FNAL, 18 October '03

Concluding Talk

G. Altarelli
CERN

An incomplete summary
(detectors, accelerator technology -> Womersley;
today talks not adequately represented)
and some opinions and conclusion

The physics case for the LHC is very strong:

- Find the Higgs or what is taking its role

- In any case: the SM should fail around o(TeV)

→ Find the new physics

The NLC is motivated as a complementary tool for the same energy range

The problem is: can we afford it?
which technology?
where?

G. Altarelli

LHC Perspective

L. Maiani, Sept. 13, '03

–Commissioning 2007;

–Physics: 2007- 2022 (at least);

–Consolidation programme

–Luminosity upgrade can prolong LHC lifetime and extend its discovery potential by 15-30% in mass;

–Energy upgrading rather costly and further in the future, as it requires the development of new high field magnets (Nb3Sn?15 Tesla ?).

Upgrading the LHC

- Higher luminosity $\sim 10^{35} cm^{-2} s^{-1}$ (SLHC)
  - Needs changes in machine and particularly in the detectors
  - Start change to SLHC mode some time 2012-2014
  - Collect ~3000 fb^{-1}/experiment in 3-4 years data taking.

Cost: ~0.4B$ (~0.25B$ SPL)

- Higher energy (LHCx2)?
  - LHC can reach $\sqrt{s}$ = 15 TeV with present magnets (9T field)
  - $\sqrt{s}$ of 28 (25) TeV needs ~17 (15) T magnets ⇒ R&D needed!

Cost: ~1-2 B$

THREE PHASES:

Phase 0 – maximum performance, no hardware changes: $L = 2.3 \bullet 10^{34}\ cm^{-2} s^{-1}$

Phase 1 – maximum performance while keeping LHC arcs unchanged
Luminosity upgrade (β^*= 0.25 m, # bunches,...) → $L = 5\text{-}10 \bullet 10^{34}\ cm^{-2} s^{-1}$

Phase 2 – maximum performance with major hardware changes to the LHC
Energy (luminosity) upgrade → E_{beam} = 12.5 TeV

Summary

A. De Roeck

The LHC luminosity upgrade to 10^{35} cm^{-2}s^{-1} (SLHC)

- Allows to extend the LHC discovery mass/scale range by 25-30%
- Could allow the first measurement of Higgs self-coupling (20-30%)
- Allows further access to rear decays such as $H \rightarrow \mu\mu$, γZ, rare top decay
- Improved precision on TGCs, Higgs branching ratios,…

It will be a challenge for the experiments/needs detector R&D starting ı
especially if one wants to be ready to "go" soon after 2013-2014

In general: SLHC looks like giving a good physics return for modest cos
$\Rightarrow$ Get the maximum out of the (by then) existing machine

An LHC energy upgrade to $\sqrt{s}$ ~ 28 TeV

- Will extend the LHC mass range by factor 1.5
- Will be easier to exploit experimentally (at 10^{34} cm^{-2}s^{-1})
- Is generally more powerful than a luminosity upgrade
- Needs a new machine, magnet& machine R&D, and will not be cheap

The case for the VLHC1,2 is clearly more difficult to be formulated now

The main argument is:
it is not conceivable that the desert is so close that the LHC will solve all questions (a bit too generic)

Certainly the LHC will clarify the issue of where to go next

Before the LHC, we can only take models of new physics and for each of them say what the LHC can do and what will be left for the VLHC

S.Chivukula

Physics at 10 TeV

T. Han, S. Dawson

- Possible Views from (1 TeV)$^{-1}$
  - SUSY I. Hinchliffe
  - Strong Dynamics/Technicolor K. Lane
  - Composite/Little Higgs G. Kribs, U. Heintz
  - Extra Dimensions G. Giudice, T.Rizzo, S. Nandi
    - AdS/CFT: the desert blooms

$$\pi \frac{M_W}{\alpha_W} \simeq 7.5\ \mathrm{TeV}$$

Also: D.Rainwater, A.Ringwald

Higgs at S/VLHC

EW Instanton scale

G. Giudice

LHC is the machine to study the scale of EW breaking

NEW THEORY

Desert, e.g. conventional susy ⇨ need for precision measurements after LHC

m < TeV

Multi-TeV linear collider?

New thresholds around 10 TeV ⇨ need for energy increase to make next step of discoveries

e.g Extra Dim.
Little Higgs

VLHC ?

VLHC not meant to push new-physics limits by an order of magnitude, but to explore a well-motivated (after some LHC discoveries) energy region

SUSY is the most “desertic” option

Sofar the most complete scenario

Well compatible, actually supported by GUT’s

Well defined and computable up to M_{GUT}

Even in this case it is difficult to imagine that the LHC can finish the job

T. Han, I. Hinchliffe, S. Dawson

DESERT — G. Giudice

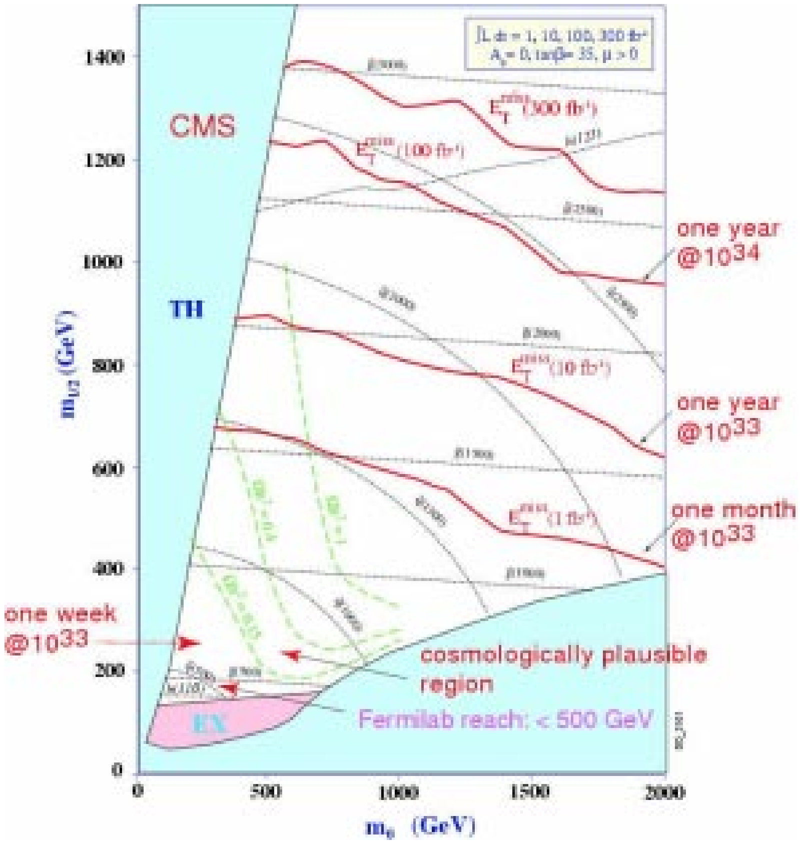
- Connection with GUT, strings, quantum gravity
- Gauge-coupling unification
- Neutrino masses
- Suppression of proton decay and flavour violations
- Setup for cosmology (inflation, baryogenesis)

