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A PERSPECTIVE ON LEPTON-PHOTON PHYSICS

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ABSTRACT

This paper reviews some key experiments of the past in which the same basic physical processes are attacked both through lepton-photon interactions and by using hadron machines as primary tools. Not surprisingly, it is concluded that the basic distinction between lepton-photon physics and elementary particle physics in general is unreal but that the tools and methodology can be very different indeed. A look is then taken into the expected future evolution of particle accelerators. Existing accelerator technologies both for proton and electron colliders are approaching basic limits as the collision energy in the constituent frame is raised. At this time no clear path exists for electron-positron colliders to compete with the SSC as far as energy reach is concerned. but the superior clarity and coverage of phenomena not accessible to hadron colliders-makes it absolutely essential that the development of both electron-positron and hadron colliders be pursued vigorously. It is concluded that accelerator R&D effort underway is insufficient if a large hiatus in productivity in particle physics is to be avoided. Electron-positron linear colliders are the most promising approach for the extension of knowledge beyond LEP and beyond the SSC, but the difficulties to reach an electron-positron energy of 15 TeV or beyond in the constituent frame look formidable. Both electron-positron and proton colliders appear to face severe future detector limitations, the former due to electron-positron pair creation during the collision and the latter due to the enormous hadronic background event rates.

INTRODUCTION

The organizers of this conference have assigned me the title of "A Perspective on Lepton-Photon Physics." The advantage of the term "perspective" is that it applies both looking backward and forward; let me start by practicing some hindsight.

The program of this and the preceding conferences makes it abundantly clear that the subject of lepton and photon physics as an isolated topic does not really exist; the more we learn the less valid is that distinction. Traditionally, the separation originated principally through the tools used, rather than the physical interest expressed. Let me illustrate this pattern by reflecting on some past experiments where the same physics has been attacked, starting with hadrons and with photon and lepton beams.

THE 3-3 RESONANCE

The A states of the proton were first seen in photon beams from electron synchrotrons. R. R. Wilson and collaborators[1] at Cornell charted the approach to the resonance, and the Cal Tech group took the data over the peak. Copious production of the A became evident at the Chicago Synchrotron and the unambiguous identification of the spin parity of the A then became possible.

Figures l(a) and l(b) juxtapose some graphs from the photoproduction and scattering experiments. I leave it to the audience to judge whether this is lepton-photon physics or hadron physics.

DISCOVERY OF THE π^0

Extensive theoretical conjectures that there should be what is now recognized to be a neutral pion were developed before the war from cosmic ray evidence. At the 184-inch hadron synchrocyclotron at Berkeley, B. J. Moyer and collaborators[2] observed the gamma-ray spectra originating from hadron–hadron collisions in internal targets, and these spectra were clearly consistent with decay of a neutral pion into two photons. However, the real identification of the neutral pion came from the experiments of J. Steinberger and collaborators[3] in the photon beam of the Electron Synchrotron at Berkeley by observing gamma-gamma coincidences from neutral pion decay. This experiment constituted a dramatic demonstration of the decay kinematics unique to the neutral pion. Figures 2(a) and 2(b) show results of these two experiments in juxtaposition.

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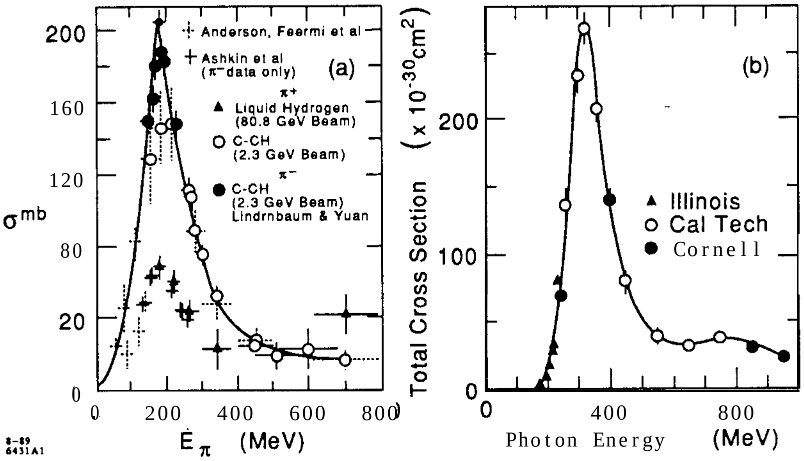

Fig. 1. The 4 resonance.

