

Conclusions and personal recommendations

Villars meeting of the SPSC, Sept 22-28 2004



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- The contents of this presentation are not intended to reflect the opinion of the SPSC: independent, subjective view, from a non-member of the SPSC

For the official SPSC Recommendations, see J.Dainton's CERN seminar, Oct 7, 2004

- I have been asked to

“... listen and discuss thru the programme with the committee and with those attending, and then to bring words of wisdom to the last 60 minutes.”

- All we've seen is **extremely exciting and worthy**, but I cannot do properly my job by simply recommending that all should be done. I will therefore allow myself to formulate *personal* evaluations, whose contents may not be shared by many of you, by the SPSC or by its individual members
- As much as possible, and with clear limitations, will force myself to use YES and NO answers instead of MAYBE

Introduction

(as a way of exposing my personal bias and perspective)

D.G. day 1: “Let **progress in physics** guide your evaluation.”

**Which physics? How far off the main path of the
HEP exploration is CERN interested in going,
motivated to go and should be allowed to go?**

- **Two levels:**
 - **leading the quest for new physics**
 - **direct searches:**
 - **LHC, CLIC**
 - **indirect evidence:**
 - **Leptons:** neutrino masses and mixings, LFV
 - **Quarks:** K, B hadron decays
 - **CPT violation searches (AD), Axion searches**
 - **exploring dynamical issues**
 - **ancillary to the exploration of the frontier, e.g.:**
 - better PDF's for LHC studies
 - **with no obvious or direct impact on the HE frontier:**
 - hadron spectroscopy
 - polarised/transverse/generalized/... PDFs
 - HI
 - ...
- **On a different Riemann sheet:**
 - “Other topics”
 - **Isolde/nTOF, future Eurisol-like activities**

LHC is the highest priority

- This is the consensus of the HEP community
- We should ensure the **fullest, safest and optimal exploitation and fulfillment of its physics potential**
- We should aim at an early approval of its luminosity upgrade, and focus the AT resources towards an early, clear definition of the injector chain upgrade path
- Priorities to new SPS-based programmes should be assigned on the basis of the
 - potential to supplement the discoveries to be made by the LHC, adding to our ability to disentangle the nature of the new phenomena observed there
 - technical synergy and compatibility with the needs of the LHC upgrade
 - immediacy of the physics return: need to guarantee an alternative to the LHC, available during the time of LHC operation

- Start by reviewing subject-by-subject the physics opportunities presented in the “*proposals*”, look at them globally at the end
- Will assume my audience followed the presentations, and will not attempt to review in any detail their contents

Heavy Ions at the SPS

- **Goals:**
 - localization of the critical point in the phase diagram (μ_B, T)
 - confirmation of the chiral symmetry restoration phase
 - low mass dileptons, thermal photons
 - determination of charm rates
 - study of rates for different charmonium states
 - high pt, Cronin effect
- **Consensus that the SPS is the ideal machine to address these issues**
- **Quite clear that the field is in rapid evolution:**
 - providing unambiguous indications of the existence of new, interesting phenomena associated to a new state of matter
 - providing more and more quantitative outputs and interpretations in the context of QCD
 - showing coherent progress in theoretical understanding
- **Need for a reassessment of the potential of the SPS to play a role in the continuous progress in the field**
- **Beam available in 2009**

Main questions

- Compelling physics case?
- Adequate experimental approach, guarantee of success?
- Need to anticipate operations to before 2009?

Compelling physics case? YES

- The **critical point is a fundamental dynamical parameter of QCD**, the finite-T/finite- μ_B equivalent of m_π or Λ_{QCD} . While we do not have a 100% certainty that the NA49 scans will succeed in pinning down the CP, it is on the other hand clear that neither RHIC nor FAIR nor LHC will have this opportunity
- The exploration of **charmonium spectroscopy is a crucial element in the complete understanding of the mechanisms for J/ψ suppression**. I don't expect this will be done at RHIC, and the LHC dynamic range is very different.
- A complete study of charm production is needed to complement the study of J/ψ 's, as well as to **clarify the origin of intermediate mass dilepton excess**

Adequate experimental approach, guarantee of success? **NO**

- **I did not perceive consensus on this issue**
- The **connection between the observables** (anomalous fluctuations in various quantities: multiplicities, pt spectra, etc) **and the presence of the CP** seems still rather weak and poorly supported by theoretical considerations (or modeling)
- The excellent mass resolution shown by NA60, with the ability to separate ω and φ peaks, offers a hope to explore the modification of light mesons in the dense medium and to connect with the physics of the chiral restoration. A **firmer connection between these observables and the underlying physics should however be put forward.**
- **NA60 appears fully prepared to complete the charm and J/ψ spectroscopy programmes, and to shed new lights on these phenomena**

Need to anticipate the run to <2009? NO

- No compelling evidence that this is the case
- No apparent risk that other facilities could get in the way of significant new discoveries at the SPS
- The time frame between now and, say, 2006-07, provides an excellent opportunity for some rethinking about the most suitable experimental programme, taking into account
 - the inputs coming from the rapid progress in the field due to RHIC analyses,
 - better theoretical understanding (e.g. Lattice results),
 - **possible innovative ideas for detectors and measurements** (see the case of small detectors at RHIC)

QCD and strong interactions

- **Strong interaction** studies will play a **crucial role**: QCD is ubiquitous in high-energy physics!

Once new particles are discovered at LHC, it will be mandatory to explore parameters, mixing patterns, i.e., **we need an unprecedented ability to interpret the strong interaction structure of final states**

Synergy: kaon system, heavy flavour, spectroscopy, pdf...

