a joint technical review panel that looked at both experiments. In the report of the June, 2001 meeting, the PAC drew the following conclusions:

- "After detailed consideration of the CKM proposal for a precision measurement of the branching ratio of the decay $K^+ \rightarrow \pi^+\nu\nu$ and the reports of the technical and cost reviews, the Committee recommends Stage I approval of the experiment."
- "The experiment is based on an innovative technique that will provide redundant measurements of both beam kaons and charged-particle decay products. The redundancies will allow backgrounds to be measured convincingly from the data."
- "The Committee finds the cost of the experiment, both in dollars and in other Laboratory resources such as protons, to be high but acceptable, given the excellence of the physics."

The PAC did not recommend approval for KAMI. They decided that although the approach KAMI was taking would have as good a chance to make a precise measurement of the $K^0 \rightarrow \pi^0\nu\nu$ branching ratio as other approaches that had been developed, that it was not assured that such a measurement was possible with existing technology. The Committee said that CKM was much more likely to make a first precise measurement of a $K \rightarrow \pi\nu\nu$, and that CKM should be pursued first.

The Laboratory accepted the recommendation and gave CKM Stage I approval. Physicists working on CKM have continued to make good progress on the R&D for their detector and the RF-separated beam. This has been true despite little support from Fermilab except on the beam. The Laboratory recently conducted a first detailed Director's review of CKM, and there will be a follow-up review before they are ready for a DOE baseline review.

Summary
Fermilab has conducted a careful process over several years to develop its experimental program for the rest of this decade. The Fermilab PAC has reviewed the proposals in great detail. The criteria for approving these major experiments were that it address one of the central issues in particle physics, that it be the best of its type in the world at the time that it is operating, and that it the physics return be great for the investment required. In addition, at every stage, we considered whether the experiment could be afforded within reasonable expectations for available resources. The laboratory budget has been reduced in real terms, however, resulting in delays from the schedules originally envisaged.

With these experiments, U.S. High Energy Physics will have a strong program at domestic accelerators over the next decade, complementing its research in the LHC program and on particle physics experiments that do not use accelerators. Each experiment addresses some of the physics issues identified as the most important in the field. CDF and D0 will be alone in addressing the problems of the electroweak scale until the LHC is producing its physics. BTeV will be the best experiment doing B physics, and CKM doing K physics, in 2009 and beyond.

P5 has been asked to place the priority of these experiments in the context of the world of particle physics, both here and abroad. We encourage the members of P5 to review the documents leading to the approval of these experiments carefully, in addition to reading the documents and listening to the presentations submitted as part of the P5 process. The more thoroughly the review is done, the more positive will be the conclusion. We believe that P5 should endorse the decisions made by the Laboratory and the evaluations expressed by the Fermilab Physics Advisory Committee about how these experiments fit into the field.

LABORATORY RESPONSES

For each Project, provide 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.

Costs

The three different projects have very different scopes and are at different stages of development. These factors determine the precision of the cost estimate. In what follows, we characterize the status of the cost estimate and provide background information on elements of the cost estimates. The detailed project information, primarily that associated with the construction costs of the experiments, is to be found in the sections associated with the individual projects and/or their ancillary documentation.

- **CDF/Dzero upgrade detectors**

The construction costs, the R&D costs, and the funding sources and schedule are based on a complete review by the DOE Office of Science (Lehman review), which led to the existing CD1, CD2 and CD3A approval. The Lehman review for these projects had no action items and recommended that we reduce somewhat the contingency. The CD3A approval includes all obligations through the end of FY03 for both projects. Including R&D, the total DOE funds in $AY for construction of the detectors is $20M to 25M each, including labor and General and Administrative costs. By the end of FY 2003, the budget committed for R&D will be between $2M and $4M for each project and will be complete. The construction budget obligated by the end of FY03 for each project will be approximately $6-7M, with $14-18M more DOE funds needed to complete each project.

In order to decouple the shutdown schedule from the construction schedule, installation was not included in the baseline project. The full installation cost was estimated, for each project at $1.5 M, including labor.