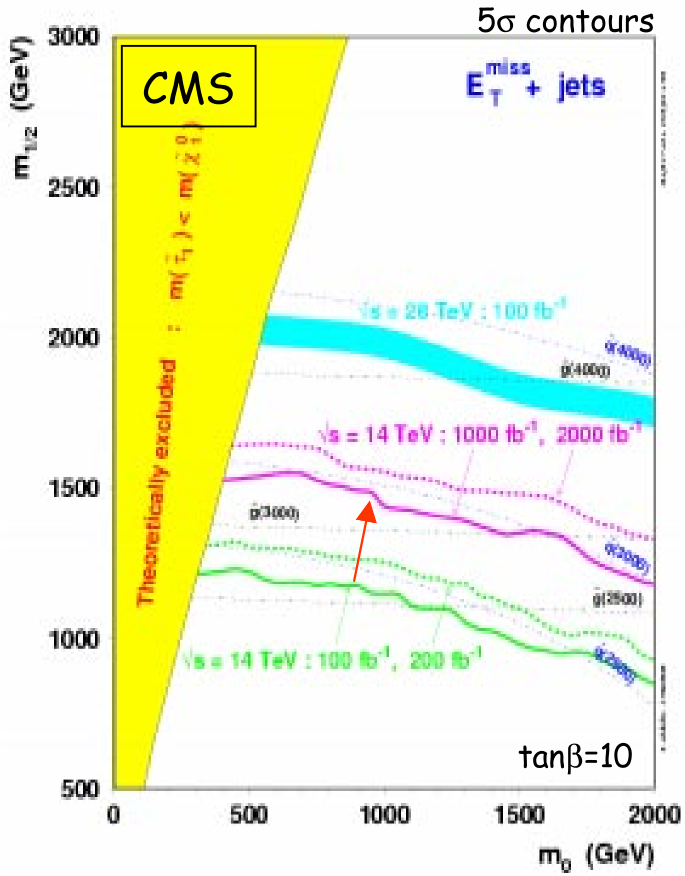
M. Turner

G. Altarelli

TeV Scale SUSY likely to be discovered at the Tevatron or LHC

S.Dawson





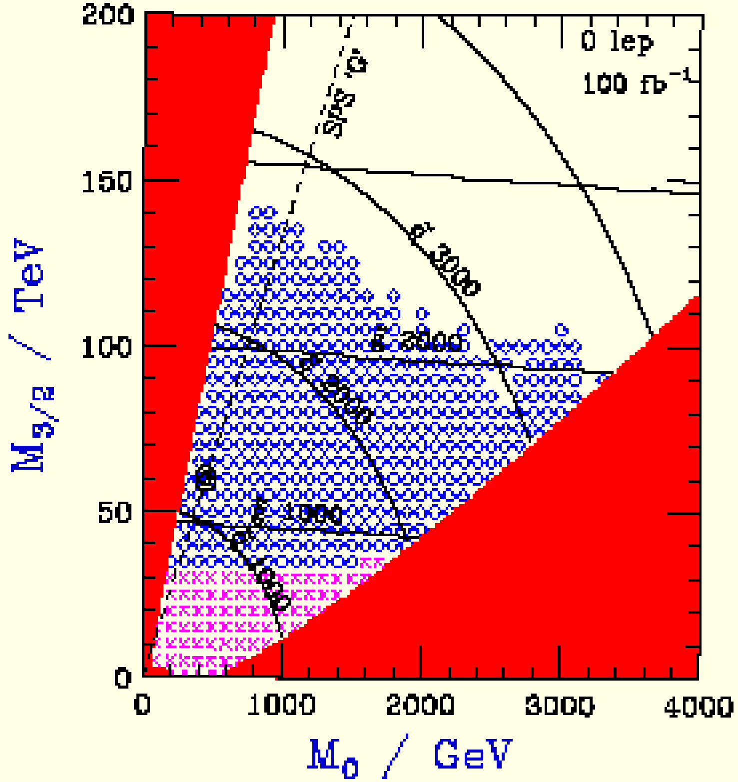
SUSY can be found with low luminosity….but what is it?

SLHC increases discovery reach by ≈ 500 GeV

Squarks and gluinos up to 3 TeV

I. Hinchliffe

Reach is similar in other models
Example of anomaly mediated model
Shaded pink region is excluded by LEP



In general reach depends mainly on $M_{\tilde{g}}$ and $M_{\tilde{q}}$ provided $M_{\tilde{\chi}_1^0} \ll M_{\tilde{g}}, M_{\tilde{q}}$
rather model independent

To improve naturalness:

T. Han

The Little Hierarchy persists:

* $m_{\tilde{q}} \gtrsim$ several TeV or $m_{\tilde{q}_i}$ highly degenerate.

- "inverted hierarchy":[†] $m_{\tilde{t}} \sim 1$ TeV; $m_{\tilde{q}_{1,2}} \gtrsim 4$ TeV.

- "focus point" scenario:[‡]
  Heavy m_0, so that $\tilde{q}$, $\tilde{l}$ several TeV.
  H_u insensitive to m_0; low μ keep the naturalness.

- "more minimal MSSM"[*]
  New scale $F \approx 5-20$ TeV, so that $m_{\tilde{t}} \ll m_{\tilde{q}_{1,2}}$.

- ... ...

Many models have
heavy particles, beyond LHC range

*Arkani-Hamed, Schmaltz; TH, Kribs, McElrath.

†Bagger, Feng, Polonsky, Zhang

‡Feng, Matchev, Moroi

*Cohen, Kaplan, Nelson.

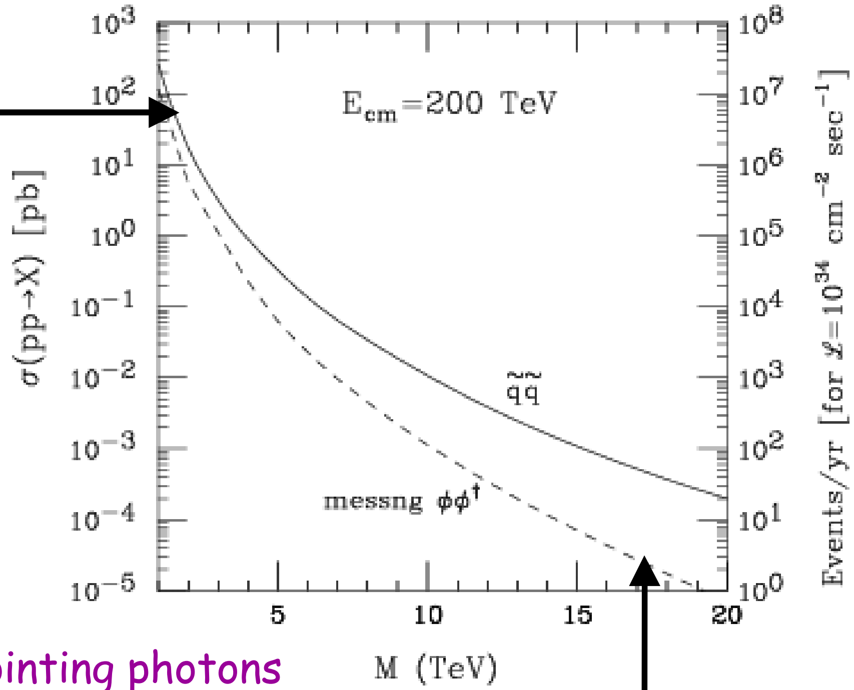
F.Gianotti

What can the VLHC do for SUSY ?

