A Vision for High Energy Physics

Teruki Kamon
Jorge Lopez
Peter McIntyre
James White

Texas A&M University
Department of Physics
College Station, TX 77832–4242

Abstract

Following the termination of the Superconducting Super Collider, there is an urgent need to develop a strategic plan for the future of high energy physics and an accompanying vision to guide the priorities of the U.S. program. This document proposes such a strategic plan and presents a singular opportunity for the U.S. program. The existing hadron collider at Fermilab could be upgraded to create a major discovery potential for supersymmetry, one of the most profound concepts in the world of elementary particles. Using a single ring of SSC magnets in the existing tunnel, the recently improved understanding of SUSY phenomenology, and the upgraded detectors in place at the Tevatron, the DiTevatron could be doing physics within five years and reach most of the range of parameters permitted in SUSY models. We propose that it be funded as a worthy component of the SSC termination, bringing to fruition both the technology and the science of the SSC at very modest cost.

With the advent of the Superconducting Super Collider, the U.S. high energy physics community embarked on a quest which would have taken a decade to begin operation, and aspired to leap more than an order of magnitude in all measures of performance beyond anything that exists today. Its charter mission was to search for the Higgs boson - the carrier of a scalar field which is thought to be the origin of mass for the gauge bosons of the electroweak interactions. With the untimely termination of the project, our community faces one of the biggest turning points in its history. There is good news and there is bad news. To the good, the search for the elusive top quark is reaching a climax, with the focus of attention at Fermilab. To the bad, the aggregate U.S. funding of high energy research, \( \sim \$650 \text{ million/year} \), is likely to be cut by 4\% next year and prospects are little better in future years.
Can we develop a realistic strategic plan which has the potential for milestone discovery within the next ~6 years? If our field of science is to prosper, indeed if it is even to survive, we must develop a vision of the future which can earn the public support which we have come to expect. At the present budget level, the U.S. would fund nearly $4 billion to high energy research in that time. Ironically, that is the original cost estimate for the SSC itself. The American people, their government, and our colleagues in other fields of science expect that we develop a strategic plan which has reasonable prospect of yielding new discoveries about nature which are commensurate with this level of support. If we fail to develop such a plan, or if we fail to communicate it effectively, we will likely receive less support in each succeeding year.

**Historical context.** In order to set a context for milestone discovery, we can examine the history of our field. Table I shows a chronology of such discoveries in elementary particle physics. Each of these discoveries has transformed the way we understand the fundamental particles and interactions of nature. The field began with the discoveries of natural radioactivity in 1896 and the electron in 1897, Rutherford scattering from the nucleus in 1911, and the neutron in 1932. After World War II a steady stream of milestone discoveries, averaging one every five years, has brought the field to its current understanding of the Standard Model. Most of these milestones have been recognized by a Nobel Prize.

The latest such milestone may be just around the corner: the top quark. The top quark is the “missing link” in the bestiary of fermions in the Standard Model. From the measurement of the $Z^0$ boson decay width at LEP (one of the latest milestone discoveries), we believe the top quark to be the last Standard Model fermion to be discovered. The search for top is the primary focus of effort in the CDF and D0 experiments at Fermilab, and both experiments will be announcing new results this Spring. The efforts of both collaboration are distinctly international in character. CDF includes groups from Canada, Italy, Japan, and the US; D0 is a collaboration including Brazil, Colombia, France, India, Mexico, Russia, and the US.

As we look to the future, it is of course pointless to resort to the oxymoron of predicting discoveries. We will however be judged in the context of the revolutionary advances we have made in the past forty years, and of the enormous cost of our future research. We must therefore look with clear vision at the potential for milestone discovery which attends the several elements of the world high energy research program, and target the U.S. program to maximize that potential, recognizing the constraints of funding at home and the legitimate ambitions of our colleagues in Europe and Japan.

**The potential for future milestone discoveries.** The task of assessing the potential for milestone discoveries is guided by our concepts for the major unsolved puzzles of the time, and the concepts which might solve them or provide fundamental insights. Three of the most important examples of such concepts today are the Higgs field, which would explain the mystery of mass in gauge theories; the origin of
CP violation, a puzzling dimension of the gauge fields which may explain the imbalance of matter and antimatter in cosmology; and supersymmetry, a new fundamental symmetry which treats fermions and bosons in a unified way, and actually relates the internal symmetries to space-time itself. Our vision of discovery potential for each of these concepts is however clouded by our uncertainty about the masses of the carriers of the new fields and their couplings to the particles we know.