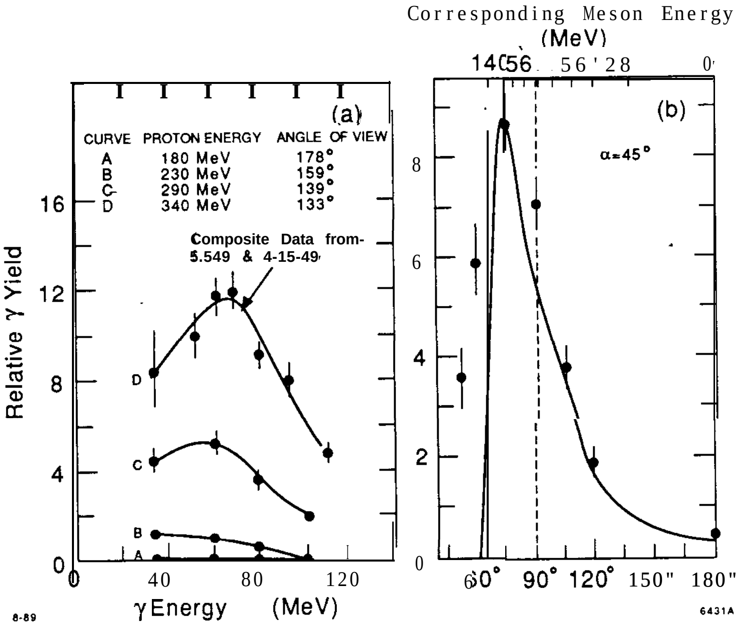

Fig. 2. **Experimental history of** the π^0.

THE PION PROTON INTERACTION AND THE SPIN PARITY OF THE PION

In 1948, the absorption of negative pions on the proton at rest resulted in a gamma-ray spectrum which proved directly measurable. The pions were produced in an internal target struck by the proton beam of the 184-inch cyclotron at Berkeley!’ At the same time, the gamma-ray spectrum also revealed the charge exchange process leading to neutral pions. You can judge the progress of instrumentation days by considering a “biomechanical” coincidence circuit that was used to register electron-positron pairs produced by the gamma-rays observed from the chamber in which negative pions were captured. This coincidence circuit consists of a square array of nails arranged in a 15 x 15 matrix. When flashing lights indicated a coincidence between arrival of an electron and a positron, a washer was thrown by the experimenter over the relevant nail in the matrix and the accumulation of the piles of washers in the matrix in-

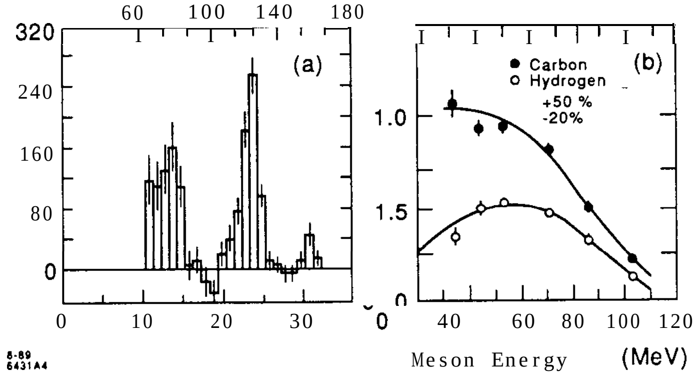

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Fig. 3. Photoproduction/absorption **of pions.**

dicated the spectrum generated. There has been a bit of progress in instrumentation since then! While this might be considered a hadron experiment, the inverse reaction which gives similar information by detailed balancing is the photoproduction of charged pions on the nucleon. Such photoproduction of π^+ and π^- mesons was brought under investigation at the 300-MeV electron synchrotron at Berkeley[5] at the same time at which the π^- absorption experiments were done at the proton synchrocyclotron.

Figures 3(a) and 3(b) give the two results in juxtaposition. These experiments deal with essentially the same basic matrix elements observed in hadron and photon machines measuring mutually inverse processes, albeit at different energies.

PRODUCTION OF VECTOR PARTICLES BY VIRTUAL PHOTONS

This topic has a long history too complex to cover here. I will here only compare production of vector particles by electron-positron annihilation in storage rings with production of the same objects through the Drell-Yan process from hadron-hadron collisions. These two processes have been pursued in parallel throughout. The best known example is, of course, the discovery of the *J/Q* simultaneously in Brookhaven by the observation of lepton pairs from hadron collisions and at SLAC from e^+e^- annihilation. These results are shown in Figs. 4(a) and 4(b). This was followed by the detailed exploration of psion spectroscopy at SLAC. Then there is the discovery of the Υ in the muon pair spectrum generated from a hadron beam, followed by elaboration of the Υ spectroscopy in electron-positron annihilation, first at DESY and then at Cornell.

