



Fermi National Accelerator Laboratory

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**The Future of High Energy Physics in the United States
Statement for the 1994 HEPAP Subpanel**

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Cornell's CESR and CLEO

The high luminosity of CESR, together with the CLEO detector, has allowed the CLEO collaboration to make the most accurate measurements of many of the properties of the B_d and B_u mesons. It has provided the best opportunity to detect and measure the properties of rare but informative decay modes of these B mesons. The CLEO detector is also the source of some of the best measurements of charm and tau decays. In the near future, once CLEO acquires a vertex detector, its capability of separating charm and tau decays from other processes will be significantly improved. It is not unreasonable to expect that CESR will reach a luminosity of $3 \times 10^{33} \text{cm}^{-2} \text{sec}^{-1}$ before the end of the decade. Not only will CESR be a B factory, but it will be a very powerful charm-tau factory as well. In the light of CESR's projected performance, the need for another charm-tau factory is questionable. With adequate funding for operation and improvement, CESR can be a very productive facility well into the next decade.

Fermilab's Tevatron

The Tevatron defines the energy frontier in particle physics today and will continue to do so for another decade; it provides research opportunities not found anywhere else. The Tevatron's energy gives the experimenters using CDF and DØ the unique capability of discovering the top quark and measuring its basic properties. The Tevatron is the only facility in operation that can produce W bosons in sufficient quantity to make precise measurements of the W's properties. The first observations of W pair production will be made at the Tevatron before the current run ends, and the data from that run will also provide a meaningful limit on the anomalous magnetic moment of the W. The only competition for the Tevatron in this arena will come a few years hence from the Large Electron Positron Collider (LEP) at CERN, once it has sufficient superconducting rf to reach the W-pair production threshold and thus allow LEP experimenters to make precise measurements of the properties of the W.

When the Tevatron is operated as a proton synchrotron, it provides the highest energy fixed-target beams in the world. This capability, unique by definition, is likely to remain so for the indefinite future. Fermilab's neutral K beams, hyperon beams, photon beams, and neutrino beams are providing superb opportunities. The search for direct CP violation in neutral K decays at the level of one part in ten thousand can only be carried out at CERN and Fermilab. The KTeV program of experiments at Fermilab will certainly yield a meaningful measurement of ϵ'/ϵ before the decade is over. It will also yield the most precise measurements of rare decays of neutral kaons. The hyperon experiments at Fermilab have defined the static properties of hyperons and have successfully measured the branching ratios of some of their rare decays. Since plausible proposals have been submitted to detect CP violation in hyperon decays, it is likely that such experiments will be done in the not-too-distant future. Photoproduction and hadroproduction experiments have provided the best measurements of charm particle lifetimes, and now they are providing the best measurements of the form factors of charm mesons. Fermilab neutrino experiments have provided the best measurements of structure functions

particle physics, and indeed in all science. In the 1890s the great British physicist William Thomson, Lord Kelvin, one of the founders of the science of thermodynamics, explained it this way: "When you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meager and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science." To get at the truth and answer our questions in particle physics, we need more than one set of measurements, made by different, complementary means. That is why the diversity of high-energy physics laboratories is so important to the progress of discovery in particle physics.

The Near-Term Requirements of the Base Program

It is worth recalling why the U.S. accelerator laboratories, capable of providing world-class opportunities at the forefront of the field, nevertheless find themselves without enough funding to achieve their potential. This crippling shortfall is a consequence of the very high priority that the field of high-energy physics gave to the construction of the SSC. Funds to operate and upgrade the rest of the high-energy research program—the base program—slowly declined over the past five years, as funds for SSC construction rose rapidly. When Congress voted to terminate the construction of the SSC, the funding of the base program continued on its downward trajectory. The U.S. high-energy physics community not only lost the SSC but also the funding to take good advantage of the opportunities for significant discovery in this decade. The program now needs a modest 10 percent increase above FY1994 funding, about \$60 million, exclusive of funds for construction, if the program is to fulfill its potential for discovery in this decade. Without such an increase, it will be necessary to restructure the base program by eliminating some of its diversity. This will be difficult and damaging in the short term, but it will be necessary if we are to have a program that produces forefront results.

