# Highlights from Villars SPSC meeting (Sept 22-28 2004)

PAF/POFPA, Sept 15 2005

Michelangelo Mangano, TH-PH, CERN

Drawn from my presentation "Conclusions and recommendations" and from the official SPSC Report SPSC-2005-010 (see link from PAF/POFPA web pages)

A mix of personal considerations, and official SPSC conclusions

# The programme

| | | | | | |
|---|---|---|---|---|---|
| Wednesday, September 22 | 9:00 - 9:30 Welcome, Introduction, CERN perspective | 9:30 - 12:30 CERN Accelerators and Beams | 14:00 - 14:45 Discussion | 14:45 - 16:00 MMWatt and HIF workshops | 16:30 - 19:30 Heavy Ions |
| Thursday September 23 | 9:00 - 11:30 Heavy Ions | 11:30 - 12:30 Discussion Heavy Ions | 14:00 - 19:30 Neutrinos | | |
| Friday September 24 | 9:00 - 12:00 Neutrinos | 12:00 - 13:00 Discussion Neutrinos | 14:00 - 20:00 Soft and Hard Protons | | |
| Saturday September 25 | 9:00 - 11:30 Soft and Hard Protons | 11:30 - 12:30 Discussion Soft and Hard protons | 14:00 - 19:30 Antiprotonic Physics | | |
| Sunday, September 26 | 9:00 - 11:45 Antiprotonic Physics | 11:45 - 12:45 Discussion Antiprotons | 14:00 - 19:30 Heavy Flavor | | |
| Monday, September 27 | 9:00 - 10:35 Heavy Flavor | 11:00 - 12:00 Discussion Heavy Flavor | 13:30 - 15:40 Other Experiments | 15:40 - 16:30 Discussion Other Experiments | 16:50 - 19:30 Discussion for Summary and Conclusions |
| Tuesday, Sept 28 | 9:00 - 10:30 Discussion for Summary and Conclusions | 10:50 - 11:50 "Conclusions and recommendations" M Mangano | 11:50 - 12:30 Closing Remarks J Engelen J Dainton | | |

# SPS short-term issues (<2011)

## from the final report:

- Lack of p.o.t. to complete the approved COMPASS and CNGS programmes by 2011
  - act ASAP on accelerator complex to increase pot's
  - priority for beam to Compass in 2006, until Opera is fully operational with its nominal target mass
  - no new experiments competing for pot's with Compass/Opera until they are done
  - no HI in the SPS until after 2009 (no source/injectors lines now, LHC commissioning first once HI injector system completed)

Few proposals were presented for possible datataking in this timeframe:

- NA49 with hadron beams (Pentaquarks, etc)
- NA49 with ions (Cronin effect, high-pt suppression)
- NA60 with p beams (D->mu+mu-)
- NA60 with Pb-Pb (open charm and charmonium)
- **NA48/3 (K+ -> pi+ nu nubar)**

# PS short-term issues (<2011)

## from the final report:

- Completion of DIRAC by 2008 (already approved)
- AD unique facility until end of the decade. Full support to continuation beyond 2005. However:
  - progress will only come from R&D in both experimental techniques (high-rate antiH trapping) and accelerator (ELENA, pbar decelerator 5.3MeV-> 0.1MeV).
  - Concerns about the fragmentation of the collaborations, lack of a clear roadmap with med-long-term goals/needs
- HARP: programme completed, but likely more future requests.

# The future beyond 2010

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, and motivated 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 fronteer, 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 accelerator 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 programme alternative to the LHC, available during the time of LHC operation

# Heavy Ions at the SPS

- **Goals**:
  - localization of the critical point in the phase diagram ($\mu_B$,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-$\mu_B$ equivalent of $m_\pi$ or $\Lambda_{QCD}$. 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

These statements reflect the experimental proposal presentations made in Villars

- **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)

# The long-term future

- Ensure that the ability to carry out a HI programme after the refurbishing of the injector chain is maintained

- Explore new opportunities offered by the upgrade of the injector complex (=> engage Alice community, which has meanwhile been activated)

  - HI collisions in the SuperSPS?
  - higher intensity sources?
  - detector upgrades
  - HI collisions during the SLHC era?

- **While the proper formulation of a compelling physics case may have to wait the first LHC HI data, make sure we don't preclude a long-future for HIs**

# 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 more 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 uncertanties)
  - 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
  - => **unlikely to require further exploration at CERN >2011**
- **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

# Renaissance of hadron spectroscopy

- **Quarkonium:**
  - $\eta_c$' (Belle, CLEO, Babar)
  - X(3872) (Belle, CDF, D0, Babar)
- **Narrow charmed states:**
  - $D_{sJ}$(Babar, CLEO, Belle) (parity partners of $D_s^{(*)}$ )
  - $D^+_{sJ}(2632) \to \eta\, D_s^+$ (Selex) (?? Tetraquark ??)
  - $\Xi_{cc}$ (Selex) ($\tau$~30fs, predicted ~400fs!)
- **Pentaquark candidates:**
  - $\Theta^+$(1540) (Chiral soliton model prediction (Polyakov talk); diquarks; prod properties?)
  - $\Xi^{--}$(1862) (NA49, $\Xi^-\pi^-$)
  - $\Theta^+_c$(3100) (H1, $D^{*-}$ p)

