EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

CERN-EP/88-01
January 6th, 1988

FUTURE FACILITIES FOR PARTICLE PHYSICS

Carlo Rubbia

CERN, Geneva, Switzerland

Talk given at the
International Symposium on Lepton and Photon Interactions at High Energies
Hamburg, July 27-31, 1987

FUTURE FACILITIES FOR PARTICLE PHYSICS

Carlo Rubbia

CERN, Geneva, Switzerland

The present situation of Elementary Particle Physics is generally considered as highly productive. Major progress has been achieved in the last years in identifying what are believed to be the basic constituents of matter and the mediators of the fundamental forces acting between them. Experiments now operational at the proton antiproton colliders of CERN and Fermilab, the future e^+e^- colliders (LEP and SLC) and the e-p collider HERA will—no doubt—perfect further our confidence in the Standard Model.

However the Standard Model, very popular and today almost universally accepted, taken literally does require for its complete understanding the observation of additional new phenomena, occurring very likely at energies which are beyond the range of these facilities. Even more fundamental is the question if the Standard Model represents in principle the correct answer to the ultimate structure of matter. To this effect there are several critical issues which await clarification, like the existence of the Higgs sector, the true nature of the quark confinement, possibility of compositeness of at least some of the elementary particles and so on.

There is no doubt that a new generation of higher energy accelerators is necessary in order to further perfect our picture of matter. As a broad guess, it is believed that the next energy domain $O(1 \text{ TeV})$, in other words about one order of magnitude higher energy in the constituents frame, will eventually be rich of experimental surprises. Only colliding beams, in various forms of $[p-p]$, $[e^+e^-]$ and $[e^-p]$ can provide the practical instruments to have access to these energies.

In addition to the highest energies there are also new, crucial experiments at low energies. The new, important discovery of the B^0-B^0 oscillations, first made at CERN-UA1 and now beautifully confirmed here at DORIS has opened a new, powerful way of exploring the origin of CP violation. A "*Beauty Factory*", namely a e^+e^- collider of very high luminosity $L \geq 10^{34} \text{ cm}^{-2} \text{ s}^{-1}$ tuned on Υ resonances around 10 GeV is a very rich and promising machine which should be built. In spite of the enormous efforts in studying the CP violation phenomenon — which most certainly has relevance also with Cosmology and the early moments of the Universe — we do not know today much more of what we knew twenty years ago, at the times of the early discoveries. It is worth mentioning here the last and very accurate experiment on

interference between K_L and K_S at CERN reported at this Conference and which probably shows for the first time evidence of deviation from "superweakness".

Besides the new generations of colliding beam devices, a growing interest is developing for the possibility of non accelerator experiments and in particular to large underground detectors. The recent Supernova event has evidenced some of the most extraordinary possibilities for elementary particle physics of such phenomenon, provided it may repeat itself within our lifetime and when the new detectors under construction or planned will become operational at the Gran Sasso Laboratory, Kamiokande II and elsewhere.

In spite of the lack of success of the last round of experiments, proton decay not found at the level of the naive SU(5) superunification model, remains in the list of "*most wanted decays*". It is perhaps the only way, unless for instance relic monopoles exist and they can be found, of "peeking" in the regime of the Planck mass — even though, so to speak, through a key-hole. Indeed in the recent years the fundamental role of phenomena occurring near the experimentally unattainable $m_{\text{Planck}} \approx 10^{19}$ GeV, has grown considerably, mainly because of the impressive developments of the "superstring theories". In spite of much progress however —to my opinion —the connection between the Planck mass and the real world is still cloudy because of the tremendous extrapolation to connect for instance the physical electron with $m_{\text{electron}} = 511$ keV to the world of the Planck mass energies i.e. $m_{\text{Planck}} \approx 10^{19}$ GeV.

There are other phenomena which have direct relevance with elementary particles and which can be studied with the help of new and more powerful underground detectors. The mystery of the missing solar neutrinos has most likely an explanation which involves elementary particle physics. In some sense the Sun has become another "large elementary particle facility"! As well known, the radio-chemical experiment of Davis et al., operating since 1968, has shown that the flux of electron neutrinos from ^8B decay in the Sun is about $2 \times 10^6 \text{ cm}^{-2} \text{ s}^{-1}$ at the Earth, at least three times lower than the Standard Solar Model prediction of $5.8 \times 10^6 \text{ cm}^{-2} \text{ s}^{-1}$. At this Conference we have heard of the result of the Kamiokande light water Cerenkov detector has confirmed the presence of a solar anomaly with an upper limit of the high energy neutrino flux which is about one half the one of the Standard Solar Model.