EXAMINATION OF HADRON STRUCTURE BY INELASTIC LEPTON SCATTERING

The quark hypothesis derived from the interpretation of the rapidly evolving data on resonant states of the nucleons as induced primarily at the Bevatron at Berkeley, followed by work at other hadron machines. A more direct revelation of the quark substructure of the hadrons came from the deep inelastic electron scattering experiments at SLAC carried out by the SLAC/MIT collaboration. This was followed with work at other electron laboratories and then by results from neutrino and muon beams from hadron machines reaching much higher momentum transfers but generally lower statistics. Figure 5 shows a comparative graphical summary.

THE Z^0

Charged and neutral intermediate vector bosons were predicted prior to their experimental discovery as part of the electroweak unification of Weinberg and Salam. Strong experimental indications on the existence of these particles originated from many

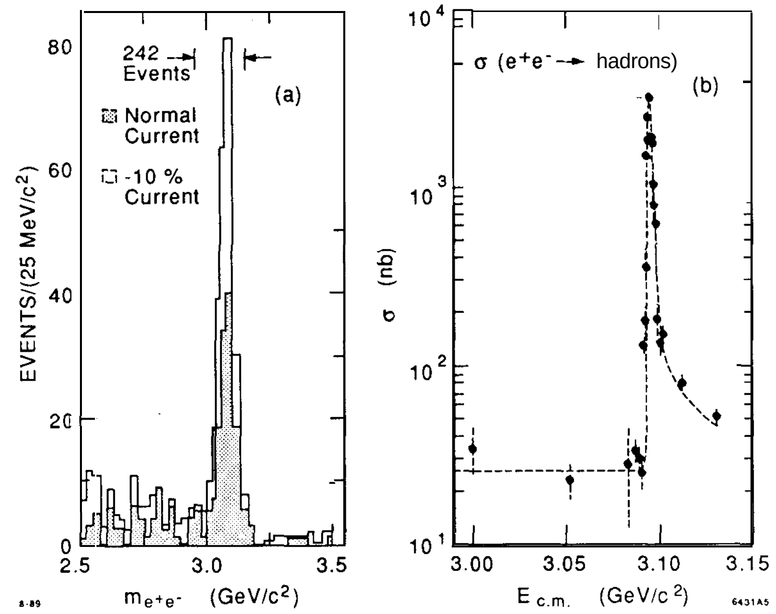

Fig. 4. Discovery of the J/ψ.

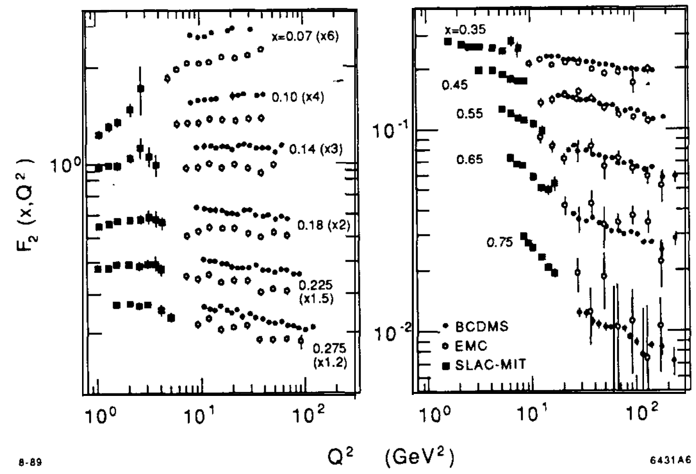

Fig. 5 The proton structure function $F_2^p(x,Q^2)$ front deep inelastic scattering of charged leptons on hydrogen targets. Where necessary, the SLAC-MIT and EMC data are interpolated to the x bins of the BCDMS data.

sources including the angular asymmetry of lepton pairs produced in electron-positron annihilations and other evidence of interference between electromagnetic and weak interaction channels. The bosons themselves were discovered at CERN in the $Sp\bar{p}S$ and have recently been more copiously produced in the proton-antiproton collider-the Tevatron at Fermilab. Recently, as will be reported later in this conference, well above 100 Z^0's have been observed in e^+e^- annihilations. Figures 6(a) and G(b) present a comprehensive data summary. Again. the comparison is illuminating: while the electron-positron annihilation data still are very sparse, they give superior mass and width measurements of the Z^0, and both lepton and hadronic decay channels can be detected with high efficiency, while in hadron colliders only the lepton channels can be clearly isolated. On the other hand, lots of information which is not accessible to e^+e^- colliders has been generated, and continues to flow from the higher energy $p\bar{p}$ colliders.

