HIGH ENERGY PHYSICS HORIZONS*

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The Program Committee chose the title "High Energy Physics Horizons" for this talk. "Horizon" has several characteristics: It is a boundary beyond which you cannot see and it is also a boundary over which the sun sets. In this talk I will assume the former and not the latter.

The previous sessions have borne witness to the fact that high energy accelerator and storage ring technology is a subject of continuing vitality. Nothing dramatizes this more convincingly than the chart (Fig. 1) which shows the growth in energy of the world's accelerators in time. If one includes in this chart the equivalent laboratory energy of existing and projected colliding beam storage rings then the chart reflects an exponential increase starting from 1930 rising in laboratory energy at the rate of approximately one decade every six years. Clearly this growth cannot go on forever but if the next generation of "super" storage rings which have been discussed during this session become reality on the approximate schedule their proponents project, then this exponential increase is maintained. This graph has many implications — some good, some bad. It bears witness to the fact that as any one accelerator technology became fully exploited new ideas have produced a new and successful attack on the problem. One result of this pattern has been that the cost per GeV has gone down almost as dramatically as the energy has gone up. Consequently the range of investment in each new installation starting from the MeV region to the many hundred GeV covers only one, or at most two, orders of magnitude. A conclusion one can draw from this fact is that no region of the world seriously participating in high energy physics can afford to stand still because otherwise one would be frozen in a situation of much less effective technology. This growth rate has many complicating implications in the present climate of fiscal constraint. While the laboratory energy increases by a factor of 10 every six years or so, the lead time from proposal to completion of a new installation has averaged around ten years in the past. Therefore it is no surprise that laboratories which have just acquired new facilities should at the same time be worrying about the next step of innovation. This circumstance has drawn a great deal of criticism from supporting agencies and the public, but in view of the time scale shown here there is no other way unless of course there is an overall drastic slowdown in the evolution of this field on an international scale.

From this general overview let me turn to more specific considerations.

A question often raised asks whether the technology of the field is running out. The answer seems to be "No" in the foreseeable future. The scaling laws pertaining to the cost of each accelerator, even if the cost varies no faster than linearly with energy, tends to make each technology non-competitive in a relatively short time. However, storage rings, and to a lesser extent superconducting technology, make at least the next logical step visible today. Whether "collective effect" accelerators will become practical as a next step in energy is too early to tell. What is more important is to examine the utility of all these new technologies to expected results in high energy physics. At the risk of grave oversimplification let me project a second chart (Fig. 2) which tabulates the world's high energy accelerators and colliding beam devices on a single diagram. The chart gives center-of-mass energy as the abscissa and the "effective luminosity" as the ordinate. It is clearly meaningless to associate a specific single value for the effective luminosity with any one accelerator since this quantity depends on the target thickness used and whether interactions are measured in primary or secondary beams. In the chart it is assumed that a 1-meter long liquid hydrogen target is used in a primary beam; luminosities involving secondary beams are also plotted but only for secondary mu meson beams since such beams are in direct competition with electron machines as far as studies of nucleon structure are concerned. It should be noted that the luminosities in this chart cover an enormous range — roughly 10 orders of magnitude!

It is not surprising that the luminosity of conventional machines using primary beam interactions greatly exceeds that attained with storage rings. It is also not surprising that the center-of-mass energies which now appear to be within reach of colliding beam technology greatly exceed energies which one could dream of attaining with conventional accelerators. Considering this state of affairs two questions are dominant in forecasting the future: (1) What is the minimum luminosity required for colliding beam machines at super high energies to be productive in high energy physics? (2) What is the minimum energy advance considered useful for conventional accelerators if they are to make a useful contribution, considering the potential advances of storage ring technology? Let me discuss the first question: The answer depends clearly on the projected cross sections for reactions at very high energies. If we assume that the electromagnetic interaction between electrons and positrons retains its pointlike character then the total cross section would vary inversely as the square of the center-of-mass energy, and therefore the luminosity required to exceed a certain threshold counting rate, say one count per hour, would have to increase as the square of that energy. It could, of course, happen that the cross section will decrease more slowly than that; there is some indication that this is the case for $e^+ - e^-$ annihilation leading to hadron channels from the recent Frascati and CEA results. It is also possible that at extremely high energies the inverse will happen, that is the cross sections will decrease more rapidly. The line on the overlay on Fig. 2 marked electromagnetic interactions assumes that the reactions $e^+ + e^- \rightarrow$ hadrons will exhibit the same variation of cross sections with energy as does the purely electromagnetic cross section $e^+ + e^- \rightarrow \mu^+ + \mu^-$ and that the latter remains pointlike. It is seen that useful interaction rates in the region of center-of-mass energies near 100 GeV would require minimum luminosities in the 10^{32} cm^{-2} sec^{-1} regime, a figure which experience shows to be within reach of current technology.