- Many **intellectual puzzles** still **open in QCD**
 - Confinement, chiral symmetry breaking, vacuum structure, hadron masses, origin of spin etc.

S. Malvezzi

- QCD studies have historically played a primary role in CERN's physics programme
 - ν and μ DIS Structure Function Measurements
 - spectroscopy
 - high- Q^2
 - jet discovery (ISR, UA2/UA1)
 - LEP, first QCD precision measurements
- The current programme at the SPS is a QCD programme (COMPASS)!

Is there a scientific case for further QCD studies at the SPS? YES

- A solid control of QCD will be required for the best use of the LHC data
- The LHC itself will provide an immense amount of QCD-related data
- Many recent experimental and theoretical developments have opened new avenues, whose role in a possible future SPS programme it is mandatory to explore

Comment

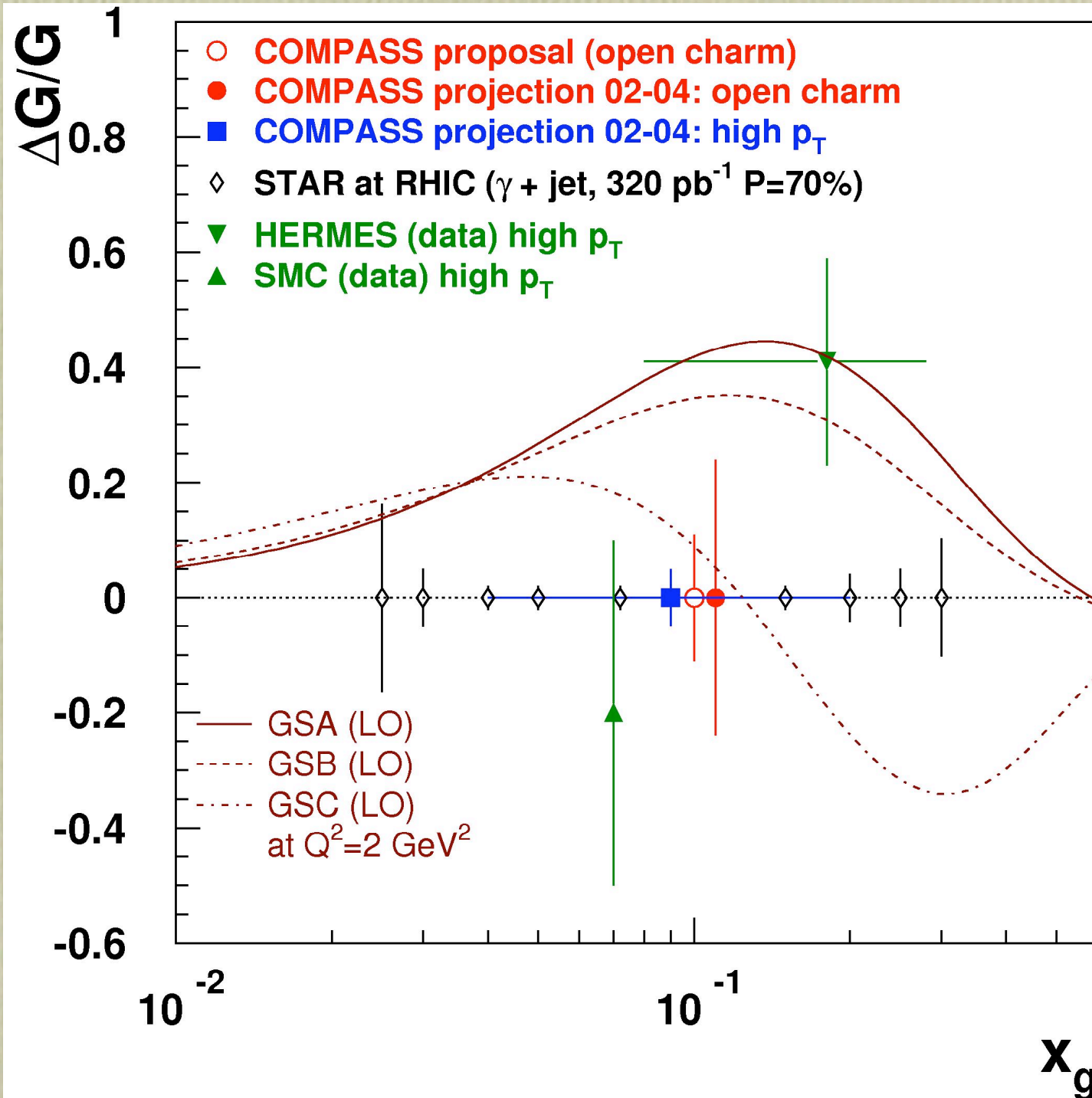
However:

- Several proposed measurements aim at improving existing results or clarifying some outstanding issues (see later)
- In these cases, the proof of ability to collect larger statistics or to explore new dynamical domains should not be good enough a motivation to support the proposals.
- I would expect to see clear and convincing evidence that the outstanding issues will be solved, and to see concrete quantitative statements about the eventual physics progress.

