# Physics At Fermilab in the 1990's

John Peoples
Fermi National Accelerator Laboratory

It has been four months since I shared my thoughts on the physics that could be done at Fermilab in the 1990's. Jon Rosner preceded me that day and he gave a truly excellent summary of the work that the participants had done. I had the opportunity to read his contribution while I was preparing my own. Since Jon shows very convincingly that there is a great deal of very good physics that can be done at Fermilab in the 1990's, I chose to comment here on how that physics might be done and how Fermilab might support those efforts. While the text is quite a departure from the remarks I made in Breckenridge, I think that it is an appropriate set of comments to append to the very fine work that the participants have contributed to these proceedings. I will not reproduce Jon's comments, although I will borrow extensively from them in order to set out my points of view. Jon identified measurements that can and should be done, since there are no technological problems that have to be solved before they can be made. He also identified experiments that should be done, but cannot be done until very substantial technological problems are solved. For example, the ratio of W mass to $Z^o$ mass, the ratio of the W width to the $Z^o$ width, and the forward-backward asymmetry of the leptonic decays of the $Z^o$ belong to the former category. The CDF and D0 collaborations will make these measurements with increasing precision as the luminosity increases and their detectors are upgraded. The observation of CP violation in the decay of B hadrons produced in the Tevatron Collider is the most extreme example of the latter, because it is not clear that all of the technical problems that must be solved can be solved.

The testing of the electroweak model at Fermilab is something that should be done. These tests need not be limited to collider experiments, since the determination of the weak mixing angle, $\sin^2\theta_W$, can be made using precise data from high-energy neutrino-nucleon cross sections and neutrino-electron cross sections. The importance of significantly improving the different ways of measuring $\sin^2\theta_W$ lies in the fact that each

measurement provides a slightly different test of electroweak unification. Collectively, these determinations could give us a glimpse of the way our standard model will have to be modified to be compatible with nature.

The importance of making accurate measurements of the quark mixing matrix and the continuation of the search for the source of CP violation was pointed out repeatedly as was the opportunity to do this work at Fermilab. During the next six years, fixed target experiments will make measurements of the lifetimes and the semi-leptonic branching ratios of charm and beauty hadrons that could, in principal, provide accurate determinations of several of those matrix elements. Fermilab fixed target experiments dedicated to measuring the weak decays of charmed hadrons may find very rare decays that are forbidden in the standard model lurking in samples of $10^7$ charmed particles.

The ultimate prize in B physics is the observation of CP violation in the decay of hadrons containing b quarks. In excess of $10^6$ B hadrons decaying into a specific final state have to be detected and reconstructed in a single experiment to test CP. In order to acquire such a sample, about $10^{10}$ B hadrons have to be produced. At Fermilab this can be done most easily at the Collider. Since the b quark production cross section in $p\bar{p}$ collisions at 2000 GeV is about 20 μb, a luminosity of $5 \times 10^{31} cm^{-2} sec^{-1}$ is sufficient to produce $10^{10}$ B's in six months. The technical problems that need to be solved to demonstrate that a detector can detect B's in hadron collisions are extremely challenging. The BCD collaboration has been engaged in this effort for several years. At Breckenridge several others entered the contest, convinced that it can be done.

Fermilab fixed target experiments are collectively in a commanding position to complete the spectroscopy of the charmonium system and of the light quark-charm quark system during the next five years. If the improvements in proton intensity, detector rate capability, and data acquisition performance that were forecast at Breckenridge occur, then the spectroscopy of the light quark-b quark system could be brought to the same understanding in ten years. If the microvertex detector technology can be advanced sufficiently in the next five or six years, then direct observations of both $B_d$ and $B_s$ decays should be possible. It is very likely that these observations will require transferring the

relevant data acquisition techniques that are being developed for the fixed target experiments to the Collider.

The neutral K experiments done at Fermilab have had a long tradition of pushing back the frontiers of precision. This tradition will continue into the future. Precise measurements of the branching ratios of rare decays of the $K_S^0$ and $K_L^0$ may make a key contribution to the unraveling of the CP puzzle. Fermilab experiments will be the best of this genre for the foreseeable future. However, they will require the proton intensity that the Main Injector can provide in order to achieve the necessary precision.

