Quale futuro per la fisica delle alte energie?

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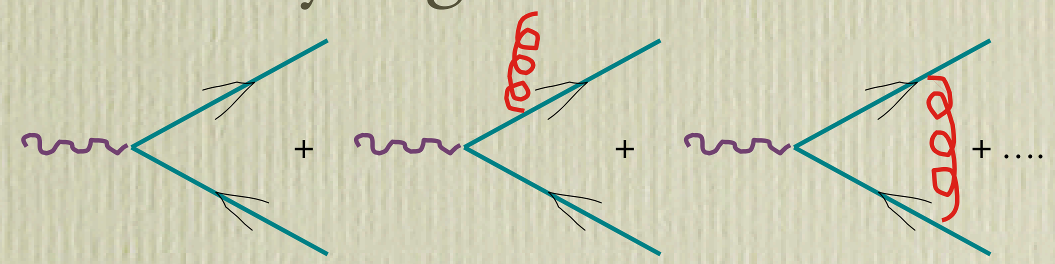
Worries about the future of HEP are encouraged by recent decisions and current negative prospects:

Termination of mu, K, D and B physics programmes in the US → which future for BNL, Cornell, Fermilab, SLAC?

Termination of HERA → conversion of DESY in a XFEL facility

The "present" of HEP:

- Renormalizable Quantum Field Theory
- Gauge symmetry principle, with group structure (SU(3)xSU(2)xU(1)) dictated by experimental evidence
- Reliable perturbation theory. E.g.
  - Z->hadrons=



- Well tested against data:
  - U(1) sector to $O(1/10^{11})$
  - SU(2) sector to $O(1/10^{3})$
  - SU(3) sector to O(1/100)

The future of HEP should be driven by the key questions left unanswered by the above picture:

Formal questions:

- Why gauge theory?
- Are particles really pointlike? Strings?? Membranes?

Phenomenological questions:

- Why 3 families of quarks and leptons? => flavour issues
- Why some particles have mass? => EW SB
- Why m(neutrino) ~ 10^{-7} m(e)? => again flavour
- Why is there a matter-antimatter asymmetry in the Universe? => sources of CP violation
- Origin of DM? Dark energy? => ?? possibly EW SB
- Why $F_{gravity} \sim 10^{-40}\ F_{electric}$? => again EW SB
- Why D=3+1? => Quantum gravity, strings, extra dim

More pragmatically, the two leading questions whose understanding is possibly within the reach of the forthcoming generation of experiments are:

The origin of EW SB

The origin of Dark Matter

Better understanding of the first issue is crucial to make progress on the other points (e.g. flavour, neutrino masses, CP violation) and to plan the future of HEP.

Other HEP topics: “QCD dynamics”

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

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)

LEP's heritage is a strong confirmation of the SM, and at the same time an apparent paradox:

SM fits: m(H)=117+45-68; on the other, SM radiative corrections give

$$\delta m_H^2 = \frac{6G_F}{\sqrt{2}\pi^2}\left(m_t^2 - \frac{1}{2}m_W^2 - \frac{1}{4}m_Z^2 - \frac{1}{4}m_H^2\right)\Lambda^2 \sim (115\mathrm{GeV})^2 \left(\frac{\Lambda}{400\mathrm{GeV}}\right)^2$$

How can counterterms artificially conspire to ensure a cancellation of their contribution to the Higgs mass?

The existence of new phenomena at a scale not much larger than 400 GeV appears necessary to enforce such a cancellation in a natural way!

The accuracy of the EW precision tests at LEP, on the other hand, sets the scale for "generic new physics" (parameterized in terms of dim-5 and dim-6 effective operators) at the level of few-to-several TeV.

This sets very strong constraints on the nature of this possible new physics: to leave unaffected the SM EW predictions, and at the same time to play a major role in the Higgs sector.

Supersymmetry, among others, offers one such possible solution

In Supersymmetry the radiative corrections to the Higgs mass are not quadratic in the cutoff, but logarithmic in the size of SUSY breaking (in this case M_{stop}/M_{top}):

$$m_h^2 < m_Z^2 + \frac{3G_F}{\sqrt{2}\pi^2} m_t^4 \left[\ln\left(\frac{M_S^2}{m_t^2}\right) + x_t^2 \left(1 - \frac{x_t^2}{12}\right)\right]$$

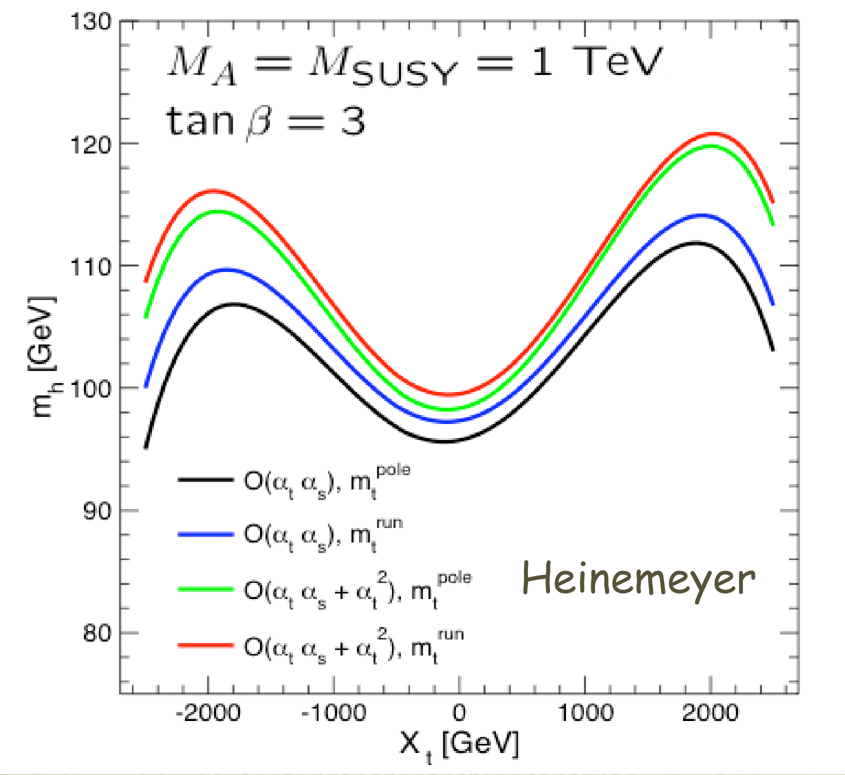
with

$$M_S^2 \equiv \frac{1}{2}(M_{\tilde{t}_1}^2 + M_{\tilde{t}_2}^2) \qquad X_t \equiv A_t - \mu \cot\beta$$

$$x_t \equiv X_t / M_S$$

For M_{susy}< 2TeV

$$m_h^{\max} \simeq 122 \text{ GeV}, \quad \text{if top-squark mixing is minimal,}$$
$$m_h^{\max} \simeq 135 \text{ GeV}, \quad \text{if top-squark mixing is maximal}$$



The current limits on m_H point to M(lightest stop) > 600 GeV. Pushing the SUSY scale towards the TeV, however, forces fine tuning in the EW sector, reducing the appeal of SUSY as a solution to the Higgs mass naturalness:

$$\delta m_Z^2 \sim (90GeV)^2 \left(\frac{M_S}{230GeV}\right)^2 \ln\frac{\Lambda_{UV}}{M_S}$$

In other words, the large value of m_H shows that room is getting very tight now for SUSY, at least in its "minimal" manifestations.