Particularly exciting is the possibility elaborated by Smirnov and Mikheyev (see Fig. 1) of matter-induced neutrino oscillations between electron neutrinos ν_e produced in the centre of the Sun and some other kind of neutrino $\nu_x \neq \nu_e$ where $x = \mu, \tau$, or other unknown type, provided the mass of ν_x is within the range "swept" by the mass shift effect due to the enormous mass of the Sun. For energies above a threshold the neutrinos will become "sterile" for the Davis experiment which is sensitive only to the ν_e component, since it is based on the neutrino induced inverse beta decay of Chlorine.

Two very important experiments are presently planned in the Gran Sasso Laboratory and are expected to shed some light on this problem. One of these experiments (GALLEX) will detect solar neutrinos coming from the proton cycle, which is believed to be the main source of energy for the Sun. Uncertainties on the solar Model will then significantly reduced.

Figure 1. The region of neutrino parameters Δm^2 , $\sin^2(2\theta)$ for which resonant amplification of oscillations takes place in the Sun. Inside the region limited by the full lines the suppression factor for Cl-Ar and ICARUS and/or Ga-Ge(GALLEX) exceeds 10%. In the shaded region the regions suggested by the Davis result, namely a factor 3 in Cl-Ar. The dashed lines restricts the region of 10% for vacuum effect alone.

This very important radio-chemical experiment need to be complemented by direct counting experiments. A liquid Argon detector (ICARUS) especially suited for resolving this problem is also in preparation for the Gran Sasso. The high density and

the excellent spatial resolution of the liquid Argon image chamber make this novel type of detector particularly good for the direct observation of solar neutrinos from ^8B decay. Neutrinos from the ^8B decay in the solar interior can produce in the liquid Argon detector both absorption ($\nu_e + {}^{40}\text{Ar} \Rightarrow {}^{40}\text{K} + e^-$) and neutrino-electron scattering ($\nu_x + e^- \Rightarrow \nu_x + e^-$) which instead is sensitive to all types of neutrinos. The dominant neutrino absorption is a superallowed Fermi transition to the analogue state at 5.88 MeV which can be very accurately predicted. Comparing the two processes as a function of the recoil electron energy can measure both the spectrum and the nature of the neutrino radiation arriving from the Sun and help in identifying specific models and processes.

Another fantastic tool for elementary particle physics has proven to be the collapse of a Supernova, like the one which has occurred only a few months ago. We must unfortunately admit that this exceptionally rare event has caught the elementary particle community substantially unprepared. While the observation of this event in the optical astronomy has been almost perfect, we have missed the gravitational impulse because the most sensitive antennas were accidentally not operational. The recordings of the neutrino burst are also not consistent, with two different times of arrival which have split apart the observations of Kamiokande and IMB on one side and of the Mont Blanc on the other. Furthermore the detectors were not at their best: the IMB detector had a large fraction of the photomultipliers off and Kamiokande had difficulties in recording the time of arrival within several seconds. Finally, can we *really* put everybody in agreement, postulating that the star has given *two* shots?

It remains that the Supernova collapse is an extraordinary natural phenomenon which produces huge amounts of all possible types neutrino-like objects in its "melting pot", provided they are relatively light ($\leq$ few MeV). Because of the brevity of the event, lasting only a few seconds, it offers the possibility of separating their mass spectrum in a fantastic cosmological time-of-flight experiment. In order to detect the presence of such particles one must depress the large signal due to the ν_e due to the inverse β -decay of the free protons in the target using a target with bound protons, like for instance Argon and enhancing the directional signal of the other kinds of neutrinos $\nu_x \neq \nu_e$ through the reaction $\nu_x + e^- \Rightarrow \nu_x + e^-$. In order to increase the chances of observing another of such events in the next several years the detector mass must be increased to at least 100,000 Tons! Such a device would also be a fantastic proton decay experiment. It remains the fact that underground detectors of several hundred thousand tons and even millions of tons are been considered (Kamiokande II, DUMAND etc.) and they are of the "right size" for the next generations of experiments, if we wish them to have a significant role especially in elementary particle physics.