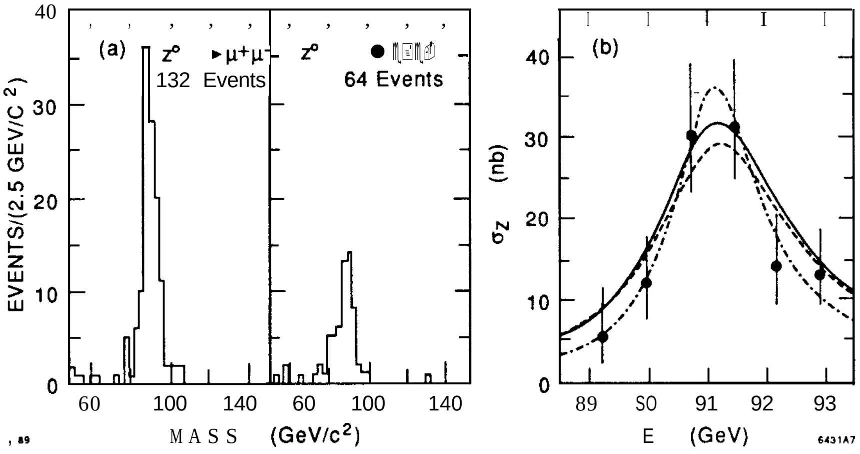

Fig. 6. (a) FNAL/CDF; (b) SLAC/Mark II.

All the previous juxtapositions are, of course, only a sketchy overview of a complex situation, most of which is now ancient history well known to all of you. However, one can reflect on some general observations quite apart from the fact that these dual graphs dramatically show the overall unity of lepton and hadron physics.

One observation is that discoveries of the fundamental lepton and quark family members are divided among electron machines, cosmic rays and hadron machines with the electron machines having made a fair share of the discoveries and elaborating on the full spectroscopy of quark mesonic states. Not surprisingly, the electron was discovered by J. J. Thompson in an electron accelerator! The Mu meson was discovered in cosmic rays and the Tau lepton in an electron-positron collider. Direct observation of neutrinos requires extraterrestrial sources, nuclear reactors, or high energy hadron beams.

Following the quark hypothesis devised to explain early phenomena in hadron spectroscopy, the confirmation of the existence of up and down quarks can experimentally be attributed to deep inelastic electron scattering experiments; strangeness was discovered in cosmic rays but the "strange" spectroscopy was elaborated in hadron colliders. The charmed quark became credible from electron-positron annihilation and through the detection of lepton pair spectra from targets bombarded by protons. The basic charmed quark spectroscopy unfolded from electron-positron storage rings. The *b*-quark was discovered in a hadron collider but its spectroscopy elaborated in electron-positron colliders. Overall, it is clear that hadron colliders have generally reached considerably larger momentum transfers and larger collision energies in the constituent frame, while the clarity of data tends to be considerably greater in the lepton-photon domain.

The reason for greater clarity of data and, more specifically, better signal-to-background ratio is, of course, well known. The total hadron–hadron cross sections as a function of energy are nearly constant and are in fact increasing logarithmically with energy, while cross sections for producing new objects of a given mass or leading to momentum transfers of a given magnitude decrease as the square of those masses or momentum transfers. Thus, as interest focuses on these higher mass or momentum transfer events, the signal-to-background ratio for hadron colliders degenerates as the square of the energy. In contrast, in lepton collisions both signals and background decrease quadratically together. Moreover, in electron-positron collisions leading to particles having the same quantum number as the virtual photon produced in the collisions the signal of interest can be larger than any other event. Therefore in high energy electron-positron colliders, the main problem is that of reaching adequate absolute rates rather than data analysis isolating signal from background.

What I call "background" here can, of course, be in itself frequently of scientific interest. After all, yesterday's signal tends to be today's background. The joke: "In an electron-positron collider either you find something new or you find nothing; while in a hadron collider, when you find nothing new you can always study the background" overstates the case. There is, of course, major scientific interest in accumulating systematic data on hadron collisions and on understanding QCD phenomena at an increasing level of detail and precision.