The current operating cost of each experiment is in the range of $5-6 M per annum. This decreases with time as the need for dedicated technician support is decreased. The costs are shared among the funding agencies involved. As far as possible, the costs are prorated by the head count of participating Ph. D. physicists. Fermilab provides the share attributed to the US physicists, and the additional costs deemed not sharable by the relevant International Finance Committee. In addition to the operating costs for computing, there is a sizable annual expenditure on computing equipment. Since major contributions in this area by participating non-US institutions are possible, this will be incorporated into the algorithm by which operating costs are assigned. Thus FNAL has provided $2 M ($1.5 M in FY2003) to each of the experiments. There are no identifiable pre-operating costs for these projects since they are fully subsumed by operating costs of the ongoing experiments.

- **BTeV**

The construction costs for the BTeV experiment have been developed at a detailed level and were given a Director's Review (Temple) in September 2002. The technical aspects of the Director's review were very successful. Modest adjustments to the cost were recommended: "The BTeV team has done an admirable job of developing a good basis for the estimate at this early stage of the project". The total cost, including labor and general and administrative costs is $122 M in $FY02. This becomes a total of $135 M in $AY for a construction period ending in FY09. For comparison, the cost estimates that resulted from the Temple review of the CDF and DZero upgrades were considered to be too high by the subsequent Lehman review. The schedule for the work was relatively less well developed and is considered the major work to be done before the project is ready for baseline review by the DOE (Lehman Review).

In addition to the experiment, it will be necessary to complete the outfitting of the C0 infrastructure and hall to accommodate the installation of the BTeV experiment. This can be done in 2006. In the accelerator, the C0 Intersection Point (IP) needs reconfiguring as a normal straight section, and some instrumentation needs repositioning. The baseline plan for the low Beta intersection region is to reuse magnets moved from the B0 or D0 intersection points, when the high transverse momentum program at those intersection points is complete. The cost of all this work is about $10 M, and the time for making this transition is estimated to be three months. To make the transition as quickly as possible, it would be desirable to construct new intersection region magnets, with a total cost of about $27 M more than the baseline plan. Options that might cost less are still to be explored. All these costs are in $FY03 and include 30% contingency and the appropriate general and administrative costs.

Building new IR components for C0 would make it possible to maximize the scientific productivity of the Tevatron during the transition period. If the LHC were delayed, for example, it would make it possible to schedule a short commissioning run for BTeV before returning to a last run for the CDF/DZero program. This would put BTeV in a position to start full physics operation closer to the startup of LHC-b, while giving CDF and Dzero a full opportunity for discovery before discontinuation of the high-p_T program. We should be in a position to decide in 2005 whether to commission such a dedicated IR insertion for C0, based on the information at that time on the LHC schedule, on the BTeV schedule, and on the budget prospects for the field and the laboratory.

The R&D materials and services costs associated with the experiment were approximately $4M through FY2003. Given the advanced state of the R&D, they are expected to be a further $1.5M in materials and services expenditures through the start of construction. Some of the advanced triggering and computing techniques being developed for BTeV have attracted funding from the NSF Information Technology Research program, recognizing the fact that the BTeV trigger is an ideal platform for computer scientists interested in testing fault-tolerant system design. University physicists in the BTeV collaboration have also received NSF support for R&D on the pixel detector, the muon system, the RICH detector, and the electromagnetic calorimeter. If BTeV is approved for construction, the university physicists participating expect to get additional NSF support. The INFN has also provided support for the R&D on the silicon microstrip detector, and it is likely that there will a significant Italian contribution to BTeV if the project is approved for construction. Finally IHEP, Protvino has provided support for work on the electromagnetic calorimeter including test beam operation.

The operating costs for the BTeV experiment (over that of operating the Tevatron Collider) have been estimated in two ways: first by attempting a bottoms-up estimate and, second, by scaling the costs from the existing collider experiments. In both cases the estimate is about $4M per annum.