Parton Distribution and Structure Functions (Compass, μ beam)

- **Longitudinal gluon polarization**
 - Original goal: $\Delta G/G=0.14$. Expectation at the end of '02-'04 analysis
 - from charm: $\Delta G/G=0.24$
 - inclusive high-pt hadron $\Delta G/G=0.05$ (plus large th uncertainties)
 - Future prospects:
 - $\Delta G/G \rightarrow 0.17$ (0.11) with 1 (3) yr after '06
 - ?? after '10
 - Competition: RHIC, jet-jet, similar or smaller error, larger x range
 - Recommendation: **flagship measurement**
- **Generalised parton densities** Knowledge of transverse structure of the proton: go to the infinite-P frame, how are partons distributed on the flat disk as a function of x?. Goal: extend accuracy and range
 - Timescale: >2010.
 - Competition: rich program at DESY, JLab, but not in this domain of Q and x. eRHIC with similar kinematics, but not before 2015.
 - Recommendation: **No rush.**
- **Inclusive PDFs:** improve accuracy of old CERN experiments.
 - Not obvious. Not obvious that this will contribute to LHC (timescale not adequate to have an impact)
 - Timescale: > 2010

Expected error on $\Delta G/G$



Chiral perturbation theory (π , K beams):

Very important measurements, **extraction of fundamental parameters of low-energy QCD**, useful for the description of several phenomena, e.g. in K decays

Very accurate theoretical predictions (2%), crucial tests of the theory possible

- $\pi\pi$, πK atoms (DIRAC, PS/SPS): improve the $\pi\pi$ accuracy, perform a (accurate) πK measure; complements related measurements at Dafne (DEAR/Siddhartha)
- Primakoff production (Compass): improve, increase statistics. Lower theoretical accuracy, due to higher energy scale
- $K \rightarrow \pi^+ \pi^0 \pi^0$, K_{e4} (Cabibbo, '04) (NA48/2): new technique, potential for measurements as accurate (more?), as DIRAC's.

Renaissance of hadron spectroscopy

- **Quarkonium:**
 - η_c' (Belle, CLEO, Babar)
 - X(3872) (Belle, CDF, Do, Babar)
- **Narrow charmed states:**
 - D_{sJ} (Babar, CLEO, Belle) (parity partners of $D_s^{(*)}$)
 - $D_{sJ}^+(2632) \rightarrow \eta D_s^+$ (Selex) (?? Tetraquark ??)
 - Ξ_{cc} (Selex) ($\tau \sim 30$ fs, predicted ~ 400 fs!)
- **Pentaquark candidates:**
 - $\Theta^+(1540)$ (Chiral soliton model prediction (Polyakov talk); diquarks; prod properties?)
 - $\Xi^{--}(1862)$ (NA49, $\Xi^- \pi^-$)
 - $\Theta_c^+(3100)$ (H1, $D^{*-} p$)

Diquarks

Jaffe, Wilczek

$3 \times 3 = 6 + \overline{3} \Rightarrow qq$ in the antisymmetric colour state is attractive

Energy favours spin=0 state (Cooper pairs), and Pauli requires antisymmetric flavour ($\Rightarrow I=0$ for SU(2), $\overline{3}_F$ for SU(3))

$[qq]$ = qq pair in the fully antisymmetric state

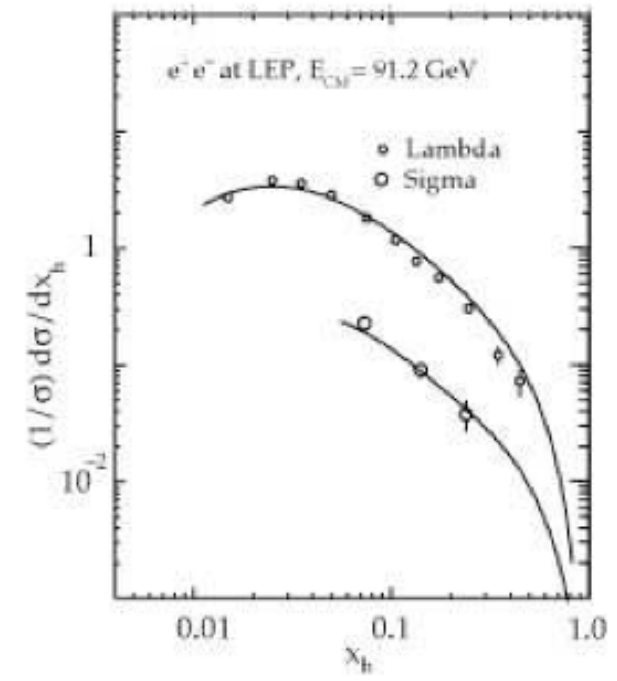
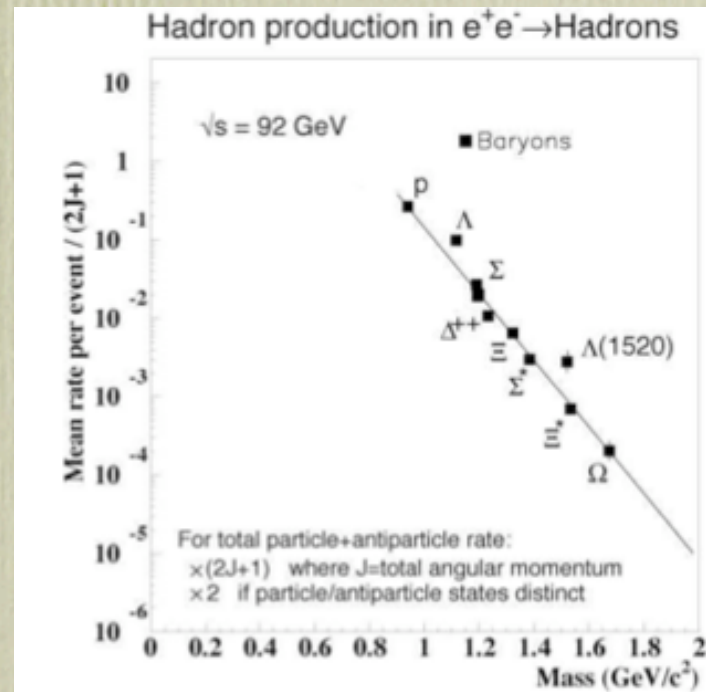
$[q q]$ = Cooper pairs at the Fermi surface of dense, large systems (n-stars?)