Coming back so to say on Earth, there are several reasons which suggest that the energy domain up to ≈ 1 TeV may contain the answer to the many unsettled questions. The few examples which follow have the orientative purpose in the choice of the main parameters of the new facilities. It should be clear however that this exercise reminds significantly the one of peeking in the crystal ball and that Nature very likely may have decided otherwise !

1.—*What is the spectrum of the "elementary" particles ?* Experimentally UA1 has set limits $m_L > 45 \text{ GeV}/c^2$ for the mass of the charged sequential lepton, $m_{\text{Top}} > 55 \text{ GeV}/c^2$ for the Top quark and $m_{B'} > 55 \text{ GeV}/c^2$ for the forth generation quark (superbeauty) B' , pushing them outside the range of Z^0 decays and out of reach for SLC and at least LEP-I. With the absence—that we hope provisional — of the Top quark, there are today three known families of quarks and leptons. We know that the number of lepton families cannot be too large, since (1) the number of neutrinos is limited by the Z^0 width Γ which acquires an additional contribution $\Delta\Gamma(Z^0 \Rightarrow \nu_x + \bar{\nu}_x, x \neq e, \mu, \tau) \approx 160 \text{ MeV}$ for each extra species, provided $m_x \ll m_Z/2$, (2) by cosmological arguments related to the formation of primordial Helium and (3) by the lack of observation of the reaction $e^+ + e^- \Rightarrow \nu_x + \bar{\nu}_x$.

Indirect evidence exists from the apparent good agreement between the masses of the Z^0 and $W^\pm$ and the predictions of the $SU(2) \otimes U(1)$ Model which provides an *upper limit* to the mass difference between the m_T ($q=2/3$) and the m_B ($q=-1/3$) quarks of any family of fundamental fermions. Fermion loops corrections to the value of :

$$\rho = \frac{M_W^2}{M_Z^2 \cos^2(\theta_w)}$$

which are of the form:

$$\rho = 1 + \Delta\rho = 1 + \frac{3G_F}{8\sqrt{2}\pi^2} (m_T - m_B)^2 + \dots$$

The present UA1/UA2 mass values insure that $(m_T - m_B) < 250 \text{ GeV}/c^2$ and hence $m_{\text{Top}} < 250 \text{ GeV}/c^2$. Note that although suggestive, the argument is not completely binding since even within the $SU(2) \otimes U(1)$ Model the two loop corrections may be important. In particular if $m_{\text{Higgs}} \approx 10 \text{ TeV}$, $\Delta\rho^{(1 \text{ loop})} \approx -\Delta\rho^{(2 \text{ loops})}$ and the argument is no longer valid.

The spectrum of elementary particles seems however lacking of several species, like fundamental scalars, spin 3/2 particles etc. Supersymmetry—as well known—tries to overcome to all this introducing a new spectroscopy of several "s-particles", which besides fulfilling a beneficial role in several aspects of the theory, they may even provide the basic, stable ingredient of the cosmological dark matter.

Experimentally, so far Nature has resisted to all attempts of discovering them and upper mass limits in the region $50 + 100 \text{ GeV}/c^2$ have been reported. It is clear that a new, generation of higher energy accelerators is mandatory in order to further explore the spectrum of elementary particles. In the case of supersymmetry, if we wish them to fulfill a reasonable role within the schemes so far put forward, their masses cannot too far from the ones of the ordinary fermions, Z^0 and $W^\pm$ particles and the like.

Hence in the quest for new elementary particle states a next step of about a factor ten in the centre of mass energy looks promising, although it should be clear that no one can really tell if and where new families of particles exist.

2.—*The origin of the Fermi scale.* One of the most crucial problems of particle physics today is the origin of the so-called Fermi scale of mass. While massless propagators, like the photon and the gluon(s) imply a potential between the charged or colored particles of the type $V \approx 1/R$, with his long range tail, the Z^0 and $W^\pm$ mediated weak interactions have a short range of the type $V \approx \exp(-Mr)$, due to the massive boson propagator, characterized by the quantity $M_{\text{Fermi}} = G_{\text{Fermi}}^{-1/2} = 293 \text{ GeV}/c^2$. The characteristic distance corresponding to the Fermi scale is $1/M_{\text{Fermi}} = 7 \cdot 10^{-17} \text{ cm}$.

