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The future for CERN

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1 Introduction

We are celebrating today CERN's past successes. Thirty years from the first observation of the weak neutral currents and twenty years from the discovery of W and Z, it is reassuring to see the Laboratory completely committed to the construction of the LHC, a project at the cutting edge of physics and technology, of a dimension never tried before in particle physics. It is a heartening signal of the vitality of the Laboratory and of the strong support that we have been constantly receiving from our Member States.

We have gone through some difficult circumstances over the past two years and I have been impressed by the determination shown by the CERN staff to keep the LHC on the road and to remain at the front of particle physics. This must be the starting point of any thought about CERN's future. In addition, CERN is a very open laboratory – we have about 6000 users – and it is impossible to speak about CERN and the future of CERN in isolation from the rest of the community; the two things are quite interleaved.

Before going into the matter, let me recall that the issue of the future of CERN has been discussed many times during my mandate. Discussions in the Laboratory have started in early spring 2001 [1] just after the closing of LEP, and working groups have been created to study the different aspects. The issue was later addressed by ECFA, with a detailed study on the future of European particle physics finalised in summer 2001 [2], and by the CERN Scientific Policy Committee, then chaired by George Kalmus, with a study presented to the CERN Council in December 2001 [3].

After this report, there was an interval of about two years in which we have been more busy taking care of the present of CERN, rather than of its future. Discussions started again in March 2003, when Council considered the possible participation of CERN to the current projects on an electron–positron Linear Collider [4], followed by various meetings on the same subject during summer 2003 [5].

Finally, let me stress that I am going to present to you here strictly personal views, which do not commit in any way the next management of CERN, due to start in a few months.

Luciano Maiani

2 The future of CERN: the overall view

In a way, CERN's future is a trivial matter to describe: the future of CERN is the LHC [6]. A glance at the dates shows that this is indeed the case, to a good approximation.

We will commission the LHC in 2007 to produce physics, we believe, for some 10 to 15 years. There could be a luminosity upgrading, to be discussed presently, that can prolong the LHC life time and extend the mass range for discovery by some 20%. This is a sort of obvious thing and in fact it is even partially foreseen in the present CERN long-term plan. Thus fully exploiting the LHC can bring us to 2020 or so.

In the same framework, another aspect I want to put on record here is the consolidation programme. CERN has not been renovating its infrastructure for long time, due to the effort to produce the LHC. We have been consistently pointing out to the Council that after the start up of the

Table 1. Comparison of LHC with upgrade possibilities

	LHC	SLHC	LHCx2
Energy (TeV)	14	14	28
Luminosity in 1 year (fb^{-1})	100	1000	100
M_{Squarks}(TeV)	2.5	3	4
M_{WLWL}	2σ	4σ	4.5σ
$M_{Z'}$(TeV)	5	6	8
Extra-dim, $\delta = 2$ (M_D, TeV)	9	12	15
M_{q*} (TeV)	6.5	7.5	9.5
$\Lambda_{\mathrm{compositeness}}$(TeV)	30	40	40

Table 2. Comparison of performances of VLHC, TESLA and CLIC

	VLHC	LC (TESLA)	LC (CLIC)
Energy (TeV)	200	0.8	5
Luminosity in 1 year (fb^{-1})	100	500	1000
M_{Squarks}(TeV)	20	0.4	2.5
M_{WLWL}	18σ		90σ
$M_{Z'}$(TeV)	35	8*	30*
Extra-dim, δ=2 (M_D, TeV)	65	5–8.5*	30–55*
M_{q*} (TeV)	75	0.8	5
$\Lambda_{\mathrm{compositeness}}$(TeV)	100	100	400

(*) indirect reach (from precision measurement)

LHC significant resources will have to be dedicated to a long due consolidation programme, and I think this has to be confirmed.

So, why bother to make a talk about CERN's future? Well, the LHC cannot make us forget that there are important particle physics problems and important sectors of the scientific community that are not covered by the high-energy frontier embodied by the LHC – neutrinos among others – and I think that we must maintain the idea of diversification in particle physics. In a way this is what Georges Charpak was trying to tell us: we cannot continue to be always fully engaged into a single project. It is not in the interest of CERN and not in the interest of particle physics.

The other reason, of course, is the discussion that has started about an electron–positron Linear Collider (LC) in the energy range of 0.5 to 1 TeV. This issue is now in front of the community and we must discuss how CERN can contribute to it.

In this context, the LHC energy doubling has also to be kept in the picture. The energy upgrading is on a completely different scale than the luminosity upgrading, as it would require replacing the magnets of the LHC by new magnets that, by the way, we still do not know how to make. However, it is an option that has to be considered: costly as it may be, it will be much less expensive than making a new machine.