- **CKM**

The construction costs of the experiment were developed in order to support the proposal of the experiment. Fermilab gave CKM Stage I approval in 2001. A few technical issues were flagged as key to the success of the experiment and the collaboration has concentrated its limited resources over the last two years on executing the R&D addressing those issues. These included the superconducting radio-frequency cavities for the separated charged kaon beamline, the operation of the straw tracker in a vacuum, the photon veto system and the trigger and data acquisition. Recently a Director's review of the project was held in order to understand better the current status of the technical issues and and of the cost estimate. The experiment, the RF elements and the conventional construction and beamline elements were all considered in the same review although the latter had received no technical support during the past two years.

With respect to the technical issues the committee perceived considerable progress. They wrote "Concern is much reduced for the Vacuum Tracker and the Photon Veto systems. Early prototypes have gone a long way toward meeting the superconducting rf specifications and a major test is scheduled for the first quarter of 2004…."

As a result of the review, a modest increase in the experimental cost was recommended. Including these increases, and management, the experiment is estimated to cost $53M. The cost of the superconducting RF system was assessed to be $18M. These costs, in $FY01 include labor, contingency and general and administrative costs. The committee reported that the R&D had made major progress on all fronts and considered there to be no serious technical risks outstanding.

The design of the conventional beamline was known to be immature because the Laboratory had not been able to assign sufficient engineering to the problem. This led to the reviewers recommending a significant increase for a total of $30M, to give an upper bound for this effort. They recommended the application of technical effort over the next several months to achieve a reliable cost estimate for the conventional beamline and civil construction. The total project cost estimate is thus $101.5M, which includes the estimated upper limit on conventional beamline components and associated civil construction.

The materials and services costs of the R&D are included in the cost estimates for the various subsystems of the experiment. The research and development of the superconducting radiofrequency system will be completed in FY2005 at which point the expenditure will have been $5.3M.

The operating costs of the experiment should be significantly less than that of a collider experiment, primarily because the number of identifiable subsystems is reduced. The current estimate suggests a total of $2-3M per annum. The preoperating costs will be relevant for two years prior to full operations and we estimate 30% of the full operating costs.

- **Infrastructure operating costs**

We have estimated the marginal incremental cost of operating two components of the accelerator complex which can be attributed cleanly to operation of particular components of the program.

We have estimated the annual cost of operation of the **Tevatron Collider** beyond a base which includes operation of the proton source (Linac and Booster) and the Main Injector. The elements of the cost include the salaries wages and fringe costs of 181 people in the necessary operating departments. These include the antiproton source, some of the cryogenics department, some of the operations department, and an appropriate fraction of the support departments. We can identify approximately $4.2M in electricity costs and $2.3M in the cost of cryogens. Including some smaller items yields a total of $25M per annum with an uncertainty of approximately 20%.

It is important to note that the present collider program requires significantly more budget partly because it requires operating the collider facility at the same time as executing an aggressive upgrade program. Around 2006, the upgrade program will be complete and priority will be given to steady operation. No additional accelerator R&D or upgrades are needed for BTeV, with the possible exception of the dedicated IR.

The **CKM beamline** contains both superconducting radiofrequency components and conventional elements. We have estimated the operating costs for this specific beamline beyond a base of operations of the Proton Source and Main Injector. The manpower required, including the cryogenics operation is 20-25. The cost of electrical power for the beamline is $1M per annum with between $0.1 and $0.5M for power, depending on the configuration of, and capital investment for the refrigeration plant. Taking account some modest materials and services, we estimate a total of $3.5M-$3.9M per annum. Preoperations in the one year before the experiment takes data should be approximately 25% of the full operating cost, say $1M.

