The Future of High Energy Physics as seen from Fermilab

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Discuss the 20-year Plan for Worldwide HEP

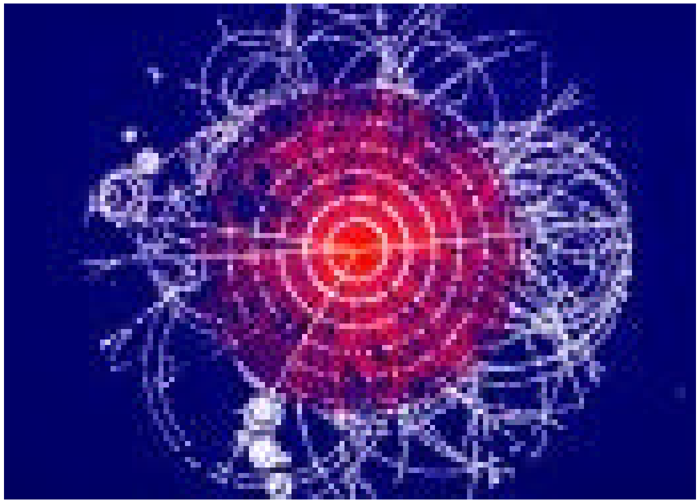
? A consensus exists within the HEP community on
  - the most important physics issues
  - the accelerators and experiments needed to address them.

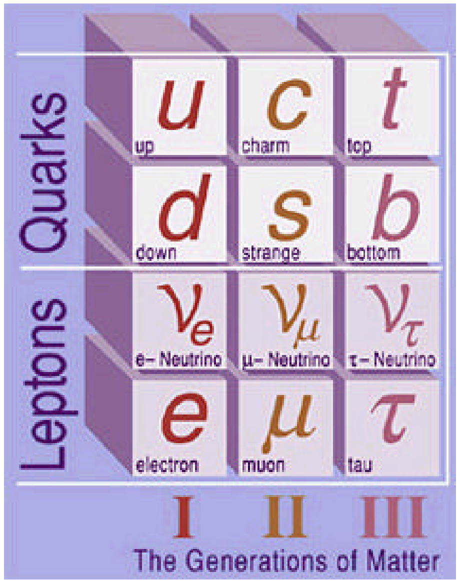
? It is important to outline a 20-year plan that reflects this consensus
  - Science drives the program.
  - Overlap with Asian and European plans adds strength.

? This plan should form the basis for recommendations on
  - construction of a major accelerator facility in the U.S.,
  - support for the immediate research program,
  - the need for R&D on major future accelerators,
  - a program of midsize experiments needed to address some critical questions.

The Central Experimental Issues in 2001

- The Fundamental Scales of Mass and Energy
  - The weak scale of 246 GeV
  - New physics at the TeV scale
  - Supersymmetry
  - Observable effects of large hidden dimensions





- Source of neutrino masses
- One source for CP violation?
- Cosmology, Particle Astrophysics
  - Dark matter, dark energy
  - Highest energy cosmic rays

Worldwide Program for High-Energy Physics

2001-2011

- Operating program: Tevatron, B experiments, neutrinos, and particle astrophysics experiments, with some new starts.
- LHC construction and operation
- Linear Collider construction
- R&D on VLHC, Neutrino factory, two-beam linear accelerators, and advanced acceleration techniques
- Construction of intense proton sources for HEP, NP, and other uses

2012-2022 (with some selection to be done as physics and technology become clearer)

- Operating program: LHC, LC, neutrino, astro experiments and upgrades
- VLHC construction
- Neutrino factory construction, if technology and physics work out
- Two-beam Linear Collider construction, assuming successful R&D
- R&D on accelerators of the next decade

? What is built in the U.S.? What does U.S. HEP participate in elsewhere?

