Physics Opportunities in Fermilab's Futures

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- Opening remarks
- Expectations for Run II
- Aspirations for Run III
- Fermilab and the LHC
- e^+e^- linear collider
- The path to a $\mu^+\mu^-$ collider
- Beyond the LHC
- Inventing Fermilab's Futures

Late Night Thoughts on Listening to Bruckner's Fourth Symphony*

Multiple time scales
Overlapping time scales
The importance of patience

¶ I will not mention every important physics issue.

¶ I will not bless or curse individual experiments.

¶ I will focus on issues we need to think about—and work on—together.

*With apologies to Lewis Thomas.

Optimize the Physics Program

- A golden age for Fermilab:
  Tevatron is the highest energy collider; fixed-target experiments advance the sensitivity frontier.
  Both have great discovery potential.
- Today's physics influences tomorrow's experiments and accelerators.
- Success is rewarded!

Develop Tomorrow's Technologies

Many Promising Ideas

Much Work to Do

Get on with Run II

Fermilab Tevatron + Main Injector
$\bar{p}p$ collisions at 2 TeV
CDF and DØ detectors

- Run I: 100 pb^{-1} @ 1.8 TeV 1994–1996
- Run II: 2 fb^{-1} @ 2 TeV in 2000–2002

Goals:

- Discover CP violation in $B^0 \to \psi K_s$
- Exploit the physics of the top quark
  Begin to determine $|V_{tb}|$ in $q\bar{q} \to W^\star \to t\bar{b}$
- Refine M_W
- Search for superpartners and new strong dynamics

The Problem of Identity

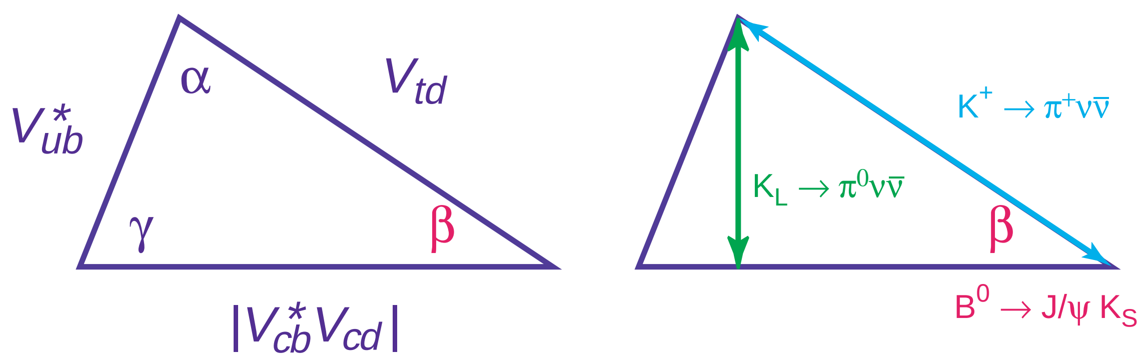
Part of the physics that determines
the machine beyond the LHC.

Accessible at Fermilab: CP violation, ν mass, ...

- Kaons: KTeV et seq.
- B mesons: Tevatron Collider

Three-generation unitarity:

$$V_{ub}^* V_{ud} + V_{cb}^* V_{cd} + V_{tb}^* V_{td} = 0$$



Fermilab can be first to $\sin 2\beta$ from $B^0 \to \psi K_{\rm S}$.

- Large asymmetry expected
- Ample rate: 10 – 20 kHz $b\bar{b}$ in Run II
- CDF has developed tagging techniques

Measuring $|V_{tb}|$

CDF measures

$$B_b \equiv \frac{\Gamma(t \to bW)}{\Gamma(t \to qW)} = \frac{|V_{tb}|^2}{|V_{td}|^2 + |V_{ts}|^2 + |V_{tb}|^2} = 0.99 \pm 0.29$$

With three generations,

$$\Rightarrow |V_{tb}| > 0.76 \;\; (95\% \text{ CL})$$

Without the unitarity constraint, learn only that

$$|V_{tb}| \gg |V_{td}|, |V_{ts}|$$

Expected improvements in δB_b:

Run II: $\pm 10\%$ Run III: $\pm$ few % LHC: $\pm 1\%$

Direct measurement of $|V_{tb}|$ in single-top production

$$\bar{q}q \to W^\star \to t\bar{b} \qquad gW \to t\bar{b}$$

$$\sigma(t) \propto |V_{tb}|^2$$

Expect $\delta|V_{tb}| = \pm(10\%, 5\%)$ in Run II and III, using both $W^\star$ and gW fusion.

LHC: gW fusion cross section is $100\times$ larger

S. Willenbrock, "Top Quark Physics for Beautiful and Charming Physicists," hep-ph/9709355.

Top and W Measurements

- $\delta m_t \approx 3$ GeV/c^2 in Run II, 1 GeV/c^2 in Run III, LHC
- $\delta M_W \approx 40$ MeV/c^2 in Run II
- $\Rightarrow$ infer $\delta M_H / M_H \approx 40\%$
- $\delta\sigma(t\bar{t}) \approx 8\%$ in Run II, 3% in Run III, $\pm$ few % at LHC
- $\delta \dfrac{\Gamma(t \to bW)}{\Gamma(t \to qW)}$ will improve to $\pm 10\%$ in Run II, $\pm$ few % in Run III, $\pm 1\%$ at LHC
- $\delta|V_{tb}| \approx \pm 10\%$ in Run II, $\pm 5\%$ in Run III
- Searches are under way for $t\bar{t}$ resonances, rare decays, and other signs of new physics.

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thinkshop: top-quark physics for Run II.
QCD and Weak Boson Physics Workshop: first meeting March 4–6, 1999.
Links at http://www-theory.fnal.gov/.

Run II: Extensive search for light-scale supersymmetry

Now is the time to find supersymmetry!

LEP 2

Tevatron Run II

Run II Workshops: Supersymmetry & Higgs

- New simulation tools
- Analysis schemes
- New signatures
  - R-parity–violating decays
  - Signatures of extra dimensions
  - Search for long-lived particles (macroscopic decay lengths) by photon pointing or heavy ionization

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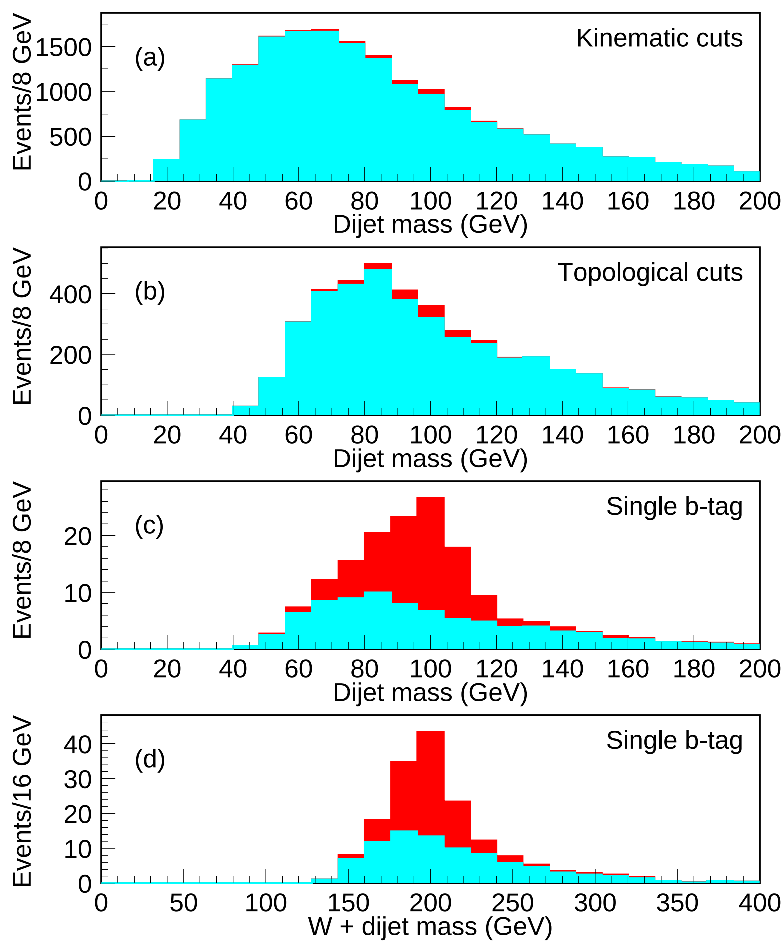
First draft of "Yellow Book" chapters due 1/29.
SUSY99 at Fermilab, 14-19 June 1999
Links at http://www-theory.fnal.gov/.