$[q q] [\bar{q} \bar{q}]$ = tetraquarks: scalar nonet? Selex $D_S(2632) \rightarrow D_S^+ \eta$?

$[q q] [q q] \bar{q} = (\overline{10} \oplus 8^{\text{flavour}}, J^P = 1/2^+)$

Maiani et al

Evidence for diquarks from LEP. The **ud** pair in the Λ^0 is in a $[qq]$ state, contrary to the case of the $\Sigma \Rightarrow \Lambda^0$ production favoured



Spectroscopy (Compass, p beam):

- **light mesons, glueballs, exotics (5-quarks):**
 - clarify outstanding issues (e.g. association of known resonances to glueballs): what are the new elements brought to light by these measurements?
 - study diffractive production dynamics
 - explore new issues (e.g. 5-quark production mechanisms and spectroscopy): **interesting, very active, open and competitive field**
- **doubly charmed baryons:** confirm FNAL observation, increase statistics ($\times 50$), improve accuracy of lifetime measurements, extend spectroscopy
- **Timescales:**
 - Compass: p runs from '06 on
 - Dedicated experiments at Super-PS / Super-SPS (charm): >2012-'14:
 - clarify which improvements in our understanding (aside from simple statistics) can be achieved, vis a vis the timescale and the likely progress from other experiments
 - justify the request for such high intensities
 - detail a complete research programme, and explore synergies/competition with other potential activities (e.g. rare K decays)

One comment

- How do we compare and grade the scientific value of measurements such as GPDs and exotics' spectroscopy?
- They both deal with the issue of understanding the hadron structure. The proton is more fundamental, but perhaps diquarks could open new avenues for the understanding of strong interactions (see e.g. colour SC/neutron stars, large- N Super YM)

$$\text{SU}(N)_{\text{YM}^+} \begin{array}{|c|} \hline \square \\ \hline \square \\ \hline \end{array} \xrightarrow{N \rightarrow \infty} \text{Super SU}(N)$$

- At the end of the day, any judgment will reflect very personal viewpoints, **unless a path is given, indicating which and how progress in other areas of physics will be driven by these measurements:**
 - better understanding of LHC bg's?
 - better understanding of QCD effects in K/B decays?
 - better tools for precision measurements in other machines?
 - how does CERN's overall physics programme benefit?

K decays

Strangeness \Rightarrow SU(3)

K

$\varepsilon_K \Rightarrow$ CP violation

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

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

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

decreasing SM contrib.

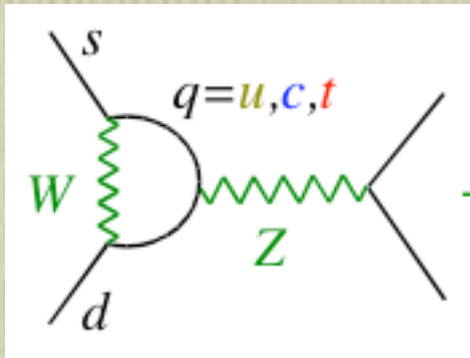
	$b \rightarrow s (\sim \lambda^2)$	$b \rightarrow d (\sim \lambda^3)$	$s \rightarrow d (\sim \lambda^5)$
$\Delta F=2$ box	ΔM_{B_s} $A_{CP}(B_s \rightarrow \psi\phi)$	ΔM_{B_d} $A_{CP}(B_d \rightarrow \psi K)$	$\Delta M_K, \epsilon_K$
$\Delta F=1$ 4-quark box	$B_d \rightarrow \phi K, B_d \rightarrow K\pi, \dots$	$B_d \rightarrow \pi\pi, B_d \rightarrow \rho\pi, \dots$	$\epsilon'/\epsilon, K \rightarrow 3\pi, \dots$
gluon penguin	$B_d \rightarrow X_s \gamma, B_d \rightarrow \phi K$ $B_d \rightarrow K\pi, \dots$	$B_d \rightarrow X_d \gamma, B_d \rightarrow \pi\pi, \dots$	$\epsilon'/\epsilon, K_L \rightarrow \pi^0 l^+ l^-, \dots$
γ penguin	$B_d \rightarrow X_s l^+ l^-, B_d \rightarrow X_s \gamma$ $B_d \rightarrow \phi K, B_d \rightarrow K\pi, \dots$	$B_d \rightarrow X_d l^+ l^-, B_d \rightarrow X_d \gamma$ $B_d \rightarrow \pi\pi, \dots$	$\epsilon'/\epsilon, K_L \rightarrow \pi^0 l^+ l^-, \dots$
Z^0 penguin	$B_d \rightarrow X_s l^+ l^-, B_s \rightarrow \mu\mu$ $B_d \rightarrow \phi K, B_d \rightarrow K\pi, \dots$	$B_d \rightarrow X_d l^+ l^-, B_d \rightarrow \mu\mu$ $B_d \rightarrow \pi\pi, \dots$	$\epsilon'/\epsilon, K_L \rightarrow \pi^0 l^+ l^-,$ $K \rightarrow \pi\nu\nu, K \rightarrow \mu\mu, \dots$
H^0 penguin	$B_s \rightarrow \mu\mu$	$B_d \rightarrow \mu\mu$	$K_{L,S} \rightarrow \mu\mu$