If we look at weak interactions the situation is reversed. Weak interactions cannot be studied with storage rings at presently accessible energies, or at least it looks exceedingly difficult; however if the Fermi interaction remains pointlike up to the limit set by unitarity then the cross sections should increase with the square of the center-of-mass energy up to an energy of about 300 GeV. Therefore the luminosity needed for useful studies decreases as the square of the energy. As is shown on the figure overlay the lines for weak and electromagnetic interactions cross over somewhat above 100 GeV and therefore luminosities in the 10^{32} cm^{-2} sec^{-1} range should be useful for studying both weak and electromagnetic interactions in the 100 GeV range. The very fact that the strength of electromagnetic and weak interactions becomes equal in the region above 100 GeV

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center-of-mass energy has given rise to numerous theoretical speculations that profound changes in theory might be expected at such energies, and that a unified description of these two interactions might become possible. This fact is in itself a major reason why one expects that totally new physics will be uncovered if storage rings in the over 100 GeV center-of-mass energies are built.

The strong interactions yield, of course, an adequate cross section to permit their study at high interaction rates using storage ring techniques as has been amply demonstrated at the ISR. However most interest focuses on strong interactions involving very high momentum transfers and here again luminosities of the order of 10^{32} cm^{-2} sec^{-1} appear to be required if momentum transfers for strong interactions comparable to those at which electromagnetic and weak interaction processes are expected to exhibit new features are to be studied also.

In contrast to the storage ring situation where the type of reasoning outlined above gives a dominant expectation for finding new facts with super high energy storage rings, it is somewhat too early to predict what new results might become accessible if the energy of conventional proton and electron accelerators were extended beyond energies attainable by NAL-CERN II and SLAC-RLA. The answer depends rather critically on what will be found with this generation of machines and whether and where any new energy threshold for new phenomena emerge. It is interesting to note that historically proton accelerators generally have uncovered new particles and particle states in a given center-of-mass energy region while exploration of the structure of such particles required electron machines at comparable center-of-mass energies.

In the preceding discussion based on a rough outline of past and expected progress of accelerator and colliding beam technology I have concluded that the rapid gains in that field have shown no indications of slowing down, provided of course that support of this type of work is maintained at roughly current levels. The question is whether the rate of discoveries in high energy physics will keep up with the promise set by the machines. Any such assessment is of course a subjective matter; I remember many times during the evolution of the accelerator art where "wise men" assembled in committees have said that the field is saturated and that future installations will only fill in details of previous work but nothing genuinely new will be uncovered. Subsequent experience has always contradicted such gloomy forecasts in the past.

The next chart (Fig. 3) tabulates those discoveries in elementary particle physics which I consider to have profoundly shaken man's concept of nature; again such a list involves much subjective judgment and others might produce a list differing in considerable detail from the one given here. However the conclusion is sustained that there is no real indication that the rate of truly profound discoveries in elementary particle physics has been slowing down in the post-war period. We are therefore again facing the question: While this conclusion may be true in the past will it be true in the future? Will the future bring only an "extensive" filling in of spectroscopic levels rather than "intensive" experiments yielding new discoveries?

There are many indications that future technology will make <u>both</u> future systematic measurements and new basic discoveries possible. The remarks made above in connection with the expectations of ultra-high energy storage rings as they reach center-of-mass energies where electromagnetic and weak interactions become equal certainly indicate strongly that very profound and new revelations will accrue once such machines are built. To predict specific additional discoveries is of course speculative, but it might be useful here to list questions which should in principle be answerable by experimentation in elementary particle physics in the future and which, if answered, would lead to very profound conclusions indeed. Naturally such a list is again a subjective tabulation and only gives the sketchiest of possible outlines of known open questions in elementary particle physics.