Another point of comparison derives, of course, from our clear quantitative understanding of quantum electrodynamics and almost as clear an understanding of the electroweak interaction. A specific consequence of that understanding is the power of lepton-photon physics to establish "positive denial" of the existence of conjectured objects or processes. If the dynamics of generation of such objects of processes is understood, then nonobservation has specific evidential value. Thus particle searches originating from lepton and photon collisions permit sharper interpretations. In addition experiments uniquely isolating quantum electrodynamics or electroweak processes, which are independent of or at least insensitive to hadronic processes, can be used to examine the limits of validity of quantum electrodynamics and electroweak theory.

The fact that QED and electroweak theory is understood and validated down to distances of at least 10^{-17}cm means that high precision measurements in lepton and photon physics and the examination of small branching ratios can be sensitive to conjectured higher mass states. Therefore, such searches can constitute large mass reach experiments if colliders to reach such masses directly are not available.

The previous brief retrospective view selects some corresponding results from lepton-photon physics and hadron physics, or more precisely, lepton-photon collisions and hadron collisions. From these I should now like to turn to some "perspective" into the future.

Most conferences in high energy physics, including those dedicated to lepton and photon physics. encompass the "standard speech" on the "Standard Model." This speech summarizes the conference saying that no deviations from the Standard Model have as yet been seen including those results reported at the conference. However, the Standard Model cannot be the whole story for many well-known reasons — too many arbitrary constants, no explanation for the number of generations of flavors, no experimental evidence for the existence of specific agents which establish the mass scale among particles of the same basic quantum numbers, and finally, no experimental data which relate gravity to particle physics phenomena. In other words, the "standard speech" persuasively argues that there must be physics beyond the Standard Model. Others at this conference will no doubt address these issues, so I would like to confine any futuristic remarks to the instrumental expectations.

Last year, Carlo Rubbia concluded his summary talk of the previous lepton-photon conference with the phrase "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 . ." I would not take that sharp a position as to the alternatives we face. Rather, the question is which parameters are in fact attainable during the next one or two decades. I would, however, agree that the rate of progress of lepton-photon physics, and of elementary particle physics in general, is paced by instrumental developments in the collider arts, particle detection. and data analysis.

I have illustrated from past history the critical role played by electron machines. One can even strengthen the case by pointing out that in the 1970's many of the profound contributions by hadron machines occurred through the use of external lepton beams, that is, neutrinos and muons. In fact, the '70's can well be designated as the decade of the leptons.

The question is how to extrapolate from this to future expectations. There is no question that formally, measured in terms of the energy in the "constituent" frame, *i.e.*, the lepton or quark frame, proton machines will be able to reach much further in the coming decades and can do so more cheaply. There is no expectation that electron-positron collider technology can match the reach of the SSC, as measured by that single parameter. The question is how accessible the resulting information is. Here an enormous amount of work has been done in workshops, at Snowmass summer studies, and through specific contributions by individuals and groups. I will not present even samples of the results of these efforts. In general, such studies generate Monte Carlo data—making assumptions about projected phenomena, be they Higgs particles of various mass, supersymmetric particles, second-generation of W's and Z's, or recurrences of other classes of particles. Background is projected based on known phenomena from the Standard Model and on QCD calculations. In general, such studies project the "reach" measured in terms of the maximum mass of the particles conjectured (Fig. 7). At the same time, such studies specify what type of segmentation of detector is required, in what radiation environment it has to live, and how vast are the imposed data processing requirements.

For the SSC, the numbers are indeed impressive. In rough numbers one starts out with 10^8 interactions *per* second, each generating perhaps 10^6 bytes of information. Trigger systems have to reject all but a few Hertz' worth of event rate. Offline cuts then have to isolate the interesting events which in most cases number in the 100 to 1000 per year range for the more interesting phenomena. In other words. we are talking about a rejection ratio of about 10^{12}. Of course, this numerology overstates the case somewhat, since many interactions generate events which are totally swallowed by the beam pipe.

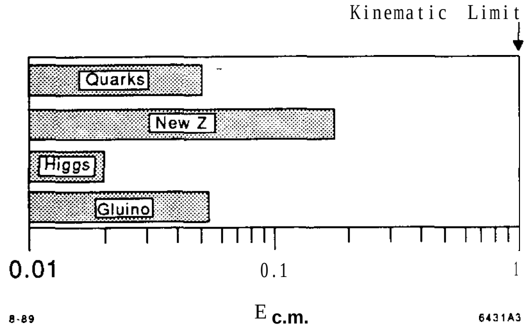

Fig. 7 Discovery "reach" versus kinematic center-of-mass energy for the SSC (Ref. 6).