2 "compelling" examples ...

① Inverted hierarchy models : $\tilde{q}$ of first 2 families heavy (m up to ~ 20 TeV)

→ LHC+ LC observe only part of SUSY spectrum
→ VLHC can observe heavy squarks



② LHC finds GMSB SUSY :

F ≡ SUSY breaking scale (hidden sector)
M ≡ Messenger scale

SUGRA : $M=M_{Pl}$
GMSB : M ~ 10-100 TeV possible

GMSB $LSP \equiv \tilde{G}$ NLSP : $\chi^0_1 \to \gamma\,\tilde{G}$ non-pointing photons
$\tilde{\ell} \to \ell\,\tilde{G}$ kinks / displaced vertices in tracker

F can be measured from $c\tau_{NLSP} \approx 100\mu m \left[\frac{100\text{ GeV}}{m_{NLSP}}\right]^5 \left[\frac{F}{100\text{ TeV}}\right]^4$

VLHC L upgrade to 10^{35} useful

→ together with sparticle spectroscopy can constrain M to ≈%-30% (LC or LHC)

If M < 20 TeV → VLHC can observe GMSB Messenger fields Φ (e.g. $\Phi \to W/Z/\gamma + E_T^{miss}$)

Need good calorimetry (granularity, compensation), b-tag of high-p_T (dense) jets

Strong Dynamics/Technicolor

S.Chivukula

$SU(N_{TC})$ strong/confining theory,

$$\Psi_L = \begin{pmatrix} U \\ D \end{pmatrix}_L \qquad U_R, D_R$$

with massless fermions

$$\begin{aligned}\mathcal{L} &= \bar{U}_L i \not{D} U_L + \bar{U}_R i \not{D} U_R + \\ &\quad \bar{D}_L i \not{D} D_L + \bar{D}_R i \not{D} D_R\end{aligned}$$

- Pions: $\pi^{\pm}, \pi^0 \Leftrightarrow W_L^{\pm}, Z_L$

We know too much … this simplest theory is ruled out!

cf. talk by Ken Lane

In modern versions of technicolor theories (walking technicolor, topcolor…)

K. Lane

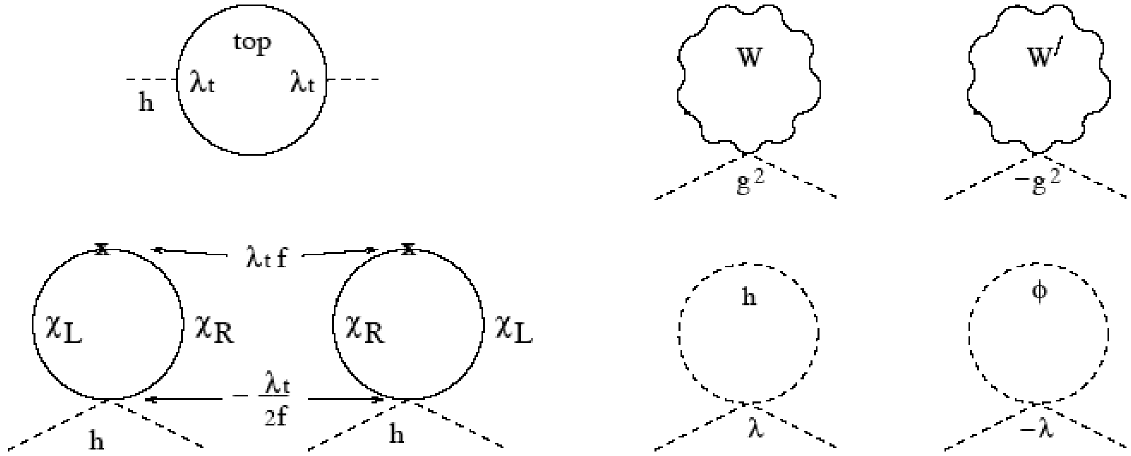
LHC: lowest fringes of the spectrum
technipions, lightest technirho….

VLHC: the bulk of the spectrum up to ~100 TeV

Interesting new perspective:
connection AdS/CFT ⟷ walking technicolor

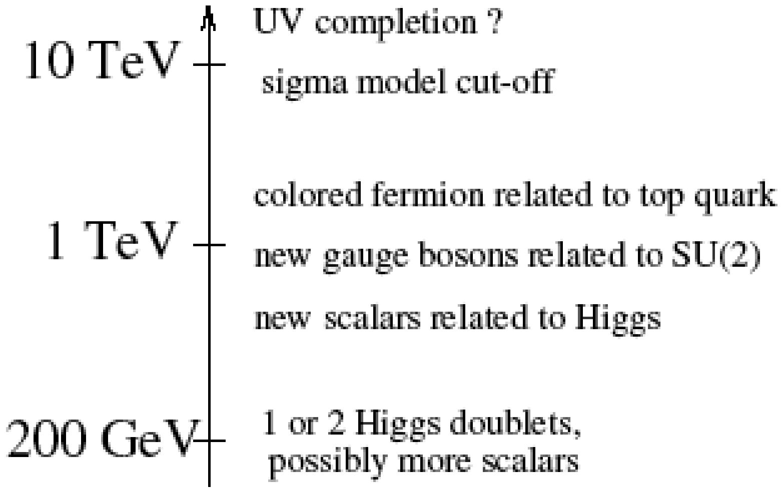
Composite/Little Higgs

S.Chivukula



VLHC

LHC



cf. talk by Graham Kribs

From M.Schmaltz hep-ph/0210415

S.Chivukula

Extra Dimensions

Small Dimensions: Warped Geometry[†]

Hierarchy from 5-d AdS geometry:

$$ds^2 = e^{-2kr_c|\varphi|} d^2x_4 + r_c^2 d\varphi^2 \ ,$$

gravity localized at $\varphi = 0$, SM at $\varphi = \pi$.

Yields cutoff $\Lambda_\pi \simeq M_{Pl}\, e^{-kr_c\pi}$ for SM!

As inspired by AdS/CFT conjecture:
a weakly coupled QFT in this space
dual to one in 4-d coupled to a large-N
strongly-coupled conformal field theory….