= exp. error $\sim 10\%$
 = exp. error $\sim 100\%$

Highlighted in red modes where theory uncertainty $< 10\%$

Guiding rationale

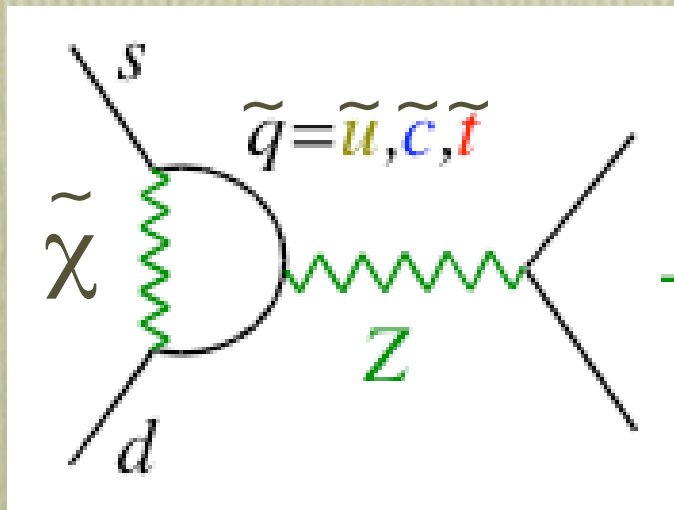
In the SM:



$$\propto C m_t^2 \lambda^5, \quad C=\text{complex}, \quad \lambda=\sin\theta_c$$

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

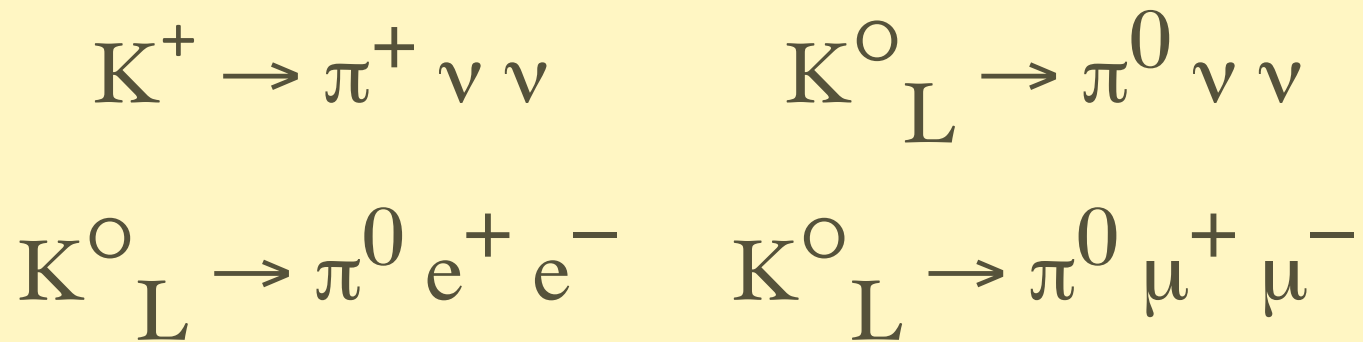
In Supersymmetry (similar examples in other BSMs):



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

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

A measurement of the 4 decay modes



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

Experimental landscape

- E949 at BNL: stopped² $\mathbf{K}^+ \rightarrow \pi^+ \nu \nu$
 - Terminated by DoE after 12 weeks or run
- CKM at FNAL: in flight $\mathbf{K}^+ \rightarrow \pi^+ \nu \nu$
 - “Deprioritized” by P⁵ after PAC approval
- KOP10 $\mathbf{K}_L^0 \rightarrow \pi^0 \nu \nu$, at BNL AGS
 - Late stage of R&D, \$30M in ‘05 President’s budget
 - **>40 events, S/B=2/1**
- P940, $\mathbf{K}^+ \rightarrow \pi^+ \nu \nu$, modified CKM based on KTeV.
 - Proposal to PAC ‘05, **Data taking at t=“Funding-approval + 1yr”**
 - **100 events /2 FNAL yrs**
- E391a at KEK, $\mathbf{K}_L^0 \rightarrow \pi^0 \nu \nu$
 - First run ‘04, more data in ‘05. Sensitivity 10^{-10} , below signal
- L-05 at JPARC, $\mathbf{K}_L^0 \rightarrow \pi^0 \nu \nu$
 - Proposal to PAC ‘05, **beam available Spring ‘08**
 - **100 events/3 yrs**
- L-04 at JPARC, $\mathbf{K}_L^+ \rightarrow \pi^+ \nu \nu$
- NA48/3 at CERN: in flight $\mathbf{K}^+ \rightarrow \pi^+ \nu \nu$
 - 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: $\mathbf{K}^0 \rightarrow \pi^0 \ell \ell, \pi^0 \nu \nu$, sensitivity dep on integrated Lum

Conclusion for K's

Absolutely clear physics case, to be pursued with the strongest determination in a global context of healthy, aggressive and very competent competition

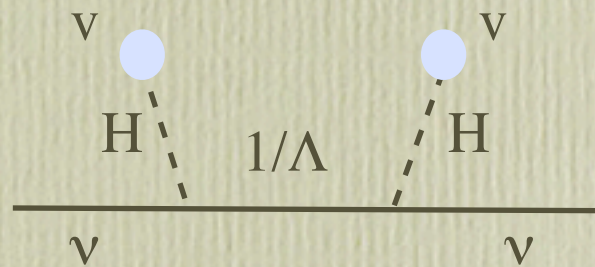
The discovery of Supersymmetry at the LHC will dramatically increase the motivation for searches of **new phenomena in flavour physics**.

The K physics programme will find a natural complement in the B physics studies at the LHC, and in new Lepton Flavour Violation searches.