In the Standard electroweak theory where heavy borrowing is made from the theory of phase transitions (ferromagnetism, superconductors etc.) the solution is found with two ingredients:

—spontaneous symmetry breaking

—the Goldstone theorem is bypassed by the Higgs mechanism, so that the bosons become massive.

One must recognize that the Higgs solution is rather "clumsy". For each of the ≥ 6 quarks and ≥ 3 charged and massive leptons one has to introduce an ad-hoc interaction $g_\Phi \Phi \bar{\Psi}_L \Psi_R$ and the large mass ratios m_{Top}/m_u and m_t/m_e imply also large and unexplained g_Φ ratios. Furthermore the hypothesis of a fundamental Higgs introduces the "hierarchy problem", namely the so far unresolved problem of the interplay between the Super-unification and the Fermi mass scales. There a number of attempts have been made in order to make it more acceptable. Supersymmetry helps, other solutions remain opened like technicolor, composite Higgs, composite W and Z, new constituents etc. One should not forget however that the theory of electroweak unification, combining the massless photon to the heavy W and Z vector particles has been largely tributary to ideas developed to describe superconductivity and other solid state phenomena. In particular the Standard Model reminds significantly the Landau-Ginzburg theory of superconductivity where also a fundamental scalar was postulated. In that domain, only few years later, the observation of Cooper has shown that instead the answer was to be related to the existence of an attractive force between electrons in which the phonons played a fundamental role. In other words that the effect was to be

associated not to a scalar field but rather to a dynamical property of the basic fermions. Will history somehow repeat itself also for the Higgs sector ?

In particular one must take with great skepticism the so called "no loose" theorem, widely advertised as a basic design criterion for the new hadron colliders and a "money back guarantee" for multi billion dollars investments. The argument is essentially as follows: either the mass of the Higgs is lower of ≈ 1 TeV and therefore is discovered directly or it is more massive and then Weak Interactions become strong and the perturbative regime breaks down. Born amplitudes for longitudinal W/Z or Higgs scattering violate unitarity and the onset of new regime is predicted as inevitable, in which anomalies are observed in the amplitudes $HH \Rightarrow HH$ and $W_L W_L \Rightarrow W_L W_L, Z_L Z_L \Rightarrow Z_L Z_L$. Excited W's, bound W-W states, etc. are also possible. The Higgs itself will become broad, with a width $\Gamma_H \approx \frac{1}{2} [m_H(\text{TeV})]^3$.

3.—*The origin of the quark and gluon confinement.* The fact that quarks and gluons can exist only inside nuclear matter is — to my opinion — still a true mystery. Are they "particles" like the leptons for instance or are they instead the expression of some underlying, strong interaction dynamics ? Both experimental and theoretical efforts are needed to elucidate in general non perturbative QCD.

It is worth mentioning on this point the importance of (1) new, more systematic studies of the spectroscopy of hadronic states for instance with high statistics experiments on fixed target and new very high intensity beams (Hadron Factories), (2) the high energy heavy ion colliding beams to the search for quark-gluon plasma and (3) the development of new, powerful computational machines for lattice calculation as for instance the APE project.

There is therefore ample scientific justification for a new bold step in energy of colliding beams. Such a facility can realistically be either

(1) a hadron-hadron collider with a luminosity $L \geq 10^{33} \text{ cm}^{-2} \text{ s}^{-1}$ and a centre of mass energy $\sqrt{S} \approx 20 - 40 \text{ TeV}$, namely the SSC or the LHC or

(2) an electron-positron linear collider with $L \geq 10^{33} - 10^{34} \text{ cm}^{-2} \text{ s}^{-1}$ and a centre of mass energy $\sqrt{S} \approx 2 - 4 \text{ TeV}$, which is studied presently in Europe (CLIC), USA, Japan and in the Soviet Union.