This makes the case for an early observation of SUSY at the LHC quite compelling

Dark Matter

- Clear cosmological evidence: CMB fluctuations, structure formation
- Whatever its origin, it must be coded somewhere in the Lagrangian of HEP => **it is "our" problem**
- Main ingredients:
  - stable weakly interacting particle
  - mass vs annihilation rate such as to decouple (freeze-out) at the appropriate time and with the appropriate density
- It so happens that the required numerics works out to match the expected behaviour of particles with mass O(100 GeV) and weak coupling:

  $\sigma \sim \alpha^2_W / M^2_W$

It is unavoidable to speculate that the origin of DM is directly linked to the phenomena responsible for EWSB

It is not surprising that most alternative approaches to the “Higgs” problem (little Higgs, extra-dimensions, etc) provide a possible DM candidate:

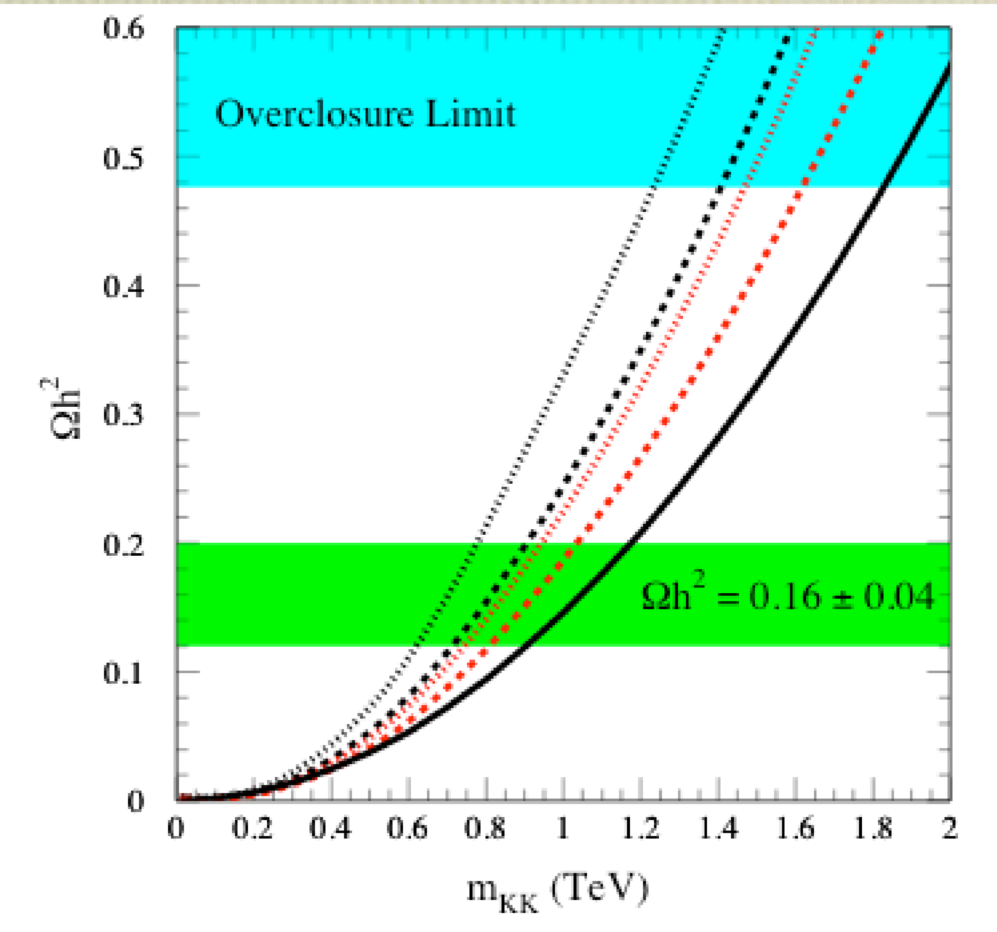
Mass scale / coupling strength are inherited by the link to EWSB

Stability is associated to discrete symmetries (like SUSY’s R parity)

Example from Universal Extra Dimensions (DM=1st photon/ neutrino KK mode)