Let me divide this list into the headings of "Strong Interactions", "Weak Interactions", "Electromagnetic Interactions", and "General Questions".

1. <u>Strong Interactions</u>

What is the behavior of cross sections at ultra-high energies? Will the so-called Pomeranchuk theorem be satisfied which predicts that particle and anti-particle cross sections become equal for all species? Will more detailed structure disappear from the curves which describe cross sections as a function of energy — that is, will there be no more resonance "bumps" of any kind beyond energies of a few GeV? At higher energies can the angular distribution, and particle multiplicities be described by the Feynmann scaling variables which reduce the number of independent kinematic parameters needed to describe the phenomena? Do some of the specific models such as those describing reactions at ultra-high energies in terms of either the fragmentation of the target or the bombarding particle retain quantitative validity? Will new qualitative features emerge in ultra-high energy reactions which point toward other models? Will the present exploration of spectroscopic levels of mesons and baryons reveal any new states beyond those describable by the quark model? Specifically, are there "exotic" states which require more than two quarks for mesonic levels and three quarks for baryons? Are quarks real and observable and if so, what are their properties? If quarks are not observable, what is the dynamics which prevents their emergence into the real world?

All these questions are part of the overall problem of the strong interactions: Will the combination of phenomenology of cross sections and observation and analysis of hadron spectroscopy lead to a real understanding of the dynamics of strong interactions? Strong interaction physics is now in the situation in which optical spectroscopy found itself before the invention of quantum mechanics; many systematic regularities have been observed and much quantitative data has been gathered but no unifying dynamics is yet at hand.

2. <u>Weak Interactions</u>

The dominant question remains that identified above in relation to the required technical characteristics of ultra-high energy storage rings: What is going to be the modification of the theory of weak interactions at energies so high that the interaction among the four particles involved can no longer be considered pointlike? At such an energy how is the "field" of such a weak interaction carried? Will it be transmitted by a new particle given the name of the "intermediate boson W?" If so what are its properties? Is it possibly an already existing hadron? Present experiments have only established limits on the mass of the intermediate boson, should it exist; these limits are not sufficiently stringent to draw general conclusions.

Another important question is the relation between structure of the hadrons and the description of weak interactions in which such hadrons are involved. With respect to electromagnetic interactions this question is illustrated by the electric and magnetic form factors which have been measured extensively with electron machines. In regard to weak interactions the corresponding form factors are more numerous and the high intensity neutrino beams, hopefully available at NAL and CERN II, appear the most promising tool for their exploration. On a different topic the question persists as to how the so-called CP violation, and presumably the violation of time reversal invariance discovered in neutral kaon decay, relates to the overall theory of weak

interactions. Why has this violation exhibited itself only in the weak decays of the neutral kaon system? Why have all other decays and interactions refused to exhibit deviations in this respect?

3. Electromagnetic Interactions

A dominant question remains whether the description of electromagnetic forces by quantum electrodynamics remains quantitatively valid even in the next accessible region of energies or the region after that. Currently quantum electrodynamics represents the only known physical theory giving a quantitative description which appears to remain valid from cosmic distances down to 10^{-15} cm or so. Thus the question whether the finiteness of electromagnetic masses is or is not associated with possible breakdowns of quantum electrodynamics at small distances remains to be answered. Associated with this problem is the question whether the electron or the muon will exhibit any structure at very small distances and the even more puzzling question of electron-muon universality, that is the identity (with the exception of their masses) of electrons and muons in all respects; thus far all experiments once sufficiently refined have confirmed this identity. There are some tantalizing discrepancies remaining, for instance in electron and muon scattering on nuclei, but they are too tenuous to be taken as definite results. All this means is that the question of the muon's role in nature remains as obscure as ever, or to put it in Rabi's words when referring to the muon: "Who ordered that?" Directly associated with this problem is the question whether the electron and muon in combination with their associated neutrinos constitute the entire family of leptons or whether other probably heavier members will be discovered at higher energies.