In general, analyses indicate that although a giant and expensive effort, is required and the challenges to detector design are huge, the problems, addressed as pre-identified issues, are soluble. Yet the lingering doubt remains that although specific analyses aimed at examining the discoverability of conjectured phenomena and yielding detector and data analysis requirements give positive results, this extreme filtering process required for proton accelerators may throw away evidence of the "truly unexpected." Moreover, even ignoring this possibility, there are bars to discoveries in specific regions. For instance, if the Higgs mass lies in the band between twice the Z^0 mass and the Z^0 mass, it will be swamped by the general QCD background, and similar discovery bars can arise at higher masses.

It is interesting to note that one of the specific prominent designs for hadron detectors is to design and build an instrument which essentially throws away most information except that identifying penetrating leptons. Indeed, as we have seen before, this has been a fruitful avenue of discovery in the past, but it also eliminates an enormous amount of data at the source which could be obtained only with hadron machines. Based both on the physics and on history.

the question can be legitimately asked whether that information which manifests itself through detecting lepton pairs from hadron collisions is not accessible with greater clarity from electron-positron colliders — *if* such colliders can be built to reach a competitive energy range.

Thus, the net which a high energy proton machine will cast will indeed yield a vast product but some interesting fish will surely get away. But in the foreseeable future proton machines can cast that net further into the ocean. All this does in no way speak against the need for a major assault upon the next generation of hadron machines. It simply means that the lepton frontier *must* be covered.

And this is the crux of the matter. How credible are the projections and studies which extrapolate beyond LEP and the SLC towards the attainment of electron-positron linear colliders of higher energy? There seems to be a consensus that LEP is the last of the highest energy electron-positron colliders based on electron-positron storage rings. The reason is the well-known argument which leads to the cost and radius of such machines increasing roughly quadratically with energy. This conclusion stems from a balance of those costs growing linearly with orbit radius against costs related to radiation loss which vary with the fourth power of the energy divided by the orbit radius. This argument is matched against the conventional wisdom that the cost of linear colliders is linear, that is, it goes up proportionally to the beam energy. If the SSC tunnel were a suitable housing for an electron-positron collider (which it is not!) it would extend the energy frontier by less than a factor of 2 beyond LEP. But does the cost of a "linear" collider scale linearly in practice? And what are the realistic coefficients of the scaling laws?

Scaling law arguments are often used to justify a new type of accelerator or collider. At the end of World War II, Luis Alvarez argued persuasively that a proton linear accelerator would be the machine of the future since at that time cyclotron costs varied roughly as the cube of the energy, and proton linear accelerators would exhibit a linear cost-scaling relationship. Unfortunately for linear proton accelerators but fortunately for physics, the rules were changed; the invention of phase stability by McMillan and Vexler together with the invention of strong focusing by Christophilos and Livingston, and Courant and Snyder, also changed the scaling laws for a circular proton machine to an approximately linear relationship. The higher cost per unit energy made the proton linear accelerator noncompetitive at higher energy, although of course it has remained the injector of choice for all high energy proton machines. Similarly, in comparing linear and circular electron machines, one has to be mindful of future changes both in the coefficients of the scaling relationships as well as in more fundamental respects.

Recently there have been numerous and extensive reviews and workshops dedicated to examining the status and promise of electron-positron linear colliders. Major studies and experimental activities are being pursued in Novosibirsk, in Japan, at CERN, and at SLAC. There is now a general consensus that for some decades the basic accelerator for linear colliders has to be based on "conventional" RF structures, albeit operating at shorter wave length than the customary 10 cm or longer now in use. A fundamental basis for this conclusion is that the average beam powers have to be very large for electron-positron linear colliders going well beyond SLC and LEP energies employing any means of particle acceleration. If adequate luminosities are to be attained, this need for high average beam powers in the megawatt range is derived through very general considerations. This, however, demands that the efficiency of power transfer from wall plug to beam be reasonably high. Thus, although very large gradients are in principle attainable by novel methods of acceleration, such as those based on using electromagnetic fields in lasers, plasma wakefields, and other "collective" methods. such approaches look wildly improbable today when overall power efficiency is considered, and also when the demands for highly precise accelerating conditions are to be met.

