Physics goals for future hadron accelerators

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What do we know at the start of the LHC?

The Standard Model (SM):

- Relativistic quantum field theory (only known framework to formulate a theory consistent with QM and Special Relativity)
- Local internal (gauge) symmetries, governed by a SU(3)xSU(2)xU(1) algebra, leading to strong and electroweak interactions mediated by gluons, W/Z and photons
- Three families of quarks and leptons, with
  - a diverse mass spectrum: **10^{-10} GeV** (ν) → **10^{2} GeV** (top, W/Z)
  - transitions between quarks generations, with violation of CP
  - transitions between lepton generations only observed so far in the neutrino sector

Overall extremely complete and successfull description of known phenomena

Which issues are open for the LHC and beyond?

To start with:

- Identify the Higgs boson, namely the particle responsible for the breaking of the SU(2)xU(1) (a.k.a. electroweak, EW) symmetry and for the generation of mass for W/Z bosons, and for quarks and leptons
- Explore the nature of the Higgs boson, and establish the detailed dynamical mechanism with which the EW symmetry is broken (EWSB):
  - pure SM?
  - Supersymmetry?
  - Extra dimensions?
  - Higgsless?
  - ... ?

- Observing the Higgs boson will seal the SM box, the solid platform standing on which we'll be looking for new horizons and intellectual challenges, which will define the future of HEP:

  - what is **Dark Matter** ?
  - origin of the Baryon Asymmetry of the Universe?
  - ......

→ well posed questions within the SM, but no positive answer

  - why SU(3)xSU(2)xU(1)?
  - why 3 generations, why their properties?
    - mass spectra
    - mixing patterns

→ no way to formulate as mathematical problems within the SM

  - .....
  - why gravity? why D=3+1?
  - .....

→ no way to formulate as mathematical problems within standard quantum field theory

- There are many good ***theoretical*** arguments suggesting that the SM is incomplete or additional structures are required:
  - understanding of quantum gravity
  - hierarchy problem, naturalness of the EW scale
  - couplings' unification at the GUT scale
- **Neutrino masses,** as well as **DM** and **BAU**, provide concrete ***experimental*** indications that we're missing something

➡ Regardless of our personal level of pragmatism and indifference towards theoretical speculations, as scientists **we have to accept the existence of physics Beyond the Standard Model (BSM)**, and as HEP physicists we have a duty to search for it.

- Formulating **plausible** and **calculable** BSM scenarios is today the best we can do to help establish directions and priorities for the field.

What will be the main driving theme of the exploration of new physics ?

the gauge sector
(Higgs, EWSB)

the flavour sector
(ν mixings, CPV, FCNC, EDM, LFV)

The High Energy Frontier

LHC
SLHC
VLHC
LC
CLIC
....

The High Intensity Frontier

Neutrinos:
super beams
beta-beams
ν factory

Charged leptons
stopped μ
$\ell \to \ell'$ conversion
e/μ EDM

Quarks:
B factories
K factories
n EDM

Physics objectives and objects for HE colliders

- Discovery of new phenomena:
  - high-Et observables:
    - direct: production of new particles, M up to 20÷30% E_{beam}
    - indirect: anomalies in expected spectra, e.g. high-Et jets
  - low-Et observables:
    - low-rate production of "light" objects (e.g. HH production)
    - rare decays ($H \to \mu\mu$, $B_s \to \mu\mu$, $t \to Zc$, ...)
  - Notice: final states of very massive objects will still be often dominated by few-100 GeV observables:
    - massive SUSY states will cascade decay
    - W/Z/H/top are often part of the decay chains, and the reconstruction of dijet inv masses in the 100-200 GeV range will be crucial
- Precision measurements:
  - SM parameters
  - BSM parameters (masses, couplings)
  - SM dynamics above EW scale (e.g. WW scattering)

High E

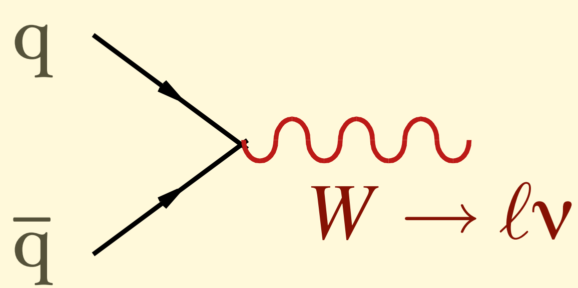
High L, clean environment

High L, clean environment

High E

Energy vs Luminosity

Example: production of new W-like gauge bosons of mass M



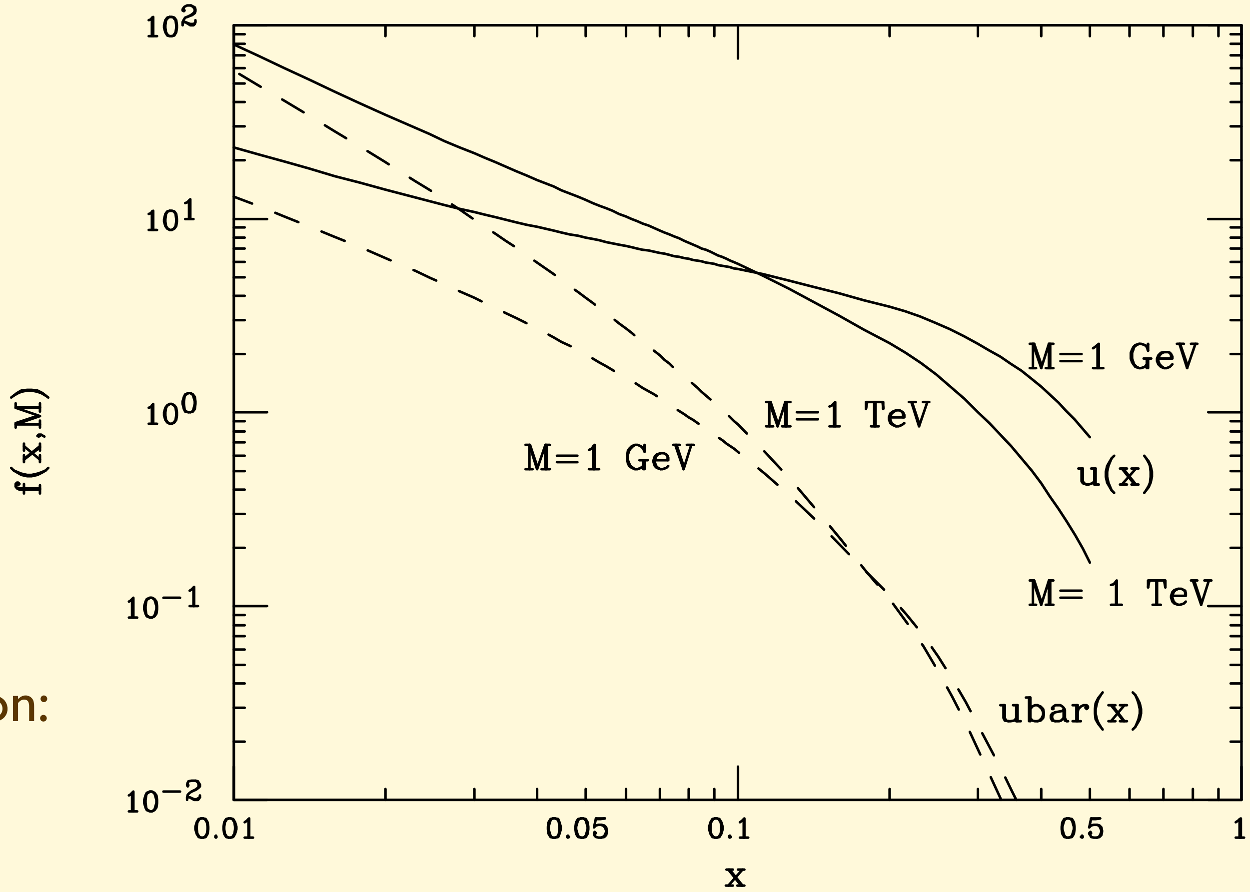
$$\sigma = \frac{A}{M^2} \int_{\tau=M^2/S}^{1} \frac{dx}{x} f_q(x, M^2)\, f_{\bar{q}}(\tau/x, M^2) \equiv \frac{A}{M^2} L(\tau)$$

f(x,M): density of quarks carrying a faction **x** of the proton momentum, in a collision leading to a quark-antiquark CM energy M:

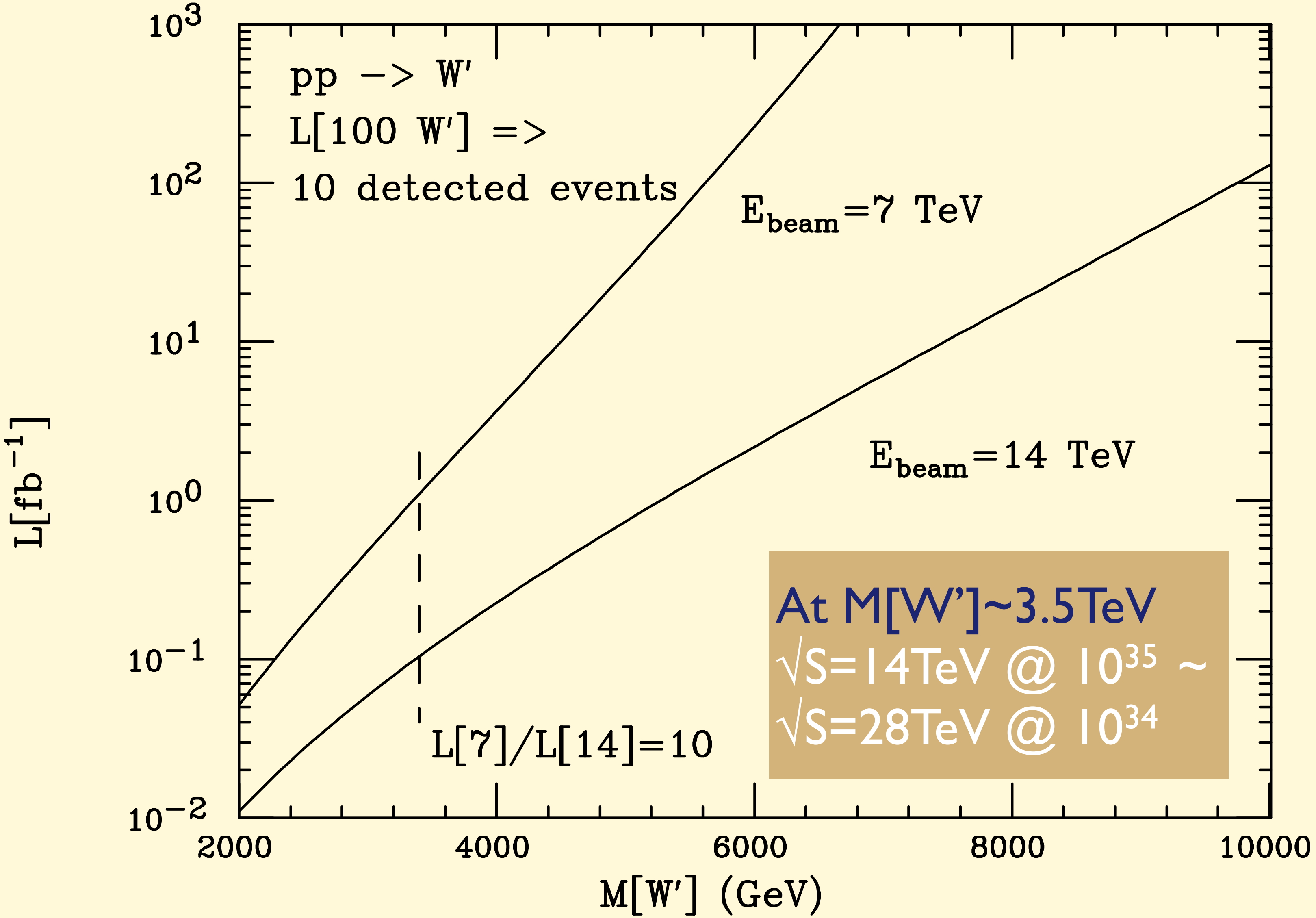
$$M = 2\sqrt{x_1 x_2}\, E_{beam}$$

with, for a central q-qbar collision:

$$M = 2xE_{beam}$$



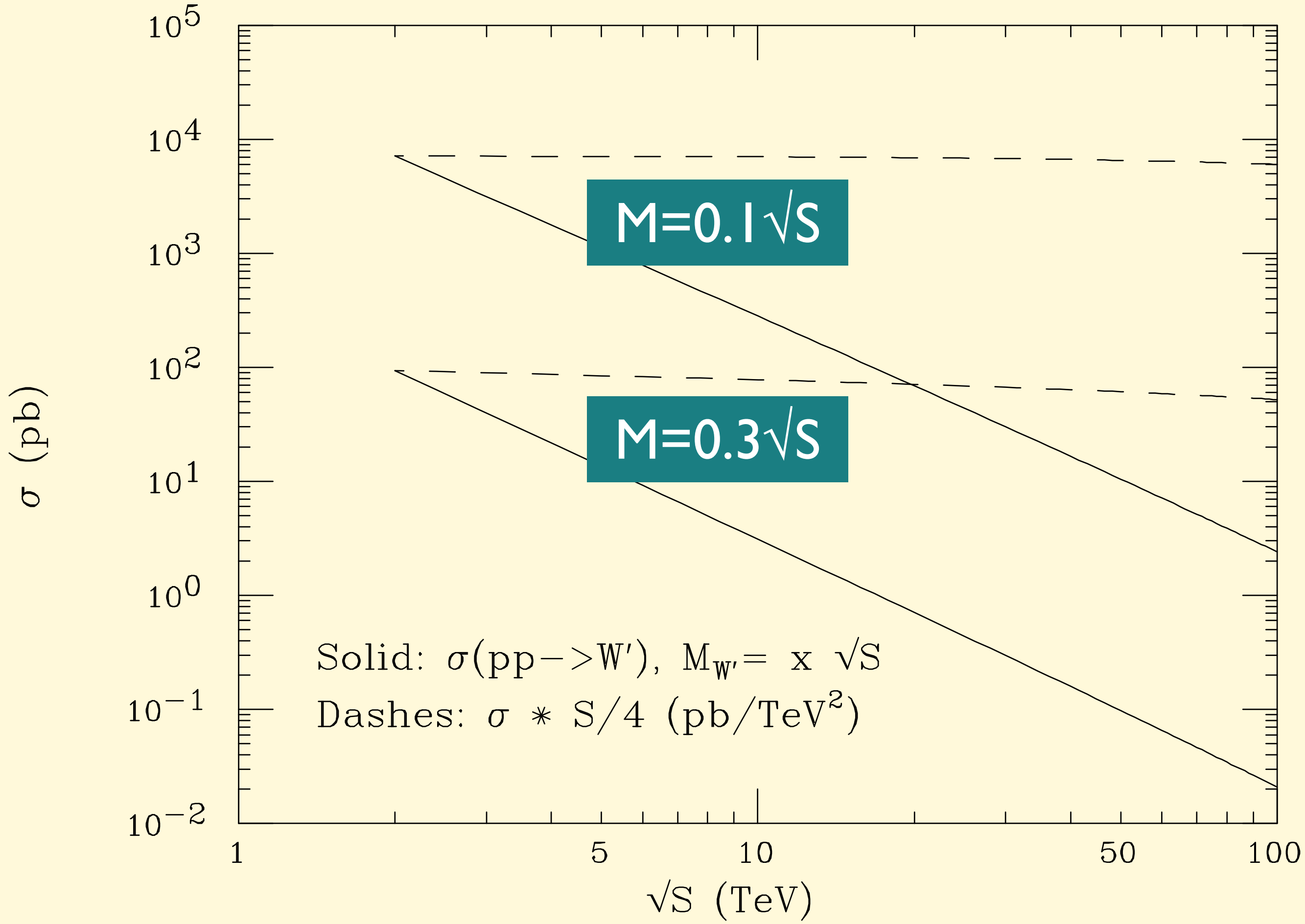
At low mass, the energy-dependence of the cross section is weaker, and a factor x10 in Lum is better than a factor of x2 in Ebeam