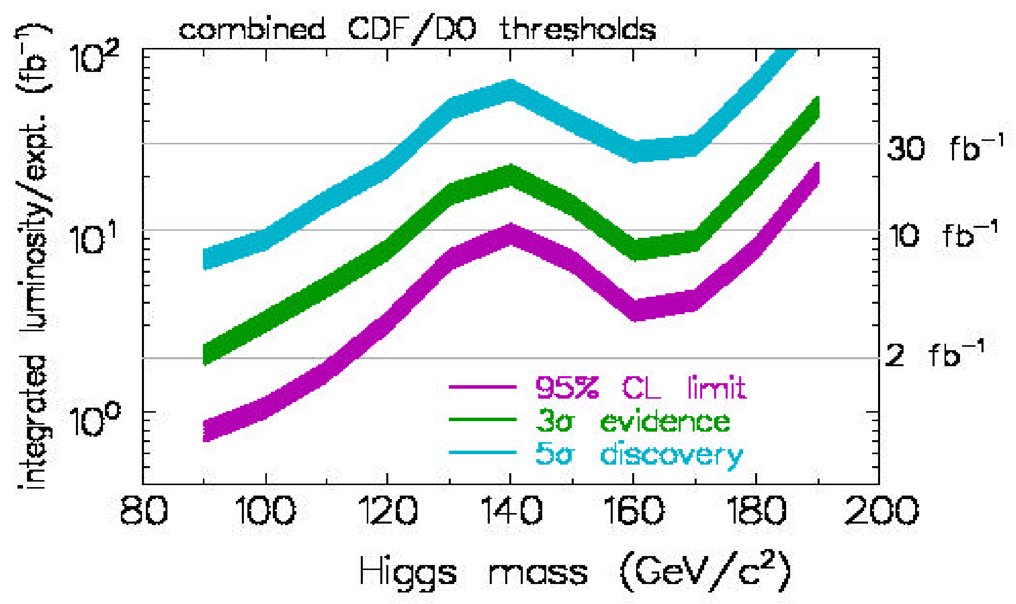
The Proper Balance

- The science issues dictate some diversity of experimental types in our plan.
  - The most ambitious experiments and the largest facilities should be focused on our leading problem, the nature of new physics at the TeV scale.
  - Midsize projects are needed to address the major issues of neutrinos, CP violation, and astro problems like dark matter.
  - Some smaller projects can provide very good physics at modest cost and broaden the field.
  - Collider experiments like CDF, D0, BABAR, BTeV do support a broad science program in addition to the headline measurements.
- Having separate lists of high-priority projects of different sizes is a good way of presenting a well-balanced plan.
  - They must be excellent experiments driven by the science.

Tevatron Run 2

- ? Our goal is to deliver as much integrated luminosity to CDF and D0 as possible by 2007.
  - Run 2a represents a big improvement in supersymmetry searches, top and W mass measurements, B physics, and QCD studies. (x20)
  - For Higgs discovery, we need the full benefit of additional luminosity upgrades. (x5-10) These need support and manpower immediately.
  - The schedule is the biggest challenge.

The Subpanel must give very high priority to the pursuit of Run 2 physics if this plan is to succeed.



CMS and ATLAS

- These will be flagship experiments of the U.S. HEP program.
- We need sustained support if the U.S. physicists are to gain full benefit of the LHC investment.
  - Software and Computing Project
  - Maintenance and Operations
- The exciting science opportunity at the LHC is one of the very good reasons that HEP deserves increased support from DOE and NSF.

The Future of Flavor Physics

- With results from the present experiments, the study of CP violation moves from establishing CKM as the primary CP̸ source to testing for inconsistencies that would signal new physics (as for the Z).

- Both BTeV and at least one upgraded e^+e^- B-factory are needed.
  - The best chance for seeing new physics is in the loop diagrams contributing to B_d and B_s mixing.
  - B_s asymmetries will only be measured by hadron experiments.
  - Precise measurements of asymmetries in some critical B_d modes probably require the rate of hadron colliders.
- The other important experiments to complete the search for new physics in CP violation are K^+ ↴ ?????and K^0 ↴ ?????
  - We should do proposed experiments if they have a very good chance to deliver 100 events with good control of background.
  - We have just given stage 1 approval to CKM, an experiment designed to meet this criterion for K^+ ↴ ?????

The Future of Neutrino Physics

- If MiniBoone confirms LSND, the most urgent neutrino experiment will be one to follow up that new physics, the existence of 4 neutrinos.
- What will be the most compelling follow-up to MINOS?
  - It may be measuring $?_{?} \to ?_{e}$ at the wavelength of $?m^2_{atm}$.
  - This would require a more intense MI beam and a detector upgrade.

- We know that neutrino physics will require new experiments and/or upgrades.
  - It is premature to propose a detailed experimental neutrino program beyond MiniBoone, MINOS, K2K, CNGS, and Solar experiments.
  - We should support continued physics and facility design studies.
- The subpanel should emphasize the scientific importance of these upcoming results and point to the need for follow-up experiments.
  - We are making new discoveries with each round of neutrino experiments, and there is good reason to expect that will continue.

The Neutrino Factory

- A worldwide R&D program through this decade will determine the practicality of a muon-based neutrino factory.
  - Fundamental differences among the various designs reflect the early stage of work toward an optimum design.
  - A few extended experiments are needed.

- Neutrino experiments operating in the period 2002-2007 will help to determine the future path.
  - Is CP violation attainable? If so, what rate is needed?
  - Is the next step to build conventional neutrino sources with more intensity?
- The community should work toward having both the accelerator R&D and physics results needed to answer these questions late in this decade.
- Intense proton sources planned and under construction could serve as the starting base.

The Future of Particle Astrophysics

- New experiments with great scientific potential will be starting to operate.
  - Dark matter detection
  - Cosmic rays at the highest energies
  - Astronomical surveys (gamma ray, visible, ir, supernova search) done as collaborations among particle and astro- physicists.