## Spectroscopy (Compass, p beam):

- **light mesons, glueballs, exotics (5-quarks/4-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 (x 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:
    - **Need to clarify** which improvements in our understanding (aside form 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

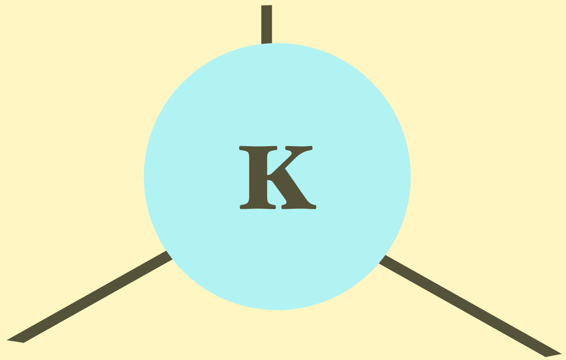
- **How do we compare and grade the scientific value of measurements such as GPDs and exotics' spectroscopy?**
- They both deal with the issue of understanding the hadron structure. The proton is more fundamental, but perhaps diquarks could open new avenues for the understanding of strong interactions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

$$SU(N)_{YM} + \begin{array}{|c|}\hline \phantom{x} \\ \hline \phantom{x} \\ \hline \end{array} \xrightarrow{N\to\infty} \text{Super } SU(N)$$

- **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)**




$K^0 - \bar{K}^0$ 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($10^{-10÷-11}$)**

A measurement of the 4 decay modes

$$K^+ \to \pi^+ \nu \nu \qquad K^0_L \to \pi^0 \nu \nu$$

$$K^0_L \to \pi^0 e^+ e^- \qquad K^0_L \to \pi^0 \mu^+ \mu^-$$

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

# Experimental landscape

- E949 at BNL: stopped[2] $\mathbf{K}^+\to\pi^+\nu\nu$
    - Terminated by DoE after 12 weeks or run
- ~~CKM at FNAL: in flight $\mathbf{K}^+\to\pi^+\nu\nu$~~
    - ~~“Deprioritized” by $P^5$ after PAC approval~~
- ~~K0PI0 $\mathbf{K}^0_L\to\pi^0\nu\nu$, at BNL AGS~~
    - ~~Late stage of R&D, $30M in ‘05 President’s budget~~
    - ~~**>40 events, S/B=2/1**~~
- ~~P940, $\mathbf{K}^+\to\pi^+\nu\nu$, modified CKM based on KTeV.~~
    - ~~Proposal to PAC ‘05, **Data taking at t=“Funding-approval + 1yr”**~~
    - ~~**100 events /2 FNAL yrs**~~
- E391a at KEK, $\mathbf{K}^0_L\to\pi^0\nu\nu$
    - First run ‘04, more data in ‘05. Sensitivity $10^{-10}$ , below signal
- L-05 at JPARC, $\mathbf{K}^0_L\to\pi^0\nu\nu$
    - Proposal to PAC ‘05, **beam available Spring ‘08**
    - **100 events/3 yrs**
- L-04 at JPARC, $\mathbf{K}^+_L\to\pi^+\nu\nu$
- NA48/3 at CERN: in flight $\mathbf{K}^+\to\pi^+\nu\nu$
    - 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

# 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**

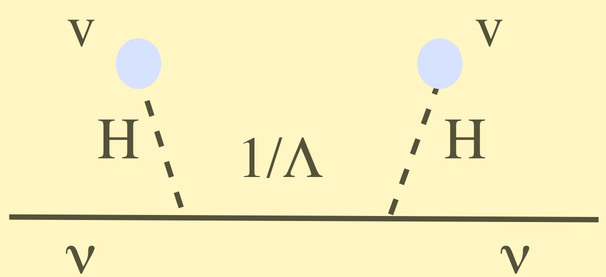
The discovery of Supersymmetry at the LHC will dramatically increase the motivation for searches of **new phenomena in flavour physics.**

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 0ν2e β-decay):




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

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

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

# 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 $\nu_\mu$ disappearance or $\nu_\tau$ appearance. They will have limited potential in measuring oscillation parameters. Not optimized for $\nu_e$ appearance ($\theta_{13}$ discovery).

**2)** 2009-2015. T2K (approved), No$\nu$a, Double Chooz. Optimized to measure $\theta_{13}$ (Chooz $\times$ 20) through $\nu_e$ appearance or $\nu_e$ disappearance. Precision measure of the atmospheric parameters (1 % level). Tiny discovery potential for CP phase $\delta$, even combining their results.