At high masses, the E upgrade is essential

Luminosity vs Energy: an example, W' production

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

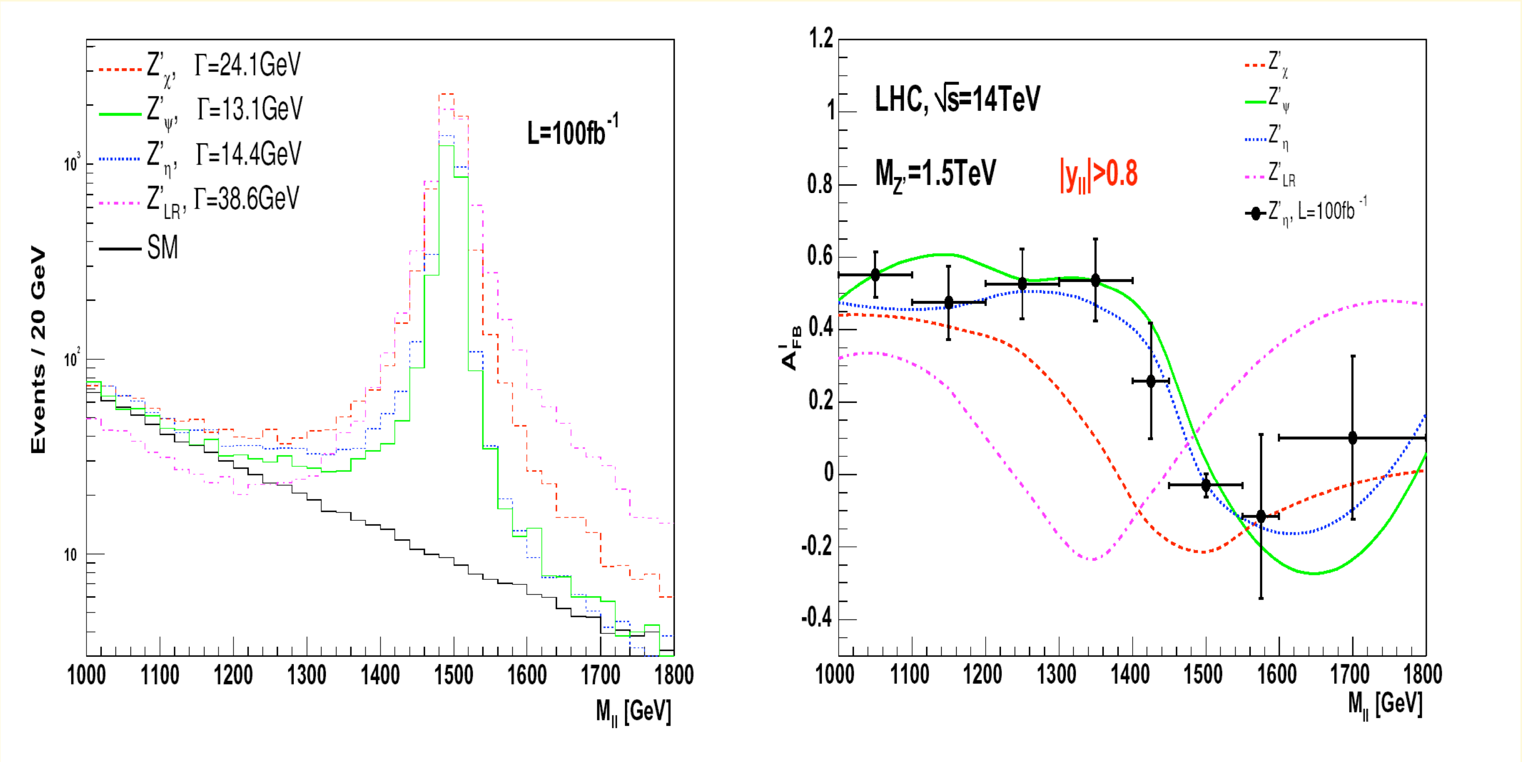


The high-mass frontier ultimately requires L∝S

SppS: 630, 10^{31} => Tevatron: 2TeV, 10^{32} => LHC 14TeV 10^{34}

The precise point at which the energy upgrade becomes more desirable than the luminosity upgrade depends on the specific process

Ex: Differentiating among different Z' models:



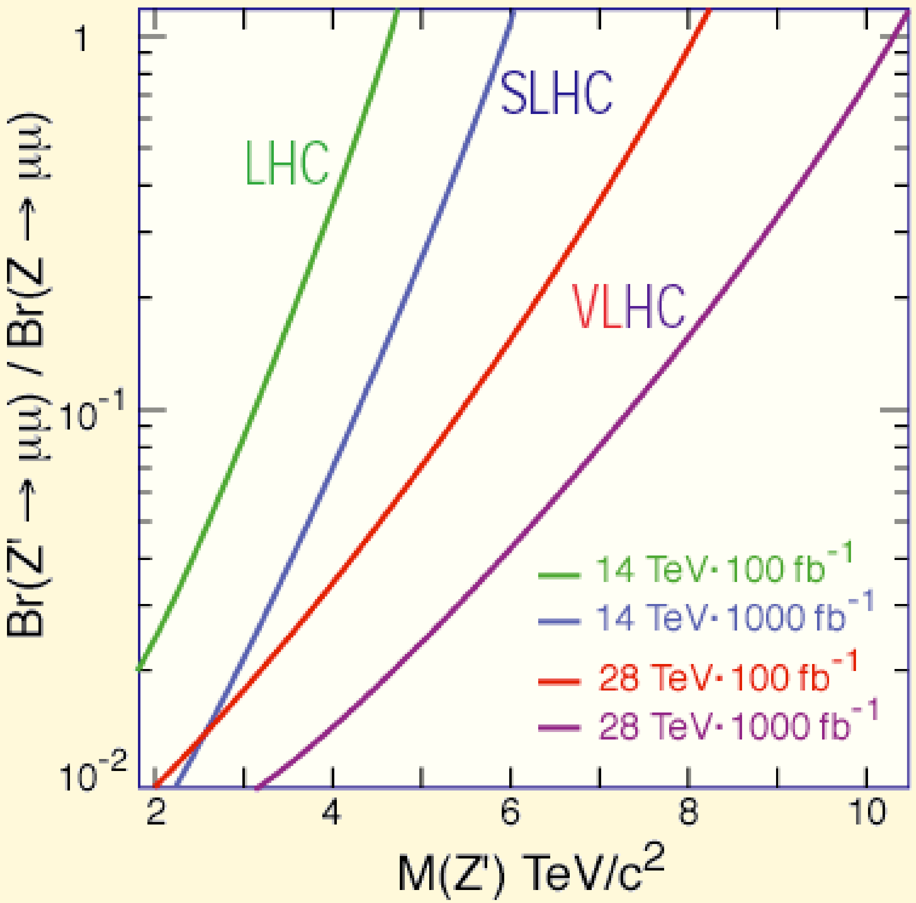
← LHC 100 fb^{-1}

reach up to ~ 5 TeV, discrimination up to ~ 2.5 TeV
here SLHC statistics would help!

Aside from the counting of events, there are obviously **experimental considerations** (ability to tag final states, to detect muons at large rapidity, momentum/energy resolution, backgrounds) which alter the balance of desirability between Lum and E_{beam}

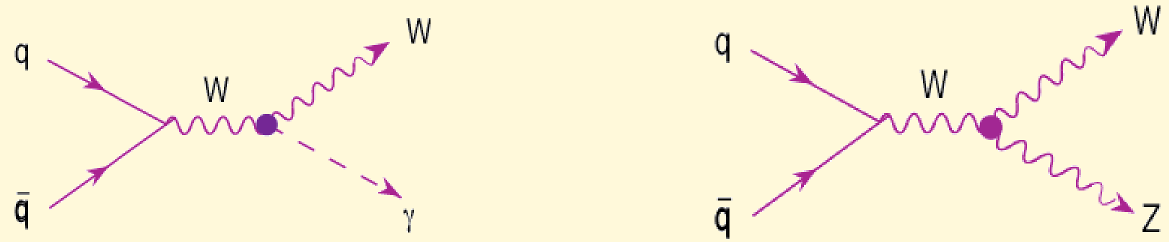
Sensitivity to Z' → μμ

Reach in M(Z') as function of Br(Z' → μμ)