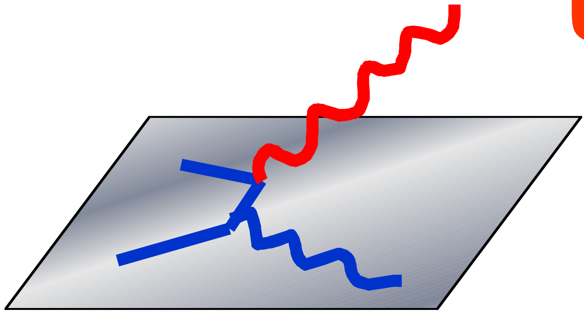
"Higgless" Symmetry Breaking = Walking Technicolor

cf. talk by Gian Giudice

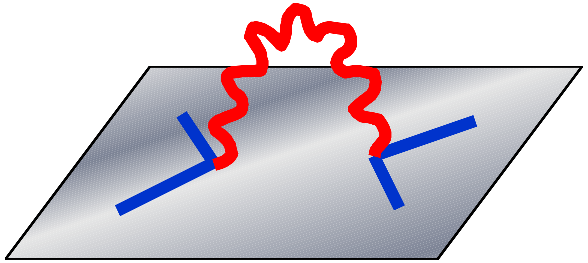
† Randall & Sundrum, hep-ph/9905221
Csaki, et. al. hep-ph/0305237, 0308038

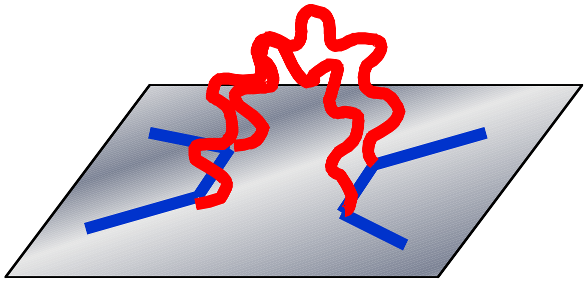
G. Giudice

QUANTUM GRAVITY AT LHC



Graviton emission
- Missing energy (flat)
- Resonances (warped)

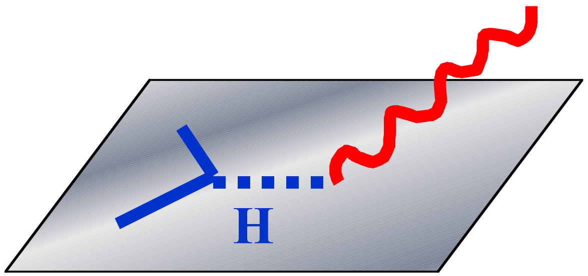




$$\frac{1}{\Lambda^4} T_{\mu\nu} T^{\mu\nu}$$

$$\frac{1}{\Lambda^2} \left(\bar{f} \gamma_\mu \gamma_5 f\right)^2$$

Contact interactions
(loop dominates over tree if gravity is strong)



Higgs-radion mixing

G. Giudice

2-jets with large M_{inv} and $\Delta\eta$
Black holes

VLHC

Transplanckian

Semi-classical approximation

QUANTUM GRAVITY

Cisplanckian

Linearized gravity

LHC

Jets + missing E_T
2-leptons

G. Giudice

Very interesting ideas:

Symmetry breaking from extra dimensions:

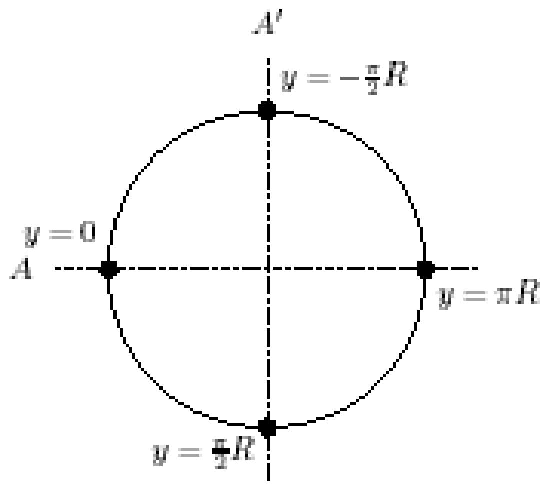
- SUSY breaking
- The Higgs as fifth component of gauge field

Extra dimensions could be an ingredient of a complex picture

G. Giudice

SUPERSYMMETRY BREAKING: AN INTERESTING EXAMPLE

Barbieri-Hall-Nomura



5D SM compactified on $S^1/(Z_2\_Z_2)$

- Different susy breaking at each boundary

→effective theory non-susy

(susy recovered at $d<R^{-1}$)

- Higgs boson mass (rather) insensitive to UV

$m_H = 127 \pm 10$ GeV

G. Giudice

CONCLUSIONS

• Extra dimensions ubiquitous ingredient in non-desert scenarios

• Physics goals of VLHC quite distinct from those of LHC

Examples:

• "Need to test the theory well above the EW breaking scale"

Transplanckian physics: new energy regime to test extra-dim gravity

• "Existence of new thresholds (new physics, not just some more KK) in the 10 TeV region"

Extra-dim theories of EW breaking require UV completion at a scale not far from EW

M. Mangano

A progress report on MC's for hadron colliders

- New techniques and new codes to describe, at LO, complex multijet final states:
  - most SM processes with up to 8 final-state hard objects (partons, gauge bosons, leptons) can be calculated
  - new techniques available to consistently merge these calculations with shower MC's
- New techniques and new codes for NLO, parton-level, event generators
  - most SM processes with 2 and some with 3 final-state hard objects (3 jets, W/Z+2jets) are calculated and encoded
- New technique and new code to consistently merge NLO calculations with full shower and hadronization evolution:
  - so far available for single and double gauge boson production, and for heavy quark pairs
- C++ versions of main shower MC's being readied, improved descriptions of shower evolution, hadronization, etc etc

Very active area of research, amazing new achievements and very powerful tools being developed for Tevatron and LHC

M. Mangano

On the role of NLO, NNLO, ...

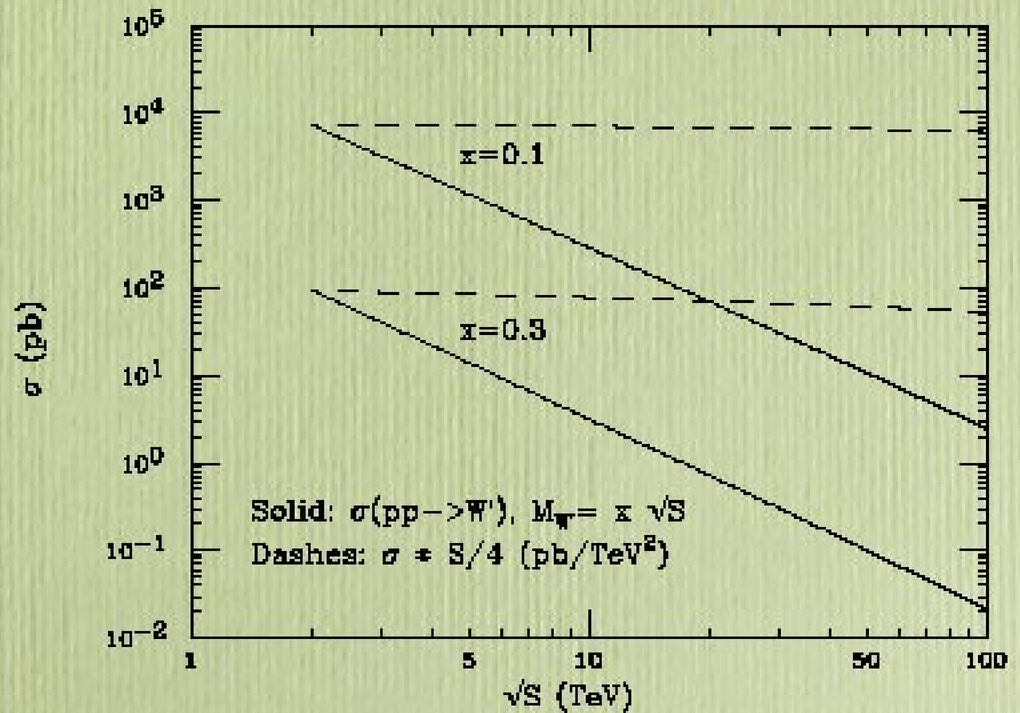
- (N)NLO calculations are essential to extract reliable estimates of the total production rates
- It is highly non-trivial, however, to establish an accurate connection between what is calculated and what is observed.
- QCD physics at LEP taught us that the concept of IR and collinear safety, while essential to justify the use of fixed-order perturbative calculations, does not guarantee the accuracy of such calculations.
- The impact of power corrections, as well as of the resummation of large logs, is crucial for a faithful description of the data. This is true even at high-Q
- **A balance between perturbative accuracy and realism in the description of the physical observables (e.g. in the description of the structure of an experimental jet) is mandatory**