Géraldine Servant [a,b] and Tim M.P. Tait

hep-ph/0206071

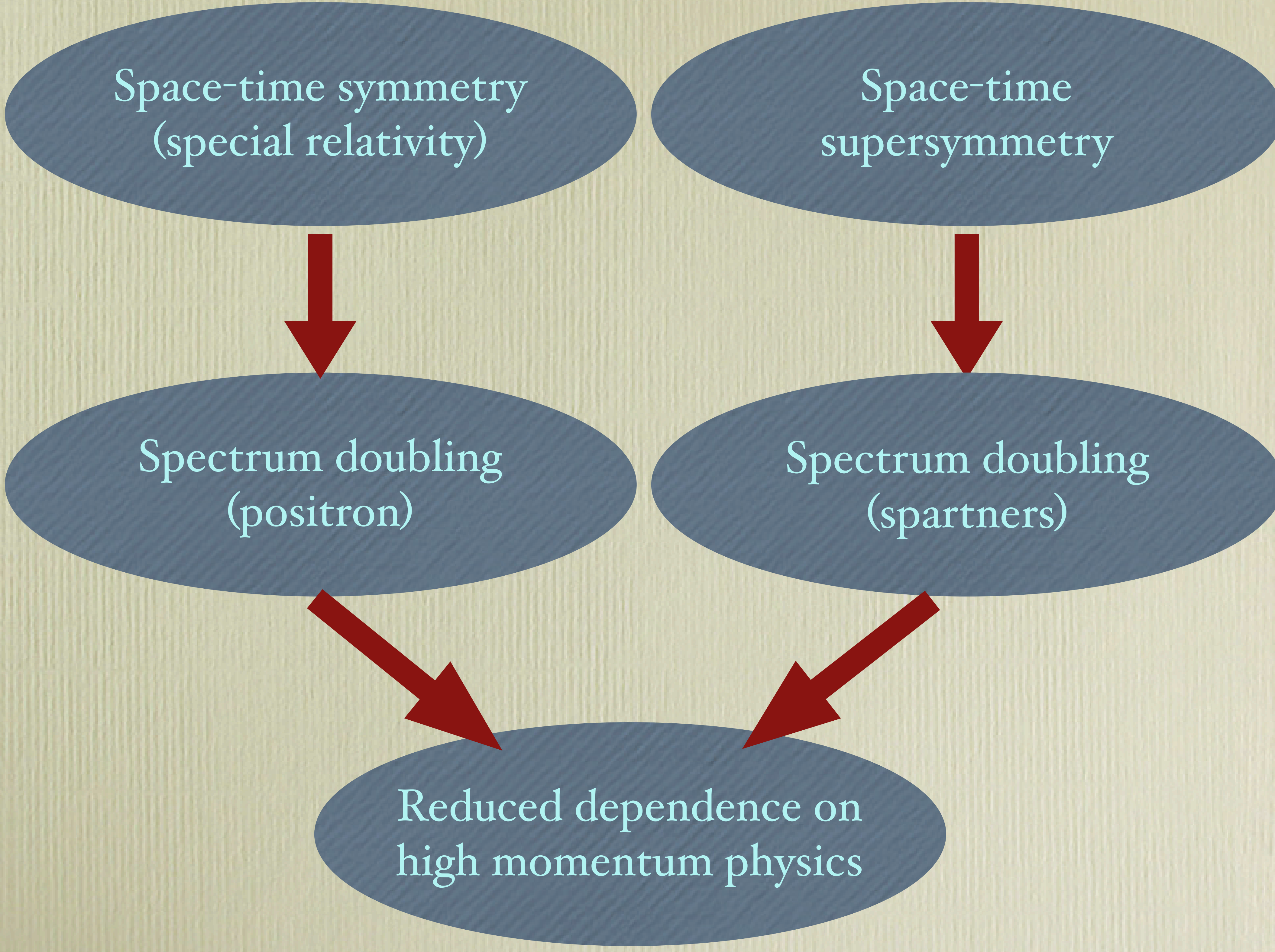


Supersymmetry: what, why, where

- Spectrum doubling: one bosonic degree of freedom (dof) of for each fermionic dof, and viceversa
- enhanced relations among and constraints on couplings/masses
- space-time Lorentz symmetry ⇒ particle ↔ antiparticle
- space-time Supersymmetry ⇒ particle ↔ sparticle
- SUSY has a priori fewer parameters than non-SUSY:
  - m(particle)=m(sparticle)
  - couplings(particle)=couplings(sparticle)
  - Higgs selfcoupling (λ) related to weak gauge coupling:

$$\lambda\phi^4 \sim g_W \phi^4$$

- All complexity and parameter proliferation of SUSY are just a consequence of SUSY breaking (SSB)!!


Space-time symmetry
(special relativity)
Space-time
supersymmetry
Spectrum doubling
(positron)
Spectrum doubling
(spartners)
Reduced dependence on
high momentum physics

- A minimal SUSY extension of the SM, with arbitrary pattern of spontaneous SUSY breaking, has over 100 extra parameters (scalar and gauge-fermion masses, mixings among SUSY partners of quarks and leptons)
- This is not much worse than an arbitrary extension to leptons and hadrons of Fermi's theory of weak interactions, before Feynman, Gell-Mann and Cabibbo, or even before LEP/SLC firmly established the parameters of the SM. One could have needed parameters to describe:
  - non V-A couplings (S, P, T, V+A)
  - non-universal couplings to hadronic currents, and to μ or τ currents
  - more complex Higgs structures
  - different realisations of EWSB
- Therefore parameter proliferation in SUSY is most likely the consequence of our current ignorance of the specific dynamics leading to SUSY breaking.

Benchmark goals for SUSY studies at the LHC:

+ GET CLUES ON THE MECHANISM OF SUSY BREAKING

+ CONFIRM THE SUSY ORIGIN (E.G. NEUTRALINO) OF DM

Supersymmetry breaking: constraints

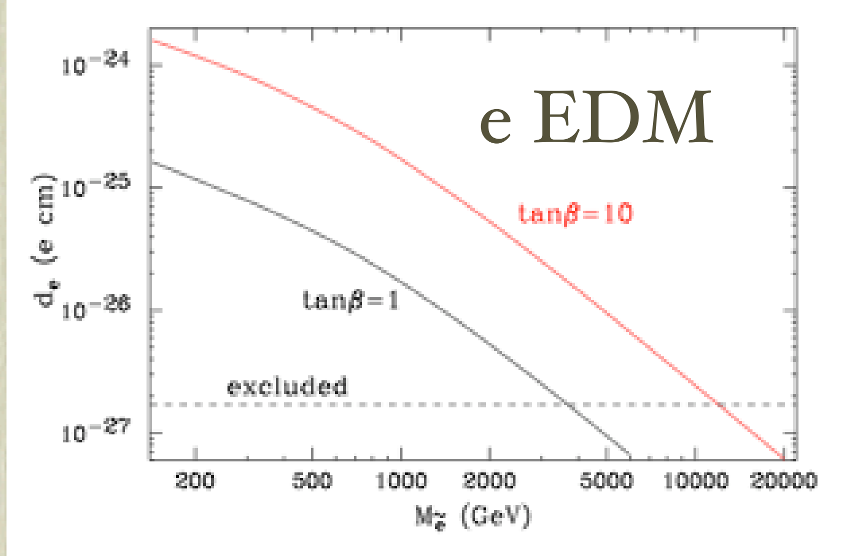
- No SUSY observed as yet: Susy particles must have masses typically larger than 100 GeV
- Nevertheless they cannot be arbitrarily large, to prevent the artificial fine tuning which justified SUSY in first place:

$$m_{\tilde{p}} \not\gg 1\ \mathrm{TeV}$$

- Generic Susy breaking (SSB) leads to unacceptable FCNC. Therefore need to require suppressed FCNC (Flavour conservation is to SUSY what GIM has been for the SM):

$$\varepsilon_K \sim \left(\frac{100\ TeV}{m_{\tilde{q}}}\right)^2 Im\left(\frac{\Delta m^2_{\tilde{d}_L \tilde{s}_L}}{m^2_{\tilde{d}}}\frac{\Delta m^2_{\tilde{d}_R \tilde{s}_R}}{m^2_{\tilde{d}}}\right) < 2 \cdot 10^{-3}$$

$$\mu \not\to e\gamma \Rightarrow \sin 2\theta_{\tilde{e}\tilde{\mu}} \frac{\Delta m^2_{\tilde{e}\tilde{\mu}}}{m^2_{\tilde{e}}} < 0.01$$



Supersymmetry breaking models: minimal Supergravity

SUSY breaking at an intermediate scale:

$$M_{SSB} \sim \sqrt{m_W \, m_{Plank}} \sim 10^{11} \; GeV$$

Universal scalar and fermion SSB masses at the Planck scale:

$$m_H = m_0$$
$$m_{\tilde{V}} = m_{1/2} \quad \forall \, V = g, \gamma, W, Z$$

Implications:

- mass splitting at EW scale induced radiatively ⇒ no FCNC problems
- mass squared for H naturally driven negative by large top Yukawa coupling
- correlation between Higgs and gaugino masses
- correlations between different gaugino masses:

$$m(\tilde{g})/m(\tilde{\chi}) \sim \alpha_s/\alpha_W$$

$$m(\tilde{B}) = (5g'^2/3g^2)\, m(\tilde{W}) \sim 0.5 m(\tilde{W})$$

Supersymmetry breaking models: gauge-mediated SSB

SUSY breaking in a strongly coupled sector, transferred to the low energy sector only via gauge interactions at an intermediate scale:

$m_{SSB} \sim$ 1-100 TeV

Consequences:

- SSB flavour independent ⇒ no FCNC problems
- Relations among SSB parameters determined by gauge couplings:

$$\frac{m(\tilde{q})}{m(\tilde{\ell})} \sim \frac{\alpha_s}{\alpha_w} \gg 1\,, \quad \text{unlike SUGRA}$$

$$\frac{m(\tilde{g})}{m(\tilde{\chi})} \sim \frac{\alpha_s}{\alpha_w}\,, \quad \text{like SUGRA}$$