3 LHC upgrading

Ideas about LHC upgrading have been presented in the ICFA seminar of 2002. As for luminosity, we speak of an increase in luminosity which would bring LHC in the order of 10^{35} cm^{-2} s^{-1}, to collect in three-four years of data taking around $3000\,\mathrm{fb}^{-1}$ per experiment [7]. If you go in this direction, you would have a first phase, to reach the ultimate LHC luminosity of $2{\times}10^{34}$ $\mathrm{cm}^{-2}\mathrm{s}^{-1}$. A second phase would follow, in which one keeps the arcs, that is the main magnets, unchanged and upgrades the luminosity by changing the quadrupoles in the straight sections, to get say a factor of 5, maybe even more. The second phase would be relatively inexpensive, perhaps in the order of few times 100 MCHF

The next possibility is to replace the present 9 Tesla magnets with say 15–17 Tesla magnets, to about double the energy of the LHC. New superconducting magnets based on Nb_3Sn are being considered in FermiLab and in Europe, but this is in no way a trivial matter and it requires a good deal of dedicated R&D. Granted that the new magnets can be developed, the substitution of 27 km of LHC cryogenic dipoles will be certainly a major step, for which the case will have to be carefully assessed. A reasonable target cost should be in the order of the cost of the present LHC magnets, about 2 BCHF.

Table 1 gives what one can gain from the luminosity and from the energy up-grading [8]. We consider a number of benchmarks: the mass that can be reached in the search for the supersymmetric partners of quarks, for strongly interacting longitudinal Ws, for Z', limits on the Mass constant of gravity in extra dimensions, excited quark or indications of quark compositeness.

For comparison, we give in Table 2 the performances with respect to the same benchmarks of other machines, i.e. the Very Large Hadron Collider, the highest energy TESLA (0.8 TeV) and the highest energy CLIC (5 TeV).

4 European participation in a subTeV electron-positron collider

The strongest motivations for an electron–positron collider in the sub TeV region is that it is clearly needed for precision Higgs boson physics. In addition, if supersymmetry applies, an LC will be crucial to distinguish the Standard Model from other models because of the different projectiles.

But we have also learnt from all the exercises that we have done in these years that we really have to be able to go further than 1 TeV in energy, to sort out which, if any, of the supersymmetric models apply or to understand whether there is other physics beyond the Standard Model.

My very personal position is that Europe should not offer a site for a sub-TeV linear collider, for three reasons. First, the presently considered LCs are in the same energy (and cost!) range and are complementary to the LHC that we are building. Also, I think that the effort in particle physics needs to be shared by the other regions. While Europe is doing the LHC, it would be reasonable, and very desirable, that the other regions take the lead to construct a LC as soon as possible. Finally, and above all, I do not think Europe can afford being a major shareholder of the linear collider as we are for the LHC.

At the same time, Europe must participate in this linear collider, if it is done in other regions, much as these regions are participating in the LHC. It would be very good for our programmes to define the degree of European participation in this enterprise as soon as possible. The rest of the world is contributing about 15% to the Large Hadron Collider. Just as an indication, I think that a European participation of the order of 10 to 15% would be very reasonable and would serve best the interest of the scientific community in Europe.

I hope the issue can be discussed as soon as possible in the CERN Council.

5 Intermediate scale projects

There have been suggestions that the resources for the European participation in the LC should come, at least in large part, from the margin remaining in the CERN budget from 2011 onwards, after the LHC has been paid for (I take the occasion to stress that the positive unspent margin in CERN's budget for 2010 is reserved for the LHC contingency and I am pretty sure that we will need it all).

This is certainly a possible suggestion but, please, don't take everything out. We need some resources, at least for the consolidation plan I mentioned before and for the LHC luminosity upgrading.

In addition, and this is a point I want to make very clearly, we are in bad need of intermediate projects, not of the big collider dimension, to adapt and to prepare for the next step. In Europe, in Russia, in the US and certainly at CERN there are infrastructures and capabilities that are going to become unused in the short term because (i) the production of LHC machine and detector components is phasing out and (ii) any activity related to a big collider is certainly not going to start so soon. So, one would like to have some project of intermediate size and intermediate time scale which would fill the needs for diversity in particle physics and would utilise these infrastructures. At present, we can identify two such projects:

- the superconducting proton LINAC in CERN (I'll say more about that soon);
- the TESLA X-ray free electron laser in DESY.

Accelerator particle physicists should consider the TESLA X-FEL as really belonging to their domain. In the spirit of a network of accelerator laboratories which work together on a common set of projects, we should put the two projects in the same basket and find a way to share resources and know-how for them.

As an important added value, the SPL and the TESLA X-FEL would establish stable links between accelerator particle physics and at least two other scientific communities. This is the dream that Bjorn Wiik pursued tenaciously in DESY, for the bio-medical and chemistry community, while Carlo Rubbia was pioneering the connection with the nuclear physics community. CERN is pursuing the nuclear physics connection with ISOLDE and the Neutron Time-of-Flight facility, but I think that with the SPL we could do it on a grander scale.