Then there is the question of the electromagnetic structure of hadrons. The scattering of leptons, and particularly electrons, has been the dominant tool in revealing the substructure of the nucleons. In particular the inelastic scattering experiments have shown that scattering cross sections at large momentum transfer were unexpectedly large and that the cross section exhibited "scaling" properties; this means that aside from kinematic factors these cross sections could be described as a function of a single kinematic variable. These phenomena in turn have given rise to the conjecture that the electromagnetic interaction carried by the scattered lepton is transmitted to pointlike constituents within the nucleon, called "partons" by Feynman. This discovery of a substructure of the neutron and proton opens up a new slate of questions: What are these "partons?" Are they the same as quarks? What is their spin and other properties? Will "scaling" persist into the next range of interaction energies accessible to the high energy electron-positron storage rings? What is the relation of the unexpectedly large annihilation cross sections for electrons and positrons into hadrons observed at CEA and Frascati to the parton or similar models? Will the new phenomena indicate a pointlike substructure of hadrons only to be followed by evidence for yet another substructure, etc., or do these new phenomena indicate something more "ultimate?" This latter problem is equivalent to the question whether scaling will persist into the next region of higher energies or will apply only in a restricted range of kinematic variables.

We have been fortunate that atomic and nuclear phenomena are separated in terms of the applicable scale of distances by four orders of magnitude; this is a consequence of the small strength of electromagnetic interactions relative to nuclear forces. Nucleons are smaller than nuclei by only an order of magnitude and going from the nucleon to its substructure appears again to descend only by one further decade in dimension. How, if at all, will this progression continue?

4. General Questions

We still do not understand why all charges are exact multiples of the electronic charge or whether magnetic monopoles exist. And then: Are there some totally new phenomena at center-of-mass energies well above 100 GeV which should be accessible to the new generation of super storage rings?

Many of the questions raised under the heading of specific interactions may of course be more general and the hope, if not the expectation, is that a more unifying picture among these forces will emerge, in particular since the cross sections governed by these different forces will tend to converge in magnitude at the highest energies hopefully accessible a decade from now. Finding a unified theory for all these forces has been a quest throughout this century. To a limited extent the search has already been successful in defining some common principles between electromagnetic and weak and between weak and strong interactions.

Let me return to the topic of this talk called "High Energy Physics Horizons." A horizon represents that boundary beyond which we cannot see and I hope that in this talk I have demonstrated that there is indeed a great deal of truly profound but unknown part of nature beyond. What may of course be true is that high energy physics exhibits another property of an horizon: As we march on in high energy physics we do indeed uncover much that is new and farreaching and modifies our view of nature as we know it; however we may also discover that the horizon of complete understanding of the inanimate structure of matter is just as far away as it has always been.