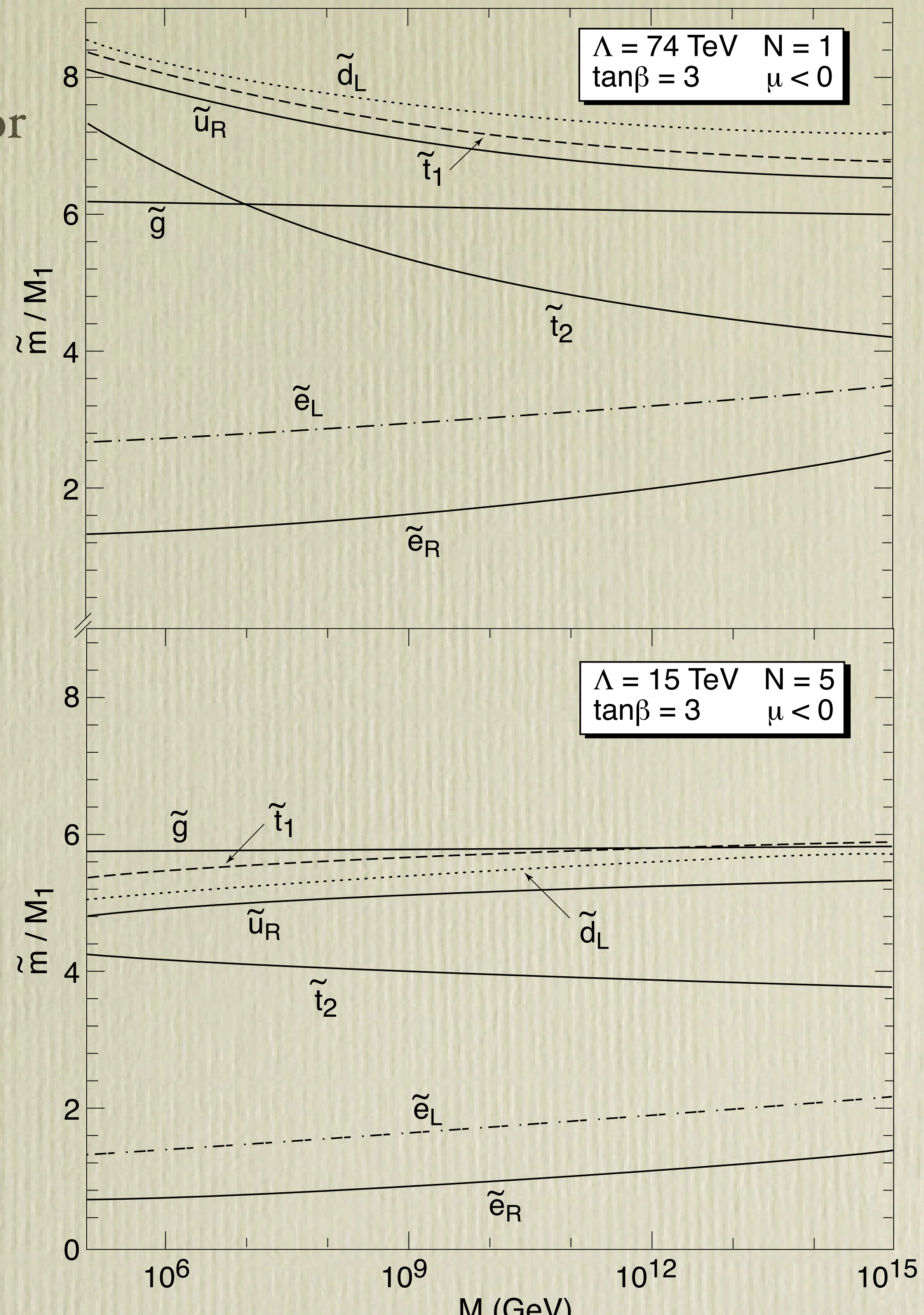
$$m(\tilde{q}) \sim m(\tilde{g})\,, \quad m(\tilde{\ell}) \sim m(\tilde{\chi})$$

$$m(\tilde{\chi}_1^{\pm}) \sim m(\chi_2^0)$$

- gravitino as Lighest SUSY Particle:

$$\chi^0 \rightarrow \tilde{G}\gamma \quad \text{or} \quad \tilde{\ell} \rightarrow \tilde{G}\ell$$

depending on which is the NLSP



SSB in Split SUSY

(see G.Giudice presentation in the BSM session)

Arkani-Hamed Dimopoulos hep-th/0405159
Giudice Romanino hep-ph/0406088
Arkani-Hamed Dimopoulos Giudice Romanino hep-ph/0409232

$m^2(\tilde{f}) \sim \tilde{m}^2 \qquad \tilde{m} \sim 10^{4-10}\text{GeV}$

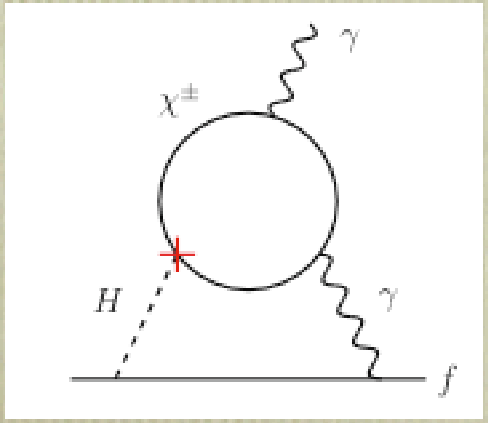
heavy scalars => no flavour problem, improved coupling unification

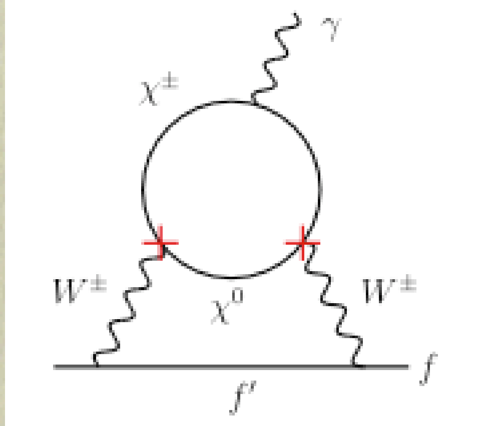
$m(\tilde{V}) \sim \frac{\tilde{m}^2}{M} \qquad M$ such that $\tilde{\chi}^0 \sim 10^{2-3}\text{GeV}$