Finally, on a smaller scale, I think we strongly need CERN participation in astro-particle physics projects. Ideas have been circulated, to have CERN as a European basis for:

- the integration of detectors for Space physics (e.g. the Extreme Universe Space Observatory – EUSO);
- Deep Underwater Neutrino telescopes (NESTOR/ANTARES/NEMO);
- Auger in the Northern Hemisphere;
- or others.

CERN has made a first step in the astro-particle area with the introduction of what we call "recognized experiments". The way is thus open to an active participation of CERN in that area, certainly less expensive than the high-energy area, after LHC commissioning.

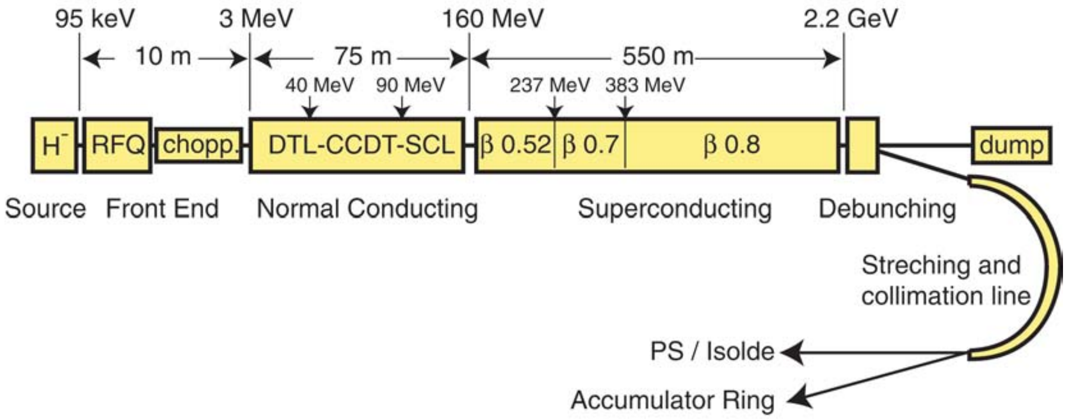

Fig. 1. Layout of the SPL and its possible location at CERN

6 The superconducting proton LINAC

A short commercial for the Superconducting Proton LINAC may be appropriate [9]. The SPL is a high intensity accelerator which drives protons up to 2.2 GeV (power on target around 4 MW). The last part of the SPL is realised with superconducting cavities; perhaps one may re-use the LEP cavities.

You can make many things with the SPL. It will make more robust the CERN injection system into the LHC, it can produce a second generation facility for radioactive ion beams (realising essentially the European project EURISOL), it will increase the intensity of the CERN-Gran Sasso neutrino beam and it can realise a new low-energy, high intensity neutrino beam. At the SPL energy one can produce pions but not K-mesons, and this would make a very pure beam of muonic neutrinos, with electronic neutrino contamination arising only from secondary muon decays. This is what is called a "superbeam" in the jargon. An underground laboratory in the Fréjus tunnel would be at the right distance for aiming this superbeam at and obtain very precise measurements of one of the two missing angles in three family neutrino mixing, θ_{13}.

A sketch of the SPL, and its possible location at CERN are shown in Figs. 1 and 2.

There is one further application of the SPL, which I consider to be extremely interesting, that is to produce the so-called "beta beams" [10]. What is a beta beam? Starting with the SPL you make an ion beam of suitable beta emitters, then you accelerate the beam to a very large energy, with the SPS, and store it in a circulating ring. The ions decay in flight to produce an absolutely