The pursuit of the Standard Model Higgs boson is the central goal of the most ambitious projects that each sector of the world high energy physics community can muster. The parameters of energy (40 TeV) and luminosity ($10^{33}$ cm$^{-2}$ s$^{-1}$) for the SSC were chosen to cover 100% of the parameter space within which our concept of the Higgs could be valid. The supersymmetric Higgs is the goal of LEP II, the energy upgrade of CERN’s $e^+e^-$ collider. The LHC collider being planned at CERN similarly targets the Higgs: its parameters (14 TeV energy and $2.5 \times 10^{34}$ cm$^{-2}$ s$^{-1}$) are bounded by the existing LEP tunnel, but can provide $\sim 70\%$ reach over the Higgs’ parameter space.

For CP violation, it will be necessary to precisely measure three amplitudes (the so-called unitarity triangle) in the Kobayashi-Maskawa matrix which describes the weak couplings between the quark families. The B factories now being designed at KEK and SLAC could measure two of the three amplitudes. With the planned Injector upgrade, the Tevatron experiments could contribute the third amplitude ($b \to s$) through their studies of $B_s$ decays. Such a coordinated effort has the long-term potential to explore CP violation where it may be strongest. There will also soon be an opportunity to extend the study of CP violation in the strange quark sector at the $\phi$ factory which is being built at INFN Frascati.

Supersymmetry (SUSY) was originally proposed two decades ago in a rather abstract context. It would couple the two classes of fundamental particles, the fermions and bosons, whose utterly distinct characters have been one of the deepest puzzles of physics. Supersymmetry has shown up again and again in many places where it wasn’t expected: the solution to the hierarchy problem and the unification of the gauge couplings in grand unified theories, the radiative breaking of electroweak symmetry, the unification with gravity (supergravity), and in superstrings. Moreover, supersymmetric models contain a natural candidate for the dark matter puzzle in astrophysics: the lightest supersymmetric particle (LSP). Its potential discovery and its impact on our understanding of nature would be as fundamental as the gauge theories themselves. This significance is in contrast with the many extensions of the Standard Model that have been suggested, which do no more than extend its gauge group and matter content.

The opportunity for supersymmetry. During the past year a remarkable opportunity to seek supersymmetry has arisen, even while the top is being chased, the SSC was being terminated, and the European and Japanese high energy communities are preparing their plans for massive colliders to chase the Higgs. Recent developments in accelerator technology, phenomenology, and detector performance have converged
to suggest a singular opportunity to extend the search for supersymmetry to cover ∼80% of its parameter space. The opportunity is one which could be mounted at Fermilab, using a modest upgrade of its present facilities, and commencing physics operation within 5 years.

The accelerator technology is the SSC magnet. A single ring of SSC magnets, installed in the Tevatron tunnel, could produce $\bar{p}p$ colliding beams with 4 TeV collision energy and $2 \times 10^{32} \text{cm}^{-2} \text{s}^{-1}$ luminosity - the DiTevatron or SUSYTRON. By operating a ring of SSC magnets at 3.5°K (feasible with the present Tevatron cryogenic system), a collision energy of 3.5 TeV would be produced. Upgrade of the cryogenic plant would enable refrigeration to 1.8°K, and extend operation to 4 TeV collision energy. As we will show below, this relatively modest energy upgrade would extend the reach of Fermilab for supersymmetry from ∼30% of the parameter space with the injector-upgraded Tevatron to ∼80% coverage with the DiTevatron.

The DiTevatron could utilize the Tevatron for injection at 900 GeV. Its magnets would then require only a 2:1 dynamic range of operation, eliminating many of the more challenging problems of magnet performance required for the 10:1 dynamic range in SSC or LHC. The 5 cm aperture of the SSC magnets would be fully adequate in this case, including provision for the corkscrew trajectory of the two separated counter-rotating beams. The magnet length would be chosen to match the Tevatron lattice and sagitta.

Figure 1 shows the arrangement of the DiTevatron and the Tevatron in the Fermilab tunnel. The beam height of the collision point would remain the same - the detectors would not have to move. The Tevatron beam would also pass through the detectors, a minor inconvenience, but it would carry beam only while the collider was being filled with beams before each store.