Low-Scale Technicolor Search

$$\rho_T^+ \quad \rightarrow \quad W^+ \pi_T^0$$
$$\hookrightarrow b\bar{b}$$



Eichten, Lane, & Womersley, "Finding low-scale technicolor at hadron colliders," *Phys. Lett.* **B405**, 305 (1997), hep-ph/9704455.
Workshop on New Strong Dynamics: next meeting April 9-10, 1999.
Link at http://www-theory.fnal.gov/.

Develop a Plan for Run III

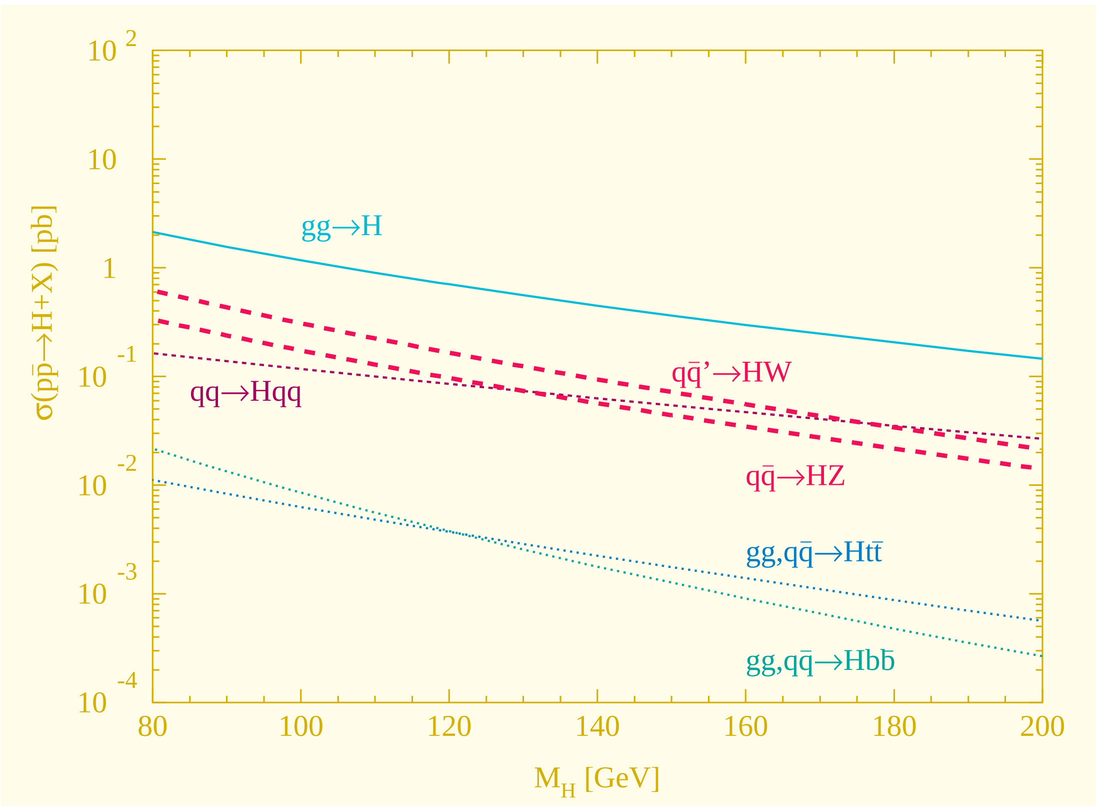
Increased $\mathcal{L}$ improves discovery reach

Target: 30 fb^{-1} by 2006

$\mathcal{L}$ motivated by search for light Higgs boson in the region favored by supersymmetry

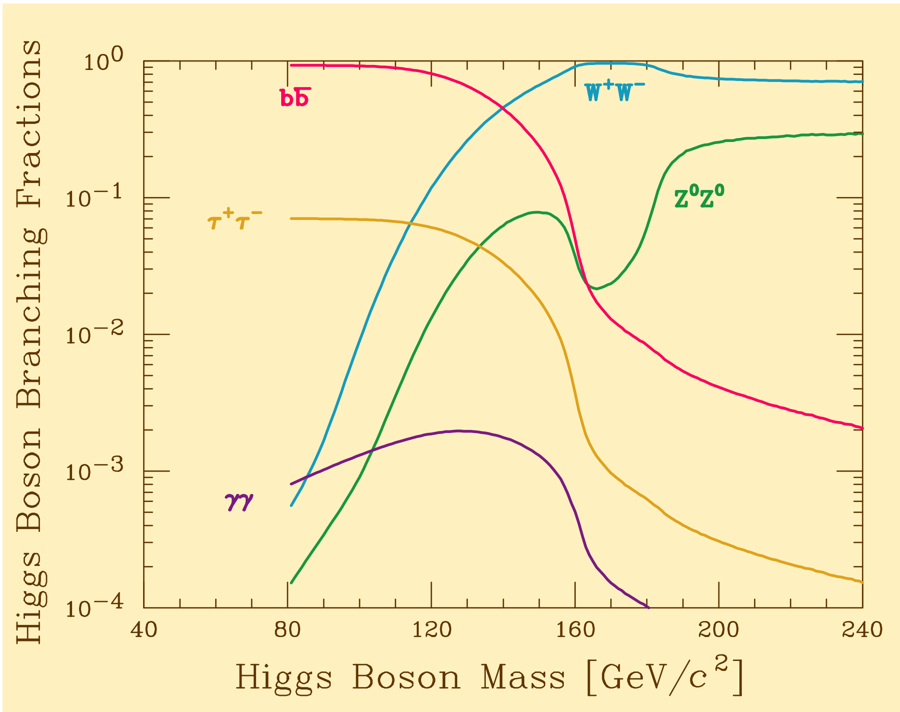
- Improvements in m_t, M_W
- Study of top production and decay
- Single-top production and $|V_{tb}|$
- Extend study of CP violation
- $B_s - \bar{B}_s$ mixing
- B_c, b-baryon spectroscopy
- Supersymmetry: extend search or exploit discovery
- Continue search for new strong dynamics

Higgs-boson production sets luminosity target



Many processes become accessible once $\mathcal{L}$ exceeds a few fb.