NLO results are available today for most processes of interest. The technique by Frixione and Webber allows their consistent merging with shower MC's. Extension to NNLO is far from being even just theoretically formulated, let alone numerically implemented.

M. Mangano

Luminosity vs Energy: an example, W' production

$$\sigma = \frac{A}{M^2}\int_{\tau=M^2/S}^{1}\frac{dx}{x} f_1(x,M^2)\, f_2(\tau/x,M^2) = \begin{cases} \log(S) & \text{for } M = \text{constant} \\ 1/S & \text{for } M^2/S = \text{constant} \end{cases}$$



The high-mass frontier requires L∝S even in hadronic collisions

M. Mangano

Conclusions

- MC development is in good health, lots of recent progress, several young and active new players
- Not enough known about the physics and detectors of a VLHC to give a clear ranking of priorities and difficulties
- Critical evaluation of what are the areas in major need of development, to ensure uniform distribution of systematic uncertainties across the different sources
- On the short term, validation against data from the Tevatron should be given high priority in the analysis plans:
  - structure of UE
  - multiparton correlations
  - description of jet structures, radiation patterns in multijet final states
- Develop analysis strategies to extract the information not available from lower energy (e.g. PDFs in new domains of (x,Q))
- At the LHC, devote enough run time at low luminosity ($10^{31\div32}$) to
  - test the quality of extrapolation from 2TeV
  - determine the missing information

Summary of reach and comparison of various machines

F. Gianotti

Only a few examples ....

In many cases numbers are just indications ....

Units are TeV (except $W_L W_L$ reach)

ILdt correspond to 1 year of running at nominal luminosity for 1 experiment

PROCESS	LHC 14 TeV 100 fb^{-1}	SLHC 14 TeV 1000 fb^{-1}	28 TeV 100 fb^{-1}	VLHC 40 TeV 100 fb^{-1}	VLHC 200 TeV 100 fb^{-1}	LC 0.8 TeV 500 fb^{-1}	LC 5 TeV 1000 fb^{-1}
Squarks	2.5	3	4	5	20	0.4	2.5
$W_L W_L$	2σ	4σ	4.5σ	7σ	18σ	6σ	90σ
Z′	5	6	8	11	35	8†	30†
Extra-dim (δ=2)	9	12	15	25	65	5-8.5†	30-55†
q*	6.5	7.5	9.5	13	75	0.8	5
Λ compositeness	30	40	40	50	100	100	400

† indirect reach (from precision measurements)

Approximate mass reach of pp machines:

- $\sqrt{s}$ = 14 TeV, $L=10^{34}$ (LHC) : up to ≈ 6.5 TeV
- $\sqrt{s}$ = 14 TeV, $L=10^{35}$ (SLHC) : up to ≈ 8 TeV
- $\sqrt{s}$ = 28 TeV, $L=10^{34}$: up to ≈ 10 TeV
- $\sqrt{s}$ = 40 TeV, $L=10^{34}$ (VLHC-I) : up to ≈ 13 TeV
- $\sqrt{s}$ = 200 TeV, $L=10^{34}$ (VLHC-II) : up to ≈ 75 TeV

VLHC 200 TeV: probes directly up to ~100 TeV with ultimate luminosity

LC 5 TeV: probes indirectly up to ~1000 TeV with ultimate luminosity

❸ **Examples of possible compelling scenarios for a VLHC emerging from LHC data ....**

F. Gianotti

LHC finds some SUSY particles but no squarks of first two generations (as in inverted hierarchy models)

$\rightarrow$ VLHC would observe heaviest part of the spectrum

LHC finds GMSB SUSY with Messenger scale M < 20 TeV

$\rightarrow$ VLHC would probe directly scale M and observe Messenger fields

LHC finds contact interactions $\rightarrow \Lambda < 60$ TeV

$\rightarrow$ VLHC would probe directly scale Λ and observe e.g. q*

LHC finds ADD Extra-dimensions $\rightarrow M_D \leq 10$ TeV

$\rightarrow$ VLHC would probe directly gravity scale M_D and above (e.g. observe black holes)

LHC finds hints of strong EWSB

$\rightarrow$ VLHC would see a clear signal and could observe massive particles associated with new dynamics

P. Limon

Conclusions (1)

- A staged VLHC starting with 40 TeV and upgrading to 200 TeV in the same tunnel is, technically, completely feasible.

- There are no serious technical obstacles to the Stage-1 VLHC at 40 TeV and 10^{34} luminosity.
  - The existing Fermilab accelerator complex is an adequate injector for the Stage-1 VLHC, but lower emittance would be better. (We should take this into account if Fermilab builds a high-power injector. Low emittance is important!)
  - VLHC operating cost is moderate, using only 20 MW of refrigeration power, comparable to the Tevatron.
  - Improvements and cost savings can be gained through a vigorous R&D program in magnets and underground construction.