The measurement of the nucleon structure function over a large range of $Q^2$ with sufficient precision and freedom from systematic error is one of the ways to demonstrate the running of the strong coupling constant. That goal has eluded all fixed target and collider experiments to date. The properties of specialized spectrometers using a high energy muon beam were discussed at Breckenridge. The conclusion was that such an experiment could measure $\Lambda$ to 10 MeV. A similar conclusion was reached by the participants who considered using a much improved neutrino detector. Measurements such as these and experiments in hadron spectroscopy are essential if the structure of hadrons is to be well understood. Again the Main Injector is a necessary ingredient if the desired precision is to reached. While QCD is almost certainly the fundamental theory that governs hadrons, it provides only a few clues on how to connect the unobservable quarks and gluons with the physical hadrons. This could well be one of the major efforts at Fermilab as the decade of the 1990's comes to a close.

The search for the top quark is the one experimental goal that stands out above all of the aforementioned. If the upper bounds on the top quark mass that are derived from $\sin^2\theta_W$ and the W and Z masses are correct, then the top quark will be discovered at Fermilab some time during the next six years if Fermilab makes this its top priority. The top quark will require the luminosity that the Main Injector can provide, either for initial discovery or for systematic study. Its initial exploitation at the level of one hundred events will certainly require the Main Injector. All of this was extremely exciting to hear in August and it reads as well

today. What is at issue is how Fermilab should provide the necessary support to insure that this discovery is made.

Many of the measurements that we will wish to do in the next six years can be done well by continuing the evolution of the existing program. A totally new accelerator is not needed to address these issues. The accelerator technology to provide the collisions from which the aforementioned physics can emerge is in hand. Moreover, the ideas for experiments that we will wish to do ten years from now will emerge from the questions that are raised by results obtained over the next five to six years. It will be possible to do these experiments if the Tevatron has been upgraded and if a carefully focussed program of detector development is carried out. If Fermilab is funded adequately during the next six years, the Tevatron will achieve a collider luminosity of near $10^{32}cm^{-2}sec^{-1}$ and a fixed target intensity of $6x10^{13}$ protons/cycle before the SSC turns on. Although the detector technology and data acquisition technology that are needed to do the most demanding of the experiments are not yet entirely in hand, there are very promising directions that can be pursued. The accomplishments made in improving detector technology over the past decade have been very impressive and there is every reason to believe that we are not at the end of the road. Fermilab and its Users are in a position to make those breakthroughs if the funds are forthcoming. In all respects, the technical problems are less daunting than the problems that must be overcome if detectors are to work in the SSC environment. As such, the R&D may be thought of as a necessary precursor to successful SSC R&D.

Since Jon Rosner's talk summarized the work of the Workshop so well, I will confine the rest of my written remarks to the way I think Fermilab will evolve to build and operate the accelerators and detectors and to create the computing environment in which to analyze the data that will be forthcoming. I will start with the "Fermilab (Accelerator) Upgrade." The goals of the "Upgrade" are to provide a luminosity of nearly $10^{32}cm^{-2}sec^{-1}$, at a center of mass energy of 2000 GeV, in the collider mode and a proton intensity of $6x10^{13}$ protons per cycle at 900 GeV in the fixed target mode.

While the scheme of creating separate, non-intersecting, closed helical orbits for the protons and antiprotons as the means of keeping

roughly forty bunches of protons and forty bunches of antiprotons separated in the Tevatron is technically quite demanding, and certainly breaks new ground, the solutions are well in hand. During the past year closed helical orbits have been set up with proton beams in the Tevatron. Studies of these orbits have demonstrated that the orbit location, tune, and chromaticity can be controlled. Preliminary tests using electrostatic separators to achieve a partial separation of beams of protons and antiprotons show that adequate beam lifetimes are achievable at injection, a time in the cycle when the beam-beam interaction can be most disruptive. The schedule for the construction of the separators will permit two major tests of the system with beam before it is used to provide luminosity for the CDF and D0 detectors in mid 1991. The magnets for the new low beta insertions represent an advance in superconducting quadrupoles. All of the magnets that have been tested have met demanding requirements for gradient strength and field quality. The production of these magnets is on schedule and all should be complete by the summer of 1990. The B0 insertion will be installed late in the summer of 1990. The installation of all of the magnets for the insertion at D0 will be deferred until the end of the 1990-1991 fixed target run, since that insertion must trade places in the Tevatron with some of the equipment for slow extraction needed during fixed target operation. This was foreseen and the equipment has been designed so that the changeover can be made efficiently in a few weeks, irrespective of direction. Thus, the completion of the two parts of the Upgrade that are the most challenging is on schedule. These systems are the main reason that we expect the luminosity to exceed $5 \times 10^{30}$ $cm^{-2}$ $sec^{-1}$ during the 1991 Collider run.