The SSC magnets are industrialized, and a magnet factory capable of mass producing them stands ready and idle in Louisiana. The production tooling and test facilities could be modified easily for the magnet length appropriate for DiTevatron. The factory could be placed in operation within a year, and could produce the required 6 km of magnets in a year of production. This industrialization and its use to propel U.S. high energy research into a potential for milestone discovery would be fitting and achievable legacies for the brave vision of the Supercollider. We propose that the funding for the DiTevatron be included in the SSC termination; we can thereby transform the failure to build the SSC into the successful use of its technology to create an equally important discovery potential.

The phenomenology of supersymmetry has benefited from the steady improvement in the bounds on the top quark mass, the improving measurements of a host of processes which bound the allowable range of parameters for the masses and couplings of supersymmetric particles, and the selection of theoretically well-motivated super-

---

1 P. Wanderer, “Status of Superconducting Magnet Development,” 1993 Particle Accelerator Conference, Washington (1993), p.2726.
symmetric models. Building upon this expertise, we are conducting a systematic analysis of the signals which are being used today to seek supersymmetry at CDF and D0, and the cross-section $\sigma$ and branching ratio $B$ for them at the DiTevatron. Two processes have been considered: the production of a pair of the super-partners of the weak bosons (Chargino $\chi^{\pm}_1$, Neutralino $\chi^0_2$), resulting in a characteristic trilepton decay signature; and the production of a pair of gluinos and squarks ($\tilde{g}\tilde{g}$, $\tilde{q}\tilde{q}$, $\tilde{g}\tilde{q}$), resulting in an equally characteristic signature of multiple jets and missing momentum.

Figure 2 shows an example of a trilepton event observed in CDF, although this particular event has been rejected in the SUSY analysis because its third lepton escapes into the end plug of the detector. Figure 3 shows another trilepton event, observed in D0 which however passes all the cuts. The following figures show the yield $\sigma B$ for the trilepton signature for three supergravity models which bound the range of theoretical framework: standard SU(5) (Figure 4), and no-scale string-inspired SU(5)xU(1) models with moduli (Figure 5) and dilaton (Figure 6) supersymmetry breaking at the unification scale. In each case, the dotted patterns show the extent of the parameter space, while the dashed line shows the discovery limit (95% CL) for the trilepton mode, extrapolating the actual experience in this search at CDF to date (present limit indicated by a solid line), and assuming a 3-year data run ($5 fb^{-1}$). In each case, the Tevatron with Injector Upgrade covers $\sim 30\%$ of the parameter space; the DiTevatron covers virtually the entire parameter space. By comparison, LEP II should be able to observe chargino masses only up to 100 GeV.

In order to determine the reach of the DiTevatron for squarks and gluinos, we studied two typical cases: (i) gluino pair production with significantly heavier squarks, as expected in the standard SU(5) model, and (ii) all gluino squark production channels such that $m_{\tilde{g}} \approx m_{\tilde{q}}$, as expected in the SU(5)xU(1) models. Figure 7 shows the missing $p_\perp$ distribution calculated for gluino pairs (case (i)), and the distribution of events from the dominant background: $Z \rightarrow \nu\bar{\nu}$. Simple selection criteria were applied (missing $p_\perp > 150$ GeV, 4 jets with $p_\perp > 40$ GeV) yielding a statistical significance $S/\sqrt{B} = 52(13)$ for a 400(500) GeV gluino mass. The above signal is only for the gluino; if the squark has a comparable mass (case (ii)), we would see $\sim 7$ times more signal. An estimate of the reach for these two cases is shown in Figure 8 in terms of the statistical significance $S/\sqrt{B}$. The figure shows that the reach exceeds 500 GeV for gluino pairs (case (i)) and $\sim 700$ GeV for all gluino-squark combinations (case (ii)). With optimized selection cuts, the reach could be larger. This covers all but a small tail of parameter space in the standard SU(5) model, and $\sim 80\%$ of the parameter space in the SU(5)xU(1) models. The analogous reach for the Tevatron

---

2 For a recent review, see J.L. Lopez, D.V. Nanopoulos, and A. Zichichi, “Status of the Superworld: From Theory to Experiment,” CERN preprint CERN-TH.7136/94.