Such a bold step in energy requires equally stringent conditions of the luminosity, which is related to the well known, dimensional argument, which implies that the study of states of mass M imply a cross section which scales like:

$$\sigma(M) \propto \frac{1}{M^2} \propto \frac{1}{[\sqrt{S}]^2}$$

and hence the requirements on luminosity grow roughly like the squared of the centre of mass energy . The effect of this drop in the cross sections has considerable implications for hadron colliders, as shown in Fig. 2 where one can see that the largest energy colliders, in spite of the very large luminosity ,barely reach the "exploratory" level, comparable to the early work with the SppS collider and even the antiprotons in the ISR.

Figure 2 . Discovery potentials for hadron colliders, arbitrarily defined as exploratory, systematic and precision and scaled as a function of the centre of mass energy with the general scaling law $\sigma \propto 1/E^2$.

The luminosity limitation are best evidenced when specific experiments are taken into considerations. In Fig.3 one shows the "discovery potentials" of the SSC for a variety of particles, as a function of their hypothetical mass expressed in units of centre of mass energy $\sqrt{s} = 40$ TeV, taken from the SSC Design Report. The calculations have been performed for 1 solid year of running time at a top luminosity of 10^{33} cm⁻² s⁻¹. Backgrounds and detection efficiencies have been taken into account as realistically as possible. this offers the interesting possibility of trading off a lower energy, for instance in the case of the LHC when compared to the SSC by an increase

in luminosity. Detailed considerations show that in this energy domain the gain in rate of a factor two in energy is in practice "recoverable" with an increase of a factor 5 + 10 in luminosity. However the "detectability" of the process may become more difficult at lower energies because of (1) lower signal-to-noise ratio and (2) rate limitation in the detectors.

Figure 3. Graphic representation of the sensitivity to some of the new possible physical processes of the SSC estimated on the basis of a luminosity of $10^{33} \text{ cm}^{-2} \text{ s}^{-1}$ and one full year of running for a 4π detector. Note that in all process one is very far from the kinematical limit. Sensitivity is primarily determined by the rate of collisions in the constituents frame.

The total hadron-hadron collision is slightly rising to something like $\sigma_{\text{tot}} \approx 100$ mbarn around $\sqrt{s} = 20 + 40 \text{ TeV}$ and therefore the signal-to-noise ratio for the physically interesting phenomena is rapidly diminishing. This is shown in Fig. 4a and 4b where one sees for instance the two jet cross section with transverse energy $0.06 \sqrt{s}$ down to about 1 nbarn, namely to $10^{-8} \sigma_{\text{tot}}$ at LHC energies. The more interesting total cross sections for a new Z' is $10^{-10} \sigma_{\text{tot}}$ for the relatively low mass of 1 TeV and a Higgs particle of 0.5 TeV occurs at a level as low as $10^{-11} \sigma_{\text{tot}}$. Decay branchnig ratios and detection efficiency are going to reduce these signals even further.

The presence of such a huge backgrounds of unwanted collisions has raised the most serious concern on the ability of detecting the kinematically allowed processes at hadron colliders. The record of the first of such machines, namely the ISR, has been somewhat disappointing. Although the ISR had plenty of luminosity and the cross sections for the interesting process were relatively large, the bulk of the observations in the Charm and Beauty sectors were outperformed by electron positron colliding beams.

Figure 4a. Proton-proton total cross section and cross section for two jets with transverse energy larger than 0.06 of the kinematic limit. One can see how while the total number of events rises slowly "interesting" physics drops roughly like E^2 .

Figure 4b. The same of Fig. 4a, but extending to much lower cross sections. In addition to the jet cross section of Fig. 4a it is shown the cross section for a 1 TeV Z' and for a 0.5 TeV Higgs particle.

For instance the cross section for Charm production at the ISR is about 0.5 mbarn, namely $10^{-2} \sigma_{\text{tot}}$ and the one for Beauty is probably $10^{-5} \sigma_{\text{tot}}$. The reasons given at the time for the apparent lack of success of the hadron collider — as well as for the experiments on fixed target at somewhat lower energies at CERN and Fermilab — had to do precisely with the "dirty" nature of the hadronic processes, when compared with the "purity" of the electrons. With the discovery of the W and Z at the SppS Collider the balance apparently moved to the other side and hadron physics seems today certainly "cleaner" and "easier" than what was thought to be sometime ago. With the useful signals becoming such a microscopic fraction of the bulk of the interactions, one may however still express the most serious concern on the manageability of the exploratory experimental programme at the LHC/SSC.