The definition of a potential LFV programme and the study of its implications for the accelerator complex should be strongly encouraged and supported

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):



$$m = v^2 / \Lambda \quad \begin{array}{l} v = O(100 \text{ GeV}) \\ \Lambda = O(M_{\text{GUT}}) \end{array}$$

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

$ \Delta m_{23}^2 $	Δm_{12}^2	m_1	$\sin^2 \theta_{12}$	$\sin^2 \theta_{23}$	$\sin^2 \theta_{13}$	δ_i
$\sim 2.6 \times 10^{-3}$	$\sim 7 \times 10^{-5}$?	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:

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

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

source

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

Perhaps too much consensus? $K \rightarrow SK \rightarrow YK \rightarrow ?K$

Need to explore new detector concepts? capabilities?

Current and planned facilities

	E_p (GeV)	Power (MW)	Beam	$\langle E_n \rangle$ (GeV)	L (km)	M_{det} (kt)	n_{mCC} (/yr)	n_e @peak
K2K	12	0.005	WB	1.3	250	22.5	~50	~1%
MINOS(LE)	120	0.4	WB	3.5	730	5.4	~2,500	1.2%
CNGS	400	0.3	WB	18	732	~2	~5,000	0.8%
T2K-I	50	0.75	OA	0.7	295	22.5	~3,000	0.2%
NO _n A	120	0.4	OA	~2	810?	50	~4,600	0.3%
C2GT	400	0.3	OA	0.8	~1200	1,000?	~5,000	0.2%
T2K-II	50	4	OA	0.7	295	~500	~360,000	0.2%
NO _n A+PD	120	2	OA	~2	810?	50?	~23,000	0.3%
BNL-Hs	28	1	WB/OA	~1	2540	~500	~13,000	
SPL-Frejus	2.2	4	WB	0.32	130	~500	~18,000	0.4%
FeHo	8/120	"4"	WB/OA	1~3	1290	~500	~50,000	

Timescale

At least 4 phases of Long Baseline experiments

2001

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).

10^{-1}

2010

2) 2009-2015. T2K (approved), No ν a, 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.

10^{-2}

2015

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 $\theta_{13} \geq 1^\circ$. In any case needed to remove any degeneracy from NuFact results (see P. Hernandez et al., hep-ph/0207080)

10^{-3}

2020

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

10^{-5}

year

$\sin^2(2\theta_{13})$

After JPARC, in the standard scenario

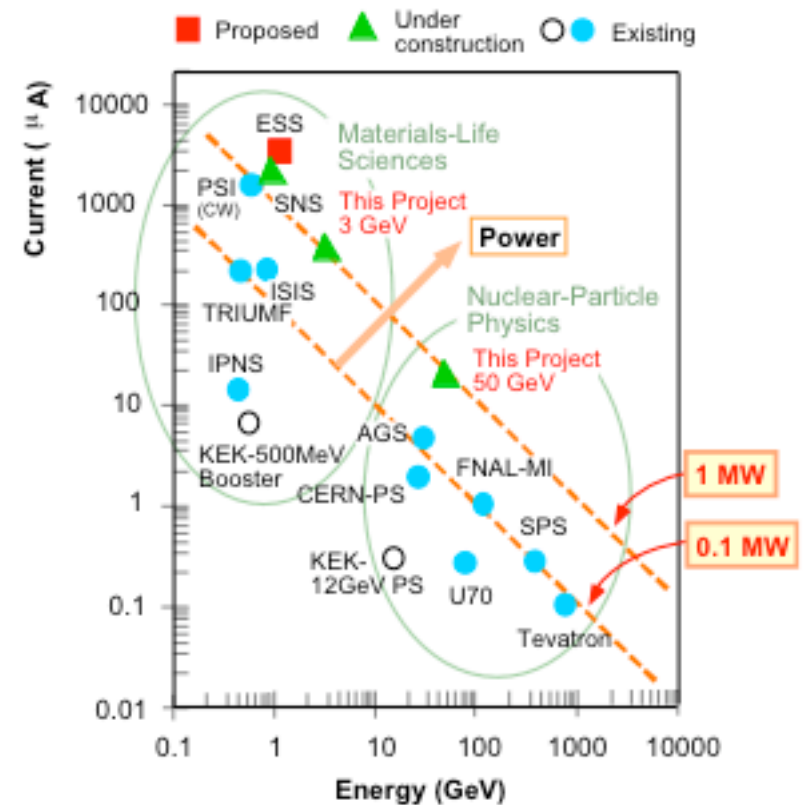
- θ_{13} , discovery or precision measure
- Mass hierarchy
- **Leptonic CP violation**

Any major improvement of JPARC will be extremely expensive:

- The proton driver is a next generation machine
- The detector is 10 times bigger of the second biggest: Minos.
- The design of close detectors system is challenging, but T2K will provide a very valuable first setup.

The knowledge of θ_{13} is necessary to guarantee the conditions to measure δ and to optimize the facility.

Any future initiative should have enough physics potential besides neutrino oscillations to justify the risk of starting the Leptonic CP violation searches without any guarantee.



Key questions for the neutrino programme at CERN

- Do the physics motivations of the Superbeam, β beam and SP+ β B programmes suffice to undertake the SPL (possibly + β beam) path, or is this **justified only in the context of a subsequent ν Fact upgrade?**
- **What if no detector at Frejus is available?**
- This must be understood clearly before the SPL road is taken, as the ν Fact option it has impact on the post-LHC programme (compatibility of the ν Fact 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 ν Fact appears unavoidable** to justify the start of a neutrino programme based on the SPL (whether or not the β beam 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

RCS PS Booster:
1.4→2.2 GeV, 0.01→4MW

??M

RCS PS:
26→50 GeV, 0.1→4MW

??M

?