P. Limon

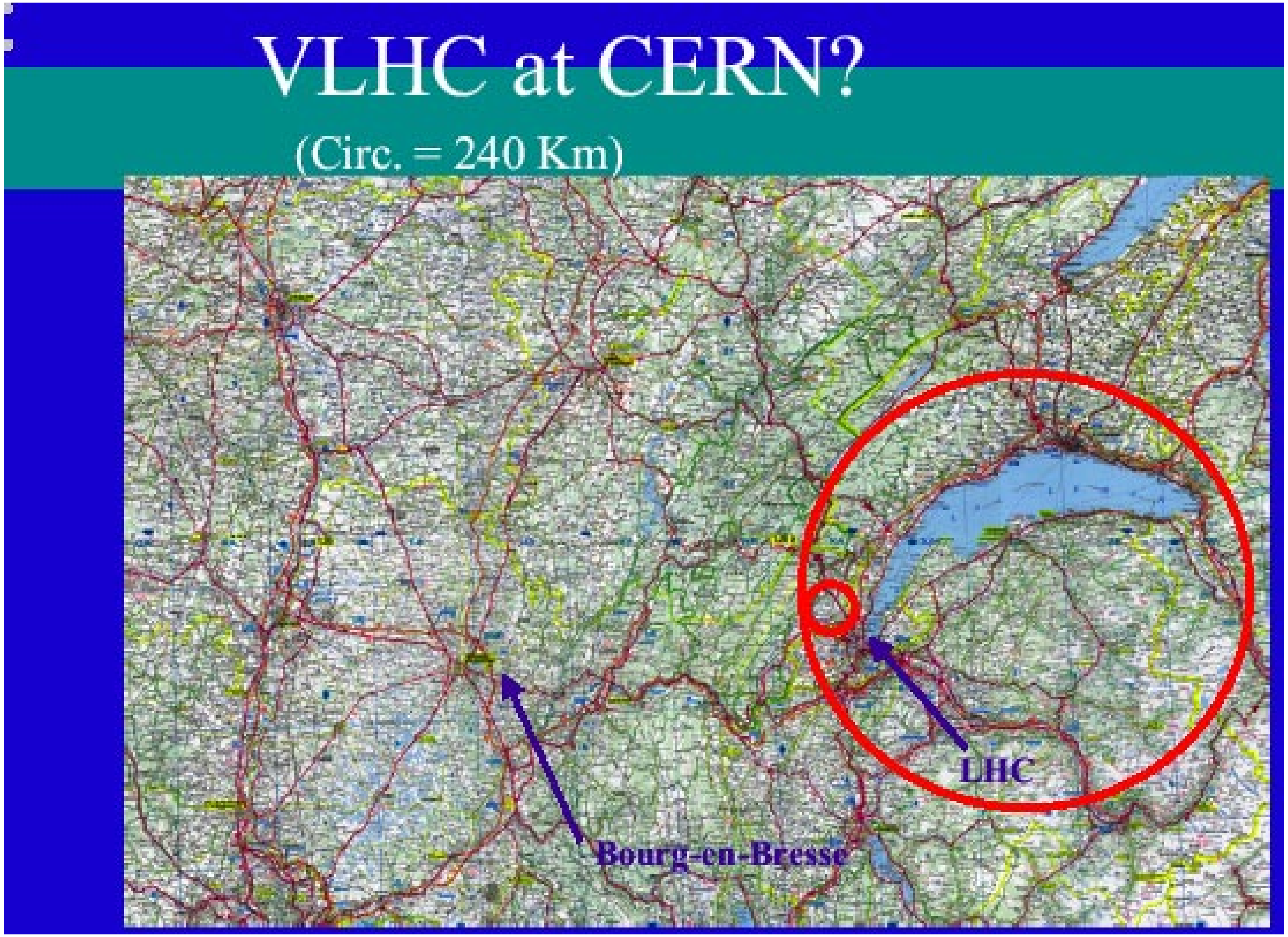
Conclusions (2)

- The construction cost of the first stage of a VLHC is comparable to that of a linear electron collider, ~ $4 billion using “European” accounting.
  - From this and previous studies, we note that the cost of a collider of energy near 40 TeV is almost independent of magnetic field.
  - A total construction time of 10 years for Stage-1 is feasible, but the logistics will be complex.
  - Making a large tunnel is possible in the Fermilab area. Managing such a large construction project will be a challenge.
  - Building the VLHC at an existing hadron accelerator lab saves significant money and time.

P. Limon

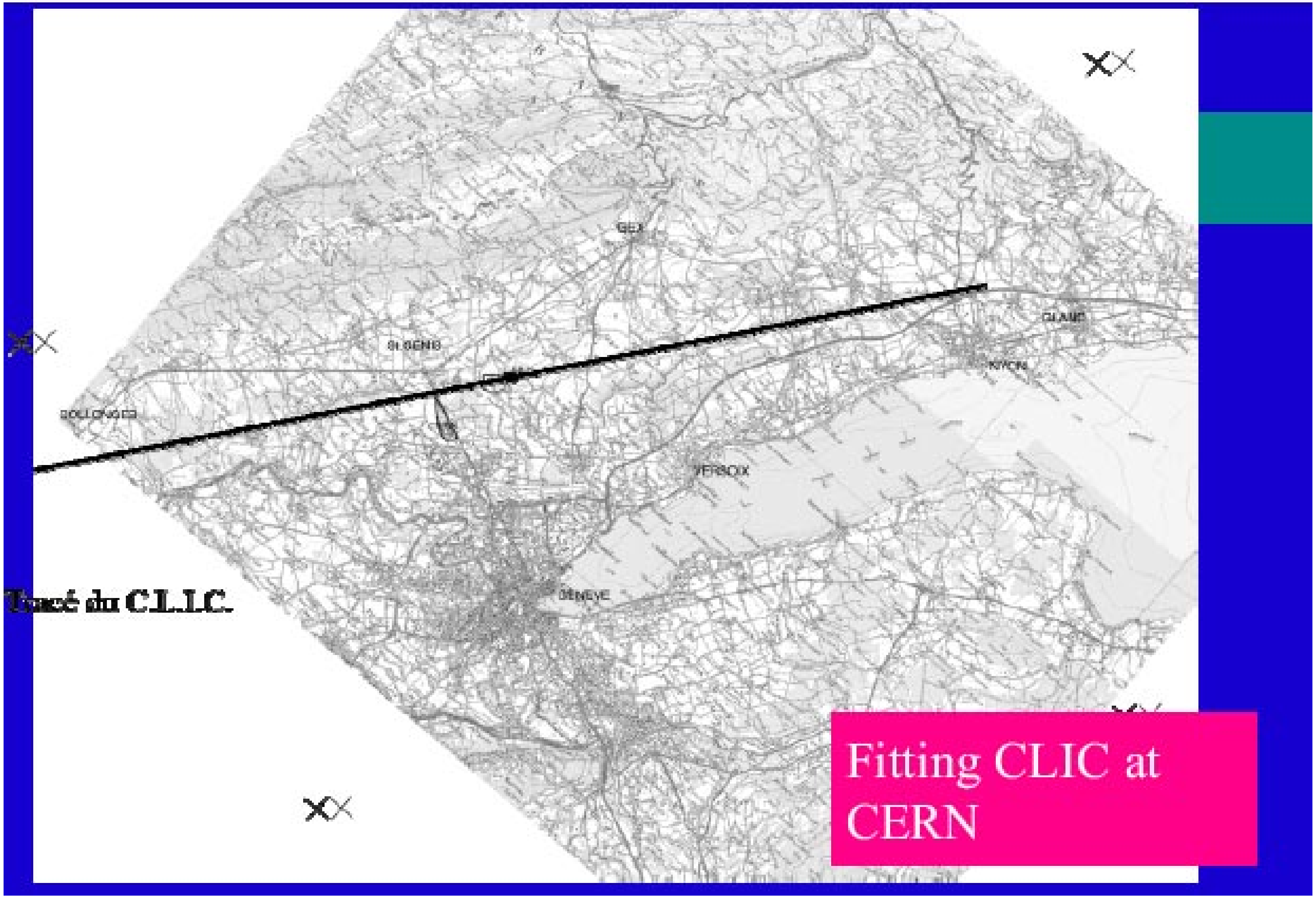
Conclusions (3)

- The Stage 2 VLHC can reach 200 TeV and 2×10^{34} or possibly significantly more in the 233 km tunnel.
  - A large-circumference ring is a great advantage for the high-energy Stage-2 collider. A small-circumference high-energy VLHC may not be realistic.
  - There is the need for magnet and vacuum R&D to demonstrate feasibility and to reduce cost.
- Result of work completed after the “Study.”
  - For very high energy colliders, very high magnetic fields (B>12T) are not the best solution.