Search for a not-too-heavy Higgs boson



- Tevatron:

$$q\bar{q} \quad \to \quad H(W, Z) \\ \hookrightarrow b\bar{b}$$

- LHC:

$$gg \to H \to \gamma\gamma,$$

$$q\bar{q} \quad \to \quad HW \\ \hookrightarrow b\bar{b},\ WW^\star,\ ZZ^\star$$

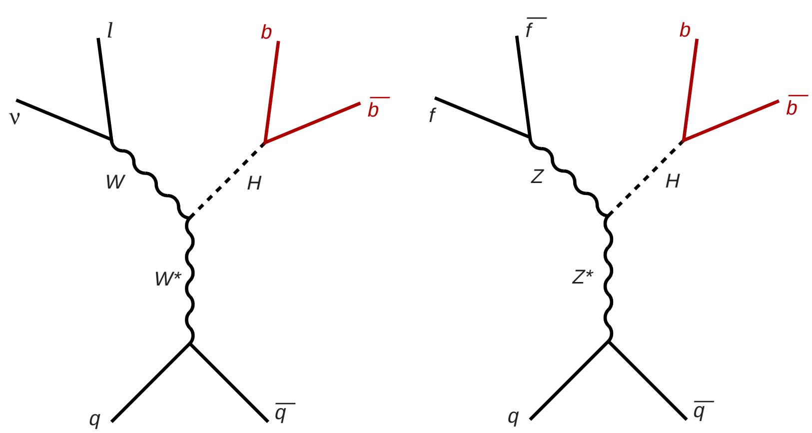
Tevatron Search Strategies

- $gg \to H \to b\bar{b}$ is swamped by QCD production of $b\bar{b}$.
  Even with 30 fb^{-1}, only < 1-σ excess.
  By-product: $Z^0 \to b\bar{b}$ observable in Run II.

- Special topologies improve signal/background and significance:

$$\bar{p}p \to HW + \text{anything}, \quad H \to b\bar{b}, \quad W \to \ell\nu,\ \text{jets}$$

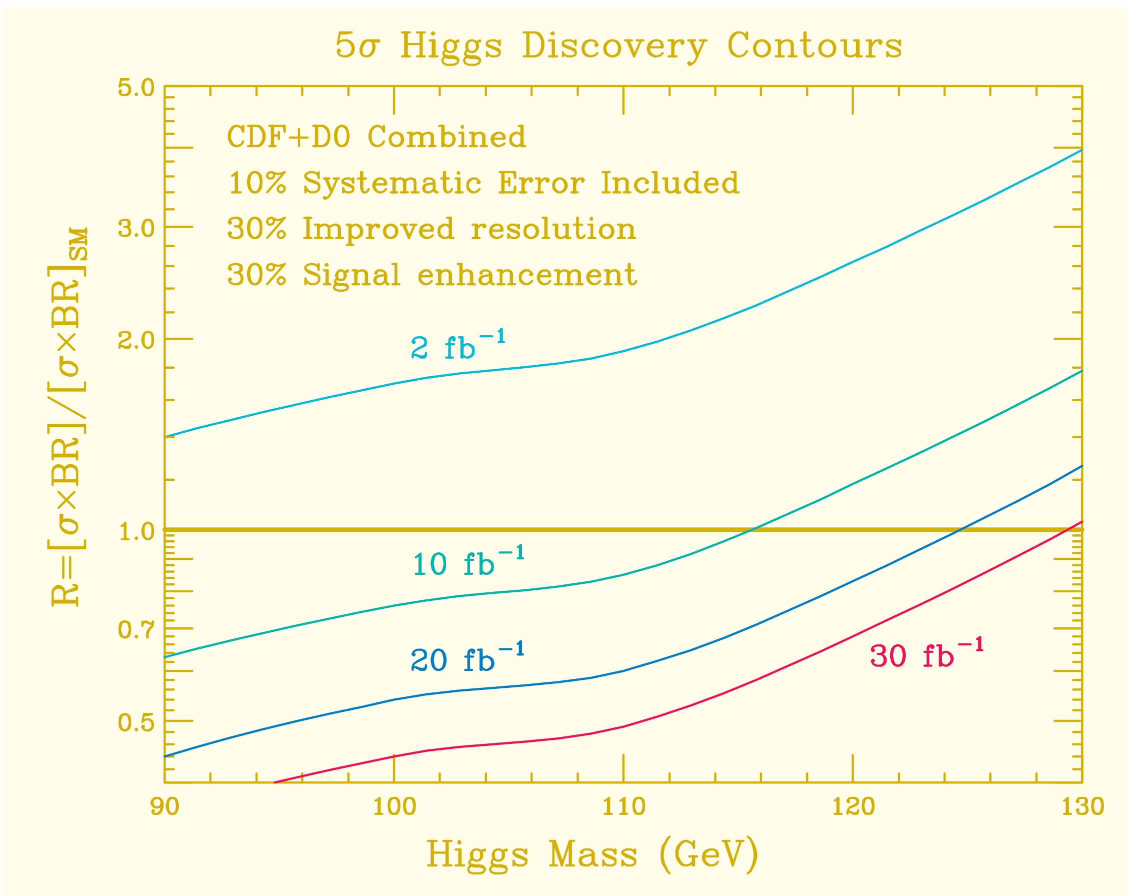
$$\bar{p}p \to HZ + \text{anything}, \quad H \to b\bar{b}, \quad Z \to \ell^+\ell^-,\ \nu\bar{\nu}$$