3 CDF Collaboration. “SUSY Search Using Trilepton Events from $p\bar{p}$ Collisions at $\sqrt{s} = 1.8$ TeV,” XVI International Symposium on Lepton-Photon Interactions, Cornell University, Ithaca, NY, 10-15 August 1993.

4 J. T. White (D0 Collaboration), to appear Proc. of 9th Topical Workshop on Proton-antiproton Collider Physics, Tsukuba, Japan, 18-22 October 1993.
with Injector Upgrade is $\sim 250$ GeV.

The third development is the coming of age of the detectors CDF\[^5\] and D0\[^6\]. The collaborations have reached a thorough understanding of their detectors, and a detailed correspondence of the observed signals for processes of interest with the simulated response from a model of the physics and the detector. This enables both teams to extrapolate the detector performance to higher energy and luminosity, and to analyze the upgrades which would be needed to reach specific signals for new phenomena.

Both collaborations today are preparing Expressions of Interest for a next generation of physics in the detectors, beginning when the Injector Upgrade will bring about an order of magnitude increase in luminosity for the Tevatron (to $10^{32}$ cm$^{-2}$ s$^{-1}$).

The likely conclusion of these EOI’s is that both detectors would be fully sensitive to the above SUSY signals. Operation at the higher luminosity will require faster data acquisition electronics and substantial upgrade of the tracking systems in CDF, and the addition of a magnet and tracking in D0. These upgrades are already envisioned and some are in progress. Otherwise both detectors are ready as they now operate to seek SUSY at the DiTevatron.

The message is then clear. **Over the entire range of SUSY model parameters, the DiTevatron could provide $\sim 80\%$ sensitivity for all of the major sparticles in the SUSY spectrum.** The DiTevatron is for SUSY what the SSC was to be for the Higgs.

Uniqueness of discovery potential. A critical issue in searching for the signals for supersymmetry is the struggle to optimize signal over background. One important source of backgrounds which has already been encountered at CDF is the superposition of several distinct interactions in a single bunch-bunch crossing. Confusion of jets and leptons from distinct interactions can produce spurious “signals” for new physics if many interactions are superposed. On average there are $\sim 2$ interactions per crossing in the Tevatron at its current luminosity. We have learned to cope with that level of multi-interactions in the searches for top and for SUSY. As the Tevatron luminosity is increased, the bunch spacing will be reduced, so that at the ultimate luminosity of the DiTevatron there would still be only 3 interactions per bunch crossing.

By contrast, LHC will operate with a luminosity of $2.5 \times 10^{34}$ cm$^{-2}$ s$^{-1}$ and a bunch spacing of 25 ns, corresponding to 60 interactions per bunch crossing! The push for ultimate luminosity is essential if CERN is to maximize its reach for the Higgs boson. In the “golden” signal for the Higgs (4 leptons from 2 $Z$ decays), the additional pair-mass constraints may make it feasible to reject combinatoric backgrounds. For supersymmetry, however, we have no such constraints. Hence the irony: although LHC will have ample energy and luminosity to make plenty of SUSY particles, it could be extremely difficult to find them. The DiTevatron is a superior machine for

\[^5\] CDF Collaboration, *“The Collider Detector at Fermilab,”* North-Holland, 1988.

\[^6\] D0 Collaboration, S. Abachi et al., to be published in Nucl. Instrum. Methods A (1994).
the purpose. The collider and its detectors could be ready to operate for physics in five years.

**The DiTevatron would also be a top factory.** Anticipating that the top quark will be discovered during the next run at Fermilab, the production cross-section would be 20 times larger at twice the collision energy. Taking this increase together with the 20-fold increase of luminosity afforded by the Injector Upgrade, the CDF and D0 experiments at the DiTevatron would each be able to identify and reconstruct \( \sim 4,000 \) top quarks/year. Such a data set would make it possible to make detailed studies of the last quark: its mass, its decay modes, the electroweak interaction with a quark which is heavier than the gauge bosons themselves.

**A World Vision.** We conceive a world vision for cooperation in high energy physics, which recognizes the legitimate aspirations of each continent’s high energy community, while working together to best use of resources.

The DiTevatron has a unique potential for a milestone discovery of historic significance - supersymmetry. It could be built in five years, as a final, constructive chapter in the termination of the SSC. Its capital cost has been roughly estimated by Fermilab at \( \sim $250 \) million. In the family of $billion facilities planned in Europe and Japan, it could prove to be an excellent bargain for Yankee ingenuity, in the same way that the Tevatron itself bootstrapped a 400 GeV accelerator into a 2 TeV collider which is today the sole source of top quarks.