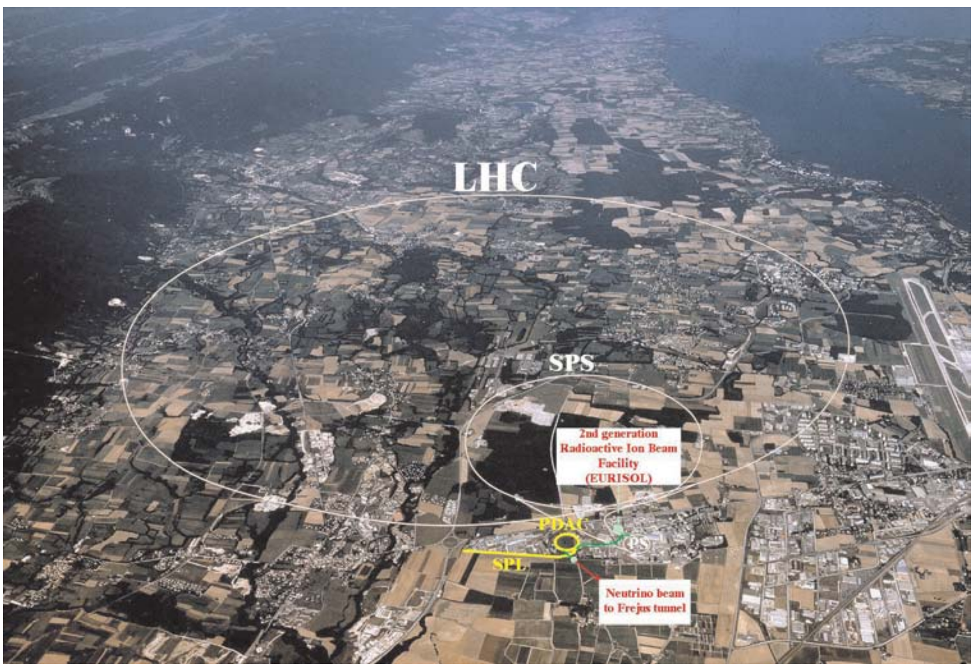

Fig. 2. Possible location of the SPL at CERN, in the Meyrin site

pure electron neutrino beam. The beam is very well collimated because of the small ratio of transverse momentum (determined by the Q-value of the beta decay) to the longitudinal momentum of the ions. Purity and collimation make this neutrino beam an ideal one for a long distance underground laboratory, e.g. Gran Sasso, to measure θ_{13} with great precision, and perhaps to observe the further angle which determines CP violation in the lepton sector.

A beta beam is less powerful than the neutrino beam from the usually considered muon neutrino factories [11], but certainly it is easier and less costly to realise. In fact, the combination of measurements made with a neutrino superbeam and a beta beam can approach in sensitivity those with a neutrino factory with 10^{21} muon decays/year [12]. There are of course several aspects of oscillation physics for which the neutrino factory is incomparable:

- the precision on the measurement of θ_{13};
- the possibility to observe $\nu_e \to \nu_\tau$ oscillation (unitarity of the mixing matrix);
- the possibility to observe and study matter effects and the matter resonance around 10 GeV.

We have a preliminary study for beams with beta minus and beta plus emitters, but much remains to be done. At this point, the beta beam is a very interesting idea whose value (and cost) remains to be assessed.

In conclusion, I think that it would be very good if just after the LHC started and during its run, CERN and Europe could develop smaller scale projects to satisfy a diversified community and to prepare for a real next step into the multi TeV.

7 A compact electron–positron linear collider

The International Technology Panel chaired by Greg Loew has recently produced, under ICFA sponsorship, assessments of the technological issues which are unsolved in the different electron–positron linear collider projects [13]. In the case of CLIC, the linear collider which is being developed at CERN, the Panel has indicated a number of crucial feasibility issues that have still to be solved. CERN is at present constructing a CLIC test facility [14] (CTF3) to address these and other issues. Within the present programme, CTF3 can produce an answer to the issues posed by the Panel by 2009 (or by 2007 if additional resources, of about 6 MCHF, are put in the programme).

If these issues are positively resolved, it would be possible, in 2010–2012, to make a proposal for a Linear Collider, capable of reaching 3 to 5 TeV. This should be done wherever it is possible and should enter operation by 2022–2025, some 15 to 18 years after LHC commissioning (with the present schedule, the LHC will come into operation 18 years after LEP).

In principle, CLIC can be staged. In case of no decision about a subTeV LC by 2010–2012, CLIC would offer a real possibility for a subTeV intermediate stage, for precision studies of the Higgs boson.

On the other hand, should a subTeV LC be decided earlier, the CLIC time scale would slide forward and, perhaps, doubling the energy of the LHC could become an attractive possibility for CERN, provided what we would have learned from the LHC by that time would justify it from the physics point of view.

All these considerations require that we do not reduce, rather increase the amount of R&D in the direction of CLIC as well as in the direction of high field magnets.

8 Conclusions

There are developments which are in our future, I would say, by default. The LHC, of course, consolidation of CERN wide infrastructure and presumably the LHC luminosity upgrade.

Besides these "normal life" options, I strongly recommend an active but restricted European and CERN participation to a subTeV linear collider, should such a facility be decided under the leadership of another region. On a shorter time scale, a new start is highly desirable in intermediate scale projects, such as the SPL at CERN and the TESLA X-ray FEL at DESY, such projects being considered within a coordinated network of allied particle accelerator laboratories. After LHC commissioning, CERN should take some initiative in astro-particle physics.

The R&D towards a multi TeV electron positron collider in the mid 2020s should be vigorously pursued from now.

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Comment by S. Glashow:

I must tell you that there is no more JLC, that the Japanese have changed the name to GLC (Global Linear Collider) – minor correction.