You will hear later in this conference about the initial results from the SLC, which again demonstrate that even a moderate amount of data from an electron-positron collider gives new physical insight in an energy region which has been accessible to hadron colliders for some considerable length of time. Yet none of these studies on electron-positron linear colliders gives absolute clarity as to what the realistic scaling laws of cost vs. energy of electron-positron colliders will be. I tend to be significantly more pessimistic than many participants in this work as to the energy to which the electron-positron linear collider art can practically be pushed during the next decades.

Many of the parameters required for electron-positron colliders vary with energy in a predictable way. The conventional line of reasoning is to specify that the required luminosity of such devices must increase with the square of the energy, due to the expected variation of the relevant cross sections. At the same time, the beam-beam interaction results in phenomena which impose limits on the number of particles in each individual bunch and on the structure of the bunches which are brought into collisions. First there is beamstrahlung, which degenerates the energy spectrum of the particles. This broadens the particle energy and thereby widens the resonance peaks for producing particles having the same quantum numbers as the single virtual photon. At the same time, the radiative tail resulting from the radiative electron-positron collisions provides an overview over a wide spectrum of electron-positron energies. In other words, the radiative broadening resulting from collisions of electron-positron bunches provides a “self energy scanning” feature of such colliders.

But then there are other consequences of the beam-beam interaction. The photons produced from beamstrahlung can result in electron-positron pair formation both in individual collisions of these photons with the opposing electrons or positrons or by coherent interaction of the photons with the electromagnetic field of the opposing bunch. These electron--positron pairs can lead to an intolerable background in the detectors. The disruption of the particles in one bunch by the electromagnetic field of the opposing bunch will cause the electrons and positrons to spray on the face of the final focusing lenses which produce the high density of interaction required for an adequate luminosity. Although ingenious tricks have been devised to reduce this problem it cannot be totally avoided.

Considerable improvement results from using flat rather than circular beams in the collisions. In that case the relationship between the mean density of colliding particles to the electromagnetic field which each particle sees can be improved. Such a flat beam is not as unnatural an object as it may appear at first glance. The dampingrings which are used to reduce the radial momenta of electrons and positrons in linear colliders have the natural characteristic of reducing the emittance perpendicular to the plane of the orbit by a much larger factor than the emittance in the plane of the orbit. If the mixing between the vertical and horizontal phase space of the particles emitted from such damping rings can be held to a low value as these particles are being accelerated and brought into final collisions, then flat beams are the natural product. Yet notwithstanding all these ingenious inventions, there are strong limitations on the number of electrons per pulse which can be usefully employed in the final collisions. Therefore, adequate data rates require either a high pulse repetition rate or a large number of electron-positron bunches within each radiofrequency pulse, or both. The first results in high average beam power and the second results in requirements to minimize the regenerative beam breakup which occurs when many successive intense electron-positron bunches are accelerated in a single radiofrequency pulse. Again, that latter problem has been attacked by ingenious methods. One is to design new accelerating structures which radiate away or otherwise damp the modes which cause the transverse beam breakup. The other is to program the phases of acceleration and the frequencies of higher modes in clever ways to reduce the instabilities.

To obtain the requisite high density in the final electronpositron collisions a focus system has to reduce the total cross section of the beam by a factor much below that currently attained in the SLAC SLC, which in itself has already achieved the spectacularly small beam size corresponding to a radius of roughly 3 μm rms. Is such a further drastic reduction attainable or not in practice and how do the means of attaining such a reduction relate to the scaling laws of cost for a linear collider of the future?

The requirement for the final focus spot to be small puts stringent limits on the radial emittance of the colliding beams, its energy width, as well as the design of the Final Focus System itself. This, in turn, not only puts demands on the design of the damping rings but also puts severe conditions on the emittance growth, both during acceleration and the beam transport after the damping ring has "cooled" the beams radially. Damping rings meeting these requirements have been designed in principle, although specific demands on kicker design, wall impedances, and tolerances are difficult to meet.

The control of emittance growth in acceleration generates a contest between competing design considerations; as the wave length of the linear accelerator becomes larger, then alignment tolerances are relaxed because wake field effects become more serious with a very high inverse power of the aperture through which the beam has to pass. However, if the wave length is shorter, then the RF power wall losses go down and the maximum possible accelerating gradients are higher at shorter operating wavelengths. How important these two factors are is open to question. Most of the RF power requirement is simply the product of the energy storage in the accelerating guide times the pulse repetition frequency: indeed, that energy storage increases as the square of the wavelength. However, if one succeeds in extracting a fair fraction of the stored energy into the beam by the use of multibunch operation during each pulse. then the overall power efficiency is not severely dependent on choice of wavelength. Also, the matter of attainable gradient need not be controlling, since setting the aesthetics of an overly long accelerator aside. and ignoring pre-established site constraints, purely economic considerations would generally not lead to the highest gradient attainable technically.