Europe plans to build LHC. It has excellent discovery potential for the Higgs boson, and could be complete in a decade. There should be opportunity for collaboration by universities and laboratories world-wide in the technology development and physics program of the LHC, driven by the project’s needs and the scientists’ interests.

Both Japan and SLAC plan to build \( e^+e^-B \) factories. These facilities will complement the DiTevatron to measure the parameters of CP violation. Together with the \( \varphi \) factory at INFN Frascati, the results may lead us to an understanding of the origins of CP violation.

The fixed-target programs at Fermilab and CERN have for 25 years been a stalwart component of the HEP scene. With the advent of the Tevatron upgrade, new experiments with rare \( K \) decays and long-baseline neutrino oscillations will dramatically extend earlier studies. Effects from all three of the above milestone-discovery interactions could be tested in these two experiments.

Japan has just announced plans to build an \( e^+e^- \) linac collider, beginning in 1996. The first stage, 150 GeV/beam, could well prove to be an excellent facility for producing top quarks as well as probing for an intermediate mass range of the Higgs boson. The ultimate stage, \( \sim 500 \) GeV/beam, would support a detailed study of SUSY Higgs physics, and of the superpartners of the leptons. The latest suggestions of converting the particle beams into photon beams just before collision may pose an even more elegant physics channel. In any case, the linac collider will be a *tour de*
force of accelerator technology, requiring a sustained international effort to prepare for it. There are today a number of international teams working to mature its technology: SLAC, CLIC, TESLA, and INP Novosibirsk. It is to be hoped that KEK will pull together an effective teamwork of these talents to mature the best possible design and its component technologies.

Technology - the art of the possible. As we begin to build accelerators and detectors for the next generation of high energy physics experiments, we should realize that we are relying on the fruits of a long, sustained effort of technology development. The innovations of beam cooling, high-\(j_c\) superconducting strand, 8 Tesla dipoles, nano-focus beams, suppression of wake fields, high-power microwave drivers, AC-coupled silicon microstrip and pixel detectors, ring-imaging Cerenkov counters, scintillating fibers, and gas microstrip chambers have required years of effort and patient support. As we begin to use these technologies, it is equally important that we do a more effective job of supporting the further innovation of new technologies that can provide an equally rich armada when it comes time a decade hence to ask again - what should we do next? At each such juncture, our vision is bounded by what we know to be possible; technology is the means of pushing back those bounds.

One example is of particular relevance to future hadron colliders. Two years ago, one of us (PM) co-invented a wholly new approach to high-field dipole design - the pipe magnet\(^7\) shown in Figure \(#9\). It is a 13 Tesla dual dipole, occupying the same space as one SSC dipole. The magnet is configured essentially as a deformed toroid with two dipole insertions. We used a conformal mapping to design a flux transformer which produces collider-quality field in each insertion. The magnetic flux is guided everywhere around its circuit, reducing peak stress in the coil by a factor 2 and eliminating persistent-current multipoles - two of the most serious limits to \(\cos \theta\) magnet performance. The pipe magnet also incorporates innovations in \(\text{Nb}_3\text{Sn}\) coil fabrication, prestress distribution, and cooling which we have learned from experience with other field geometries.

The best experts in magnet design and fabrication at Fermilab and CERN have expressed enthusiasm for the concept. Nevertheless it is only a concept. It would take \(\sim\) 5 years of intense effort by a seasoned team to build and test model magnets, and strive to mature it to readiness as a collider magnet. We have such a team and six months ago we requested funding from DOE’s Advanced Accelerator R&D Program for this purpose. The answer: there is no money in all of DOE HEP for any new programs.

If we take such an attitude to the first new technology for superconducting dipoles in twenty years, and to similar new technologies for accelerators and detectors, we will have no future as a field!

\(^7\) E. Badea, P.M. McIntyre, and S. Pissanetzky, “The Pipe Magnet: Compact 13 Tesla Dual Dipole for Future Hadron Colliders,” Proc. HEACC ’92 Conference, Hamburg (1992).
The role of the universities. As the accelerators and detectors required for our research grow ever larger and more complex, there is a natural tendency to consolidate the design, construction, operation, analysis, and supporting technology R&D at major national laboratories, where the tools supporting technical personnel and facilities can assure a competent, well-managed effort.