The Linac Upgrade and the Main Injector do not require the same degree of innovation as do the separator systems and the low beta insertions. The Linac Upgrade was authorized by Congress for construction in 1990 and, at this writing, has received its initial appropriation. The development of side coupled cavities and klystrons is on schedule and the prototypes of these devices have met the project performance requirements. The Accelerator Division expects that the upgraded Linac will be finished by the late summer of 1992. If the rhythm of the annual alternation between colliding beams and fixed target persists

as planned, the new Linac will benefit the fixed target program first. The factor of 1.7 increase in proton intensity will be a very welcome increment for the intensity limited fixed target experiments. This run is also likely to be the first to benefit from the low temperature operation of the magnet system. Since this will increase the fixed target proton energy to 900 GeV, it will help nearly all experiments. These two improvements will allow the fixed target experiments to make significant advances beyond what they expect to accomplish in 1990 and 1991. The low temperature operation of the magnets will also allow the Tevatron to operate at 1000 GeV per beam in the collider mode.

The Main Injector is not yet approved by the Department of Energy. Since the time of the Breckenridge meeting the Department of Energy has reviewed the ten year plan that Fermilab submitted in September. The Main Injector is a critical part of that plan and the Department recognizes the importance that Fermilab places on a rapid construction schedule. The Main Injector will require an addition of nearly $150 million of funds to the Fermilab budget between 1992 and 1995. If the first of these increments arrives in FY1992, then it can be completed by 1995. Within a year of completion the luminosity goal of $5x10^{31}cm^{-2}sec^{-1}$ and the proton intensity goal of greater than $6x10^{13}$ protons/cycle will be achieved. What is important to note is that there are neither uncharted technical wildernesses that have to be crossed, nor unreasonable mountains of funding requests that have to be scaled to reach these goals.

It is worth reflecting on how the experimenters might improve their detectors and how the Laboratory might adapt its way of supporting experiments in order to exploit the improved luminosity and intensity. Since the greatest changes are likely to take place in the fixed target experiments, I will begin with them. The Fermilab fixed target program is remarkably diverse. Sixteen experiments are scheduled to take data during the 1990-1991 run. While there is some overlap of the goals among experiments that are dedicated to measuring the properties of charm and beauty particles, the multiple approach is allowing a more rapid development of detector and data acquisition systems than would occur if there were but one detector. For the present, progress in meeting the technical challenges can be more rapid of the collaborations that are

working on these issues continue their diverse approaches. Two of the experiments scheduled for the 1990-91 fixed target run expect to fully reconstruct $10^5$ charm particles. Each experiment will thus observe twenty times more events than E691, the charm photoproduction experiment that set the standard for this technique. Each detector is a large acceptance multiparticle spectrometer that consists of two magnets, track chambers, Cherenkov counters, electromagnetic calorimeters, and silicon strip microvertex detectors. In addition, each employs a data acquisition system with more computation power than Fermilab had in its Computing Center a few years ago.

The proponents of the heavy quark experiments believe that with more intense beams, more selective event filters, more powerful data acquisition systems, and continued detector improvements, they can increase their samples of charm particles to $10^6$ events in the fixed target run that follows the 1990-1991 run. By that time the specialized skills and the scale of effort required to improve and operate these detectors and analyze the data that is generated will probably demand larger collaborations. The large acceptance spectrometer is not the only approach that will be pursued. During the 1990-1991 fixed target run several other rather different types of detectors will be used to acquire large data samples aimed at the study of charm and beauty particles. These detectors differ significantly in geometry or acceptance, in the filtering or event selection electronics, and in data acquisition systems. The relative degree of success of these approaches will shape the future of the fixed target heavy quark experiments.

