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FROM ACCELERATORS TO STORAGE RINGS TO ?*

W. K. H. Panofsky
Stanford Linear Accelerator Center
Stanford University, Stanford, California 94305

1. History and Motivation for Energy Growth of Accelerators

You have heard extensive reports on particle accelerators for a large variety of uses - pure and applied. In this talk I will give a general but highly subjective overview of the expectation for accelerators and colliders only for high energy physics. I will not discuss here extended developments of accelerators and storage rings for application to nuclear structure physics, synchrotron radiation, medical applications and industrial use.

Let me begin with an updated version of the usual Livingston chart (Fig. 1). This demonstrates the exponential growth in time of the beam energy of accelerators - a growth to which we all have become accustomed. I need not emphasize here that this exponential growth has been obtained through a succession of technologies with each technology reaching the practical energy limit attainable by any particular method. Let me project next a similar Livingston chart (Fig. 2) pertaining only to electron-positron colliders. Again we are seeing an exponential growth but in the past only one technology - electron-positron storage rings - has been responsible for this development. The great question before us is how long or whether the type of exponential growth reflected by these two charts can be sustained in the future.

The motive for searching for large increases in collision energy must be expectation of truly meaningful results in elementary particle physics. A problem is that with the exception of W and Z^0 physics and the still uncertain energy threshold for production of the t-quark, predicted thresholds are hard to come by. The number of new quarks may not be exhausted. There is expectation of the scalar counterparts of the elementary fermions (quarks and leptons); there may be an onset of whole families of new objects. However, apart from structures and discontinuities associated with such specific objects the general trend of cross sections tends to have only a small variation with energy. We heard recently from a well known theorist: "The collision energy of 2 TeV is too small by a factor of 10^{13} for anything really interesting to happen." I agree indeed that the predicted masses for particles leading to grand unified theories are beyond the reach of man-made devices. Thus we are in the not unusual situation that arguments specifically defining "required" energies for the "next step" are difficult or impossible to formulate. However, let me remind you again that in the past accelerators have rarely been built for the "right" reason: the most important impact of a new accelerator or collider on particle physics has generally been in areas quite different from those used by the designers for its justification. Therefore, the pressure for increased collision energies, although real and merited, has to rest largely on general arguments.

Just because the variation of cross sections with energy involving new phenomena might well be slow, one would like to maintain the historical exponential growth. Moreover, just because any one new machine is very expensive, steps in performance should be large if at all possible. Yet we are now facing the situation that truly large steps in attainable collision energy are predictably very costly, or are simply not in sight through available basic technology.

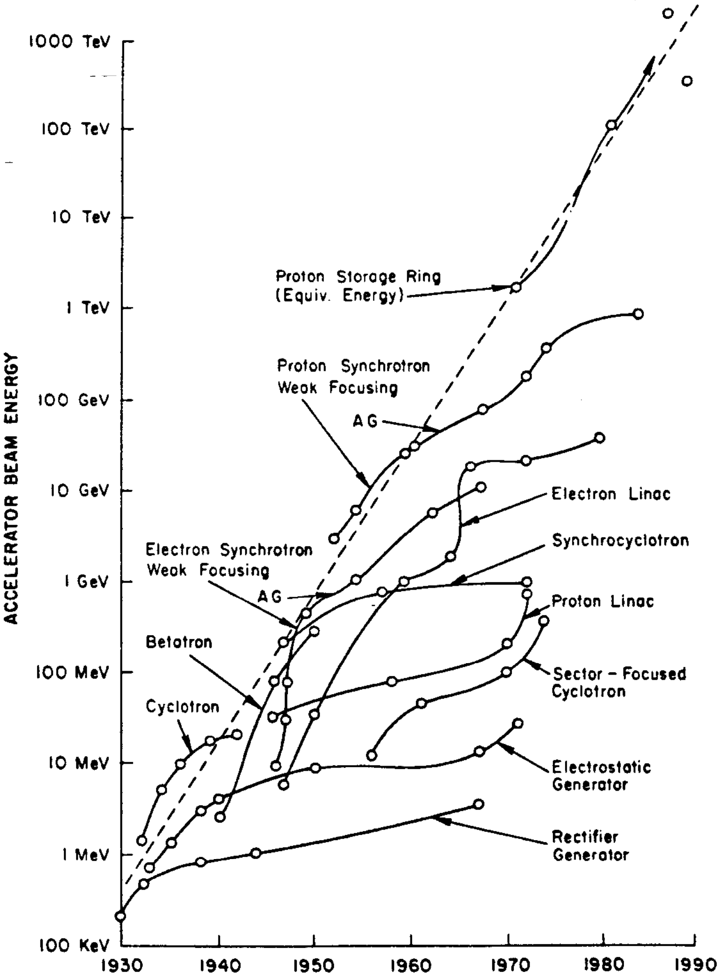

Fig. 1. Energy growth of accelerators and storage rings.