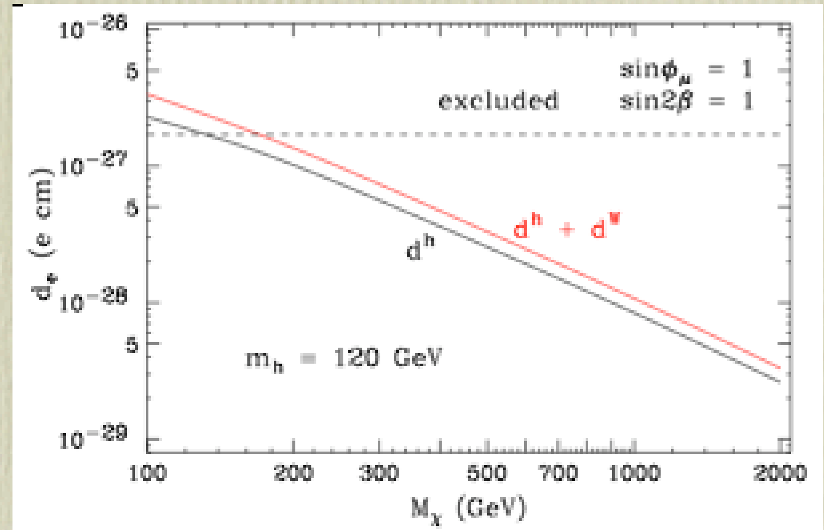
DM candidate

Potential source of CP violation in the complex higgsino / gaugino couplings

EDM contributions







N.G. Deshpande and J. Jiang
hep-ph/0503116

SUSY DM

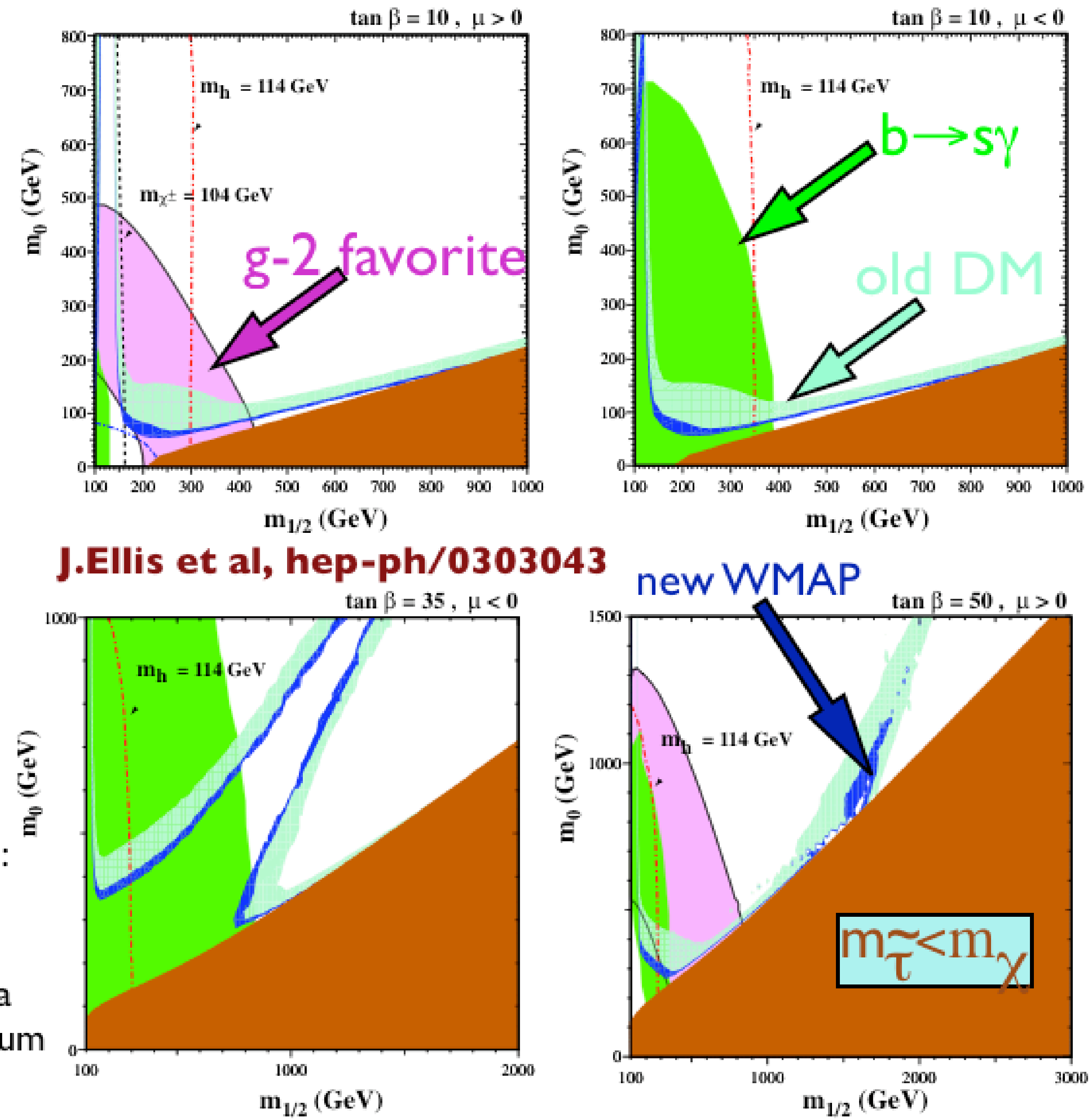
Dark matter constraints on neutralinos: a CMSSM example

old:

$$0.1<\Omega_\chi h^2<0.3$$

new WMAP

$$0.094<\Omega_\chi h^2<0.129$$



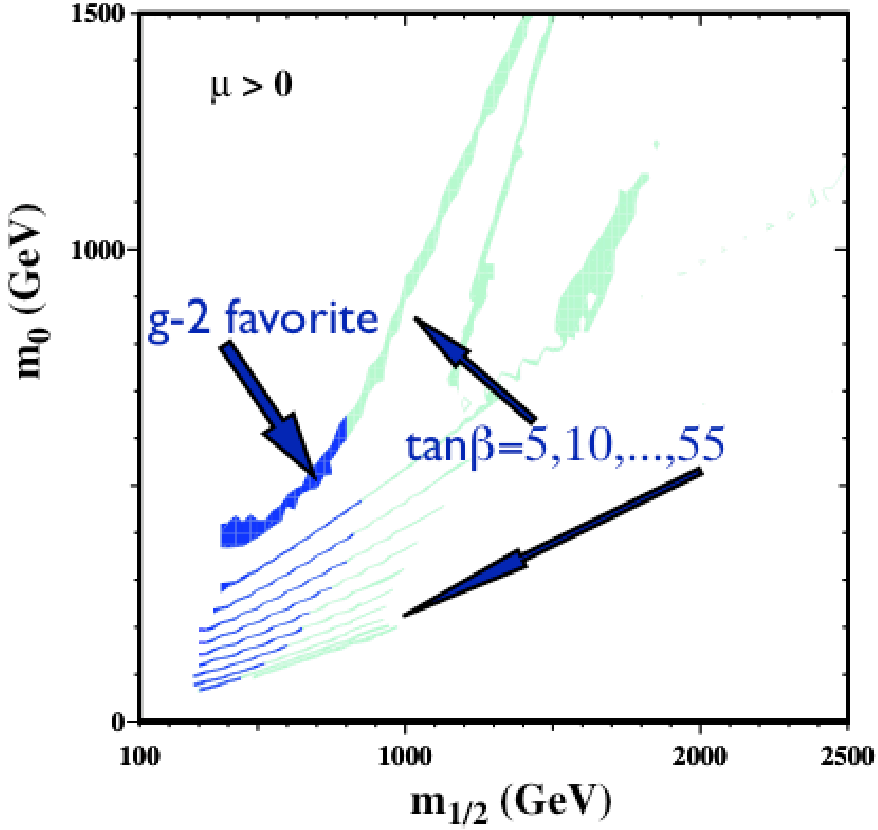
$\Omega_\chi h^2 \sim m_\chi\, n_\chi \Rightarrow$

upper limit on Ω_χ requires:

+ small m_χ, or

+ fast/efficient annihilation, a strong constraint on spectrum (to allow, e.g., $\chi\chi\to h$ at threshold or $\chi\tilde{\tau}\to\gamma\tau$)

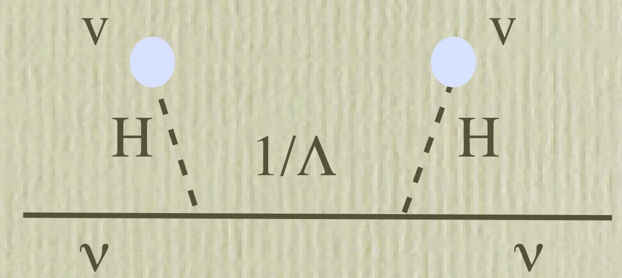
In the CMSSM the measurement of $m_{1/2}$ and m_0 (resp. m_χ and m_{slep}) will fix almost uniquely tanβ