The Workshop summary of the Fixed Target Heavy Quark Working Group identified a number of very demanding technical problems that have to be solved to produce the data acquisition systems and build the detectors that are required for the next generation of experiments. A collaboration of more than one hundred physicists will probably be needed to build and use the first detector capable of reconstructing $10^6$ charm events and $10^3$ beauty events. Such a collaboration will be fifty percent larger than todays larger fixed target collaborations, one third the size of CDF or DØ. An organization of this size will require that the Laboratory provide stable support groups, dedicated to building and operating the

detectors. There will have to be a strong focus on detector development specific to these fixed target experiments, and the development will require Fermilab support if it is to be successful. The conduct of fixed target experiments will then begin to resemble the style of CDF and D0. The ultimate physics goals of these experiments are to fully reconstruct samples of $10^7$ charm particles and $10^4$ beauty particles. The technological problems that must be solved if these goals are to be reached are extremely demanding. The physics that can be accessed by these large samples is so compelling that it is very likely that the required number of experimenters will band together and solve the problems. It is already happening. If the experiments are to be done in the next six years, the Laboratory will have to adapt the way it provides resources so that these development problems can be solved expeditiously.

An examination of the scope of an experiment designed to measure CP violation in the decay $K_L^0 \rightarrow \pi^0 e^+ e^-$ at a sensitivity $10^{-13}$ in the branching ratio shows that a large acceptance detector is also required. Moreover, the detector elements must make more accurate measurements of particle energy, direction, and flavor than before, while operating in a very high rate environment. Such a detector will be simpler than a beauty detector, but it will still require stable and significant support from the Laboratory. The same argument holds for the neutrino experiments and structure function experiments that were considered at Breckenridge. One of these experiments was designed to measure $\sin^2\theta_W$ with sufficient precision to challenge the standard model. Another was designed to search for $\nu_\mu$ to $\nu_\tau$ oscillations. While the data rates of the neutrino experiments are considerably more modest than the beauty experiments and the neutral K experiments, they are still large in comparison to what was done in the past. In the past, neutrino experiments enjoyed a greater measure of support than other fixed target experiments because they were, if nothing else, massive. Clearly, the success of these experiments in providing the best measurements of cross sections, structure functions and $\sin^2\theta_W$, is related to the modest extra support that they have received. E691 also was the recipient of significant Laboratory support. The moral to be drawn is clear; good physics requires good support for major detectors. The neutrino experiments of the scale proposed at Breckenridge would

certainly need considerable support from Fermilab if they were to be successful. Structure function experiments of the future will have to be done in a major detector facility that possesses all of the properties of large acceptance multiparticle spectrometers, although the magnetic field requirements are likely to lead to a very different looking device. The data acquisition requirements will be very demanding since data must be taken from the smallest feasible $Q^2$ to the largest if the running of the strong coupling constant is to be observed.

Not all experiments will require such support. For example, some of the experiments designed to look for oscillations over long baselines will only require that a neutrino beam, derived from the protons from the Main Injector, point at an existing neutrino detector some 500 km in the distance. While smaller experiments that require fewer people may be just the right thing to look for axions and other unknown denizens of the particle zoo, they will require far more technically advanced detectors than the experiments of this type that are being done today.

The challenge that the Laboratory must meet in the next five years will be to balance the requirements for resources among the big, medium, and small. At this writing it would seem that a successful strategy would be to gradually reduce the number of fixed target experiments from the present sixteen to a number nearer six or eight. The evolution toward this smaller number of better supported experiments can start after the 1990-1991 run. While it is likely that all of the existing large detectors that will be used in 1990 and 1991 can be used effectively again in 1992, the need for many more people to analyze the data so that it is published before the 1992 run starts suggests that some consolidation should take place before 1992. On the other hand, some of the detectors now under construction will have just come into their own in 1992 and it is reasonable to expect that an additional run will be profitable for these detectors if the data from the 1990-91 run can be analyzed prior to the 1992-1993 run.