The modest amounts of construction funds that are provided in the budget are desperately needed, to build the Main Injector at Fermilab and the B Factory at SLAC. These two projects will enhance the potential of their respective laboratories by strengthening the ability of these existing facilities to provide world-class research opportunities in the first decade of the next century. It is important to complete them rapidly, so that they can provide the U.S. with a productive program at the beginning of the next decade. The B Factory will allow a deeper study of the origin of the matter-antimatter asymmetry than is possible today. The Main Injector will allow an exploration of the basic properties of the top quark and make neutrino beams of unparalleled intensity at an energy level that will allow investigation of the question whether neutrinos have mass. The decays of the top quark may provide us with a clue to the nature of electroweak symmetry breaking and the mystery of why there are only three generations of quarks and leptons. By the early part of the next decade, Fermilab should be carrying out a new generation of collider experiments, one of which is likely to be focused on the properties of B decays,

The LHC will not be an easy collider to build and operate. The 8.65 Tesla two-in-one dipole magnets that CERN plans to employ will push the superconducting magnet technology well beyond the 6.5 Tesla single-beam-tube dipoles that had been intended for the SSC. The superfluid cryogenic system will be the largest ever built. The required performance of the interaction region quadrupoles as currently designed stretch a magnet designer's imagination and the credulity of the accelerator specialist on the street. While a liner was desirable in the SSC, it is essential in the LHC, if the design luminosity is to be attained. There is a clear need to understand all of these difficulties and develop solutions for them. If U.S. accelerator specialists are to contribute to the solution of these problems, then they will require some additional funds for magnet R&D in FY1995. These funds will be well spent, because they will allow us to develop our capability for the future.

The proposed LHC luminosity of $10^{34}\text{cm}^{-2}\text{sec}^{-1}$ will require advances in detector technology beyond what had been planned for the SSC. The groups working on SDC and GEM had carefully explored the problem of working at a luminosity of $10^{33}\text{cm}^{-2}\text{sec}^{-1}$, but $10^{34}\text{cm}^{-2}\text{sec}^{-1}$ remains *terra incognita*. The U.S. detector groups that might ultimately join ATLAS and CMS will need a small amount of money for the next few years if they are to contribute to the solutions of these difficult technical problems.

If the U.S. is to contribute to the construction of the LHC, we must be full participants in the accelerator and detector R&D programs that must precede the start of construction. It is important that our government be well informed on the progress made in solving these difficult technical problems, and this will only happen if we are collaborators in all phases of the work. In spite of all these difficulties, the LHC remains the only affordable option open to the U.S. high-energy community if it hopes to continue to work at the energy frontier. Participation in the LHC will not require large amounts of money in the next two or three years; but it does need a commitment that roughly \$50 to \$75 million per year will be forthcoming in FY1998 and beyond.

Can the U.S. Remain Among World Leaders in Elementary Particle Physics?

During the debate over the SSC, too often our more reckless colleagues pronounced that the search for the Higgs was the only worthwhile undertaking in high-energy physics. Clearly it is important, and clearly we can create the capability to search for it. But we shouldn't fall on our sword because Congress was unwilling to provide \$10 billion to complete the SSC in a time of extreme deficits. There is more to understand about elementary particle physics than the SSC could illuminate. We must take stock of what we can do within the framework of affordable budgets.

To continue among the world leaders in high-energy physics, the U.S. must have a few forefront accelerator facilities in the United States. Consider for a moment what the U.S. program would be like if it were reduced to using facilities in other regions, with no operating high-energy accelerators in the U.S. The benefits

and magnet test lab is newer and more desirable. Brookhaven and Fermilab have both the capability and the desire to resume such a magnet development collaboration. There is another important reason why these laboratories are the appropriate nucleus for U.S. participation in the LHC. Fermilab is operating a successful superconducting collider right now, and Brookhaven will be operating one before the end of the decade. The best place to keep superconducting magnet technology alive is at laboratories that use it.

While it is up to the international scientific community to recommend whether the next super-machine after the LHC will be a TeV-class linear collider or a 60 TeV proton-proton collider, it is very likely that ultimately both will be built. Since I am both the proprietor of the only working superconducting hadron-hadron collider in the world, and also the caretaker of the partially built sarcophagus of the largest one ever started, let me give you my views of how I believe one could build a 60 TeV proton-proton collider in the U.S. without risking another SSC catastrophe.