A. Stange, W. Marciano and S. Willenbrock, *Phys. Rev. D***49**, 1354 (1994); *Phys. Rev. D***50**, 4491 (1994).

Higgs boson sensitivity

- Combine CDF and DØ
- Combine W and Z channels

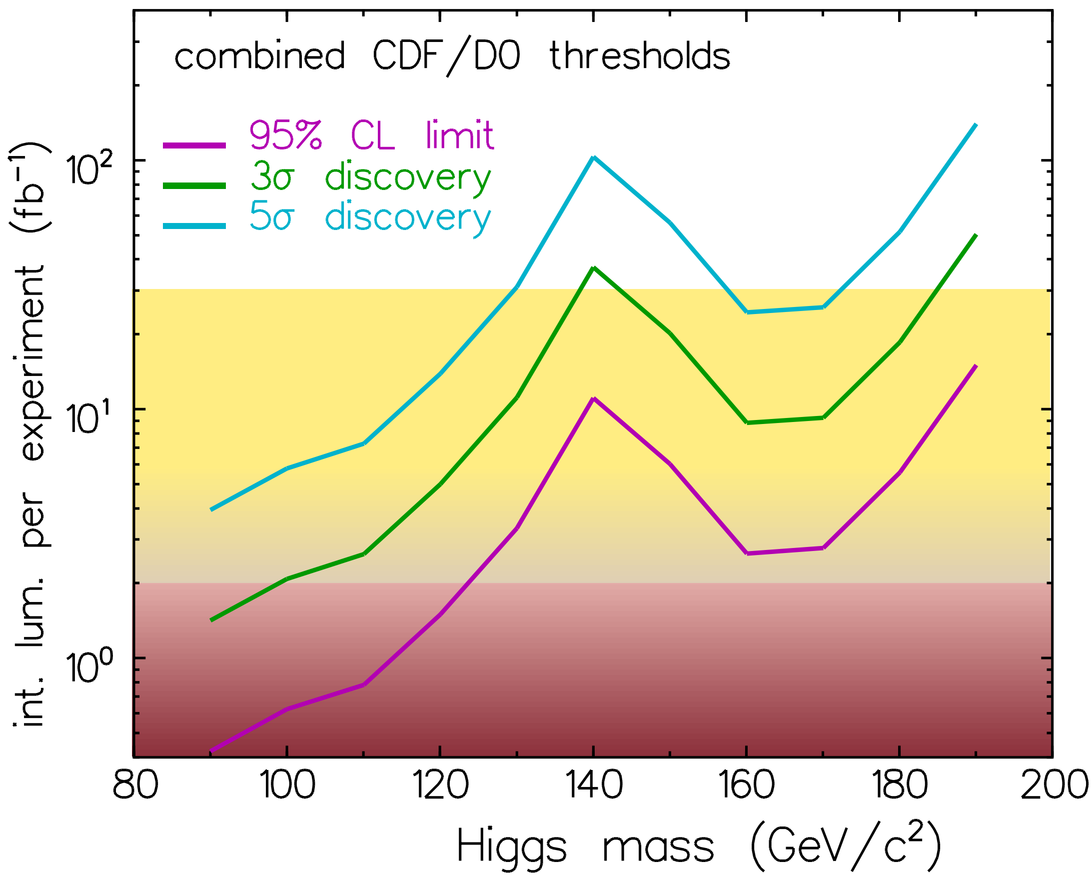


Supersymmetry/Higgs Workshop, http://fnth37.fnal.gov/susy.html.

Higgs boson search & discovery

Extend reach using $H \to WW^*$ mode?

Initial studies are promising.



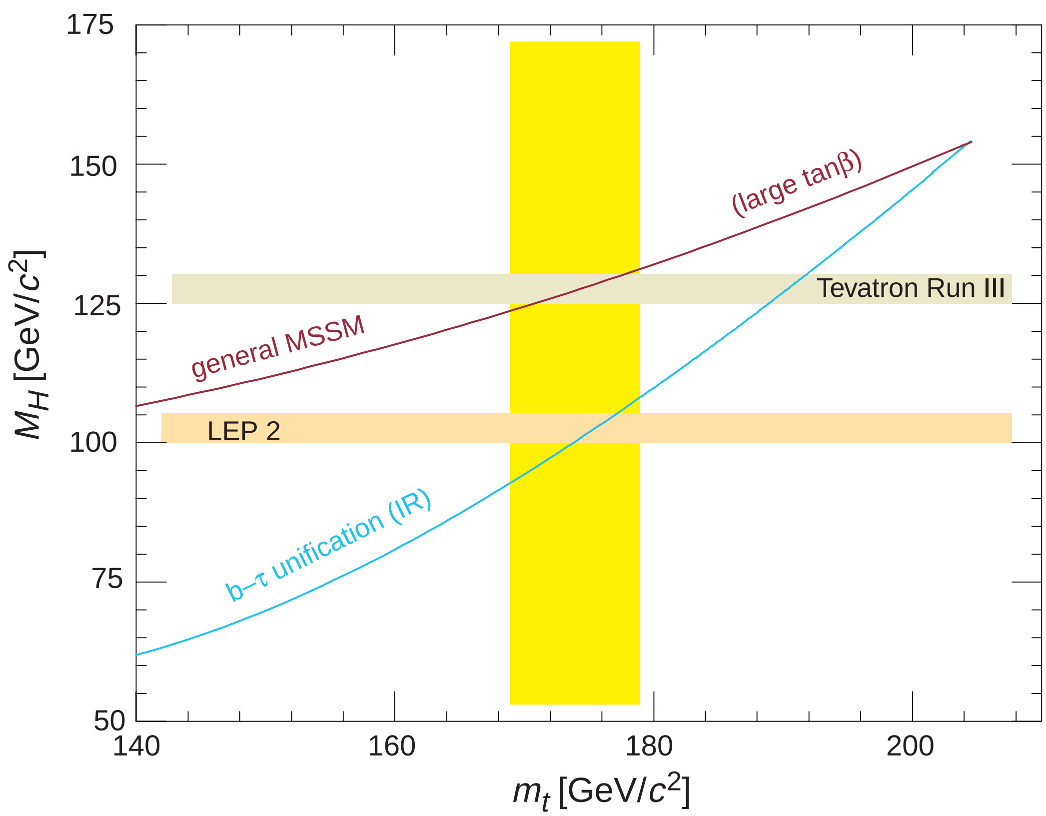
Tao Han, André S. Turcot, and Ren-Jie Zhang, hep-ph/9812275.

Precision EW data prefer a light Higgs boson, which demands new physics nearby.

SUSY solves naturalness problem of SM Higgs sector, allows perturbative unification, and provides a source of new physics that demands a light Higgs boson.

$$M_h^2 = M_Z^2 \cos^2 2\beta + \frac{3g^2 m_t^4}{8\pi^2 M_W^2}\left[\log\left(\frac{m_{\tilde{t}_1} m_{\tilde{t}_2}}{m_t^2}\right) + \cdots\right] \lesssim \left(130\ \mathrm{GeV}/c^2\right)^2$$

Upper bound on $m_h \Longleftrightarrow$ large m_A limit, ($M_s = 1$ TeV):



M. Carena, J. R. Espinosa, M. Quirós, and C. Wagner, *Phys. Lett.* **B355**, 209 (1995).

How to Realize Run III?

Be prepared to exploit Run II discoveries

(a) High peak $\mathcal{L} \to 2 \times 10^{33}\ \mathrm{cm}^{-2}\ \mathrm{s}^{-1}$, or

(b) "Level" $\mathcal{L} \approx 5 \times 10^{32}\ \mathrm{cm}^{-2}\ \mathrm{s}^{-1}$?

Avoid a long shutdown while Tevatron defines the energy frontier.

What detector upgrades are required?

If modest upgrades suffice, will CDF & DØ have adequate forces?

Total cost?

Can we do this?

CMS & the LHC

pp collisions at 14 TeV

$\int \mathcal{L}dt = 100\ \text{fb}^{-1}$ in 2005–2009

The Energy Frontier and EWSB

Physics, accelerator, and detectors all part of Fermilab's intellectual legacy . . . and future.

Tevatron experiments have changed the way we think about LHC physics.

- The great mass of the top quark
- The success of b-tagging in the hadron-collider environment

Will continue to define the physics context for LHC.

We should be strongly engaged in

- magnets
- machine design & commissioning
- detectors
- physics   CMS school in 2000 or 2001?

LHC involvement can enhance Fermilab, not sap it.

Big Questions for Future Accelerators

- What machines are possible?
  When?
  At what cost?
- What are the physics opportunities?
- Can we do physics in the environment?
  (What does it take?)
- How will these experiments add to existing knowledge *when they are done?*

The SSC was the right answer

Central problem in particle physics:
understand the mechanism of electroweak
symmetry breaking.

⇒ Explore the 1-TeV scale
Search for the Higgs boson

40-TeV *pp* collider with 10^{33} cm^{-2} s^{-1} would have been the ideal instrument.

Still the *best practical idea* we've had . . .

. . . but it's not going to happen.

Complicates the task of developing a new vision

Luckily, LHC is a very capable machine.

Challenge:

- Develop better practical ideas
- Look to physics beyond EWSB
- Imagine ways to pursue LEP2 – Tevatron – LHC discoveries

Watch this space!

Norbert Holtkamp and Alvin Tollestrup are organizing a seminar series on new machines and new technologies. They will invite technical experts (not traveling salesmen) and commission provocateurs to identify issues in need of study.

e^+e^- linear collider

A lovely idea!