Under all circumstances the tolerances which specify the level of congruence between the centroid of the beam, the electromagnetic axis of the accelerator, the beam position indicators, and the external focusing elements are extremely serious-much more so than they are in the case of the SLC. To express this in the form of a scaling law one can show that for constant average beam power, beam-beam disruption, and radiative beam-beam energy broadening, the radial invariant emittance (that is the actual radial emittance multiplied by the relativistic γ factor) has to decrease with something like the inverse eighth power of the energy. While some of the assumptions in this extremely steep scaling relationship can be modified, the severity of the emittance requirements rises sharply with energy. This problem reflects, in turn, on the precision of manufacture and alignment of components and on the demands for quality of beam position indicators, correcting elements and feedback loops; these requirements have not as yet been factored into cost estimates; this is difficult to do without detailed design.

There is one further crucial matter. What counts is the total luminosity integrated over long running periods. Therefore, as has been painfully learned during the past years, the matter of reliability is becoming of increasing importance as the complexity and number of components *in* accelerators and colliders increase, as they must as we go to higher and higher energies. This, in turn, implies that quality standards must be increased. This means as a minimum larger investments in R&D; but it may also imply higher unit costs in construction, counterbalancing the hoped-for cost reductions due to economies of scale.

The existing rough cost scaling considerations pertaining to electron-positron linear colliders are largely based on such tangible data as unit costs of modulators, RF power tubes, past experience with accelerating structures, and digging tunnels. Faced with the extremely steep scaling laws relating to tolerances and the increasing emphasis which has to be placed on reliability, I would not be sure how overall costs grow with energy for a nominally "linear" collider.

All these considerations indicate that there is a relatively clear predictable path, albeit at an R&D effort much larger than is now being invested by the four major centers dedicated to linear collider development, to an energy of perhaps 400-500 GeV in the electron-positron collision frame. Above that, predictions become speculative, both in regard to costs and time scale. However, at these lower energies such a machine would still be an enormously powerful tool for particle physics. How powerful depends, of course, on the masses of the hitherto elusive objects which are predicted "beyond the Standard Model." The detectability and ease of measurement of such objects, to the extent they exist, is, however, excellent all the way up to the kinematic limit. For instance, heavier quarks and leptons produced in pairs will decay into W-bosons in combination with the existing lighter quarks or leptons, and the signature of such processes remains clean.

So, notwithstanding the desire so frequently expressed to establish clear priorities among future colliders, the fact remains that both the proton and electron collider fronts *must* be covered. It is my view that electron-positron colliders cannot hope for a decade or two to match the energy "reach" of the SSC. However, that reach will be beset by limitations set both by the capability of detectors and fundamental gaps in coverage where general QCD background will prevent discoverability of new processes. As history has amply demonstrated, the clarity and usually also the discovery potential of electron machines is expected to remain superior to hadron colliders within the kinematic range accessible to such colliders, but extending that range by a large factor beyond that now expected to be reached by LEP-II is going to be a real battle.

The SSC is rightly billed as a conventional extension of the technology successfully demonstrated at the Fermilab Tevatron. Yet even at the SSC, synchrotron radiation of protons is already becoming a dominant design consideration, since the nine or so kilowatts of photons radiated deposit their energy at liquid helium temperature. Thus, as proton machines "beyond the SSC" are contemplated, many of the design limitations for electron-positron colliders which we have just discussed will also apply to proton machines. Thus the distinction between "hadron physics" and "lepton-photon physics" which already hardly exists in basic particle physics will also tend to disappear for machine design as we contemplate yet another leap in energy. Thus, Rubbia's pronouncement about choosing between a machine we don't know how to build and one we don't know how to use becomes a choice between hadron and electron machines, neither of which we know how to build; and a choice between electron machines we might know how to use if we can live with the blast of electron and positron pairs, and a proton machine we don't know how to use at all. A great deal of accelerator research and development has to be done before particle physics can (either with hadrons or electrons and leptons) penetrate deeply beyond the TeV region. Let me close on this happy note!

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