Planning and R&D for the next generation of 900 GeV fixed target experiments must begin soon if they are to be constructed in a timely manner, given the R&D lead time necessary for good designs, and given that the Laboratory will likely not be able to dedicate the kind of resources that are required to build the next round of these experiments until after

1993 when the upgrade of CDF and DØ is complete. We expect to begin their construction on this time scale. What is the competition at that time? By 1995, UNK should be in operation at 3 TeV. Beauty experiments done at UNK should be vastly superior to experiments now in progress at Fermilab if for no other reason than the factor of three in energy. A charm experiment could still be competitive at Fermilab if a larger acceptance multiparticle detector spectrometer were built or if an advantage in technology could be found and maintained. Specialized detectors looking for rare charm decays that exhibit lepton number violation or flavor changing neutral currents might also be competitive. One should expect some surprises in the current round of heavy quark fixed target experiments and their extensions. These surprises will be followed up in a timely way after the completion of the Main Injector if strong groups of dedicated people are willing to search for the sources of those surprises.

The experiments that can be done with the intense flux of neutral K's and neutrinos that can be produced by the Main Injector are only beginning to be considered. The questions of whether neutrinos of one generation can evolve into neutrinos of another generation are best done with the Main Injector. On the other hand, an experiment looking for the interaction of a tau neutrino in matter is done better at the Tevatron. Should a bent crystal channel prove to be a feasible method of precessing the spin of a charmed baryon, the magnetic moments of weakly stable charmed baryons might become accessible. Letters of intent have already been received for neutral K decay detectors, neutrino oscillation detectors, and a precise measurement of the nucleon structure function by muons. Each of these detectors, exclusive of the beam and buildings, is likely to cost between $15 and $20 million, roughly three times the cost of today's large detectors. It is also reasonable to expect that the construction of at least one new detector would begin in 1993 as mentioned above. At the present time the neutral K and neutrino oscillation experiments seem like the experiments that can best exploit the Main Injector intensity, since energy is not as important a criterion as intensity.

The Collider program, by virtue of the size and complexity of the detectors, requires large stable support groups to achieve its goals. The most important immediate goal of that program and the Laboratory is to

discover and study the top. Upper limits of roughly 250 GeV almost insure, with the main injector luminosity, the discovery of the top. The path to that goal is obvious because the Tevatron Collider is the only machine in operation that can produce a 100-250 GeV top quark. The D0 detector will be finished in 1991, used for the first time in the 1991 run, and then improved to handle the ever increasing luminosity. The CDF detector will also be upgraded to handle the succession of luminosity increases. Its upgrade plan was brought clearly into focus at Breckenridge. By the end of 1993 the upgrades of CDF and D0 will be largely complete. They will certainly be capable of handling forty bunches of protons and forty bunches of antiprotons by that time. If the top is lighter than 150 GeV/$c^2$ it will be discovered in the data taken during the 1991 run. If the top hasn't been discovered by the end of 1992, and if it is less massive than 180 GeV/$c^2$, then it will be found in the data taken by CDF and D0 in 1993. By 1995 both CDF and D0 will be fully capable of handling $10^{32}cm^{-2}sec^{-1}$. Once the Main Injector has been commissioned and made its contribution to the luminosity, the top will be seen if it hasn't been seen already and if it is a "standard" object. If it is seen, the goal will then change to producing and detecting enough top quarks to find out whether there is anything special about them. If it is not seen, then the standard model is incorrect and a systematic study is also called for. In addition, if there is a gauge boson that has a mass of less than 500 GeV and decays into leptons it will be discovered at Fermilab. The mass region between 100 GeV and 500 GeV will be Fermilab's private hunting ground until the late 1990's when one of the SSC turns on.

CDF has been very successful. It took data in 1988 and 1989 and within a few months of the end of its run it had both presented the results of some of the most important physics of that run, and was simultaneously embarked on the upgrade of its detector for the 1991 run. CDF was well supported by Fermilab standards during detector construction and operation. Its success is directly related to the support that the Laboratory and the collaboration was able to bring to CDF. The construction of the D0 detector is now moving rapidly. When its commissioning is completed in 1991, it will add a great deal to the Collider program. There is little doubt in my mind that there should be two excellent and complementary

detectors looking at the physics of the 100 GeV to 500 GeV mass scale over the next decade. Once that premise is accepted it follows that the support for CDF and DØ must be maintained at near its present level. In view of this, a competitive and forefront fixed target program must contain fewer experiments if they also are to receive the necessary level of support. Indeed, the model for the required level of support can be derived from that given to the Collider program.