Ex: Precise determinations of the self-couplings of EW gauge bosons

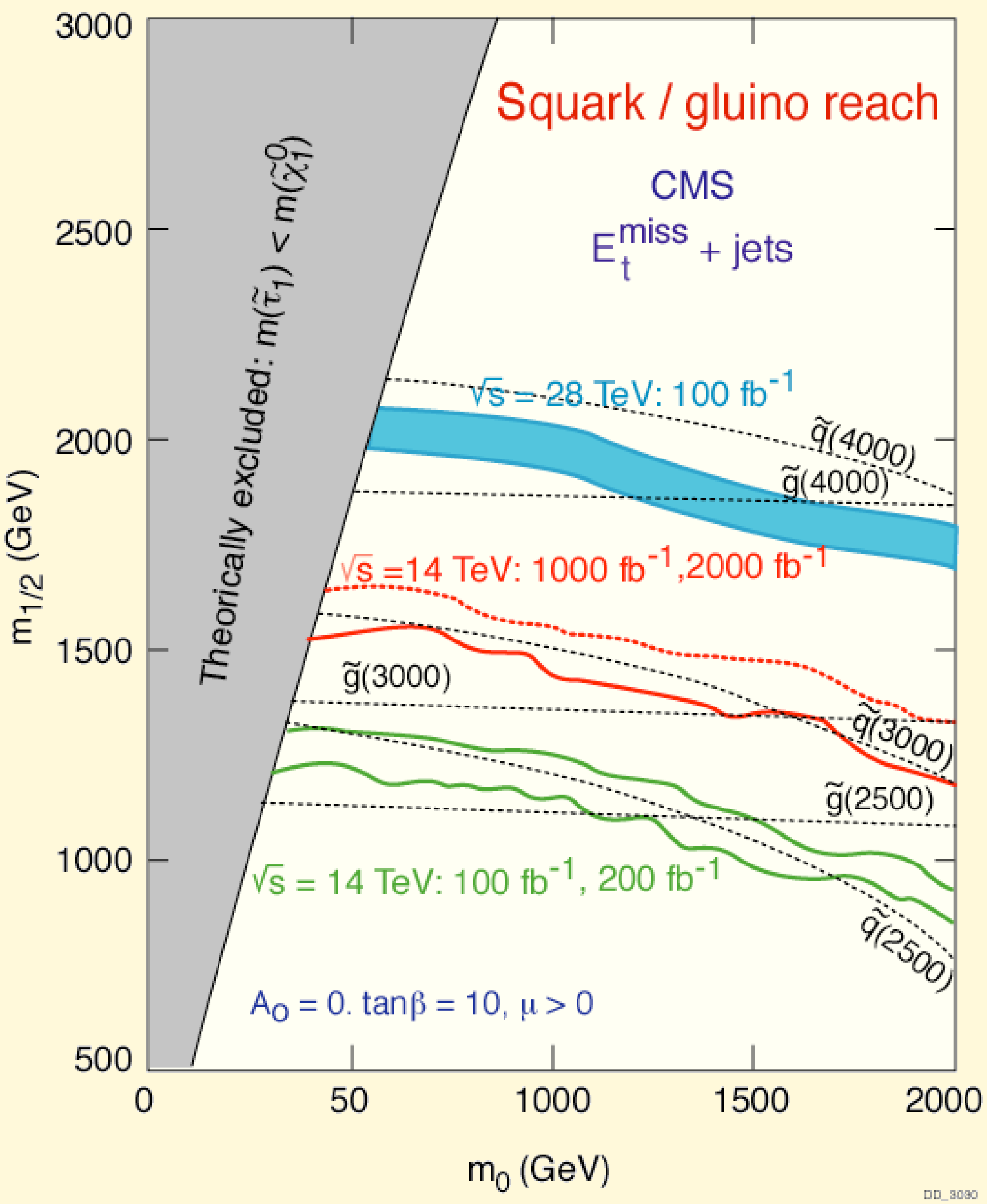
5 parameters describing weak and EM dipole and quadrupole moments of gauge bosons. The SM predicts their value with accuracies at the level of 10^{-3}, which is therefore the goal of the required experimental precision



Coupling	14 TeV 100 fb^{-1}	14 TeV 1000 fb^{-1}	28 TeV 100 fb^{-1}	28 TeV 1000 fb^{-1}	LC 500 fb^{-1}, 500 GeV
λ_γ	0.0014	0.0006	0.0008	0.0002	0.0014
λ_Z	0.0028	0.0018	0.0023	0.009	0.0013
$\Delta\kappa_\gamma$	0.034	0.020	0.027	0.013	0.0010
$\Delta\kappa_Z$	0.040	0.034	0.036	0.013	0.0016
g^Z_1	0.0038	0.0024	0.0023	0.0007	0.0050

SLHC only slightly better than 28 TeV/100fb-1, and comparable to 500 GeV LC

Ex: SUSY reach extension



SLHC: 500GeV increase in mass reach for gluinos and squarks

VLHC[28TeV]: 1000GeV increase in mass reach for gluinos and squarks

But if the scale of SUSY particles is around the TeV, statistics will be essential to study the full SUSY spectrum. Again, balance between statistics and systematics is required in the choice of Lum vs E

Ex: Longitudinal $W^+ W^+$ scattering

Unitarity requires that in absence of a Higgs-like scalar field the interaction of W_L becomes strong at energies above 800-900 GeV. These strong interactions leads, as in teh case of strong interactions between pions, to the formation of resonances. Such a behaviour, required from first principles (unitarity) would be a signal of new underlying interactions, as predicted in BSM scenarios alternative to the SM Higgs mechanism.

Their exploration demands either higher luminosities, or higher energies. Which is better depends on the details of "what's there"

Table 10: Expected numbers of reconstructed events above an invariant mass of 600 GeV (for $\sqrt{s}$=14 TeV) and 800 GeV (for $\sqrt{s}$=28 TeV) for models with a strongly-coupled Higgs sector and for the background. The significance was computed as $S/\sqrt{S+B}$.

Model	300 fb^{-1} 14 TeV	3000 fb^{-1} 14 TeV	300 fb^{-1} 28 TeV	3000 fb^{-1} 28 TeV
Background	7.9	44	20	180
K-matrix Unitarization	14	87	57	490
Significance	3.0	7.6	6.5	18.9
Higgs, 1 TeV	7.2	42	18	147
Significance	1.8	4.5	2.9	8.1

Ultimately a VLHC with a modest lum increase relative to the LHC will be the best exploration machine for these scenarios

The taste of Flavour

Flavour phenomena have contributed shaping modern HEP as much as, if not more than, the gauge principle

μ

ν

Strangeness ⇒ SU(3)

K

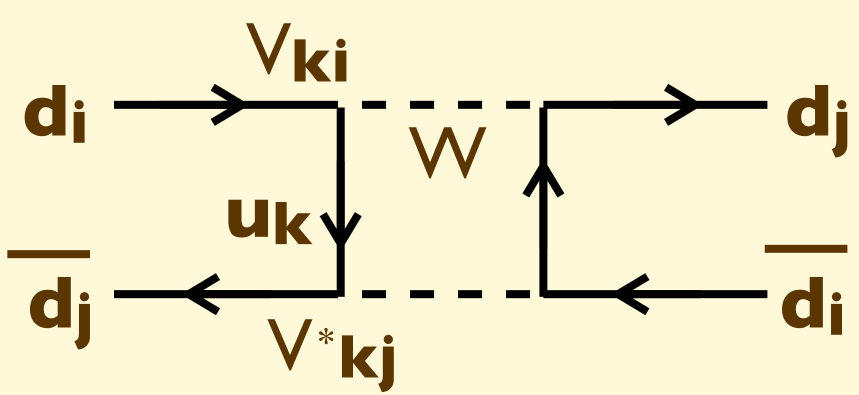
$\varepsilon_K \Rightarrow$ **CP violation**

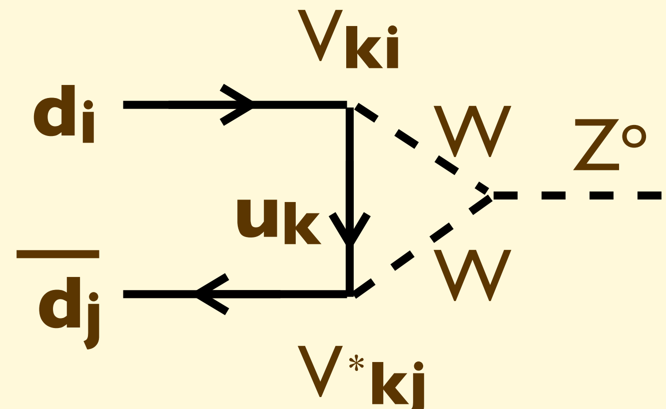
$K^0 - \bar{K}^0$ mixing/ FCNC

⇒ **GIM, charm**

FCNC and CPV in the SM

- Suppression of FCNC and CPV are guaranteed in the SM by the following facts:
  - Quark sector:
    - unitarity of CKM (GIM mechanism)
    - small mixings between heavy and light generations





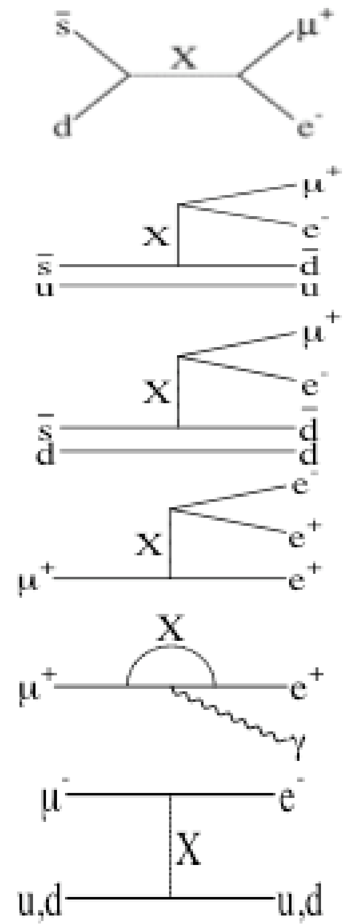
$$\Delta_{ij} \sim \sum_{k=u,c,t} V_{ki} V^*_{kj}\, f(m_k/m_W) \sim \sum_{k=c,t} V_{ki} V^*_{kj}\, m_k^2/m_W^2 \sim V_{ci} V^*_{cj} \frac{m_c^2}{m_W^2} + V_{ti} V^*_{tj}$$

  - Lepton sector:
    - mν=0 ⇒ all phases and angles absorbed by field redefinitions, no mixings/CPV at all

FCNC beyond the SM

S.Geer

- There is absolutely no guarantee that these properties be maintained in extensions of the SM
- As soon as these are released, effects are devastating!