Beware! Look back at Table I, and consider the people who made those milestone discoveries. With very few exceptions, they were faculty at universities. The universities of America are one of our greatest assets as a nation. The high energy physicists on their faculties are the driving force of creativity and sustained effort for the research which our high energy facilities are being conceived to support. They are also the originators of most of the new concepts for accelerator and detector technologies that extend the possible. Yet over the past decade there has been a steady erosion of the support of these scientists within the U.S. high energy program. If we continue such a shift in our priorities, we will have no future as a field!

The future is bright. The SSC is dead. We grieve for its passing. We hope to have demonstrated a vision for high energy physics in which the future can be bright. We are at the threshold of a milestone discovery (the top quark), and we have the realizable potential within our austere budget to reach for another one - supersymmetry. The DiTevatron could be operating for research within five years. Its construction is compatible with the current effort to discover the top quark - the next milestone discovery. It fits well into a world view in which Europe builds an LHC, Japan builds a linac collider, and international collaboration flourishes for technology development and physics experiments. We could accomplish this agenda within our likely budget constraints, but only if we develop a better ability to balance our priorities. The waning support of technology R&D and of our universities would jeopardize the field’s future were it to continue.
| Year | Discovery | Source | Anticipated Theory |
|------|-----------|--------|-------------------|
| 1896 | $\alpha, \beta, \gamma$ | Radioactive Source | |
| 1897 | $e^-, p$ | Cathode Ray Tube, H$^+$ | |
| 1911 | Rutherford scattering | $\alpha + Au$ (gold foil) | Rutherford’s atom |
| 1932 | $n$ | $\alpha + ^9\text{Be} \rightarrow n + ^{12}\text{C}$ | |
| 1933 | $e^+$ | Cosmic Ray | Dirac Theory (1928) |
| 1937 | $\mu^\pm$ | Cosmic Ray | |
| 1947 | $\pi^\pm$ | Cosmic Ray | Yukawa Theory (1935) |
| 1947 | $K^{\pm, 0}$ | Cosmic Ray | |
| 1950 | $\pi^0$ | Cosmic Ray | |
| $\sim 1953$ | Hyperons | Cosmic Ray | Strangeness |
| (1959) | $\Lambda^0, \Sigma^+, \Xi^-$ | $1.4\text{-GeV/c } \pi^-$ by PS (BNL) | Gell-Mann (1953), Nishijima (1955) |
| 1955 | $\bar{p}$ | Bevatron (6.3 GeV/c $p$, LBL) | Dirac Theory (1928) |
| 1959 | $\nu_e$ | Reactor ($\beta$ decay) | Pauli’s invention of $\nu$ (1930) |
| 1962 | $\nu_e \neq \nu_\mu$ | AGS (BNL) | No events for $\mu^- \rightarrow e^- \nu \bar{\nu} \rightarrow e^- \gamma$ |
| 1964 | $\Omega^-$ | AGS (BNL) | SU(3) - Eightfold Way |
| 1973 | Neutral Current | SPS (400 GeV $p$, CERN) | Gell-Mann (1961), Ne’eman (1961) |
| 1974 | $J/\psi$ | SPEAR ($e^+e^-$, SLAC) | Charm Quark |
| 1975 | $\tau$ | SPEAR ($e^+e^-$, SLAC) | $R_{\exp} - R_{\text{theory}} \sim 1; \sigma = \sigma_{\mu}\sigma_{\mu}$ |
| 1977 | $\Upsilon$ | PS (400 GeV $p$, FNAL) | Kobayashi-Maskawa Theory (1972) (CP Violation) |
| 1979 | $g$ | PETRA (31 GeV $e^+e^-$, DESY) | QCD ($SU(3)_c$) |
| 1983 | $W^\pm, Z^0$ | Sp$ar{p}$S (540 GeV $p\bar{p}$, CERN) | $SU(2) \times U(1)$ |
| 1989 | $N_g = 3$ | LEP (91 GeV $e^+e^-$, CERN) | GUTs |
| 1994? | $top$ | Tevatron (1.8 TeV $p\bar{p}$, FNAL) | Kobayashi-Maskawa Theory (1972) |
| 1999? | SUSY | SUSYtron (4 TeV $p\bar{p}$, FNAL) | SUSY + GUT |
| 2005? | Higgs | LHC (14 TeV $pp$, CERN) | Standard Model |
|       | New Particles | | Beyond the Standard Model |