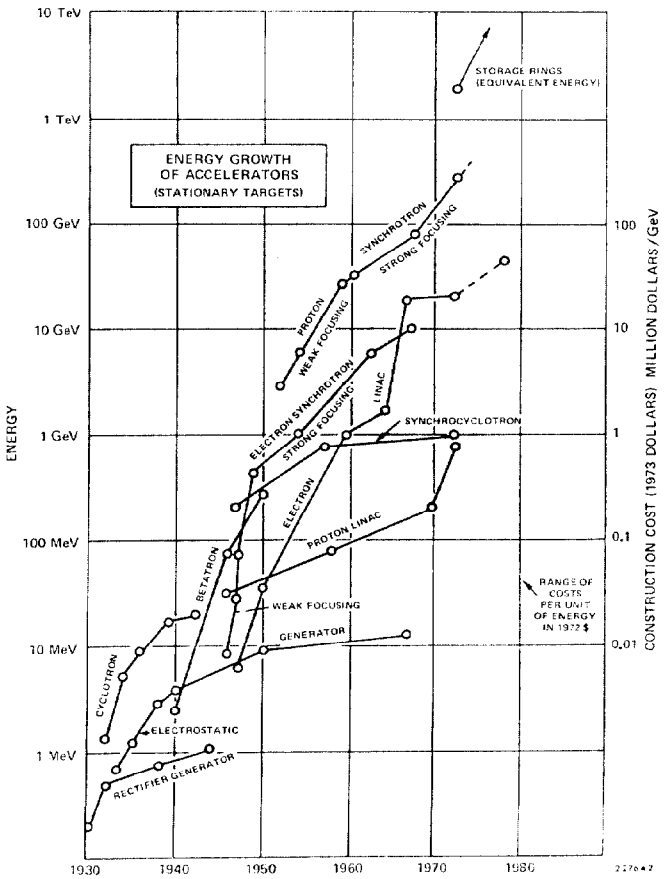

FIG. 1--Energy growth of a particle accelerator.