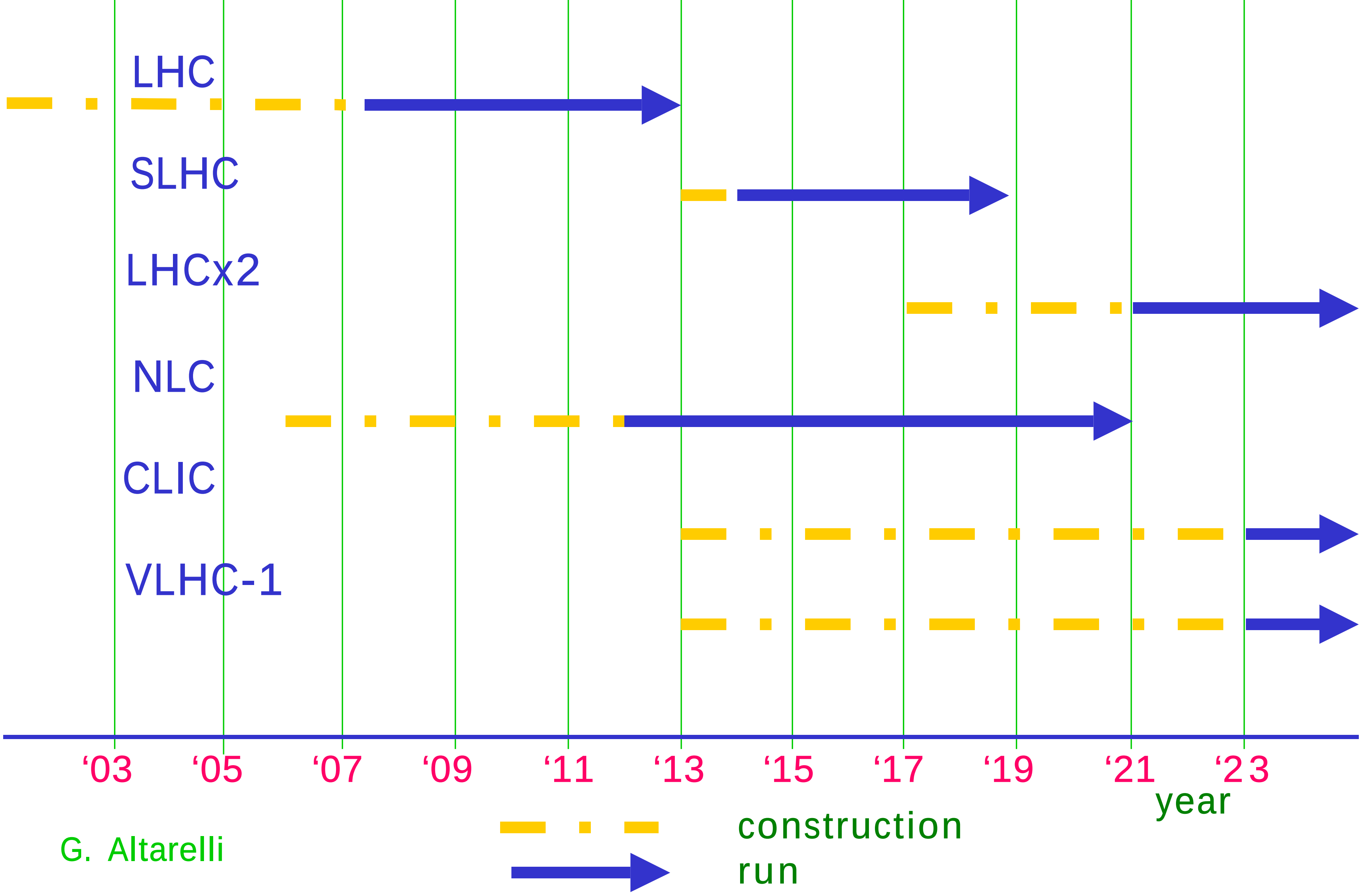
Not the right environment!

G. Altarelli



G. Altarelli

An (optimistic) sketch of time scales



G. Altarelli

Money constraints

The total money spent at present in hep labs in the world is ~ 2-2.5 B$/year (~3 CERN budgets)

Money that can be invested in new machines is ~30%

~ 6-7.5 B$/10 years

Cost of NLC ~ CLIC ~ VLHC-1 ~ 4-4.5 B$, EU accounting, no detectors, no contingency (for comparison LHC~2.5 B$)

Considering

- R&D, detectors, computing,....
- LHC is being paid till '10
- other physics (neutrinos ,p-decay, factories ...),

Either a problem of compatibility or long time scales ≥ 25 years

G. Altarelli

With tight resources and long time scales an efficient decision making would be needed.

Lack of adequate R&D funding is also a serious problem

But the large dimension of the projects need a world-wide collaboration which is difficult to aggregate

e.g. the slow evolution of the NLC case:
it is vital to reach a consensus fast
(or everything else will be blocked)

A year ago:

M. Whiterell
ICFA-CERN Oct.'02

Linear Collider R&D

The HEPAP Subpanel:

- ***We recommend that the highest priority of the U.S. program be a high-energy, high-luminosity, electron-positron linear collider, wherever it is built in the world.***
- ***We recommend that the U.S. prepare to bid to host the linear collider, in a facility that is international from the inception, with a broad mandate in fundamental physics research and accelerator development.***
- Fermilab is working to follow these recommendations, but is limited by special funding constraints.
- We are also working with other laboratories around the world on how to organize an international linear collider. (Much more about this later)

M. Whiterell
ICFA-CERN Oct.'02

Other Future Accelerator R&D

VLHC and Superconducting Magnet R&D

- Fermilab has a special role in maintaining core strength in this area.
- We are concentrating effort on high-field magnet program.
- We ar developing plans for R&D on next generation LHC low-beta quads.

Neutrino Factory R&D

- Collaboration with US universities and other laboratories on muon cooling experiments
- Planning with international partners on R&D for next steps

P. Limon

The HEP Plan

- We do not have a viable strategy for the survival of HEP.
  - **A global scrap over a linear collider does not constitute a strategy.**
    - There has been some recent progress in formulating a path to a linear collider technology decision.
- We do not even have a plan to make a plan.
  - **In the U.S., for example, the HEPAP recommendation to create a mechanism to formulate a coherent strategy has become the narrowly-focused P5.**
- HEP must change the way it does things if it is going to survive!

P. Limon

A Word About R&D

- The machines we are talking about are very costly and very complex.
  - Mistakes and delays are potentially very damaging financially, politically and scientifically.
  - It takes longer than you think to develop the components of a cutting-edge collider.
- The R&D investment for future HEP instruments will be much greater than we are accustomed to.