X M

SPL:
1.4→2.2 GeV, 0.01→4MW

520M

βBeam

500M

ν to Frejus
 θ_{13}
CPV?

Eurisol

NA48/4: first attempt at
 $K^0 \rightarrow \pi^0 \nu \nu$

new PS: 50 GeV
Optional?

Super SPS
1 TeV SC

200-400M

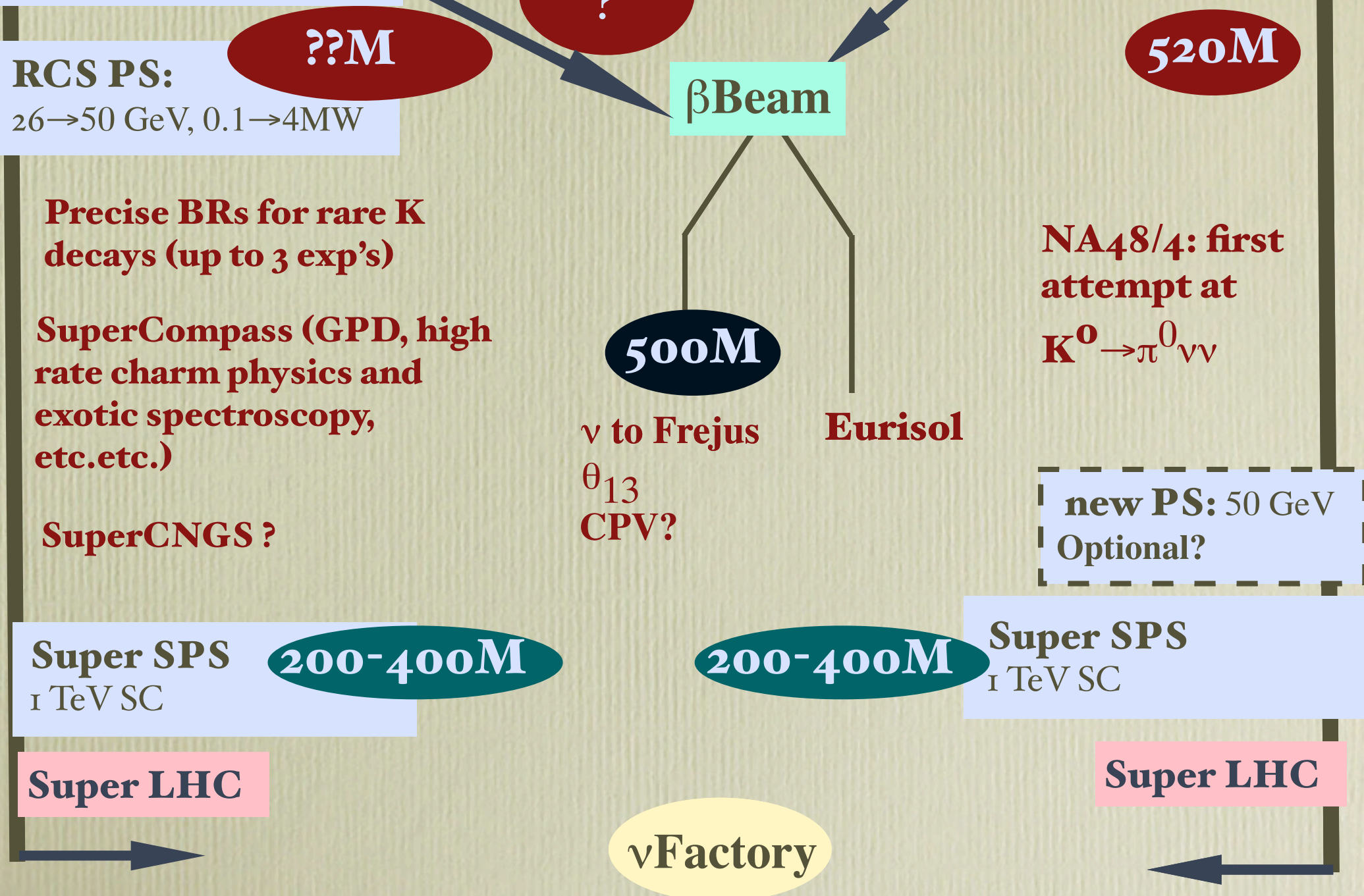
200-400M

Super SPS
1 TeV SC

Super LHC

Super LHC

νFactory



- In view of the physics case, I would bypass the superbeam/ β beam phase, and support a plan explicitly aiming at the construction of the ν Fact (to the extent that this does not jeopardize CLIC)
- The injector upgrade should be staged according to the primary needs of the LHC, with a view at a possible future ν Fact
- The compatibility between a β beam option and an RCS-based injection upgrade should be explored
- The ability to assess the feasibility and costs of a ν Fact 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 2011?, in addition to benefiting the SLHC, would open excellent new opportunities for the fixed-target programme

From the Recommendations of the High Intensity Protons WG:

- In the long term, to prepare for a decision concerning the optimum future accelerator by pursuing the **study of a Superconducting Proton Linac** and by exploring **alternative scenarios for the LHC upgrade**.

R.G. for the HIP WG

SPSC – Villars 22/09/2004

In my view this formulation is rather negative as far as the “alternative options” are concerned. A decision “prepared” by “pursuing studies” in one case, and “exploring scenarios” in the other, will prevent a meaningful and fair comparison between all options when the time comes.