Proving the direct and unambiguous link between cosmology, DM and SUSY would be, perhaps even more than the Higgs discovery, the flagship achievement of the LHC

Non-LHC HEP's future: Neutrinos

- **Physics case clear and strong:**
  - GUT-scale physics
  - Flavour structure
  - Leptogenesis (lepton-driven B asymmetry of the Universe)
  - Cosmology: WMAP => $\Omega_\nu < 0.015$, $m_\nu < 0.23$ eV
- Majorana nature favoured theoretically (implications for $0\nu 2e$ β-decay):



$m = v^2/\Lambda$ $\quad v = O(100\ \mathrm{GeV})$, $\Lambda = O(M_{GUT})$

- 2 relative masses, one absolute mass scale, 3 mixing angles, 1 CKM phase δ, 2 relative phases if Majorana

$\lvert\Delta m^2_{23}\rvert$	Δm^2_{12}	$\mathbf{m_1}$	$\sin^2\theta_{12}$	$\sin^2\theta_{23}$	$\mathbf{\sin^2\theta_{13}}$	δ_i
$\sim 2.6\times10^{-3}$	$\sim 7\times10^{-5}$	**?**	0.2-0.4	0.3-0.7	**<0.05**	**?**

Straightforward theoretical interpretation: entries of a 3x3 matrix

Clear criteria driving the experimental design/optimization:

source

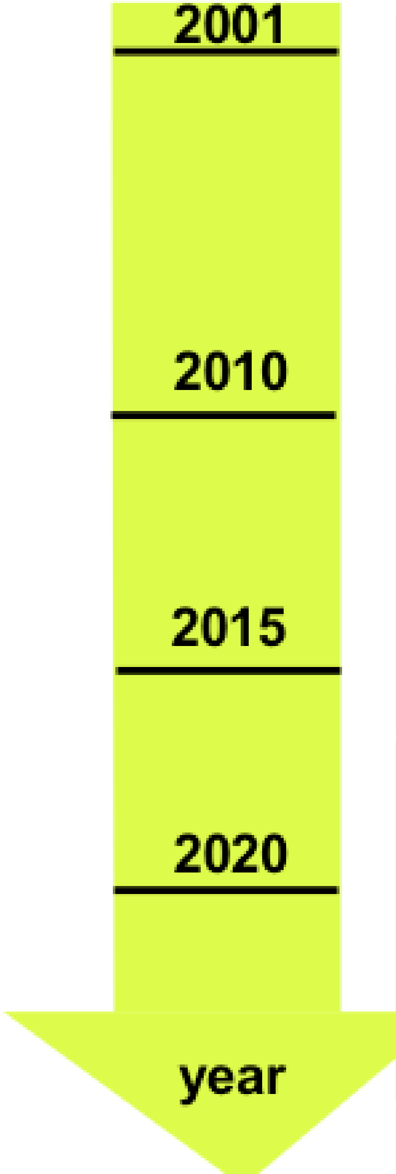
$$P(\nu_i \rightarrow \nu_j) = S \times \sin(\Delta m^2\, E / L)$$

beam purity, backgrounds — **Source power, detector Volume** — **location**

Rather general consensus on the pros and cons of different configurations:

Perhaps too much consensus? K→SK→YK→?K .....
Need to explore new detector concepts? capabilities?

Timescale



At least 4 phases of Long Baseline experiments

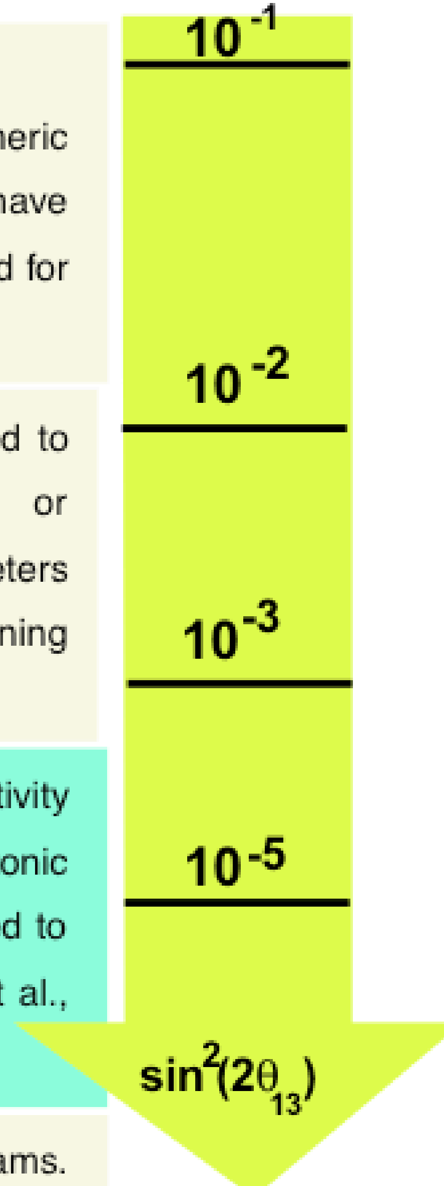
1) 2001-2010. K2K, Opera, Icarus, Minos.

Optimized to confirm the SuperK evidence of oscillation of atmospheric neutrinos through ν_μ disappearance or ν_τ appearance. They will have limited potential in measuring oscillation parameters. Not optimized for ν_e appearance (θ_{13} discovery).

2) 2009-2015. T2K (approved), Noνa, Double Chooz. Optimized to measure θ_{13} (Chooz $\times$ 20) through ν_e appearance or ν_e disappearance. Precision measure of the atmospheric parameters (1 % level). Tiny discovery potential for CP phase δ, even combining their results.

3) 2015 - 2025. SuperBeams and/or Beta Beams. Improved sensitivity on θ_{13} (Chooz $\times$ 200). They will have discovery potential for leptonic CP violation and mass hierarchy for $\theta_{13} \geq 1°$. In any case needed to remove any degeneracy from Nufact results (see P. Hernandez et al., hep-ph/0207080)

4) Ultimate facility: Neutrino Factories or high energy Beta Beams. Ultimate sensitivity on the CP phase δ, θ_{13} , mass hierarchy.



Prospects for a neutrino programme at CERN?

- Do the physics motivations of the Superbeam, βbeam and SP+βB programmes suffice to undertake the SPL (possibly + βbeam) path, or is this **justified only in the context of a subsequent νFact upgrade?**

- **What if no detector at Frejus is available?**

- This must be understood clearly before the SPL road is taken, as the νFact option it has impact on the post-LHC programme (compatibility of the νFact with CLIC??)

- Does the Eurisol physics motivation and financial opportunity suffice to undertake the construction of the SPL regardless of the answer to the above points?

Personal assessment

- The physics case for the simple superbeam option does not appear compelling
  - from the "SPL Physics case" presentation at Villars:

Q: Why proposing the SPL Superbeam if JHF will have similar results?

A1: Unique synergy with the Beta Beam

A2: Learned from the Japanese style of working, and also from CERN style, every step carries the know-how for the next step. The next could be a NuFact.