Fermilab accelerator links to SLAC & DESY

Possible goals:

- multi-TeV to complement LHC studies of EWSB
- detailed studies of top or Higgs or SUSY; threshold scans of any new channel

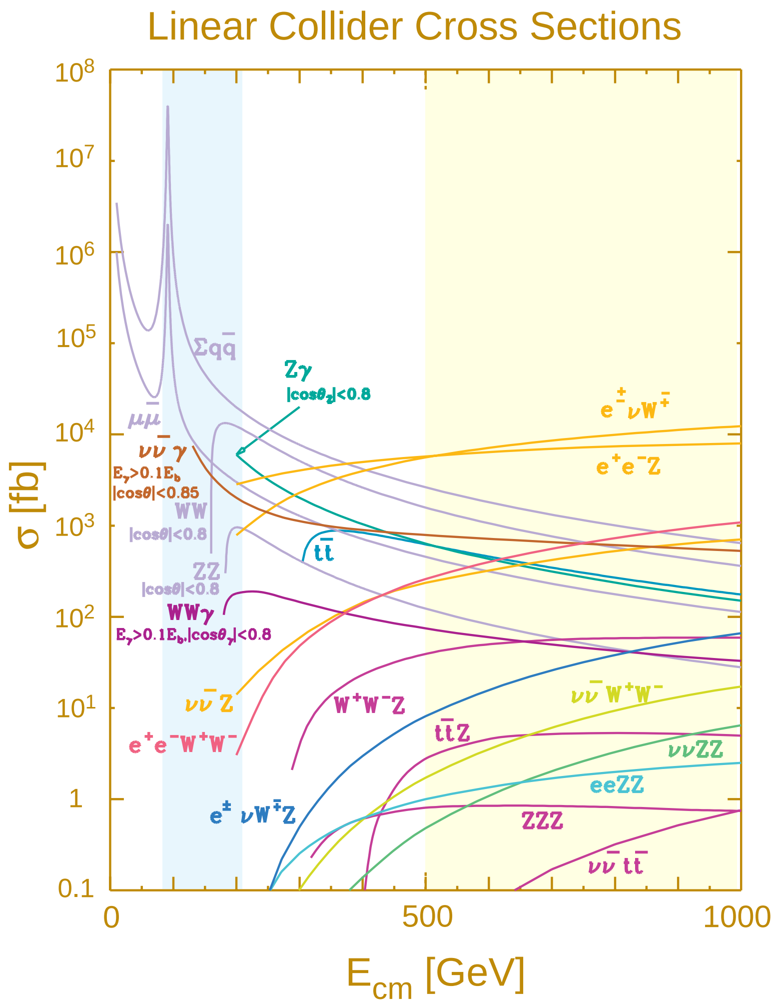
Traditional advantages:

- Point particle means full c. m. energy is available
- No background from the underlying event

Traditional challenges:

- Hard to reach very high energies
- Small cross sections demand high luminosity

Away from resonance peaks, cross sections are small . . .



. . . but many interesting cross sections are significant fractions of σ_{total}

H. Murayama and M. Peskin, "Physics Opportunities of e^+e^- Linear Colliders," *Ann. Rev. Nucl. Part. Sci.* **46,** 533 (1996).

Gilman subpanel recommendation:

The Subpanel recommends that SLAC continue R&D with Japan's KEK toward a common design for an electron-positron linear collider with a luminosity of at least 10^{34} cm^{-2} s^{-1} and an initial capability of 1 TeV in the center of mass, extendible to 1.5 TeV. The Subpanel recommends that SLAC be authorized to produce a Conceptual Design Report for this machine in close collaboration with KEK.

This is not a recommendation to proceed with construction. A decision on whether to construct a linear collider should only follow the recommendation of a future subpanel convened after the CDR is complete. The decision will depend on what is known about the technology of linear colliders and other potential facilities, costs, international support, and advances in our physics understanding.

My belief:

The US (and worldwide) HEP community will decide within five years whether to proceed with a linear collider—on its own, and not in competition with any other machine.

This decision must be based on an informed assessment of the scientific opportunities, technical risk, and cost. Building an inadequate machine "because we need a project" would be calamitous.

We cannot stand apart

- Only informed opinions will carry any weight.
- We have the standing to check claims about physics and accelerator science.
  Example: How long does it take to carry out the physics menu of a 500-GeV linear collider?
- The entire community must define the Right Linear Collider.
- Much physics is in common with the physics of a muon collider. (Profit from LC studies, learn what it takes to compete.)
- Fermilab is a possible site for a linear collider at the energy frontier. http://www-project.slac.stanford.edu/lc/local/systems/Conventional%20Facilities/compare.htm
- We can build bridges to users, and to other labs.

Can we invent a machine we want to build?

Physics Return vs. Cost and Technical Risk

(time sensitive)

Very interesting suggestions

TESLA team: $\mathcal{L} \to 10^{35}\ \mathrm{cm}^{-2}\ \mathrm{s}^{-1}$

(John Irwin: new focusing scheme)

$\Rightarrow$ Explore physics reach vs. $\mathcal{L}$

CLIC team: Evolved two-beam scheme

$\Rightarrow$ Explore physics reach vs. E_{cm}

Fermilab studies of physics vs. E_{cm} and $\mathcal{L}$

- Coordinators: Andreas Kronfeld & Sławek Tkaczyk
- Meeting jointly with Muon Collider Physics Group
- Exploring Higgs searches and heavy-flavor tagging

Physics and Detectors for Future Linear e^+e^- Colliders
http://lcwws.physics.yale.edu/lc/
C. Baltay, S. Komamiya, D. Miller co-chairs

- 2nd Joint ECFA/DESY Study
  http://www.desy.de/conferences/ecfa-desy-lc98.html
  - Orsay, April 1998
  - Lund, June 1998
  - Frascati, November 1998
  - Oxford, 20–23 March 1999
- ACFA Working Groups http://acfahep.kek.jp/
  - Beijing, November 1998 (G. P. Yeh)
  - ???
- (North) American Study
  http://lcwws.physics.yale.edu/lc/america.html
  - Keystone (Colorado), September 1998
  - (Simulations WG) SLAC, December 1998
  - (Simulations WG) Fermilab, 15–16 February 1999
  - ???

$\Longrightarrow$ International Linear Collider Workshop
28 April–4 May 1999, in Sitges, Spain

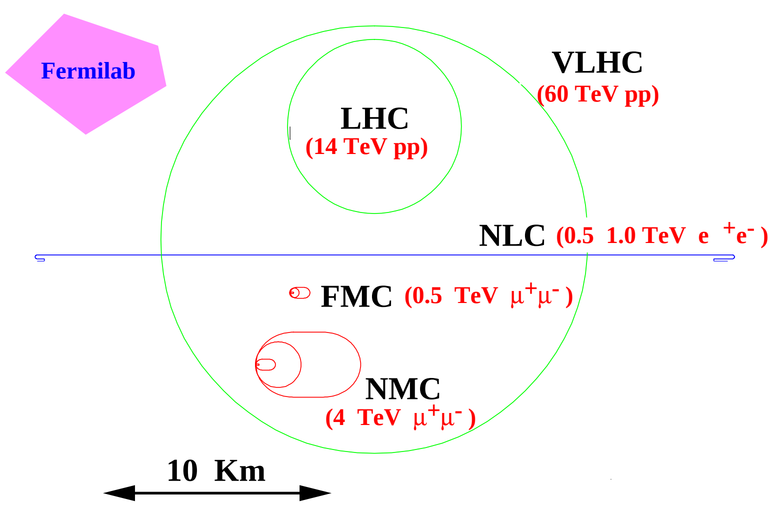
A future LC Workshop at Fermilab?