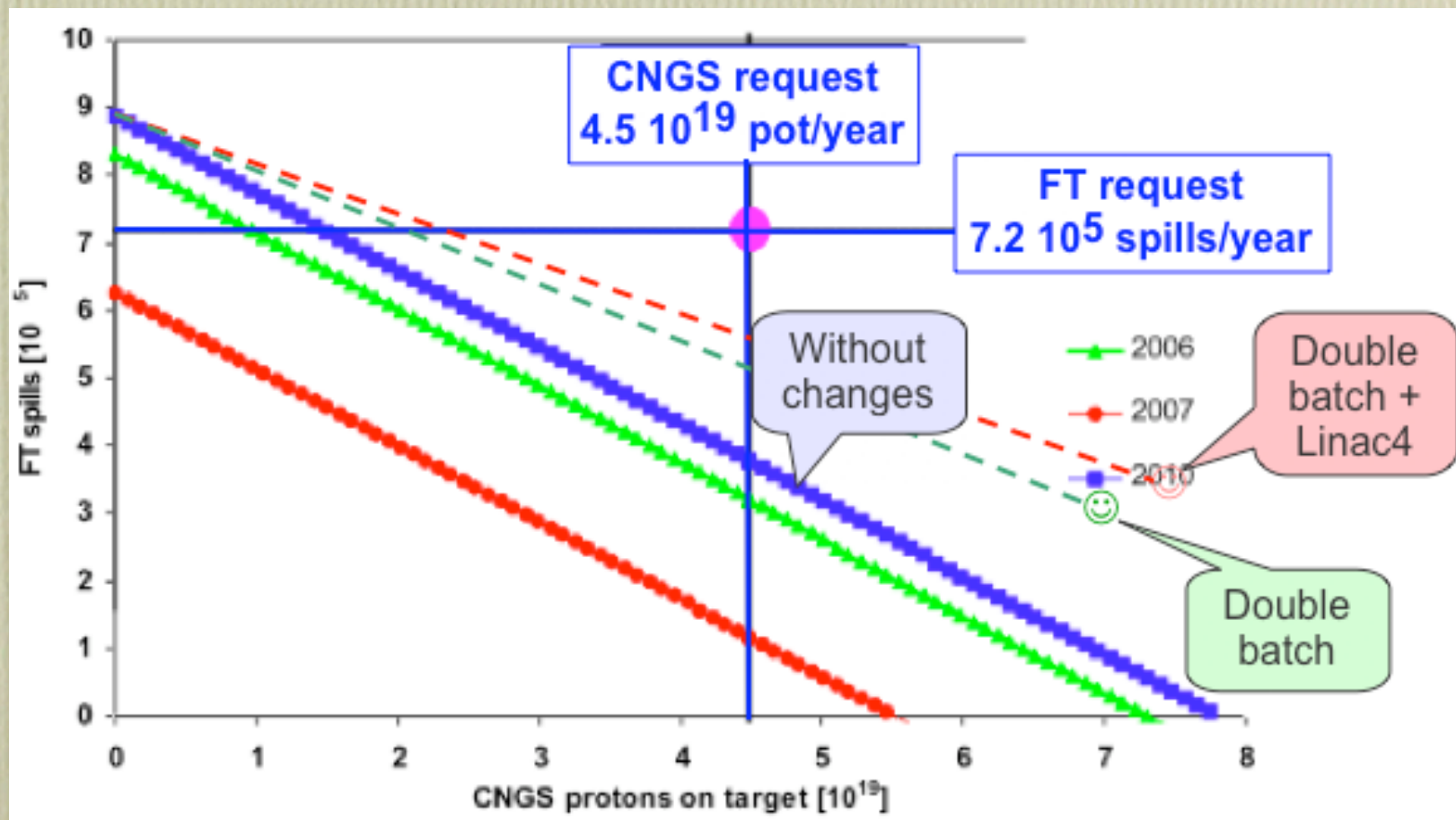
AD physics

(as seen from a man on the street)

- Probing the limits of CPT conservation is an **imperative duty** in the exploration of the fundamental laws of Nature:
 - is Lorentz invariance broken, (whether dynamically or explicitly)?
- There is **no guarantee** that any violation will ever be observed, at the AD or anywhere else.
- **If it is, the return/investment** $\rightarrow \infty$
 - \Rightarrow remember the WEB? If AD ever sees a signal, the importance of this observation will shadow any Higgs or SUSY discovery at the LHC.
- In the meantime, the AD facility offers unique opportunities for the study of complex and fascinating plasma and atom-manipulation physics

The short term (2006-2010)

- Potential for a very rich programme:
 - Compass completion (mu+p beams, **2006**→)
 - CNGS (**2006**→)
 - NA48/3 (**2009/10**)
 - Completion of the HI programme (**2009**→)
- Serious constraint from the availability of protons



- The proposed improvements to the SPS proton economics should be put on the fast track to limit the damage ASAP
- The very strong physics case, and the compatibility with COMPASS running, make the NA48/3 proposal an obvious case
- The competition of COMPASS/NA48-3 with the HI programme in the yrs ≥ 2009 poses an additional strain on the schedule. This will be evaluated once concrete HI proposals are on the table, taking into account the status of the expected competition (RHIC for Compass, P940 for NA48/3).
- Arbitration between the needs of CERN-FT and CNGS will be unavoidable, and should be based on carefully assessed priorities and opportunities (readiness of OPERA in 2006, progress of MINOS, competition of COMPASS and NA48/3 with RHIC and FNAL experiments).
- Compass beyond 2010: lukewarm about the physics programme. I will certainly develop stronger feelings once the overall framework (status of default Compass programme, prospects for SPS lum increase and NA48/3/4/... status and prospects)