Linear Collider

- The Subpanel was charged to make recommendations on the next U.S. facility.

- You must choose clearly one of three recommendations on a linear electron-positron collider:
  - The U.S. should propose to be the host for a linear collider built by an international collaboration.
  - The U.S. should not pursue building it here, but should take a strong role in building one somewhere else.
  - The U.S. should not take a major role in a linear collider.

- This decision should not be delayed further.

Questions on the Linear Collider

- Is the physics potential of an electron-positron linear collider with energy 0.5 TeV, upgradable to 0.8-1.0 TeV, great enough to pursue the possibility of hosting it in the U.S.?
  - Is it reasonable to conclude that at least one technology will be ready to build a collider with this energy and a luminosity sufficient to do the physics program?

The Case for a 500 GeV Linear Collider

P. Grannis HEPAP subpanel meeting SLAC

✍ Understanding the source of electroweak symmetry breaking is the most likely question on which fundamental progress is expected in the next 10 – 20 years.

✍The LHC (or the Tevatron) seems assured of discovering new phenomena related to EWSB, but will leave critical questions unanswered.

✍An e^+e^- linear collider at ~500 GeV should discover new phenomena, make precision measurements that illuminate the nature of EWSB, and point the way toward higher energy phenomena.

✍The $e^+ e^-$ linear colliders are well developed technically and it is very likely that we will make decisions about a linear collider within the next few years.

Linear Collider recommendation

- **The subpanel should recommend construction of a linear collider in the U.S., built as an international project, with the optimum technical design.**
  - The physics case for the LC with a 1st stage at ~ 500 GeV is very strong. We need a linear collider to study our most compelling physics issues – the physics of the TeV scale.
  - We know enough to make the choice now.

- The subpanel should also
  - emphasize the need to develop a true international collaboration;
  - encourage the comparative evaluation of technology.

Basic Principles

The Subpanel should also state the basic principles for proceeding toward a linear collider:

- There should be one linear collider in the world, built as an international collaboration.
  - HEP laboratories and community should lead the way in shaping that collaboration.
- The collaboration should develop the best possible design.
  - The technical evaluation is an essential next step in this process.

- We should also recognize and accept that the host country will be the one willing to contribute most of the finances.

Linear Collider at Fermilab

? **We propose to the U.S. and to the international HEP community that we work together to build a linear collider at or near the Fermilab site.**
  - There is a consensus in the HEP community that the site should be near an existing laboratory if possible.

? Fermilab is an excellent site for a linear collider.
  - strong base of expert manpower and infrastructure
  - excellent locations nearby
  - none of the problems, including political, associated with a green field site
  - good geology
  - political environment as good as any other U.S. site

International Collaboration

- We can organize the accelerator project as a true collaboration of HEP laboratories.
  - Each laboratory will have a critical role and will be able to remain a vital accelerator laboratory.
  - This will establish the principle that we can build large international projects.
  - It will show a way to sustain the vitality of many HEP laboratories in an era of only 2 or 3 large accelerators in the world.
  - We will insure that the U.S. will have a flagship accelerator.
  - It will also establish the U.S. as “among the world leaders” in accelerator science as well as in particle physics

- Recommending that the U.S. bid to host the collider is completely compatible with encouraging that the collider should be an international project.
  - It substantially increases the chance that such a collider will be built.

VLHC

- The most promising path to explore physics at the 10-TeV scale is a proton collider with higher energy than the LHC.
  - This will follow up on the exciting physics that is sure to come from the LHC.
  - The basic technology of superconducting magnets is in hand, work on developing specific VLHC magnet designs is under way.
  - The excellent study recently completed establishes a feasible design that makes it possible to explore various possible designs. It also provides a proper basis for directing the R&D program.
  - The community needs to address issues such as physics reach vs. cost. What is the right energy step after LHC?

VLHC R&D

- We need to continue a strong R&D effort on technologies critical for a VLHC.
  - It is very likely that the community will push for a VLHC start early in the next decade.
  - Well-developed designs will make it possible to move a project from concept to construction start as quickly as possible.
  - Other uses include special magnets for Tevatron upgrades, magnets for muon storage rings, and possible 2nd generation LHC quadrupoles.
- We should develop an international collaboration on VLHC R&D.
- We should start an international series of workshops on physics and detectors of hadron colliders beyond initial LHC.

A Final Word

- **We have a historic opportunity to advance the cause of High Energy Physics worldwide and to establish that the U.S. will remain at the forefront of the field.**

- **The subpanel should recommend construction of a linear collider in the U.S., built as an international project, with the optimum technical design.**
  - The physics case for the LC with a 1st stage at ~ 500 GeV is very strong.
  - We know enough to make the choice now.