$\mu^+\mu^-$ collider

Possible path to a few-TeV $\ell^+\ell^-$ collider
to study electroweak symmetry breaking
and explore . . .

μ: an elementary lepton $\Rightarrow$ energy efficient
synchrotron radiation not crippling

$\Rightarrow$ small device reaches 1-TeV scale



?? modest size $\Rightarrow$ modest cost ??

Ultimate goal is $\sqrt{s} \sim 4$ TeV

(keep eye on ball)

But ... How to start?

Fermilab Workshop on Physics at the First Muon Collider and at the Front End of a Muon Collider

¶ A First Muon Collider?

- high-luminosity Z factory
- Higgs factory
- W^+W^- threshold
- $t\bar{t}$ threshold
- $\sqrt{s} \approx \frac{1}{2}$ TeV to explore SUSY or Techni world

¶ Front-end physics

- intense low-energy hadron beams
- a copious source of low-energy muons
- intense ν_μ and $\bar{\nu}_e$ or $\bar{\nu}_\mu$ and ν_e beams

The Front End

- A high-intensity proton source.
  CDR by 1/1/2001: Weiren Chou, *et al.*

- A system for pion production, collection, and decay to capture about 1.5×10^{21} μ^+ and μ^- per year.

- A muon cooling channel to concentrate the muons in six-dimensional phase space.

- A muon acceleration system to raise the captured muons quickly to the desired energy.

Higgs Factory at $\sqrt{s} = M_H$

If Higgs is light ($M_H \lesssim 2M_W$) and narrow,

$$\mu^+\mu^- \to H \to b\bar{b} \text{ and other modes}$$

may allow a comprehensive study of Higgs properties.

Assume Higgs discovered and $\Delta M_H = (100\text{-}200)$ MeV/c^2.

- modest luminosity (0.05 fb^{-1}/year) and high momentum resolution ($\sigma_p/p = 3 \times 10^{-5}$): $\sigma_{\sqrt{s}} \approx$ a few MeV $\approx \Gamma(H \to$ all).
- standard luminosity (0.6 fb^{-1}/year) and momentum resolution ($\sigma_p/p = 10^{-3}$): $\sigma_{\sqrt{s}} \gg \Gamma(H \to$ all).

Two-phase study:

1. Determine mass with $\Delta M_H \approx \sigma_{\sqrt{s}} \approx 2$ MeV/c^2. Requires 0.15 fb^{-1}, about three nominal years.
2. Scan at $\sqrt{s} = M_H, M_H \pm \sigma_{\sqrt{s}}$ to determine $\Delta M_H \approx 0.1$ MeV/c^2, $\Delta\Gamma_H \approx 0.5$ MeV $\approx \frac{1}{6}\Gamma_H$, $\Delta(\sigma \cdot B(H \to b\bar{b})) \approx 3\%$, and $\Delta(\sigma \cdot B(H \to WW^\star)) \approx 15\%$. Requires 8 years to accumulate 0.40 fb^{-1}.

Unparalleled measurements, luminosity at a premium.

Neutrino Beams from Stored Muons

Neutrino beam generated in

$$\mu^- \to e^- \nu_\mu \bar{\nu}_e$$

contains ν_μ and $\bar{\nu}_e$, but no $\bar{\nu}_\mu$, ν_e, ν_τ, or $\bar{\nu}_\tau$.

Muon charge, momentum, polarization determine composition and spectrum of the neutrino beam.

¶ New element: storing a millimole of muons per year, giving intense beams for

- Oscillation studies over a wide range of distance/energy and at very great distances
- Deeply inelastic scattering on thin targets

Requires less development of cooling technology than a muon collider.

Long-Baseline Possibilities

¶ Muon storage ring of two semicircular arcs + two equal straight sections ⇒ 25% of νs emitted in desired direction.

¶ Sources can be small:

$$\text{Arc length} \approx 75\ \text{m} \times \left(\frac{p_\mu}{40\ \text{GeV}/c}\right)$$

Can slant at steep angles without going very deep.

¶ Distinguish expected reactions

$$\begin{aligned} \nu_\mu N &\rightarrow \mu^- + \text{anything} \\ \bar{\nu}_e N &\rightarrow e^+ + \text{anything} \end{aligned}$$

from the oscillation-induced reactions

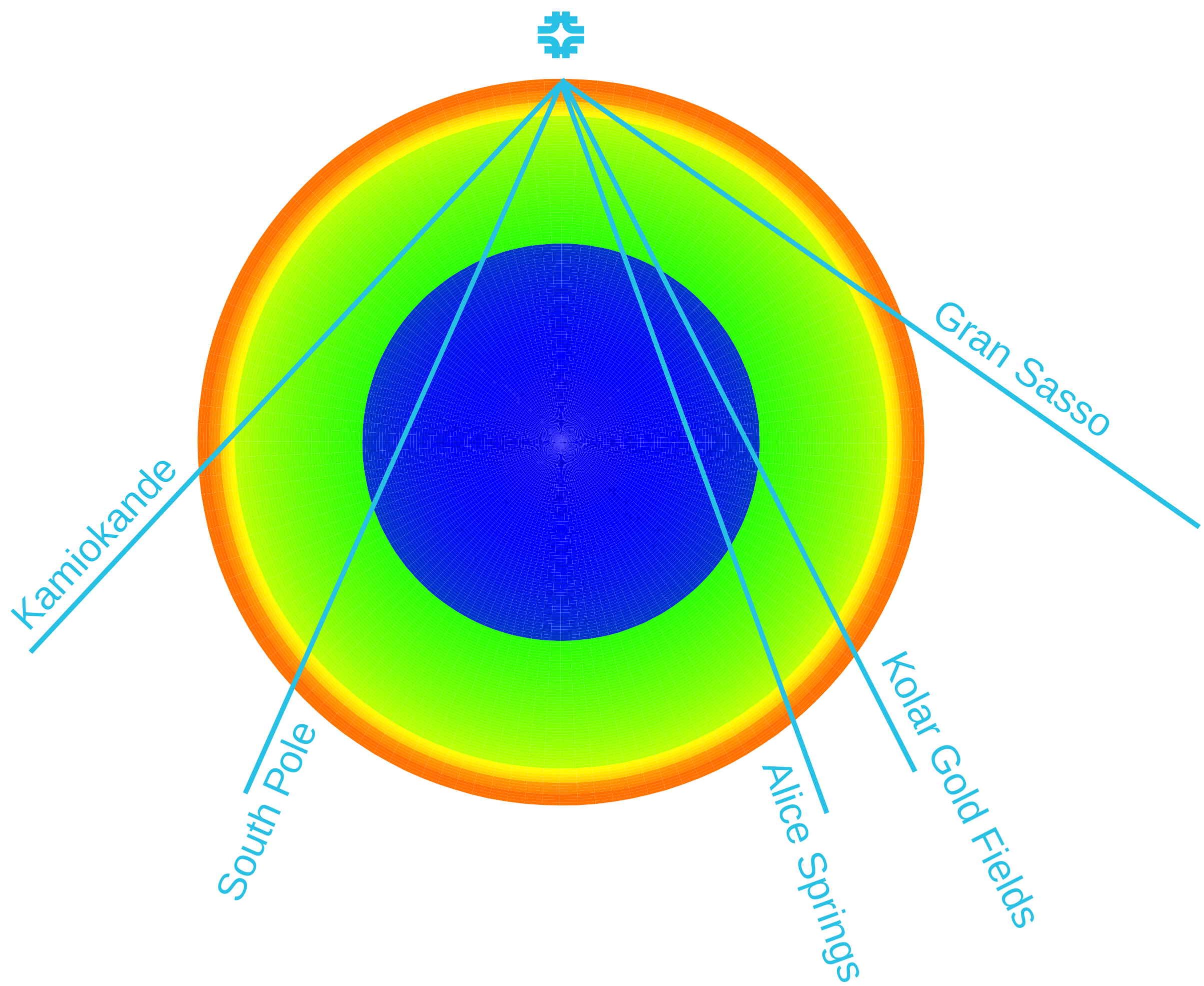
$$\begin{aligned} (\nu_\mu \rightarrow \nu_e) N &\rightarrow e^- + \text{anything} \\ (\bar{\nu}_e \rightarrow \bar{\nu}_\mu) N &\rightarrow \mu^+ + \text{anything}\ . \end{aligned}$$