The suitability of a given production reaction (namely hadrons or electrons) and the corresponding detector technology depends crucially on the kind of process one wants to study. So far hadron initiated experiments have almost exclusively made discoveries using the Drell-Yan signature of the emission of a pair of energetic leptons ($J/\Psi, Y$ and Z^0) and more recently the lepton-missing energy coincidence ($W^\pm \Rightarrow e\nu, \mu\nu$ and $\tau\nu$). On the other hand, hadronic final states and quark spectroscopy are far more difficult, as one can see from the difficulties encountered for instance by UA1 in identifying heavy quarks, and looking for supersymmetry, Higgs and the like. *Unfortunately, the main expectations in the field of $O(1 \text{ TeV})$ physics are new particle states of which we would like to map the spectra. One must recognize that in this domain the record of hadron colliders has been — so far — quite modest.*

In addition, with respect to today's detectors, the detector for the LHC/SSC will have to operate at ultra high rates of unwanted events and they will suffer a significant radiation damage. Can one then hope to build a suitable detector? I believe the answer is probably a yes with cautious optimism and provided an entirely new technology of detectors is developed. In fact at those rates one can no longer use scintillators, light pipes, phototubes, wire chambers. What is then left of a today's detectors? In addition there will be the need of a very sophisticated trigger technology, capable of selecting one good event out of $10^8 + 10^{10}$ triggers. New and expensive calorimeters, very fast electronics with a very large number of channels, new vertex detectors have to be invented. Many ideas are around but a coordinated effort is now necessary.

Even if large superconducting hadron colliders (LHC/SSC) are perfectly adequate to conduct a first phase of exploratory physics in the $O(1 \text{ TeV})$ energy domain, a second, independent and complementary exploration with electron-positron colliders is mandatory. Hadron colliders—if successful—will provide crucial information on the definition of the parameters, like energy, luminosity and energy resolution for a

suitable electron positron machine. This second phase is necessary, since there are a number of fundamental physics issues which can only be answered with an adequate electron positron collider:

(1) there is no known method of searching for the Higgs with hadron colliders if heavy and below the threshold for W-W and Z-Z production. On the other hand the most reasonable guesses would suggest that $m_{\text{Higgs}} \approx (0.5 + 1.5)m_W$ and therefore an important window is inaccessible by hadronic methods.

(2) There is no way of performing a spectroscopy of new particle states, either new quarks, supersymmetric partners and so on. Like in the case of the Charm and Beauty quarks, electron-positron initiated experiments are necessary as soon as some new phenomenology is unveiled by the hadron collider.

The realization of an electron-positron collider in the TeV range requires necessarily new accelerator developments, the natural limit of circular machines being already attained by LEP. Although it is not within the scope of this report to discuss in detail accelerator schemes, some of the critical point must be mentioned. There are in principle two schemes:

(1) a beam from a Linac of energy E_1 collides against a low emittance beam of energy $E_2 \approx 70$ GeV stored in a large circular ring (LEP). The centre of mass energy $E_{\text{cm}} = \sqrt{S} = 2\sqrt{E_1 E_2}$ is rapidly limited as soon as $E_1 \gg E_2$. However it is possible to obtain for instance $E_{\text{cm}} = 500$ GeV with $E_1 = 800$ GeV and $E_2 = 70$ GeV. The scheme has the advantage that the stored beam can be a high current positron beam while the linac accelerates a tiny electron current. One can arrange the emittances in such a way as to have a destructive beam-beam interaction only for the beam from the linac.

(2) both electron and positron beams are accelerated by linacs and collide head-on following the technique being pioneered by SLAC (SLC). The typical conditions at the collision point are summarized in Table 1. They refer to the CERN project, although the other designs are not fundamentally different. One has assumed a centre of mass energy of 2 TeV and a luminosity of $10^{33} \text{ cm}^{-2} \text{ sec}^{-1}$. Compared to SLC the factor 150 increase in luminosity is realized (i) increasing the repetition rate of a factor 35, (ii) reducing the β -value at collision by a factor 2, (iii) the remaining factor 2. being associated to the beam dimensions and currents. The power of the beams is now 20 times larger than the SLC, achieved reducing the number of particles per beam to compensate the increase in repetition rate and in the beam energy. The emittance must be correspondingly reduced to keep a sufficiently large disruption parameter.