**3)** 2015 - 2025. SuperBeams and/or Beta Beams. Improved sensitivity on $\theta_{13}$ (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)

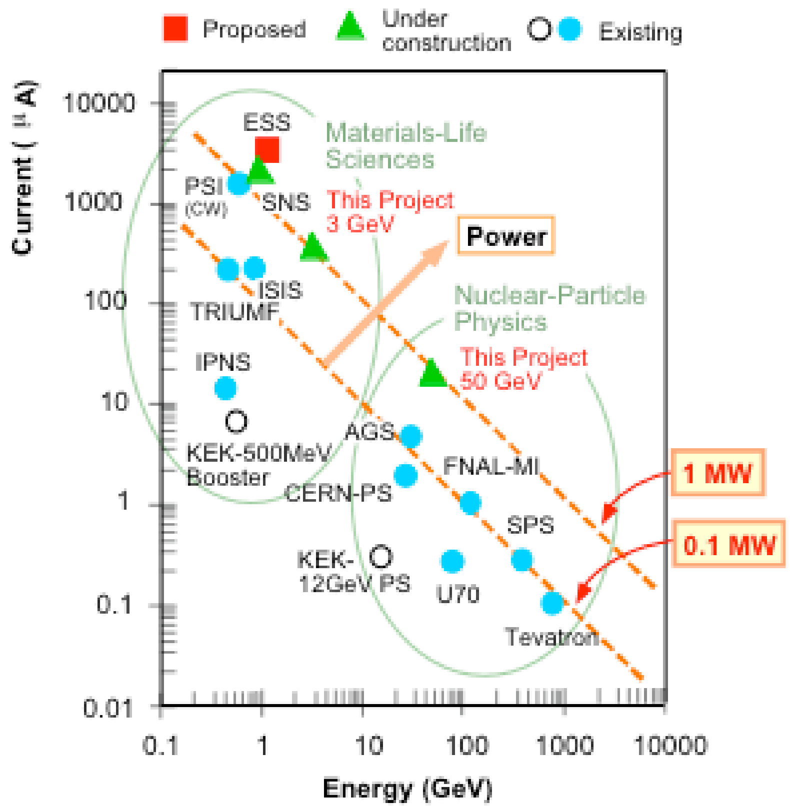
**4)** Ultimate facility: Neutrino Factories or high energy Beta Beams. Ultimate sensitivity on the CP phase $\delta$, $\theta_{13}$ , mass hierarchy.

2001
2010
2015
2020
year

$10^{-1}$
$10^{-2}$
$10^{-3}$
$10^{-5}$
$\sin^2(2\theta_{13})$

## After JPARC, in the standard scenario

- $\theta_{13}$, 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 $\theta_{13}$ is necessary to guarantee the conditions to measure $\delta$ 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, 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 has impact on the post-LHC programme (compatibility of the νFact with CLIC, S/D-LHC??)

- **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 $\theta_{13}$, 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 $\theta_{13}$, 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

From the SPSC recommendations:

- *The super-beam option does not* ***alone*** *appear the most attractive option*
- *The evaluation of the possibility of an* ***experiment*** *with a beta-beam should be completed ASAP*
- *CERN should support hadron-production exp's with nu beams*
- *CERN should support the nu-factory Scoping Study*
- *CERN should support, partecipate and coordinate R&D for cheaper, lighter,large area and magnetized detectors*

# 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. 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?

New ideas and alternative options have been proposed recently, which bypass the use of an SPL+Frejus detector by exploting higher $E_\nu$ 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: $\gamma_{He6}$=350, $\gamma_{Ne18}$=580) to LNGS (40kton Pb detector) (P.Hernandez et al, hep-ph/0312068, Donini et al 2005)
- Higher yet betabeam (LHC: $\gamma_{He6}$=2488, $\gamma_{Ne18}$=4147) to LNGS or to very-long baseline (Migliozzi, Terranova, hep-ph/0405081)

## CONCLUSIONS, part 1

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.

We therefore need to review Roland'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:

$\gamma$~100 (Frejus)

$\gamma$~350-600 (LNGS)

★ $\nu$ Factory

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

# Options for the upgrade of the CERN accelerator complex

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

# CONCLUSIONS, part II

Flavour physics in the quark and charged-lepton sector provides a most compelling case for a diversified exploitation of CERN’s accelerator complex.

So far the case for future high-intensity flavour physics at CERN has been studied only in the context of the neutrino factory complex (namely assuming the SPL+etc)

We should review this study following criteria like:

+ what are the minimal requirements are in order to achieve results of top scientific value?

+ what is the impact on (S)LHC operations?

## CONCLUSIONS, part III, a proposal for classification

- What are the minimal scenarios for a full exploitation of the LHC
- What are the additional elements/costs/etc required for a flavour physics programme (plus possibly QCD studies)
- In the above two frameworks, what are the extra requirements for a continued Relativistic HI Collisions programme
- What are the additional elements/costs/etc required for different options in neutrino physics:
    - super beam to Frejus
    - beta beam to Frejus
    - high energy beta beam (e.g. to LNGS)
    - high-power PS beam to LNGS
    - nuFact
- What are the additional elements/costs/etc required for Eurisol