A3: Different condition to repeat the same measurement. In particular different background.

  - if T2K-I measures non-zero θ_{13}, SB will come in late, and will be in competition with T2K-II
  - if T2K-I fails, SB will at best detect a non-zero θ_{13}, but will not be in the condition to perform an accurate measurement, or to firmly establish CP violation
- **the upgrade to a νFact appears unavoidable** to justify the start of a neutrino programme based on the SPL (whether or not the βbeam option is available)
- In all cases, it is mandatory that an independent physics case be developed, and independent resources be confirmed and allocated, for the construction of the **required** detector at the Frejus

- In view of the physics case, I would bypass the superbeam/ βbeam phase, and support a plan explicitly aiming at the construction of the νFact (**to the extent that this does not jeopardize CLIC**)
- The upgrade of CERN's injector complex should be staged according to the primary needs of the LHC, with a view at a possible future νFact
- The compatibility between a βbeam option and an RCS-based injection upgrade should be explored
- The ability to assess the feasibility and costs of a νFact by the time similar info is available for CLIC (end '09?) would put us in the best position to determine CERN's future options
- The availability of the RCS PS by 201?, in addition to benefiting the SLHC, would open excellent new opportunities for the fixed-target programme

Non-LHC HEP's future: K decays

Strangeness ⇒ SU(3)

K

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

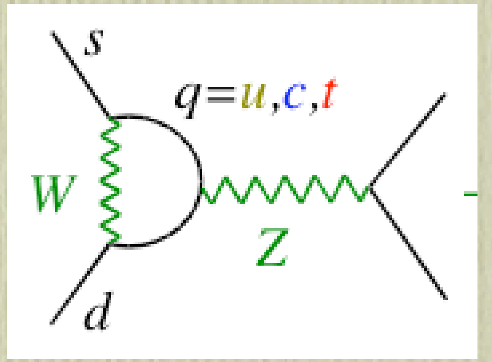
$K^0 - \bar{K}^0$ mixing/ FCNC ⇒ **GIM, charm**

More: ε'/ε, CKM parameters, CPT tests (m(K) vs m(Kbar)), etc.etc.

New frontier: very rare decays, $O(10^{-10 \div -11})$

Guiding rationale

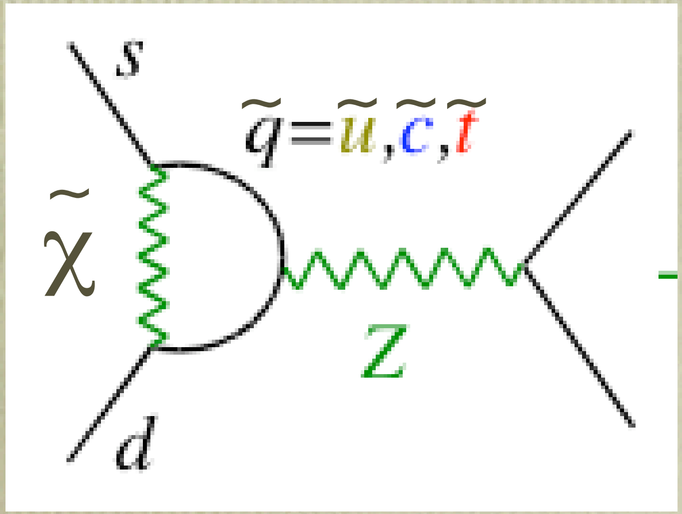
In the SM:



$\propto C\, m_t^2\, \lambda^5$, C=complex, $\lambda = \sin\theta_c$

GIM suppression of light-quark contributions, dominated by high mass scales

In Supersymmetry (similar examples in other BSMs):



$\propto f(\Delta m_{\tilde{q}}^2, \lambda^a)$, $a \geq 1$

Sensitive to whether GIM suppression operates in the scalar quark sector: tests of scalar quark mixings and mass differences

Gino Isidori

decreasing SM contrib. →

decreasing SM contrib. ↓

	$b \to s$ $(\sim\lambda^2)$	$b \to d$ $(\sim\lambda^3)$	$s \to d$ $(\sim\lambda^5)$
$\Delta F=2$ box	ΔM_{Bs} $A_{CP}(B_s \to \psi\phi)$	ΔM_{Bd} $A_{CP}(B_d \to \psi K)$	ΔM_K, ε_K
$\Delta F=1$ 4–quark box	$B_d \to \phi K$, $B_d \to K\pi$, ...	$B_d \to \pi\pi$, $B_d \to \rho\pi$, ...	ε'/ε, $K \to 3\pi$, ...
gluon penguin	$B_d \to X_s\gamma$ $B_d \to \phi K$ $B_d \to K\pi$, ...	$B_d \to X_d\gamma$, $B_d \to \pi\pi$, ...	ε'/ε, $K_L \to \pi^0 \ell^+\ell^-$, ...
γ penguin	$B_d \to X_s \ell^+\ell^-$ $B_d \to X_s\gamma$ $B_d \to \phi K$ $B_d \to K\pi$, ...	$B_d \to X_d \ell^+\ell^-$, $B_d \to X_d\gamma$ $B_d \to \pi\pi$, ...	ε'/ε, $K_L \to \pi^0 \ell^+\ell^-$, ...
Z^0 penguin	$B_d \to X_s \ell^+\ell^-$ $B_s \to \mu\mu$ $B_d \to \phi K$ $B_d \to K\pi$, ...	$B_d \to X_d \ell^+\ell^-$, $B_d \to \mu\mu$ $B_d \to \pi\pi$, ...	ε'/ε, $K_L \to \pi^0 \ell^+\ell^-$, $K \to \pi\nu\nu$, $K \to \mu\mu$, ...
H^0 penguin	$B_s \to \mu\mu$	$B_d \to \mu\mu$	$K_{L,S} \to \mu\mu$

● = exp. error ~ 10% ○ = exp. error ~ 100%

Highlighted in red modes where theory uncertainty < 10%

A measurement of the 4 decay modes

$K^+ \to \pi^+ \nu \nu$ $K^O{}_L \to \pi^0 \nu \nu$

$K^O{}_L \to \pi^0 e^+ e^-$ $K^O{}_L \to \pi^0 \mu^+ \mu^-$

is a crucial element in the exploration of the new physics discovered at the LHC.

Accuracies at the level of 10% would already provide precious quantitative information

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!

The K physics programme will find a natural complement in the B physics studies at the LHC and at SuperBELLE, and possibly in new studies of the charm sector and searches for Lepton Flavour Violation phenomena.

Experimental landscape

- E949 at BNL: stopped[2] $K^+ \to \pi^+ \nu\nu$
  - Terminated by DoE after 12 weeks or run
- CKM at FNAL: in flight $K^+ \to \pi^+ \nu\nu$
  - "Deprioritized" by P^5 after PAC approval
- KOPIO $K^0_L \to \pi^0 \nu\nu$, at BNL AGS
  - Late stage of R&D, jeopardized by RSVP extra costs?
  - **>40 events, S/B=2/1**
- P940, $K^+ \to \pi^+ \nu\nu$, modified CKM based on KTeV.
  - Proposal to PAC '05, **Data taking at t="Funding-approval + 1yr"**
  - **100 events /2 FNAL yrs**
- E391a at KEK, $K^0_L \to \pi^0 \nu\nu$
  - First run '04, more data in '05. Sensitivity 10^{-10} , below signal
- L-05 at JPARC, $K^0_L \to \pi^0 \nu\nu$
  - Proposal to PAC '05, **beam available Spring '08**
  - **100 events/3 yrs**
- L-04 at JPARC, $K^+_L \to \pi^+ \nu\nu$
- NA48/3 at CERN: in flight $K^+ \to \pi^+ \nu\nu$
  - tests on beam '04, proposal to SPSC in '05
  - **ready for beam in '09**
  - **>100 evts in 2 CERN yrs, S/B=10/1**
  - NA48/4-5: $K^0 \to \pi^0 ll$, $\pi^0 \nu\nu$, sensitivity dep on integrated Lum

DAFNE!?