¶ Oscillations characterized by

$$\sin^2\left(1.27\,\frac{\Delta m^2}{1\ \text{eV}^2}\cdot\frac{L}{1\ \text{km}}\cdot\frac{1\ \text{GeV}}{E}\right)$$

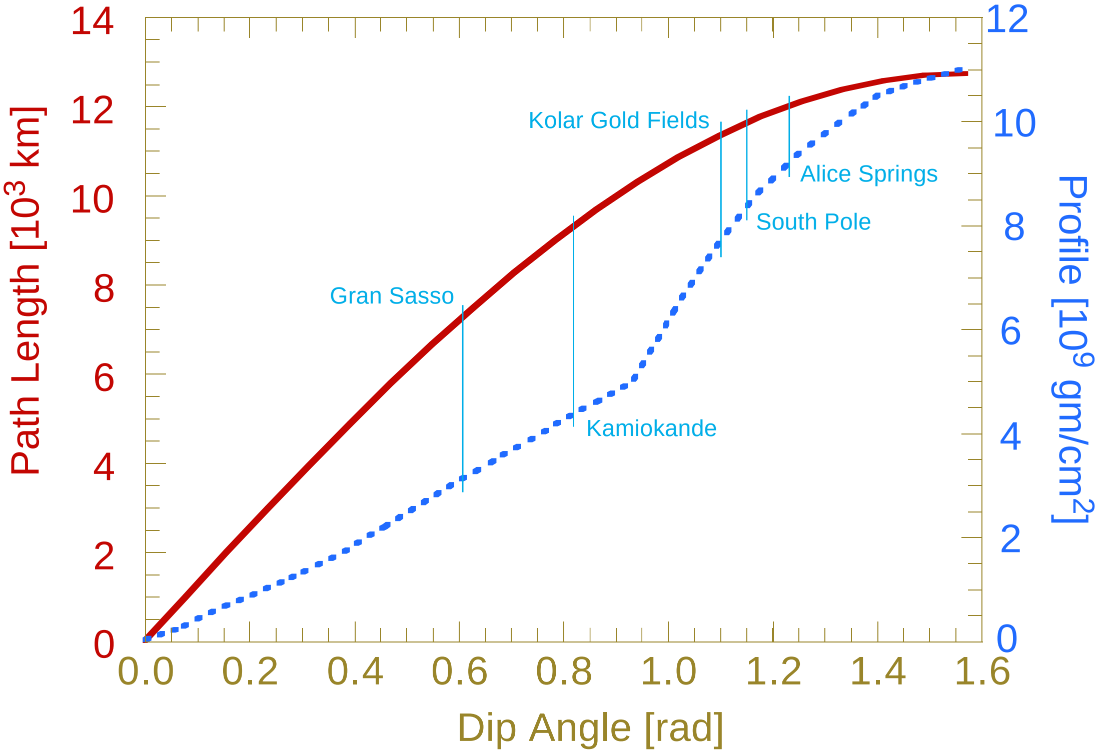
20-GeV muon beam ⇒

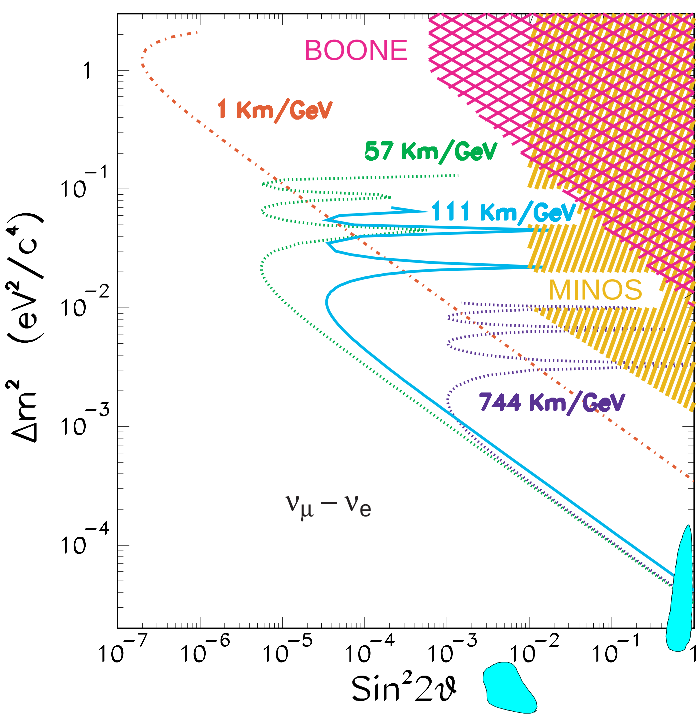
- few $\times\, 10^{10}\ \nu/\mathrm{m}^2$/year at Gran Sasso or Kamioka
- about 100 charged-current events / kiloton-year



At Soudan: 10× Main Injector flux

Detector design needed





Background: S. Geer, "Neutrino Beams from Muon Storage Rings: Characteristics and Physics Potential," hep-ph/9712290, *Phys. Rev. D***57**, 6989-6997 (1998), **59**, 039903 (1999).

Deeply Inelastic Scattering

Neutrino beam from last acceleration stage (RLA 3, 70 to 250 GeV), $\langle E_\nu \rangle \approx 135$ GeV; or a 250-GeV $\mu^+\mu^-$ collider ring, $\langle E_\nu \rangle \approx 178$ GeV:

1-meter LH_2 target @ 600 m $\Rightarrow 10^7$ CC events / year.

- Measure parton distributions on H_2, not Fe.
- $\delta M_W = 30$ - 50 MeV/c^2
- Extend to smaller x_{Bjorken}
- Reconstruct 10^5 charms / year, to improve measurements of $|V_{cd}|$ and study the strange sea.
- Polarized targets to probe spin distribution in proton, perhaps polarization of s and $\bar{s}$.
- Silicon target-detectors for heavy-flavor studies?

Observations on the FMC

- The various machines discussed as the First Muon Collider all are luminosity poor. The interesting—and unique—program that has been outlined for a Higgs factory would be a far more compelling prospect if it could be carried out over a few years, rather than a decade.

- A program that includes many collider rings dedicated to specific studies: a Higgs factory, a top factory, a $\frac{1}{2}$-TeV collider, etc., appears very rich. We have to keep in mind the realities of the muon economy: not all elements of a multiring complex will operate at once.

- Even modest polarization can be highly useful, especially if it can be controlled flexibly, and separately for μ^+ and μ^-. It is an advantage if polarization can be reversed on demand.

- Single-muon-ring devices do not seem to lack intensity. The capabilities of the intense neutrino beams produced in the decays of stored leptons appear very well matched to the demands of the physics.