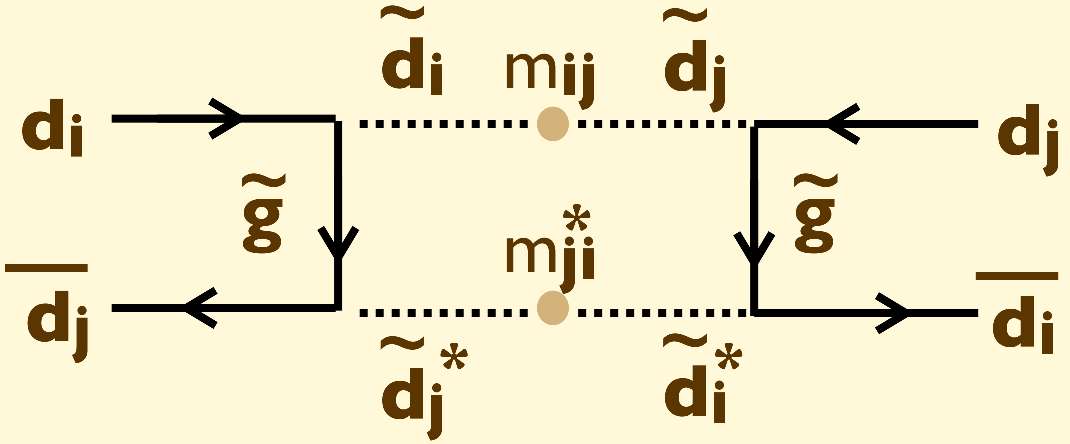


$B(K_L \to \mu e) < 4.7 \times 10^{-12}$	$M_X > 150$ TeV/c²
$B(K^+ \to \pi^+ \mu^+ e^-) < 4 \times 10^{-11}$	$M_X > 31$ TeV/c²
$B(K_L \to \pi^\circ \mu^+ e^-) < 3.2 \times 10^{-10}$	$M_X > 37$ TeV/c²
$B(\mu^+ \to eee) < 1 \times 10^{-12}$	$M_X > 86$ TeV/c²
$B(\mu^+ \to e^+ \gamma) < 1.2 \times 10^{-11}$	$M_X > 21$ TeV/c²
Normalized Rate $< 6.1 \times 10^{-13}$	$M_X > 365$ TeV/c²

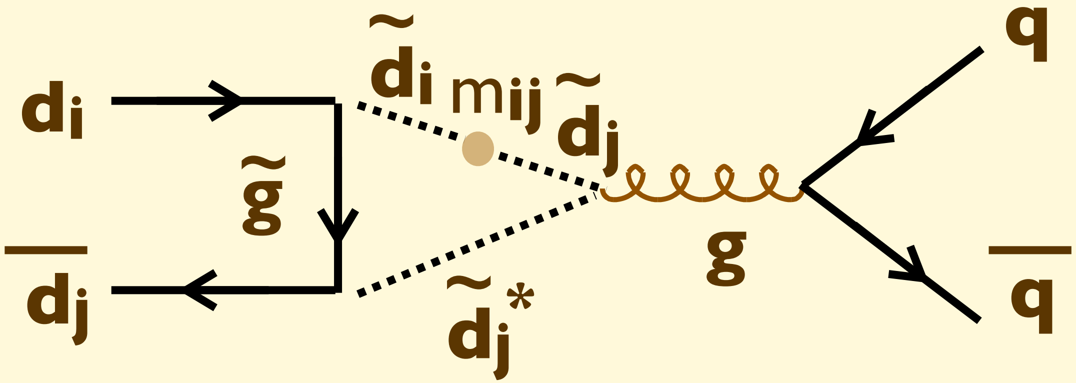
Compare the to O(10 TeV) sensitivity w.r.t. modifications of the gauge/EW sector

How can the new physics we need to understand the open problems of HEP leave no trace of FCNC?

Example: Squark mixing



$\Delta F=2$ (B-$\bar{B}$, K-$\bar{K}$ mixing)



$\Delta F=1$ penguins:
$b \to s \bar{s} s$

If the squark mass matrix is not aligned with the quark mass matrix, new sources of FNCN and of CPV

Neutrinos, facts

- LEP: 3 weakly interacting neutrinos with $m<M_Z/2$
- 2 relative masses, one absolute mass scale, 3 mixing angles, 1 CKM phase δ, 2 extra relative phases if Majorana

$\|\Delta m^2_{23}\|$	Δm^2_{12}	m_1	$\sin^2\theta_{12}$	$\sin^2\theta_{23}$	$\sin^2\theta_{13}$	δ_i
$\sim 2.6\text{x}10^{-3}$	$\sim 7\text{x}10^{-5}$	?	0.2-0.4	0.3-0.7	<0.05	?

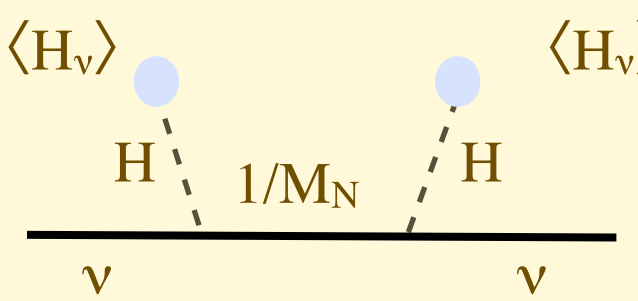
  - Iff all $\theta_{ij}\neq 0$ and at least one phase $\delta\neq 0$, then CPV
    - Leptogenesis (lepton-driven B asymmetry of the Universe)
  - Dark Matter: WMAP $\Rightarrow$ $\Omega_\nu<0.015$, $m_\nu<0.23$ eV
- Neutrino masses themselves do not provide a new *theory*, but give us constraints and hints for the development of the BSM theory which hopefully will, in addition to explain neutrino mixings, shed light on the other enigmas of the SM

Neutrinos, implications

- Neutral partners N_i of the SM ν_i should exist. Possible mass terms:
  - Dirac mass: $L_m \propto y_\ell H_\ell L_i L_i^c + y_\nu^{ij} H_\nu L_i N_j$
    - in a minimally extended SM, $H_\nu = \bar{H}_\ell$ $\quad m_\nu \sim y_\nu \langle H \rangle \sim m_\ell (y_\nu / y_\ell)$
    - to first approximation this scenario has no other implications: the R-handed partner of the neutrino is totally decoupled, and one does not understand why m_ν is so small ⇒ **useless option**!
  - Majorana N_i : $L_m \propto y_\ell H_\ell L_i L_i^c + y_\nu^{ij} H_\nu L_i N_j + M_N^{ij} N_i N_j$

$$M_\nu = -y_\nu M_N^{-1} y_\nu^T \langle H_\nu \rangle^2$$

- in this case:



$m_\nu = \langle H_\nu \rangle^2 / M_N$ and $\langle H_\nu \rangle = O(100\ \mathrm{GeV})$

→ $M_N = O(M_{GUT})$

GUT connection!

If GUT, then Supersymmetry

- If we follow the path of neutrino masses, we are naturally led to the idea of Grand Unification. As a result of gauge coupling evolution, SUSY then becomes **almost** an *experimental fact.*

- SUSY will be one of the first theoretical ideas to be probed by the LHC, and most studies of the physics potential of the LC are based on the assumption the SUSY exploration will constitute a fundamental element of its programme.

- SUSY, on the other hand, has also several important implications which affect physics at the low-energy scale in the flavour sector

Neutrinos and SUSY

For details and refs, see: Masiero, Profumo, Vempati, Yaguna, hep-ph/0401138

The merging of neutrino masses, SUSY and GUT leads to very interesting constraints and consequences:

SUSY ⇒ Higgs field giving Dirac υ mass = Higgs field giving up-quark masses

$$L_m \propto y_\ell H_d L_i L_i^c + y_\nu^{ij} H_u L_i N_j + M_N^{ij} N_i N_j$$

GUT (e.g. SO(10)) ⇒ Yukawa ν-mass matrix = Up-quark Yukawa matrix

$$L_m \propto y_{i,j}^{d,\ell} \mathbf{16}_i \mathbf{16}_j H_d + y_{i,j}^{u,\nu} \mathbf{16}_i \mathbf{16}_j H_u + y_{i,j}^{R} \mathbf{16}_i \mathbf{16}_j H_R^{126}$$

where $\mathbf{16} = (u_L, d_L, u^c, e^c)_{10} + (d^c, L)_5 + N^c$

⇒ one entry in the neutrino Yukawa matrix is of order of the top Yukawa coupling!