For some people the room left for SUSY is too tight. Some skepticism on SUSY has emerged, and a huge effort of looking for alternatives has began few years back, leading to a plethora of new ideas (Higgless-models, Little Higgs, extra-dimensions, etc)

Some of these ideas lead to rather artificial structures, where the problem of the Higgs naturalness is shifted to slightly higher scales, via the introduction of a new sector of particles around the TeV.

The observation of new phenomena within the first few yrs of run, in these cases, is not guaranteed (nor is it asymptotically)

Few of these scenarios offer the appeal of Supersymmetry, with its clear predictions (calculability), and connections with the other outstanding problems of the Standard Model (Dark Matter, Flavour, CP violation)

Alternative scenarios

- Composite Higgs model?
  conflicts with precision electroweak data
- Interpretation of EW data?
  consistency of measurements? Discard some?
- Higgs + higher-dimensional operators?
  corridors to higher Higgs masses?
- Little Higgs models?
  extra `Top’, gauge bosons, `Higgses’
- Higgsless models?
  strong WW scattering, extra D?

Little Higgs models

- Imbed SM in a larger group
- Higgs as pseudoGoldstone boson
- Cancel top loop

$$\delta m^2_{H,top}(SM) \sim (115GeV)^2(\frac{\Lambda}{400GeV})^2$$

$$\delta m^2_{H,top}(LH) \sim \frac{6G_F m_t^2}{\sqrt{2}\pi^2} m_T^2 log \frac{\Lambda}{m_T}$$

with a new heavy T quark $m_T > 2\lambda_t f \sim 2f$ $f > 1$ TeV

- new gauge bosons, Higgses, all heavy $\gtrsim 1.2 f^2$

$M_T < 2$ TeV $(m_h / 200$ GeV$)^2$
$M_{W'} < 6$ TeV $(m_h / 200$ GeV$)^2$
$M_{H++} < 10$ TeV

No satisfactory framework for flavour, all problems shifted to the higher scale of ~ 10 TeV, not much to learn until VLHC available. Very frustrating!

Black holes at the LHC

M	$D=8$	$D=10$
5 TeV	1.6×10^5 fb	2.4×10^5 fb
7 TeV	6.1×10^3 fb	8.9×10^3 fb
10 TeV	6.9 fb	10 fb

- $M_{Planck} \sim O(1\ \text{TeV})$ =>

  pp→BH has large rates:

  $$\sigma(BH) \sim \left[\sqrt{S}/M_{Planck}^{(D-2)}\right]^{2/(D-3)} \sim S^{1/D-3}$$

- The details of the cross-section growth depend on the internal structure of the extra-dimensional space => probe geometry via BH production and decay properties

- Short distances screened by the BH => **the end of short-distance physics.** The radius of the BH grows with its M, so higher energies probe more and more IR physics (<=> string duality).

- $M_{Planck} \sim 1/(g^{1/4} L_{string}) \Rightarrow M_{string} < M_{Planck} \ll M_{BH}$

- Therefore, once BH become manifest, **experimental string physics is already accessible.**

- Since $M_{BH} \gtrsim O(5\text{-}10)\ M_{string}$ and given that we've seen no strings yet, room for this to happen at the LHC is limited.

My dream scenario

- indications of a gluino in the O(TeV) range are detected early on at the LHC (e.g. in jets+MET)
  - separate gluino from squarks, determine gluino mass scale
  - identify charginos/neutralinos, determine their mass scale
    - establish connection with DM
    - first constraints on SUSY breaking mechanism:
      - m(gluino) vs m(winos)
      - squarks seen? => no Split SUSY
      - hard photons in final states? => GMSB
  - count squarks, identify stop and sbottom, measure their mass
  - start exploration of decay chains, squark flavour separation
  - direct/indirect slepton signatures:
    - pp-> slepton+ slepton-
    - same-sign lepton FSs, ee vs mumu vs tautau <=> m(selectron) vs m(smuon) vs m(stau)
- Detect Higgs boson, establish consistency with the SUSY scenarios being outlined from the above studies:
  - m(H) vs m(stop), tan(beta), detection of extra H, associated bbH or ttH production...
- Detect Bs->mumu, search for Bd->mumu:
  - first exploration of the flavour violation phenomena in SUSY
  - indirect constraints on wino spectrum, scalar quark mixings

- If m(squark) or m(sleptons) below 400 GeV => go full speed toward a ILC
- If heavier => skip LC and go full speed toward CLIC
- In all cases, as we wait for either LC or CLIC:
  - Go full speed towards the LHC luminosity (and perhaps energy) upgrade.
  - Capitalize on the upgrade of the LHC injector complex with
    - a new programme of FT experiments, dedicated to flavour physics: very-high intensity **charm and kaon studies, $\mu \rightarrow \tau$ conversion**
    - **a higher-E/higher-intensity nu beam to Gran Sasso**
  - Full support to **SuperBelle**
  - **SuperDAFNE? τ / charm?**
  - Resurrect muon physics at BNL (**g-2, MECO**), full support to next-generation **$\mu \rightarrow e\gamma$** experiments (JPARC, PSI)
  - Full support to next-generation experiments on **EDM**, **$0\nu2\beta$**
  - Push R&D for megaton-scale detectors for **p-decay** and **nu physics**
  - **FNAL** -> ILC if m(sparticles)<400, else **-> nu fact**

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	SC PS*/** for HEP	26 → 50 GeV Intensity x 2	++	0 (if alone)	0	+
	5 Hz RCS*/**	26 → 50 GeV 0.1 → 4 MW	++	++ (ν factory)	0	+++
SPS	1 TeV SC SPS*/**	0.45 → 1 TeV Intensity x 2	+++	?	0	+++

R.Garoby

Sociological / psychological aspects:

- Hard to imagine a future for HEP without major discoveries at the LHC. To assume, as done in many phenomenological papers, that

  "... should the LHC not see the X-ion, the ILC will definitely be sensitive to its presence via loop effects in precision observables ... "

  is a compelling argument to build an ILC is, IMHO, science fiction
- We need LHC not only to discover new phenomena, but to establish a clear path for HEP, providing strong and compelling motivation to move to the next multibillion$ enterprise
  - let's not wish to discover the unexpected: the expected is rich enough to provide excitement and motivation! Not seeing the Higgs would not be progress, it would be a return to the dark ages of HEP!
  - Ex.: where do we go next if we just see a Z' at 2 TeV?
- The polarization of resources in giga-projects is a potentially fatal weakness of the field: how long can the community survive and maintain its skills without active research?
  - **The time gap between giga-$ experiences should be animated by lower-scale ancillary activities, with the potential to contribute clarifying the details of the new phenomena uncovered by the LHC/LC, etc.**
  - Prioritization should follow the criterion of "relevance to the understanding of the new phenomena"