The successful path that CERN followed to reach the LHC holds a lesson for us. Through the years, CERN has added one accelerator after another to its complex, and with each addition its scientific capability has grown. At no time was it necessary to triple the CERN budget to build a new accelerator, and at no time was the ongoing productivity of CERN threatened. The experience of CERN also teaches us that an active laboratory is an excellent nucleus for expansion in capability, because its technological capability is enhanced by the effort of designing and building an addition to an existing facility.

I believe that the surest approach to a super hadron collider is an incremental one. Fermilab is surely the best site in the U.S. for an incremental approach to a future 60 TeV-class super hadron collider. Fermilab already has a working 1 TeV superconducting synchrotron that would make a superb injector. By 1999, all of its warm machines will have been extensively modernized. The value of the injector and the associated infrastructure is at least \$3 billion. The scientific and technological capability of the Fermilab staff to plan and build a superconducting hadron collider is unique in the U.S.. The remaining steps are clear: First participate in the LHC program, thus gaining the opportunity to sharpen our skills in superconducting magnet construction and hadron collider technology and, as soon as practical, establish an international collaboration to carry out the R&D for the components of a 60 TeV collider. The R&D can proceed at a pace that matches the funding. Once it is done it will be possible to carry out an engineering design from which to develop an accurate cost of the entire facility. If the international collaboration and the participating governments decide on Fermilab as the site, only then would one begin to bore through the Illinois limestone to build an appropriate-size tunnel—the cheapest and easiest part of any supercollider. As an intermediate step along the way to 60 TeV, one could consider putting in one ring partially filled with relatively inexpensive 6.5 Tesla magnets suitable for a $p\bar{p}$ collider—a missing magnet scheme in the extreme. The energy and luminosity would be lower than that of LHC, but one could do physics with this machine and

the U.S., work is going on in smaller ways at Cornell, Fermilab, and CEBAF. Ultimately, one design will emerge as the best choice from among the various possibilities. Virtually the entire worldwide linear collider community has agreed to work in collaboration toward this choice, and to share their resources in the work. Moreover, all of the collaborators have a common vision of a linear collider that will be built as a worldwide effort. This collaborative effort—a first for accelerator builders—is an example of how big facilities will be designed in the future. Hopefully, it will also serve as an example of how they will be built. It is a process that should be considered carefully by CERN if they are to request that nations such as Japan, the U.S. and Russia contribute to the construction of LHC as the price of using the facility.

Beyond Accelerators

Particle physics research advances not only with accelerators but through the study of the early universe. In recent years, the convergence of particle physics, cosmology, and astrophysics toward a common set of issues has grown increasingly strong. The interplay of these different fields will almost certainly continue into the next century. Discoveries at accelerators will have a significant impact on astrophysics and cosmic-ray physics; and progress in those fields will in turn illuminate our understanding of the structure of matter. For some time, large underground detectors designed to observe the interaction of neutrinos from outer space have been recording data. Twenty years ago, the forerunners of these detectors were built to search for proton decay corresponding to a lifetime sensitivity of 10^{30} years. In the process, the experimenters learned to handle background resulting from the interactions of neutrinos in their detectors produced by cosmic rays colliding with the upper atmosphere. Yesterday's background is today's signal, and results accumulated to date have raised new questions about neutrinos and confirmed the role of neutrinos in supernovae explosions. More extensive observations may provide answers that profoundly affect our understanding of particle physics.

To search for the origin of the highest-energy cosmic rays, experimenters have built large counter arrays at the earth's surface, in a continuation of the earliest approach to particle physics. We have opportunities to learn much about the origin of these cosmic rays, and our growing knowledge may provide a few surprises.

For some time astronomers have known that the amount of matter needed to hold galaxies and clusters of galaxies together for the billions of years that they have existed cannot be accounted for by the luminous mass, the matter that we can see. How much dark matter is there in the universe and how is it distributed? What is this dark matter? Is some of it due to massive neutrinos or to unfamiliar particle relics from the early universe? Many searches for dark matter now underway could be sensitive to such relics. The University of California Center for Particle Astrophysics provides a focus for many university groups engaged in these searches. Fermilab has recently joined with astrophysicists from Chicago, Johns Hopkins, Princeton, and Japan to play an important part in fostering this