Europe has a roadmap → L. Maiani, Sept '03

2. CERN future: LC & other projects

•A sub-TeV e^+e^- collider is needed for precision Higgs boson physics

•Useful to distinguish SM from Minimal Supersymmetric SM;

•Multi TeV capability needed to really sort out Supersymmetry.... …
or any other Physics beyond the SM

- It is **not** in the interest of Europe to offer a site for a subTeV LC
  - LC is complementary to the LHC …and is in the same energy range;
  - HEP is a global enterprise: other regions sharing efforts and benefits is crucial for its vitality;
  - Doing the LHC, Europe **simply cannot afford** being a major shareholder also for the LC.
- Europe should **define soon** the extent of its participation in a subTeV Int. LC
  - a minority participation (10 %?)
  - **not all taken from CERN budget from 2011 onwards** (!!)...

CERN future: LC & other projects (cont'd)

- … so as to allow **intermediate scale projects to start**, using the infrastructures in allied Labs (EU, Russia, US) and at CERN, which have been instrumental to build the LHC (+ ISTC?):
  - @ **CERN**: Superconducting Proton Linac (νs, β-beams, nucl. phys.);
  - @ **DESY**: Free Electron Laser (Chem. and Biolog. applications) with TESLA technology.
- These projects will establish closer links between Accelerator Particle Physics and wide scientific communities:
  - BioChem (the dream of Björn Wiik)
  - and to Nucl. Phys. (as pioneered by Carlo Rubbia)…
  - In addition to Data GRID.
- CERN **has to participate** in AstroParticle Physics projects (choose one !):
  - Space physics (as European basis for detector integration), e.g. EUSO
  - Deep Underwater Neutrino telescopes (NESTOR/ANTARES…)
  - Auger in Northern Hemisphere
  - …??.

…in the longer term

- In 2009 (2007, if some extra resources are found) CTF3 will be able to tell if standing feasibility issues of CLIC can be solved (R1 issues);
- Around 2012 (2010), CERN should be able to launch a MultiTeV Global LC, based on CLIC technology;

- CLIC can be staged from lower energy (if no subTeV LC yet decided);
- The energy doubling of the LHC based on High Field Magnets may be a (alternative?) option to be seriously considered !

- Physics at the new facility could start around 2022-2027, i.e. about 15-20 years after the LHC commissioning

3. Summarising

i.e. Maiani's

My very personal conclusions:

- Default:
  - LHC
  - Lab consolidation
  - LHC luminosity upgrade
- Active but restricted EU and CERN participation to subTeV LC;
- Intermediate projects (SPL, FEL) made in a coordinated way by a network of allied HEP Labs;
- CERN into AstroParticle (space? underwater?Auger2?...)

Prepare now for MultiTeV in the 2020's: CLIC - or LHCx2

Europe and CERN have perhaps a clear road map

Japan is investing on neutrinos

The US should decide their way

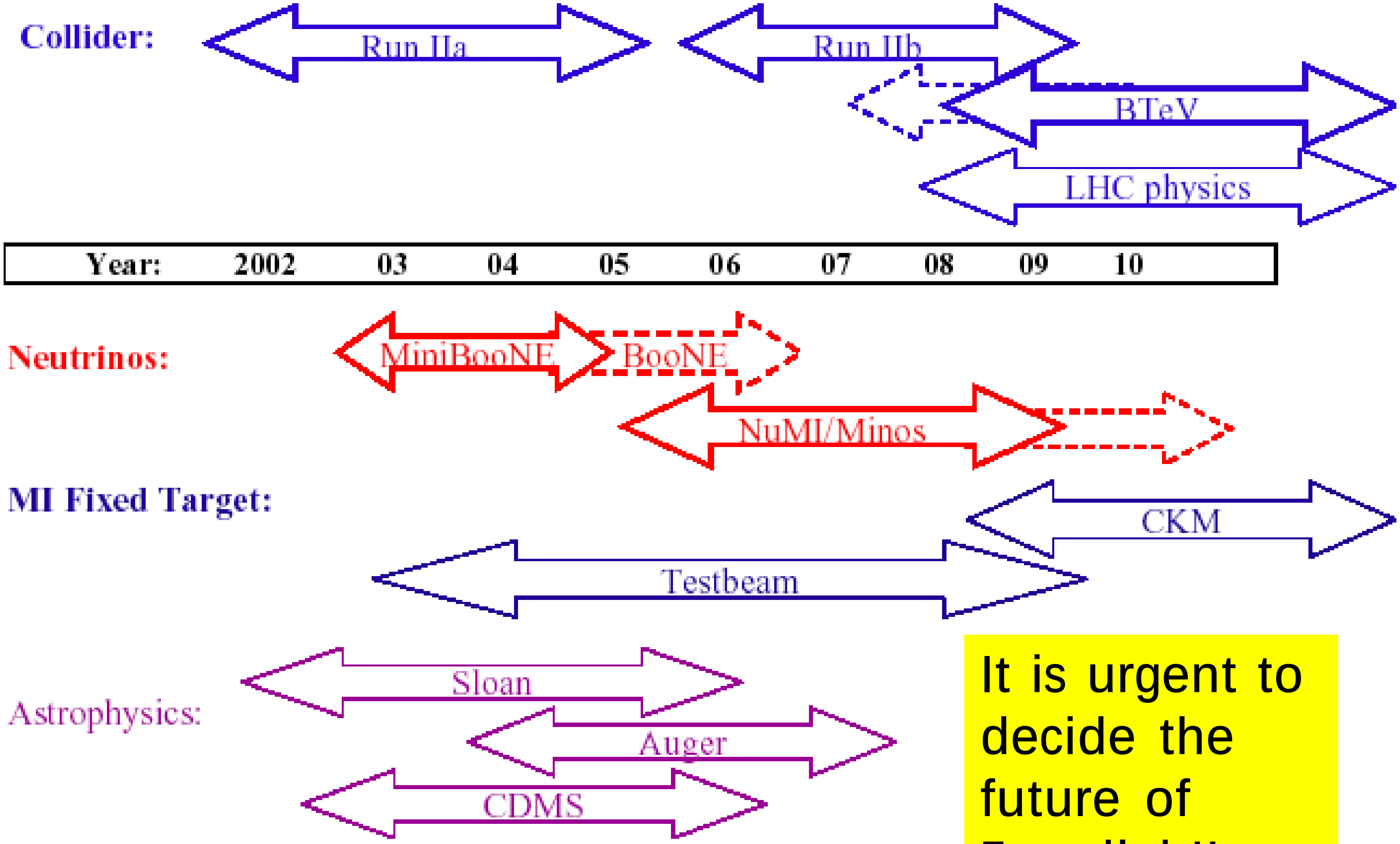
e.g. clarify the future of Fermilab

CERN: LHC, CLIC
US: NLC, VLHC

?

Fermilab Research Program

M. Whiterell
Oct.'02



It is urgent to decide the future of Fermilab!!

My personal roadmap:

to live long enough to see the VLHC start

I think that a big project in the US is crucial for the continuation of particle physics (also in Europe)

I hope that a strategy on the NLC, on the future of Fermilab will soon be formulated

A vigorous R&D programme should be continued and funded adequately

Finally I think that the case for fundamental physics (particle physics & cosmology) is strong beyond fashion and that we should defend it proudly and confidently

On behalf of all the partecipants:

I thank the Organisers of this very stimulating Workshop