After the year 2000 the high energy frontier will shift from Batavia to Waxahachie. Somewhat earlier in time, the physicists who want to pursue that frontier will shift their attention from CDF and D0 to detectors for the SSC. Fermilab will be a participant in that effort. Already the work done at Breckenridge has left its mark on several potential SSC detectors. A Fermilab group has been joined to participate with many other groups in the preparation of a letter of intent for an SSC detector. It is too early to tell what the scale of that effort will be at Fermilab although it will be substantial.

While some will find the open energy frontier of Waxahachie more inviting, there will still be important collider physics to be pursued at Fermilab in the late 1990's and in the first decade of the twenty-first century. At Breckenridge two paths were explored. One path lead to the construction of a new detector suitable for detecting B hadrons and the other to the construction of a new Collider, LINCOLN.

If LINCOLN existed today it would be a superb tool to search for the top quark, new gauge bosons, and supersymmetric particles. As magnificent as the machine could be, it seems premature for Fermilab to pick this path. The flat budget predicted for high energy physics, exclusive of the SSC, offers no prospect for a billion-dollar machine. It seems unlikely that one could obtain that amount of money for a new project until the SSC construction nears completion. By the year 2000 it is likely that there will be at least two hadron colliders, LHC and SSC, that can significantly exceed the energy and luminosity of the LINCOLN proposal. Energy has been the old reliable panacea when it appeared that progress in high energy physics was stalled. For the moment Fermilab has got that elixir bottled; in a decade the folks in Texas will get to drink stronger stuff. However, many crucial discoveries were made at machines when their

days as the highest energy machine in the land were over. If the past is a reliable guide for the future, then there is great deal to be learned from the Tevatron. With this knowledge in hand, one can contemplate a new accelerator. In the meantime, the accelerator builders at Fermilab will join others around the world in advanced accelerator R&D. From that R&D and the discoveries of the Tevatron will come the course for the twenty-first century.

The second path does not focus on the most massive particles but the bottom quark, the quark that was discovered at Fermilab in 1977. The cross section for b production at the Tevatron is about 20 μbarns. This provides almost as good a signal to background ratio as is provided in charm production experiments done in fixed target beams. The invitation to use that large cross section should not go unanswered. The technical challenge that must be met to use that cross section is challenging. The raw event rate will be about $10^7$ events/second. It is estimated that the on-line data acquisition system must have the computing power of $10^6$ MIPS to handle the rate and filter out the events of interest. The detection of beauty in hadron collisions will require the clean separation of the decay vertex from the production vertex. Present vertex detectors are an awkward match to the Tevatron's extended interaction region. They are also probably too massive. Further development is sorely needed. Particle identification is desirable. The beam properties at the interaction point will have to be tailored to those detectors. Over the next four years there needs to be an R&D program specifically focussed on the Tevatron Collider that will address each problem. A beginning of that R&D program is already in place. A microvertex detector will be used by CDF during the 1991-92 Collider run. The progress that CDF makes in separating B decays from the background will be crucial in determining whether B's can be detected at the Collider. Some of these problems are being addressed with fixed target heavy quark experiments and the experience gained there will also be valuable. It would seem natural that some of the groups working on fixed target heavy quark experiments would continue their efforts into a heavy quark experiment in the Tevatron Collider. The work of CDF, BCD, and the fixed target groups over the next few years should point the way to a collider detector that competes with detectors operating at high

luminosity $e^+e^-$ machines. The standards that a collider detector must exceed if it is to match the expectations of CLEO or a high luminosity LEP are very demanding. CLEO should accumulate $10^6$ B decays and a high luminosity LEP operating at the $Z^0$ can produce $10^6$ B's per year per interaction region. Future $e^+e^-$ colliders hope to produce 10 to 100 times more B's.

It would be wonderful if the twentieth anniversary of the discovery of the Upsilon could be celebrated simultaneously with the measurement of the top mass and with the start of operations of a detector that will observe CP violation in hadrons containing beauty. These accomplishments, as great as they seem today, might not be the best results that will come from the Tevatron. Ten years is too long a time to be devoid of experimental shocks to our theoretical friends. There is a great deal to do at Fermilab in the next ten years.