⇒ $m(N_R) = f(m_{up}, m_\nu) \approx (m_t^2 / m_\nu, \; m_c^2 / m_\nu, m_u^2 / m_\nu)$

⇒ $m_\nu > m_t^2 / M_{GUT}$ to ensure that $m(N_R) < M_{GUT}$

Even more interestingly, quark mixings induce charged **slepton** mixing via RG evolution from M_{GUT} to $m(N_R)$:

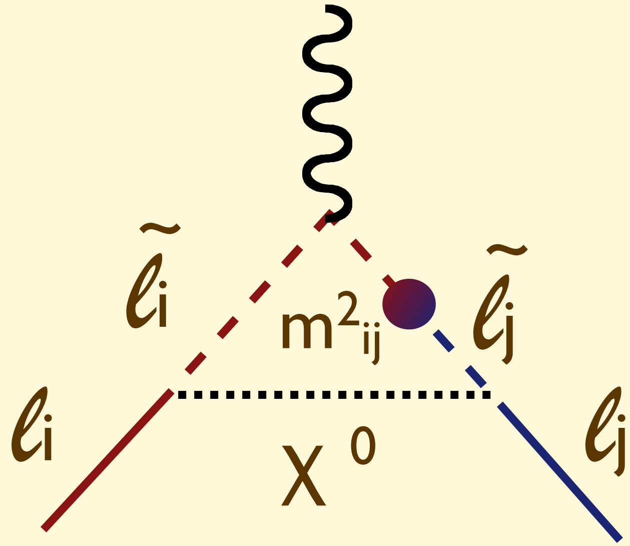
$$(m^2_{\tilde{L}})_{ij} \sim -\frac{3m_0^2 + A_0^2}{8\pi^2}\, y_t^2\, O_{ij} \log\frac{M_{GUT}}{M_{N_R}}$$

SUSY breaking param's

nu mixing param's

$$y_t^2\, O_{ij} = \sum_k y^\nu_{ik}\, y^{\nu *}_{jk}$$

$\ell_i \to \ell_j\, \gamma$ transitions:

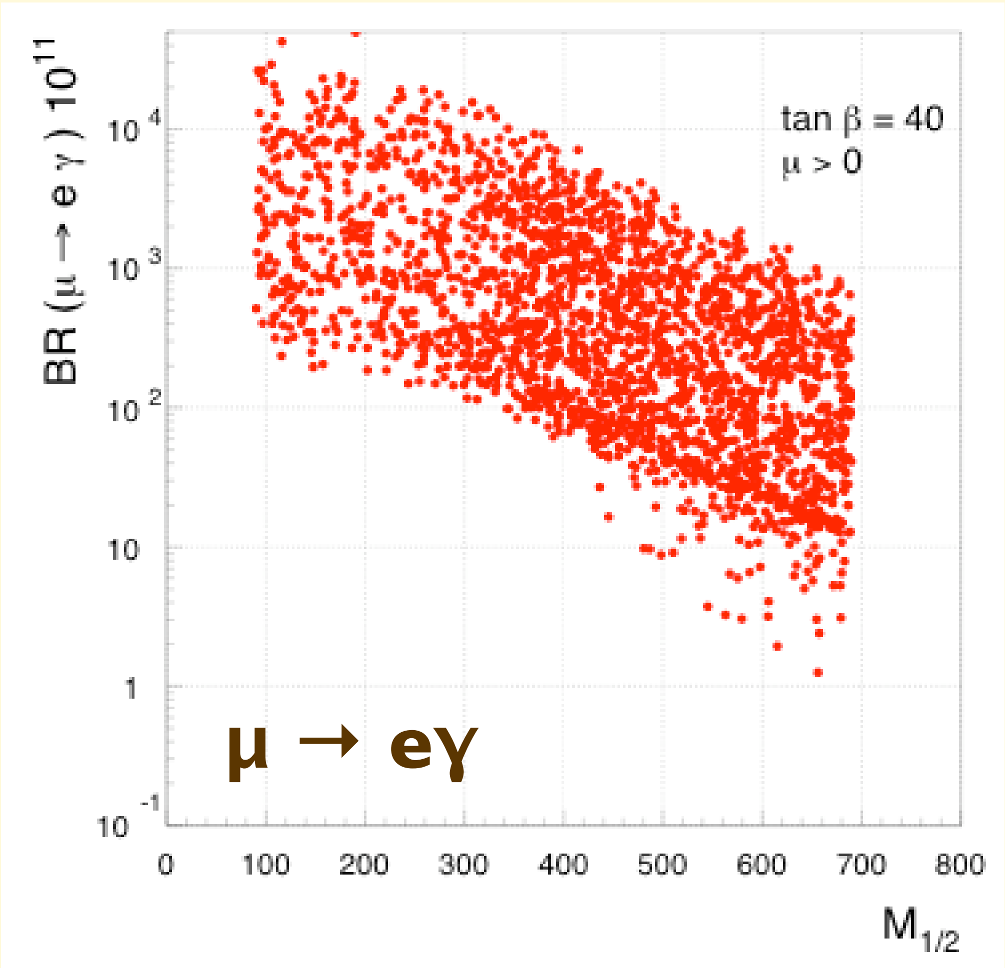


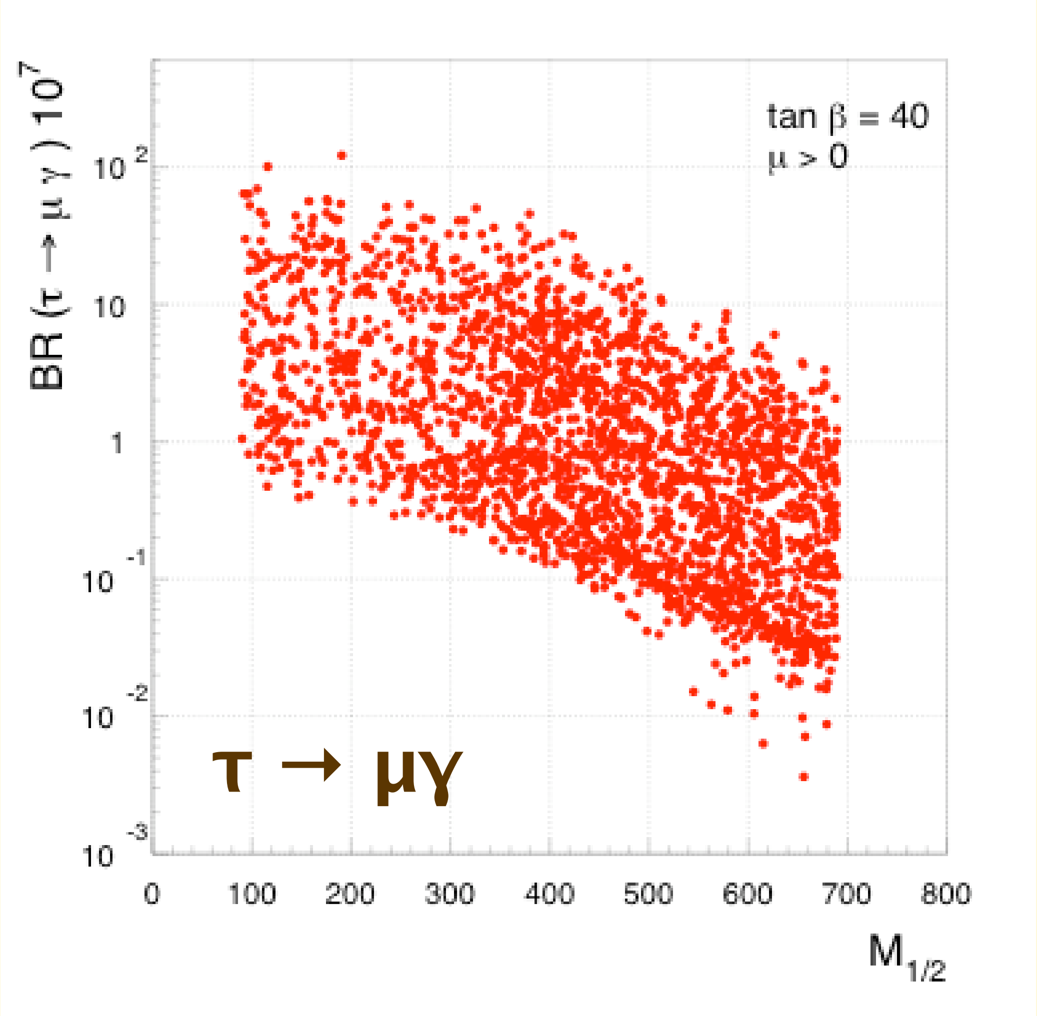
Possible scenarios:

$O_{\mu e} = V_{td} V_{ts}$ "worst
$O_{\tau\mu} = V_{tb} V_{ts}$ scenario"

$O_{\mu e} = U_{e3}\, U_{\mu 3}$ "optimal
$O_{\tau\mu} = U_{\tau 3}\, U_{\mu 3}$ scenario"

Some examples





Optimal scenario (mixing proportional to nu mixings)

The discovery of Supersymmetry or other new phenomena at the LHC will dramatically increase the motivation for searches of **new phenomena in flavour physics.**

While there is no guarantee that any deviation from the SM will be found, the existence of physics BSM will demand and fully justify these studies: we'll be measuring the properties, however trivial, of something which we know exists, as opposed to blindly looking for "we don't know what" as we are unfortunately doing today!

B physics studies at the LHC and at SuperBELLE, a rich K physics programme and possibly new studies of the charm sector, will naturally complement the measurements in ν physics and searches for Lepton Flavour Violation phenomena.

Other HEP topics: “QCD dynamics”

High-density QCD matter: Heavy Ions

- Relativistic heavy ion collisions (RHIC, LHC) are a new entry in HEP. They will open a new window on QCD at extreme densities and temperature
- This is rather unknown territory, with room for interesting dynamical surprises.
- No future is however being layed out for these initiatives (HI programme at the LHC to terminate by 2015)
  - **Question to our HI physics colleagues: is it required/desirable??**
- What are the prospects for the continuation of HI collisions at CERN after the LHC upgrade?
- Possible to conceive HI collisions in a Super-SPS?

Low-energy QCD

- **Studies of proton structure:**
  - PDF's => relevance to LHC physics (absolute determination of Xsections -> extraction of coupling constants)
  - diffractive PDFs -> diffractive H production?
  - polarized PDFs, etc: ??
- **Hadronic spectroscopy:**
  - glueballs
  - quarkonium
  - Narrow charm resonances above threshold
  - 5-/4-quarks etc
- **Low-E nuclear physics, Relativistic Ion Beams, ...**

Which role should these studies play in the future of HEP?

Options for the upgrade of the CERN accelerator complex

Present accelerator	Replacement accelerator	Improvement	INTEREST FOR			
			LHC upgrade	ν physics beyond CNGS	RIB beyond ISOLDE	Physics with k and μ
Linac2	Linac4	50 → 160 MeV $H^+ \rightarrow H^-$	+	0 (if alone)	0 (if alone)	0 (if alone)
PSB	>2.2 GeV RCS* for HEP	1.4 → >2.2 GeV 10 → 250 kW	+	0 (if alone)	+	0 (if alone)
	>2.2 GeV/ mMW RCS*	1.4 → >2.2 GeV 0.01 → 4 MW	+	++ (super-beam, β-beam ?, ν factory)	+ (too short beam pulse)	0 (if alone)
	>2.2 GeV/50 Hz SPL*	1.4 → >2.2 GeV 0.01 → 4 MW	+	+++ (super-beam, β-beam, ν factory)	+++	0 (if alone)
PS	RSS*/** for HEP	>30 GeV Intensity x 2	++	0 (if alone)	0	+
	5 Hz RCS*/**	>30 GeV 0.1 → 4 MW	++	++ (ν factory)	0	+++
SPS	1 TeV RSS*/**	0.45 → 1 TeV Intensity x 2	+++	?	0	+++

RCS=Rapid Cycling Synchrotron
RSS=Rapid Superconducting Synchrotron
SPL=Superconducting Proton Linac

* with brightness x2
** need new injector(s)

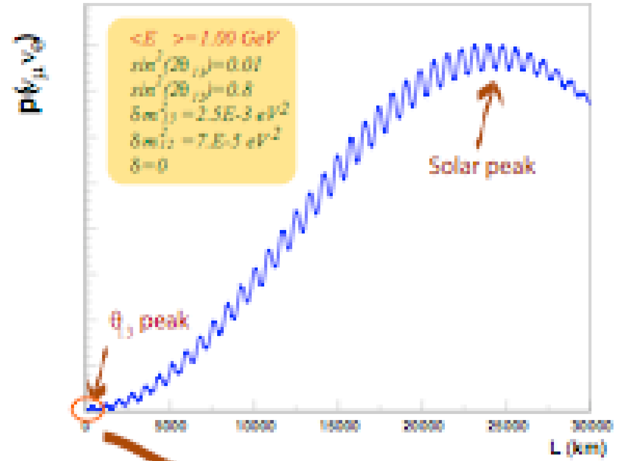
HIP report & R.Garoby

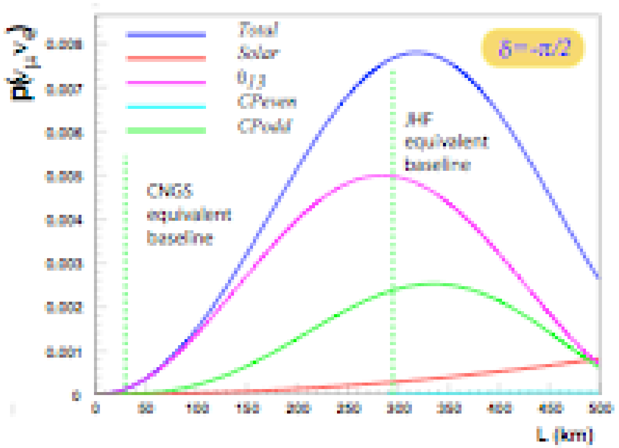
Issues for discussion

- SPL and default β-beam require a 0.5MTon detector in the Frejus tunnel
  - the SPL ν-physics potential alone does not justify the enterprise
  - $several-100M tunnel + $500M detector: who pays?
  - a detector at 130km is too close to address next-generation issues (CP violation). It will survive as a proton-decay experiment, but new detectors will have to be built for future developments (ν factory): is it a wise investment?
- Neither the β-beam, nor the SLHC or any of the possible fixed-target experiments (K decays, muons, etc) require more than few 100kW at 2 GeV
  - e.g. stopped K+ exps require 500kW@30GeV

How much can be achieved with a diversified use of the financial and HR resources required to develop an SPL+Eurisol facility?

Sub leading $\nu_\mu - \nu_e$ oscillations





L~120km(E/0.3GeV)

$$p(\nu_\mu \to \nu_e) = 4c_{13}^2 s_{13}^2 s_{23}^2 \sin^2\frac{\Delta m_{13}^2 L}{4E}$$ θ_{13} driven

$$+ 8c_{13}^2 s_{12}s_{13}s_{23}(c_{12}c_{23}\cos\delta - s_{12}s_{13}s_{23})\cos\frac{\Delta m_{23}^2 L}{4E}\sin\frac{\Delta m_{13}^2 L}{4E}\sin\frac{\Delta m_{12}^2 L}{4E}$$ CPeven

$$- 8c_{13}^2 c_{12}c_{23}s_{12}s_{13}s_{23}\sin\delta\sin\frac{\Delta m_{23}^2 L}{4E}\sin\frac{\Delta m_{13}^2 L}{4E}\sin\frac{\Delta m_{12}^2 L}{4E}$$ CPodd

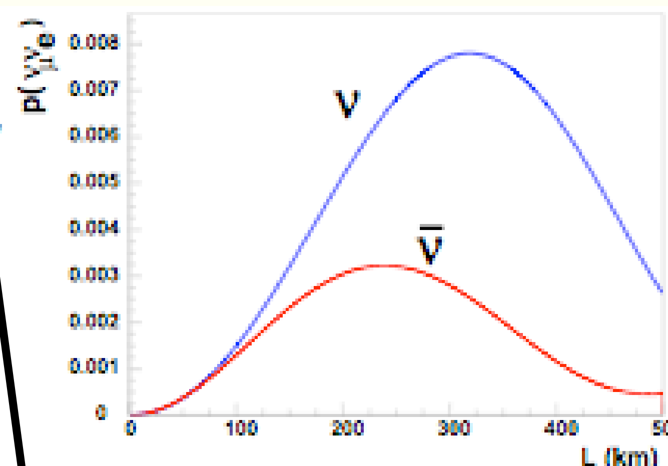
$$+ 4s_{12}^2 c_{13}^2\{c_{13}^2 c_{23}^2 + s_{12}^2 s_{23}^2 s_{13}^2 - 2c_{12}c_{23}s_{12}s_{23}s_{13}\cos\delta\}\sin\frac{\Delta m_{12}^2 L}{4E}$$ solar driven

$$- 8c_{12}^2 s_{13}^2 s_{23}^2 \cos\frac{\Delta m_{23}^2 L}{4E}\sin\frac{\Delta m_{13}^2 L}{4E}\frac{aL}{4E}(1-2s_{13}^2)$$ matter effect (CP odd)