Gilman subpanel recommendation:

The Subpanel recommends that an expanded program of R&D be carried out on a muon collider, involving both simulation and experiments. This R&D program should have central project management, involve both laboratory and university groups, and have the aim of resolving the question of whether this machine is feasible to build and operate for exploring the high-energy frontier. The scale and progress of this R&D program should be subject to additional review in about two years.

European interest:

- Report to ECFA, "Prospective Study of Muon Storage Rings in Europe"
- CERN–SPSC/98-30, "Physics Opportunities at a CERN-based Neutrino Factory"

My belief:

The muon collider path needs a plausible first step that is rich in physics. If a neutrino source can be built by the time the LHC turns on, it could offer a diverse program of experiments, including a broad assault on neutrino mass and mixing—without committing us to the muon collider as our machine on the energy frontier.

Toward a neutrino source:

- Physics and experiments:
  Workshop on the Potential for Neutrino Physics at Muon Colliders, Brookhaven, August 1998. Contacts: B. King, P. Spentzouris, H. Schellman + S. Geer.

- Accelerator complex:
  ECFA Workshop planned at CERN (+ Muon Collider Collaboration). Contact: S. Geer.
  Need Fermilab-specific design work.

Toward a muon collider:

- Muon Collider Collaboration
  link from http://www-ap.fnal.gov
  Contacts: A. Tollestrup and S. Geer.

- Detector (R. Raja) and Physics (J. Lykken)
  Simulations gathering a small group. Meetings every other Thursday (now with linear-collider group).
  http://www.fnal.gov/projects/muon_collider/physics/

- Throw deep: Montauk meeting on Muon Colliders from 10 to 100 TeV, end of September (B. King)
  http://pubweb.bnl.gov/users/bking/www/heshop/

Beyond the LHC

Discoveries at LHC could point to energies well above the 1-TeV scale $\Rightarrow \sqrt{s} \gg 14$ TeV.

- Heavy Higgs boson
- New strong dynamics
  strong WW scattering
  Technicolor (analogue of BCS)
  Gauge-mediated SUSY breaking
- New gauge boson

A Very Large Hadron Collider is the one multi-TeV machine we know we can build.

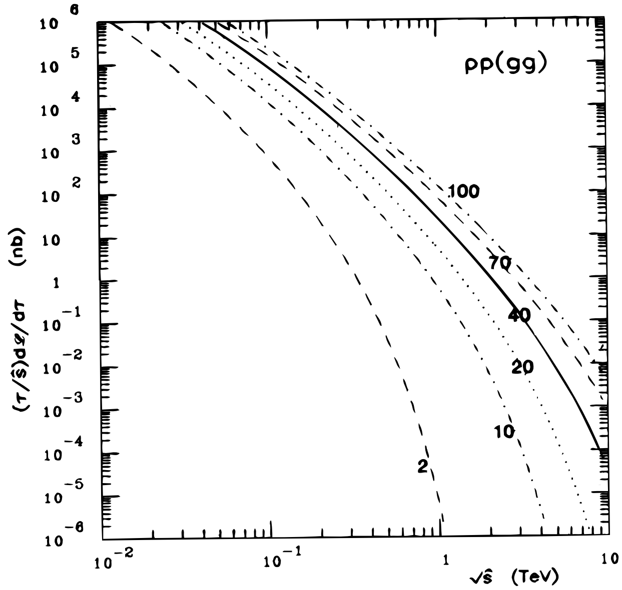
Pointlike cross sections $\propto 1/s$

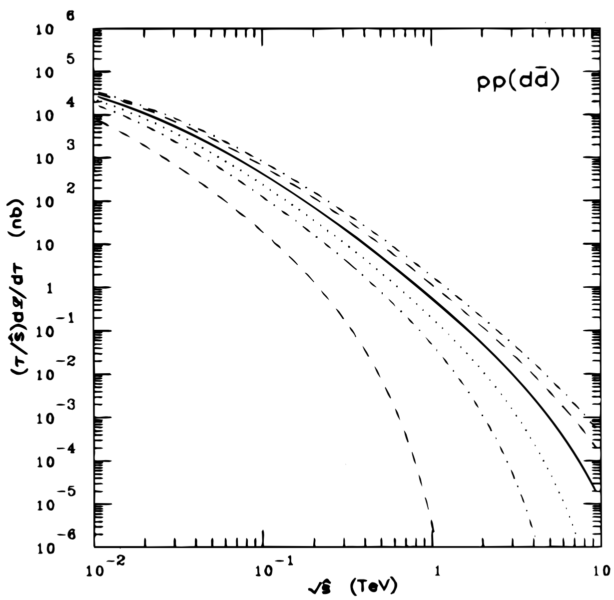
$\Rightarrow$ Luminosity goal:

$$\mathcal{L}^\star = 10^{32\text{-}33}\ \mathrm{cm}^{-2}\ \mathrm{s}^{-1}\left(\frac{\sqrt{s}}{40\ \mathrm{TeV}}\right)^2$$

For $\sqrt{s} = 100$ TeV, target $\mathcal{L}^\star \approx 10^{34}\ \mathrm{cm}^{-2}\ \mathrm{s}^{-1}$

Parton Luminosities





Background: E. Eichten, I. Hinchliffe, K. Lane, and C. Quigg, *Rev. Mod. Phys.* **56**, 579 (1984).

Possible physics targets

- nonstandard heavy Higgs boson
- strong WW scattering without low-lying resonances
- few-TeV messengers of gauge-mediated SUSY breaking
- huge reach for leptoquarks, excited quarks, ...

Gilman subpanel recommendation:

> The Subpanel recommends an expanded program of R&D on cost reduction strategies, enabling technologies, and accelerator physics issues for a VLHC. These efforts should be coordinated across laboratory and university groups with the aim of identifying design concepts for an economically and technically viable facility. The scale and progress of this R&D program should be subject to additional review in about two years.

VLHC Steering Committee (http://vlhc.org)

Peter Limon & Ernie Malamud

Fermilab contacts:

Accelerator physics: S. Mishra
Accelerator systems: B. Foster
Construction & installation: J. Lach
Physics & detectors: D. Denisov

My belief:

> We need to identify compelling physics targets before any machine proposal will be taken seriously by our colleagues or the funding agencies.

Toward the VLHC

Cost reduction essential to go beyond SSC

Example *pp* machine:
$\sqrt{s} = 100$ TeV, $\mathcal{L} = 10^{34}$ cm^{-2} s^{-1}

¶ Explore magnet alternatives

- superferric (2 teslas) "transmission line"
- moderate field (7 – 8 teslas) à la LHC
- high field ($\sim$ 10 teslas)
- very high field (14 – 15 teslas)
- high-T_c superconductors for dipoles or specials

¶ Encourage appropriate conductor R & D

¶ Look for limitations to accelerator performance à la 1979 ICFA Report

¶ Optimize cost of machine: technical + conventional components

¶ Aim at a set of reference designs (but not too soon)

Be aware of evolving physics goals
and energy /luminosity tradeoffs for detectors

Inventing Fermilab's Futures

- Commission the Main Injector and make the '99 fixed-target run a success.
- Get on with Run II.
- Define Run III and make an early commitment to it.
- Make CMS a success. Keep the energy frontier at Fermilab metaphorically, if not geographically.
- Help define the *Right* e^+e^- linear collider.
- Understand the feasibility of a $\mu^+\mu^-$ collider. Explore muon storage rings as intense neutrino sources.
- Develop the technologies for a hadron collider beyond the LHC.