Table 1. Main Parameters of the CLIC electron-positron Collider

Quantity Ratio with. SLC	Value respect to		
Centre of mass energy	2.0	TeV	20
Luminosity	10^{33}	$\text{cm}^{-2} \text{sec}^{-1}$	150
Number of interaction regions	1		1
Repetition frequency	5.8	Khz	35
RF frequency	30.	Ghz	10
R.m.s. bunch length	0.5	mm	1/3
Number of particles/bunch	$5.4 \cdot 10^9$		1/10
β -value at collision	3.0	mm	1/2
Invariant emittance of beams	$2.0 \cdot 10^{-6}$	rad.m.	1/10
Spot radius at collision point , r.m.s.	65	nm	1/20
Power/beam	5.0	MW	1/50
Disruption parameter	.9		
Average energy beamstrahlung photons	46	GeV	
Beamstrahlung power/beam	0.5	MW	

The most striking result of this extrapolation is the fact that the spot size has now an area which is 400 times smaller than the one of SLC design. It is not clear if one can insure in practice such a precise matching between two beams.

Another feature which may rise concern is the transverse emittance which is 1/10 the one of SLC. While there are methods of constructing damping rings with such an

Figure 5 . Gradient and power/meter for a 30 GHz copper cavity for 1 TeV Linac, as a function of its length

emittance, it is not obvious that it can be preserved all along the acceleration process. The positron yield is also quite large, about $3.0 \cdot 10^{13}$ / sec.

The linear collider must be based on a cavity scheme which is capable of transferring a good fraction of the RF energy to the beam, since the beam loading alone is as much as 10 MW average. In principle superconducting cavities would be ideal, except that at the present state of the art the gradient cannot exceed $5\div 10$ MeV/metre. The length of a [1+1] TeV collider would then become prohibitively long, namely $200\div 400$ km ! Awaiting for the progress in the superconducting cavities one has therefore to use copper structures or more or less conventional type. It has been shown recently that these cavities can withstand a very large electric gradient provided the wavelength and hence the size is reduced and that enormous peak powers are injected in the structure (Fig.5).

Figure 6 Normalized parton luminosities at the LHC and CLIC as a function of the effective parton centre of mass energy

Figure 7. Cross section for W_L - W_L fusion as a function of Higgs mass in proton proton and electron positron interactions.

For instance a 1 TeV Linac of 6.25 km length will have a gradient of 160 MeV/m and require a peak power per unit of length of 384 MWatt/m or a total peak power of $2.4 \cdot 10^{12}$ watt at 30 GHz.

There is no conventional RF source which can meet such requirements. Therefore the idea is been explored of using a high power, relativistic beam to excite a cavity structure, which then in turn transfers such a power to the main beam of the Linac, operating in some sense like a step-up transformer between the two beams. A number of practical arrangements are being studied actively, which differ basically in the choice of the acceleration method for the driving beam. In the US, the preferred solution seem to be the one of an electron induction accelerator — of the type developed at LLL and capable of currents of about 5 k A at about 5 MeV— in order to drive either a FEL structure or a relativistic klystron. At CERN one has considered instead an auxiliary Superconducting Linac with a ≈ 4 GeV beam. Typical parameters as a driver of a 6.25 km 1 TeV linac are: a voltage gain of 12 GeV, a frequency of 350 Mhz, an accelerating gradient of 15 MeV/m and an active length of about 800 m. The cryogenic power load is about 67 Mwatt. Clearly the new superconducting materials at

higher operating temperature, if acceptable would be very valuable. Even more exotic schemes are considered in which a relativistic proton, rather than an electron beam is used to excite the accelerator structure.

It is clearly too early to say if a 2 TeV linear collider with a luminosity of $10^{33} \text{ cm}^{-2} \text{ sec}^{-1}$ is feasible. However its impact in the energy domain $\mathcal{O}(1 \text{ TeV})$ will be comparable and under certain aspect even superior to the one of a hadron collider of LHC/SSC type, as shown in Fig 6 and Fig. 7.

I would then like to conclude with a "boutade" which however represents rather well the present dilemma :

" to choose between a machine we know how to build , but for which so far no satisfactory detector has been proposed and a machine for which the present detector technology is adequate but for which no clear machine design exists so far! "