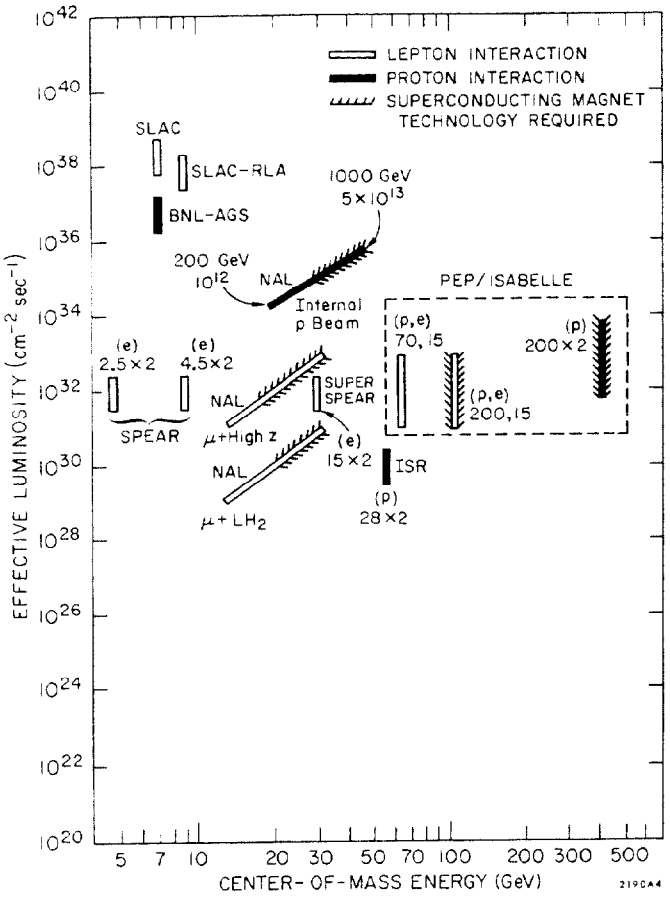

FIG. 2

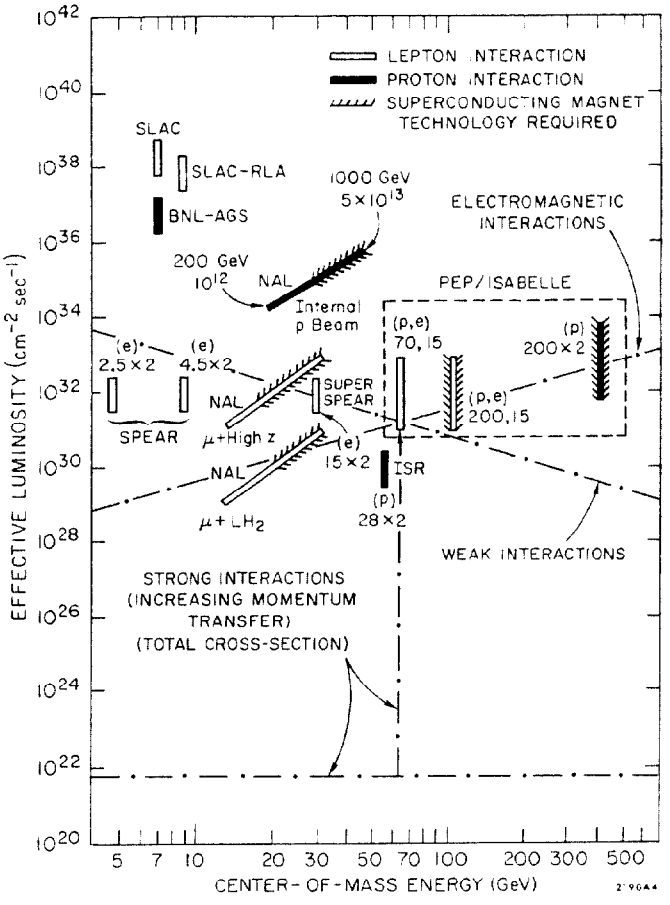

FIG. 2 Overlay

FIG. 2--Effective luminosity vs. center-of-mass energy for several accelerators and storage rings, existing and under study. The facilities shown are identified in previous figures except for the following:

(a) SuperSPEAR is a study being carried out at SLAC of the possible characteristics and uses of a colliding-beam storage ring that would store beams of electrons and positrons up to energies of 15 GeV (each beam).

(b) PEP is a study being carried out by a collaborative group from SLAC and LBL, Berkeley, on the possible characteristics and uses of a colliding-beam storage ring that would permit collisions between 15 GeV electrons and 15 GeV positrons, or between either of these particles and 70 to 200 GeV protons.

(c) ISABELLE is a study being carried out by a group from Brookhaven National Laboratory and collaborators on the possible characteristics and uses of a colliding-beam storage ring that would permit collisions between beams of protons having energies up to 200 GeV (each beam).

Figure 2 attempts to display both accelerator and storage-ring installations on a comparable scale. Naturally, such an attempt will involve some oversimplification. The data rates attainable are described by an "effective luminosity"; this is the number by which the cross section (measured in cm^2) of the reaction channel under observation is to be multiplied to arrive at a rate in events per second. This scale replaces the "intensity" figures, in microamperes or in particles per pulse, that are usually displayed for accelerators. It is assumed that the reaction in question is observed at 100% efficiency, and that the detector solid angle collects all the events of interest. To apply this concept to an accelerator, it is assumed that (unless otherwise indicated) a liquid hydrogen target of one-meter length is used. With the exception of the muon-beam entries, all figures refer to primary beams.

Center-of-mass energies are plotted under the assumption of a stationary proton target in the case of conventional accelerators. Those U. S. accelerators which are operating or are under study, and which have a center-of-mass energy greater than 5 GeV, are listed in the figure. The CERN ISR is shown for comparison with the U. S. colliding-beam storage-ring projects under consideration. CERN II is not explicitly shown, but its performance would be comparable to NAL. NAL performance is shown under a wide range of assumptions; these range from an energy of 200 GeV at 10^{12} protons per pulse all the way up to an intensity of 5×10^{13} protons per pulse at an energy of 1000 GeV. The latter values are very optimistic assumptions, both in regard to intensity and to the feasibility of the superconducting "doubler" project for NAL.

Since the engineering feasibility of large-scale superconducting magnet technology has not been demonstrated, a special notation is made in the figure to point out those devices that would require such technology.

FIG. 2 Overlay--The dash-dot lines on the figure indicate the luminosities that are required to achieve a counting rate of one event per hour, at the center-of-mass energies shown, for weak, strong, and electromagnetic interactions. The vertical dash-dot line extending upward from the strong-interaction line is meant to point out the increasing luminosities needed for rates of one count per hour for events of increasing momentum transfer.

DATE	DISCOVERY	EXPLORATION
1947	Lamb shift: "g-2" of the electron	Limits of quantum electrodynamics
1947	Properties of the pion	Pion-nucleon interactions
1952	Bubble chamber for investigation of strange particles	Interactions of strange particles
1954	Composite nature of the nucleon	Electron scattering and nucleon spectroscopy
1955	Anti-proton	Matter/Anti-matter symmetry
1956	Violation of parity conservation	Weak interactions
1961	Hadron symmetries (SU^3) and discovery of omega-minus (1964)	Whole hadron spectroscopy
1962	A second neutrino	Search for new leptons
1964	Violation of CP conservation	Search for T violation
1968	Point structure within hadrons	Deep inelastic scattering and e^-e^+ storage rings

FIG. 3