- **Fermilab Budget: ten-year view**

For discussions with the Physics Advisory Committee we develop a perspective of the long range expectations for expenditure on the experimental program in an attempt to indicate how what may be approved or rejected fits within reasonable funding expectations. We have taken all costs including the Construction costs, R&D costs, and operating costs associated with the projects under consideration and have shown how they fit within a reasonable projection of the Laboratory Budget, in Fig 1. All costs in Fig 1. are expressed in FY2003 dollars. Inflation has been assumed to be 4.5% for labor and 2.5% per year for materials and services. The flat red line shows the purchasing power of the Fermilab FY2002 budget in FY2003 dollars. FY2002 is shown as the budget was actually obligated in that year. FY2003 is the actual budget distribution for this fiscal year. FY2004 represents a budget that is increased in then year dollars from FY2003 by 2%. FY2005 is the laboratory's request to the Department of Energy as it will be in our WPAS submission this spring. FY2006 and beyond indicate how the budget would be deployed to support the currently approved program. As will be observed in Fig 1, the ten-year plan discussed here requires a funding profile that never returns to the level of the purchasing power enjoyed by the laboratory in FY2002. The data in the figure are presented as the laboratory actually prepares its internal budgets. That is, the indirect activities are not distributed over the projects but are shown as a separate activity called "Indirect Support." In this scenario the first significant funding for any new initiative, not included in the currently approved program, becomes available in FY2009. The legend in Fig 1 indicates those programs that are included. All costs for operating, maintaining and upgrading the accelerator complex are contained in the sum of the bands labeled "Accelerator Improvements for Run II" and "Accelerator Operations and Maintenance." The band labled "Other Direct Activities" includes all R&D for future and advanced accelerators, linear collider R&D, fixed target efforts other than neutrinoes, base support for the LHC/CMS programs, experimental particle astrophysics, theoretical physics and astrophysics, and a wide variety of direct support activities including central computing and networking, building and facility maintenance, and ES&H activities in the direct program.

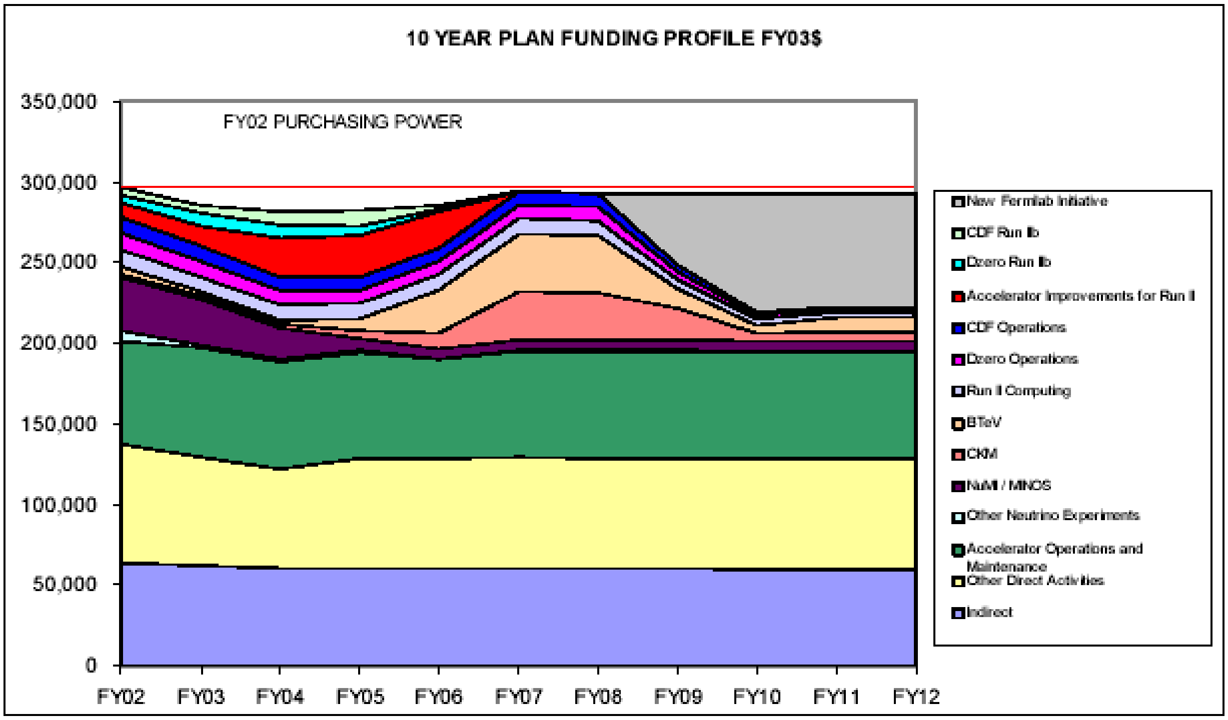

Figure 1: Tentative Ten Year Budget Plan

- **Long-range schedule**

There are clearly uncertainties in a running schedule which many years. Nevertheless, we have constructed a strawman running schedule through 2012, for the purpose of discussing key planning issues.