Notes: SC = Synchrocyclotron, PS = Proton Synchrotron
Figure 1: Cross-section of the Fermilab tunnel, showing the DiTevatron (bottom), the Tevatron (middle), and the transport line (top) in A sector for transfer of beams from the 150 GeV Injector. The beam height for collisions is the same as for the present Tevatron.
Figure 2: End projection of a trilepton event in the CDF experiment. The event contains two CMU+CMP muon candidates, plus one CMI candidate. The muons are visible as straight tracks on the right ($\mu^+$, $p_\perp = 35\text{ GeV}$), the lower left ($\mu^-$, $p_\perp = 10.5\text{ GeV}$), and a short track in the upper left which exits into the end plug ($\mu^-$, $p_\perp = 49\text{ GeV}$ imperfectly measured).
Figure 3: Side views of $ee\mu$ trilepton candidate from the D0 experiment. The event contains a muon at $\eta = 1.1$ and measured $p_{\perp} = 17.2$ GeV/c and two electron candidates (seen in the end cap) with $\eta$'s of 2.0 and 1.9 and $p_{\perp}$'s of 37.8 and 8.0 GeV/c respectively. The missing $p_{\perp}$ is $39.1 \pm 2.7$ GeV/c without, and $23.3 \pm 11.6$ GeV/c with the muon included.
Figure 4: Trilepton yield ($\sigma B$) versus chargino mass in chargino production. The dots define the range of parameters allowed within the standard SU(5) supergravity model. Results are shown for each sign of the Higgs mixing parameter $\mu$. The upper plots show the limits which could be reached in the Injector-upgraded Tevatron; the lower plots show the limits which could be reached in the DiTevatron.
Figure 5: Trilepton yield \((\sigma B)\) versus chargino mass in chargino production. The dots define the range of parameters allowed within a no-scale SU(5)xU(1) supergravity model using moduli supersymmetry-breaking at the GUT scale. Results are shown for each sign of the Higgs mixing parameter \(\mu\). The upper plots show the limits which could be reached in the Injector-upgraded Tevatron; the lower plots show the limits which could be reached in the DiTevatron.
Figure 6: Trilepton yield ($\sigma B$) versus chargino mass in chargino production. The dots define the range of parameters allowed within a no-scale SU(5)xU(1) supergravity model using dilaton supersymmetry-breaking at the GUT scale. Results are shown for each sign of the Higgs mixing parameter $\mu$. The upper plots show the limits which could be reached in the Injector-upgraded Tevatron; the lower plots show the limits which could be reached in the DiTevatron.
Figure 7: Spectrum of missing $p_{\perp}$ for events containing 4 jets (each with $p_{\perp} > 40$ GeV). Spectra are shown for signal events from 400 GeV gluinos (dashed curve), 500 GeV gluinos (dotted curve), and background from $Z \rightarrow \nu \bar{\nu}$. 
Figure 8: Statistical significance for gluino and squark events selected by the criteria $p_\perp > 150\,\text{GeV}$, and 4 jets with $p_\perp > 40\,\text{GeV}$. Bands are shown for signal $S$ from gluino pairs and squark/gluino combinations for parameters which are consistent with the standard SU(5) supergravity model and the SU(5)xU(1) supergravity models, respectively. The background $B$ is calculated from $Z \rightarrow \nu\bar{\nu}$; the bands provide for a factor of 5 deterioration of $S/B$ ratio due to additional backgrounds or inefficiencies.
Figure 9: Configuration of the pipe magnet in a cryostat appropriate for hadron collider applications. The magnet would produce a 13 Tesla dipole field in each beam tube, with multipoles suitable for collider operation. An intercept tube for synchrotron light is shown.
This figure "fig1-1.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1
This figure "fig2-1.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1
This figure "fig3-1.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1
This figure "fig2-2.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1
This figure "fig3-2.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1
This figure "fig2-3.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1
This figure "fig3-3.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1
This figure "fig2-4.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1
This figure "fig3-4.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1
This figure "fig3-5.png" is available in "png" format from:

http://arxiv.org/ps/hep-ph/9402349v1