θ_{13} discovery requires total probability greater than Solar probability

Leptonic CP discovery requires

$$A_{CP} = \frac{P(\nu_\mu \to \nu_e) - P(\bar{\nu}_\mu \to \bar{\nu}_e)}{P(\nu_\mu \to \nu_e) + P(\bar{\nu}_\mu \to \bar{\nu}_e)} \neq 0$$



M.Mezzetto, Villars 2004

sensitive to signΔm^2_{13}

long baseline

New ideas and alternative options have been proposed recently, which bypass the use of an SPL+Frejus detector by exploting higher E_ν beams towards LNGS or other long baseline, new, locations

- RCS PS: 20 GeV p, 6.5MW, towards LNGS (4kton LAr) (Ferrari et al 2002)
- Higher E betabeam (SuperSPS: γ_{He6}=350, γ_{Ne18}=580) to LNGS (40kton Pb detector) (P.Hernandez et al, hep-ph/0312068, Donini et al 2005)
- Higher yet betabeam (LHC: γ_{He6}=2488, γ_{Ne18}=4147) to LNGS or to very-long baseline (Migliozzi, Terranova, hep-ph/0405081)

Aside from the needs of the SLHC, prospects for neutrino physics will be the main driver in the selection of the path towards an upgrade of the CERN accelerator complex.

PAF+POFPA

We therefore need to review Garoby's table, using a finer structure in the area of neutrino physics, and exploring the value, as well as beam and detector requirements, of each option, individually

- ★ Superbeams
  - SPL: 4MW@2-3 GeV → Frejus
  - PS++: 6.5MW@20 GeV → LNGS
- ★ Beta beams:
  - $\gamma \sim 100$ (Frejus)
  - $\gamma \sim 350$-600 (LNGS)
- ★ ν Factory

The choices will be complicated by the uncertainty about the fate of other competing projects around the world: JPARC, FNAL

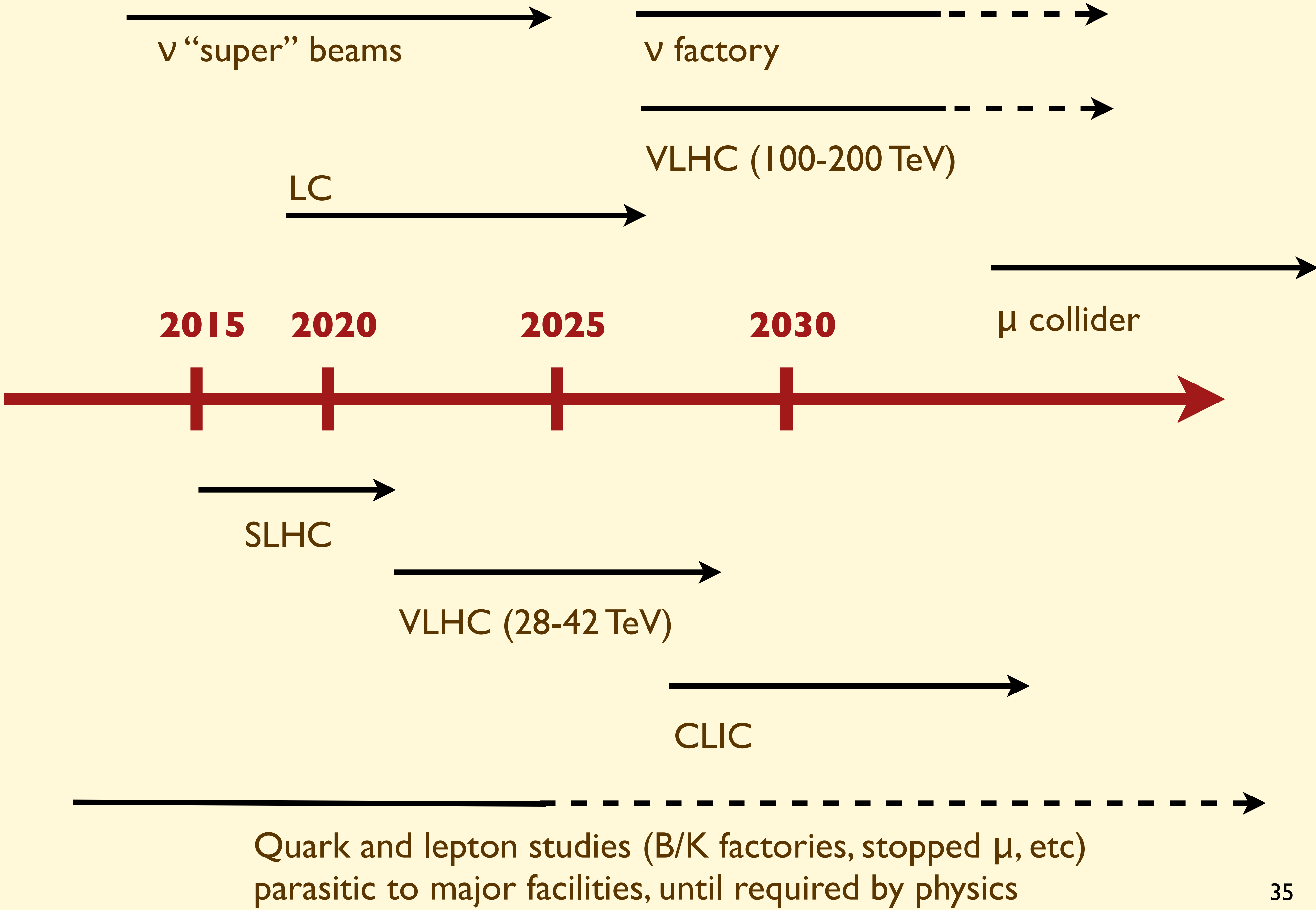
The development of higher energy accelerators is, for what we know today, an irreplaceable tool of exploration to understand the ultimate laws of Nature

Cosmology, cosmic rays, precision lower-energy measurements, are essential **complementary** tools of discovery, but cannot replace the direct observation and study of new phenomena provided by HE accelerators and experiments

This may evolve with time, but until then:

The potential of a laboratory to explore the highest energy frontier should not be compromised by the temptation of synergy with programmes which could be pursued elsewhere or which are not critical to the pursuit of the HE frontier

No compromise to get the best and the most out of the LHC and, in perspective, to get to CLIC


ν "super" beams
ν factory
VLHC (100-200 TeV)
LC
μ collider
2015
2020
2025
2030
SLHC
VLHC (28-42 TeV)
CLIC
Quark and lepton studies (B/K factories, stopped μ, etc)
parasitic to major facilities, until required by physics

Conclusions

- Full exploitation of the LHC is mandatory. Whatever new physics is observed at the LHC, its understanding will require higher statistics and higher energies. No other facility in the world can achieve this in a foreseeable future.
- The understanding of the flavour structure of the SM and of whatever BSM framework is exposed by the LHC will demand new high-statistics explorations of low-energy phenomena in the quark and lepton sectors: K/D/B decays, neutrino properties, LFV decays, EDM, etc.
- The upgrade path for the LHC allows options which would enable CERN to play a leading role in part of this research, with a marginal extra cost.
- 2nd generation, superbeam-like, neutrino physics can have immense costs, both in terms of infrastructure and experiments. These are acceptable only if they are part of a longer time scale investment, and parasitic to the LHC upgrade. However:
  - To the extent that these longer time scale programmes interfer with the pursuit of the HEP frontier, their case should be brought against these HEP alternatives (e.g. ν Factory vs. CLIC/VLHC)
  - Synergy with other non-HEP research should not justify developments which, at a fraction of the cost, could yield the same results from the HEP viewpoint

- CERN is the most powerful and complete HEP lab in the world
- Aside from FNAL, it is the only surviving lab willing to and capable of pushing the energy frontier
- The energy frontier does not stop at 14 TeV, and the ILC is NOT the machine that will take us beyond the reach of the LHC
- In this respect, CERN is the ONLY lab with the infrastructure, the expertise, and the R&D plans to provide a concrete and solid future to the exploration of Nature at its most fundamental level
- Let's focus on this goal, and set ourselves the most ambitious targets!