- **Integrated Luminosity for CDF and DZero**

We have taken the schedule shown in Figure 2 and folded in the expected Tevatron Luminosity based on the review of October 2002 including some startup penalty following the shutdowns. The resulting curve superimposed on the October 2002 Stretch and Base goals is shown in Figure 3. This properly accounts for down time associated with the shutdowns to install the Run IIB Detectors. These are shown as ocurring at the time indicated by the baseline schedules for those projects. At the time at which this shutdown occurs, the experiments will have accumulated an integrated luminosity of between 3 and 5 inverse femtobarns. This matches the expected performance limits of the present silicon detectors. The full integrated luminosity that the detectors are expected to see is 7-15 inverse femtobarns depending on the ultimate weekly luminosity attained and the final termination date.

During these years we expect that the accelerator complex will also be operational for a neutrino program, initially MiniBooNE, then NUMI-MINOS. The complex is designed to be compatible with concurrent operation of the collider and the neutrino program. What concerns remain are associated with the details of the concurrent operation of the Main Injector for antiproton stacking and extracted beam for MINOS.

We have chosen a particular date in mid 2009 to indicate the end of the running for CDF/Dzero and the installation of the intersection region for BTeV. At that point the integrated luminosity would be expected to be between 7 and 13 inverse femtobarns. There are many factors which will influence the eventual choice of the switchover date.

The most important uncertainty that we can identify now is the schedule for completing the LHC projects and commissioning the accelerator complex and the detectors. We need to put ourselves into position to make the right program planning decisions during the period the LHC is coming on, allowing for uncertainties in the LHC schedule that will not be completely resolved until the experiments are taking physics data. In this exercise we assume that the LHC accelerator project maintains the present schedule and circulates beams for the first time in spring, 2007. As in the case of the previous three superconducting colliders, it will take some commissioning period before the collider is operating at reasonably high luminosity. Commissioning the detectors and the table of triggers for high-luminosity running will also take a reasonable time, even given the fact that the collaborations will be at an unprecedented state of readiness before high-luminosity conditions are available.

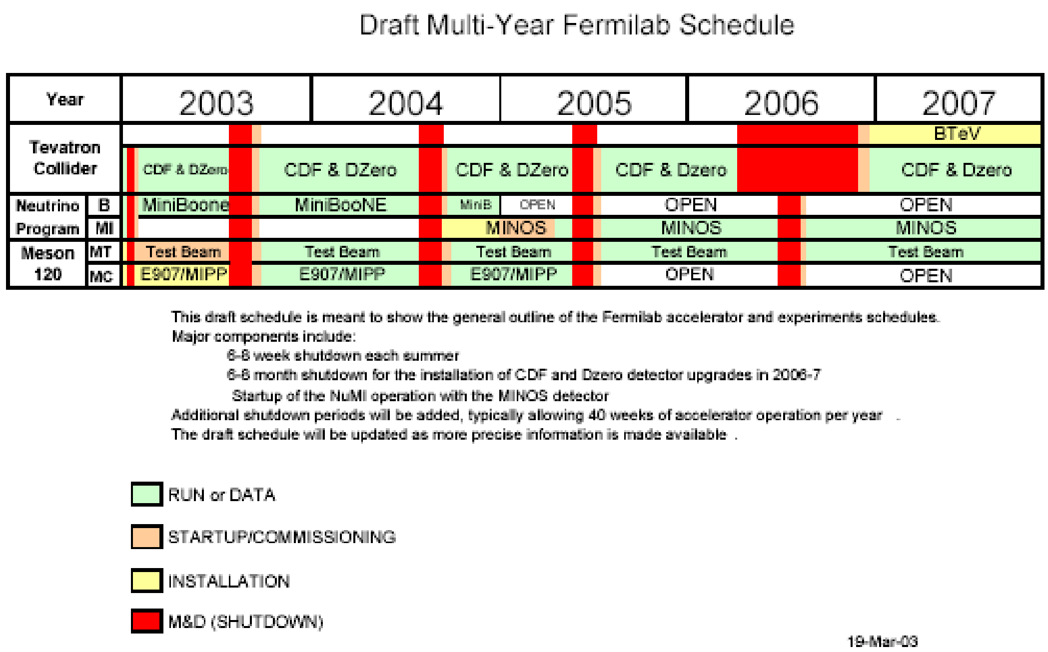

Figure 2a: Fermilab Multi Year Running Schedule: Tentative

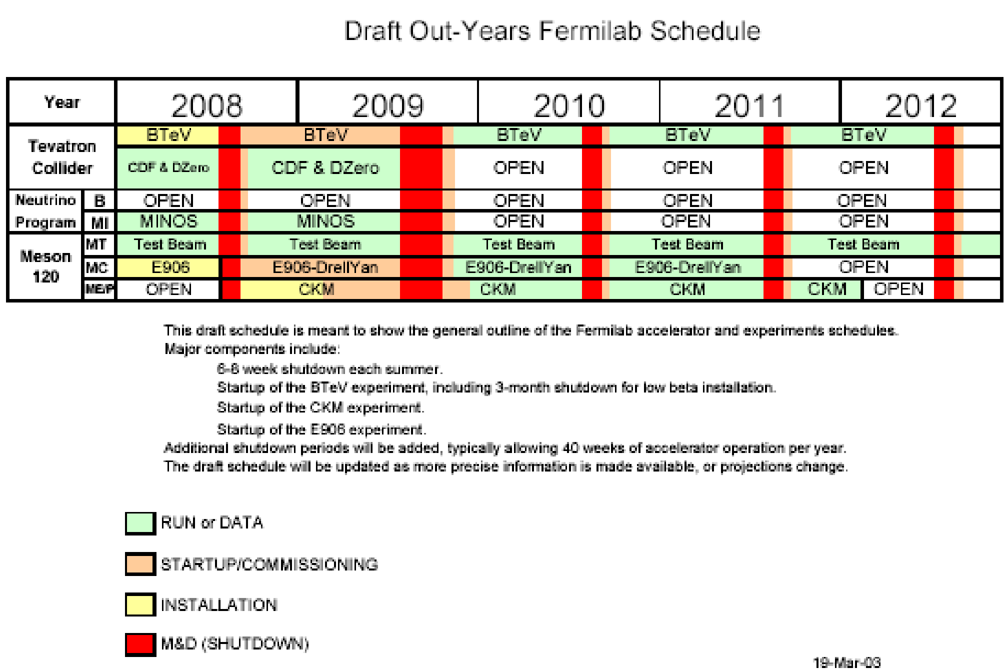

Figure 2b: Fermilab Multi Year Running Schedule: Tentative

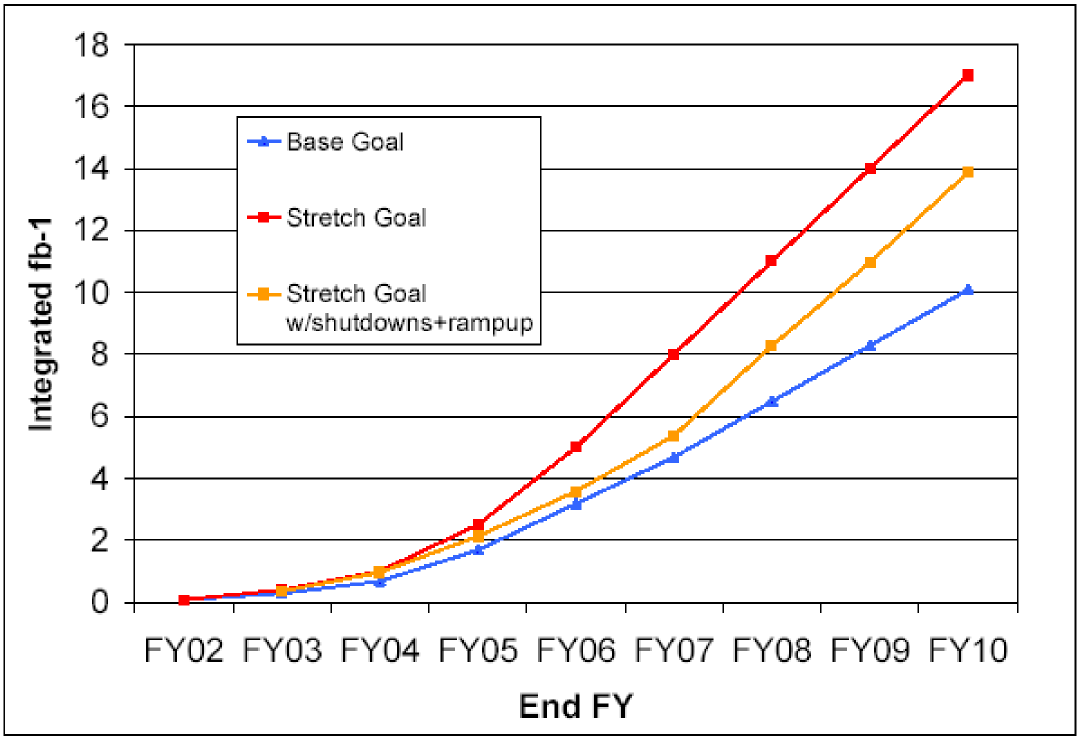

Figure 3: Integrated Luminosity for each experiment as a function of time.

Some possible LHC discoveries such as supersymmetry or a medium-mass Higgs require minimal luminosity and could be immediate. For the low-mass Higgs, which has most overlap with the Tevatron program, the discovery strategy requires the combination of both experiments and two channels ($H \rightarrow \gamma\gamma$ and ttH, $H \rightarrow bb$). This will take significant integrated luminosity and some time to operate, understand, and calibrate the detectors. We can reasonably expect that two well-tuned LHC experiments will be ready to discover a low-mass Higgs using a reasonably large data sample from 2008, with results becoming public in 2009.

We expect the end of running for CDF and Dzero and the installation of the intersection region for BTeV to come around mid-2009. This represents a reasonable compromise between the goal of getting full physics return from the CDF/Dzero program the interest in obtaining B physics results from BTeV as soon as possible. The exact timing of this important transition will be done through a process of careful program planning, based on the states of the LHC physics program, the CDF/D0 physics program, and the BTeV experiment.

Because of the non-hermetic geometry of the full detector, various components of BTeV could be installed and commissioned well before the IR magnets are installed. Commisioning would occur while operating in a test mode, using interactions of the beam halo in wire targets or low-luminosity collisions for brief periods at the end of stores. Such early operation would make it possible for BTeV to get a running start when it does get full-luminosity.

The recent review of the LHC schedule emphasizes that there is almost no float for many items. Within the conservative DOE project management system, one would set an official date for project completion that can't miss, perhaps 18 months after spring 2007. Prudent planning dictates that we should be ready to accommodate a slip in the LHC schedule of this length without a major problem. Building the new interaction region for BTeV would allow us to hold the date for completion of the BTeV detector even if the LHC schedule slides.

- **Operation of the Tevatron for BTeV**

In the main scenario above, the operation of BTeV for physics occurs after the high transverse momentum experiments have completed running. Before that time, the goal would be to provide a modest amount of running under special conditions. This could occur at the end of a store using a wire target, but would afford commissioning opportunities for BTeV.

- **Operations for CKM**

We show the CKM operations starting with some testing time during 2008/9 and full operations from late in 2009. At that stage the operation of the Main Injector will be very mature and we expect to be able to accommodate the experimental requirements. There appears to be some flexibility in the mode of operation, that is to say, at what frequency we provide the slow spill to CKM. These options appear to give us considerable latitude in handling the inevitable program planning concerns of a healthy multifacetted program. Occasional short periods with beam could be accomodated with zero impact during 2008.