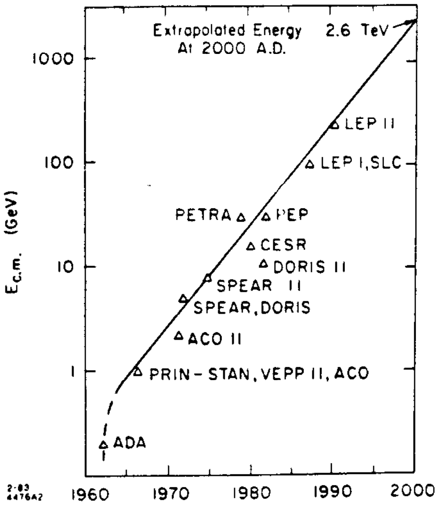

Fig. 2. Growth of electron colliders.

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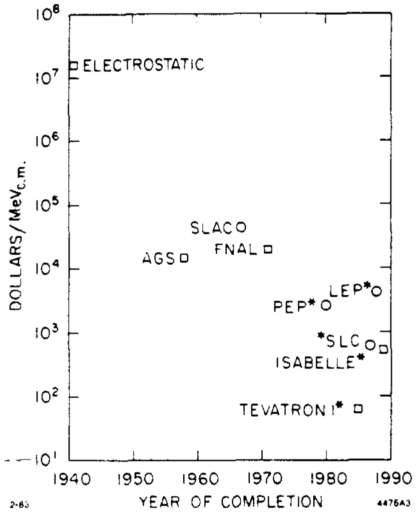
2. Scaling Laws of Energy Growth

Let me elaborate on this last, somewhat pessimistic remark. All specific existing accelerator, storage ring and collider designs can be divided into those for which the capital cost to attain an increase in the available center-of-mass energy increases roughly with the square of that energy and those for which the cost variation is approximately linear. No known technologies hold out promise of a scaling law in which the basic cost per unit energy exhibits a decrease. Yet it has been just such a decrease which has made it possible in the past, as is shown in Fig. 3, to extend the energy frontier without correspondingly large increases of the cost of each installation.



* Based on incremental costs in existing laboratories and using existing injection systems

Fig. 3.

To offset this pessimism, let me remind you of an amusing incident near the end of World War II. At that time Luis Alvarez had just proposed the proton linear accelerator, to be built from surplus military components, as an alternate to the then conventional cyclotron. He presented the persuasive argument that the cost of the conventional cyclotron increased with the cube of the energy, while the proton linear accelerator should exhibit a linear cost-energy relationship. Thus he predicted that sooner or later all proton accelerators in the future would be proton linacs. His argument was certainly correct as far as it went, but the trouble is that the designers of circular proton machines insisted on changing the rules. They invented phase stability, strong focusing, etc. Thus any general argument based on scaling assumes an absence of new basic inventions or the absence of truly drastic economies of design, an assumption which I very much hope will be proven false. Incidentally, the discussion later in this paper indicates that Alvarez' prediction might still be proven correct!

All stationary target machines and electron-positron storage rings are in the quadratic scaling categories as shown in Chart I. In the linear category are proton-proton and proton-antiproton colliding beam machines and linear single pass colliders for either electrons with positrons or for protons. Thus, cost alone seems to be imposing a serious limit on the growth of all stationary target machines and electron-positron storage ring colliders.

These scaling laws do not, of course, define absolute costs; it is always possible to change the coefficient which gives the factor of proportionality. Thus the question remains whether major economies through inspired design might reduce capital costs without introducing basic new technology. My answer is: yes, but probably not too large a cost reduction should be expected.

Chart I

Collision Energy vs Cost Scaling Laws for Accelerators and Colliders

Beams on Stationary Targets

$$E_{c.m.} \sim \sqrt{2E_{lab} M}$$

$$E_{lab} \sim \text{cost} \sim (E_{c.m.})^2$$

Colliding Beams

$P-P\,;\,P-\bar{P}$ $\qquad E_{c.m.} = 2E_{lab}$

$$\text{cost} \sim E_{c.m.}$$

$e^+ - e^-$, Storage Rings

$$\text{cost} \sim A \times \text{Radius} + B\left(\frac{E}{R}\text{lab}\right)^4 \sim (E_{c.m.})^2$$

$e^+ - e^-$, $P-P$, $\bar{P}-P$, Linear Collider

$$\text{cost} \sim \text{length} \sim E_{c.m.}$$

3. The Future of Proton Circular Accelerators and Colliders

Let me illustrate the above remarks by discussing the leading technology developed for the highest energy proton colliders which are storage rings using magnets with superconducting windings.

This year we are looking forward to the entry of the Fermilab "Saver" into production for physics. We know the cost of the components of the machine fairly well, and we also have acquired substantial experience with the extensive R&D effort needed to make a very large superconducting system work. We have learned that cryogenic and superconducting technology is very unforgiving of mistakes. Yet to extrapolate that knowledge to a future laboratory faces the unpleasant fact that in the past the construction of a new plant and the creation of initial detectors and associated facilities have each matched in cost that of the machine proper. Thus any cost reduction of the superconducting magnet ring proper should be matched by reductions in these other areas, if at all possible.

But this is not all. We really do not know which way to turn to reduce costs on fundamental superconducting storage ring systems. In one direction there are efforts to develop superconducting magnets with increased magnetic fields (perhaps 10 Tesla). In the other direction the case is being made that low fields (near 2 Telsa magnetic field) in which the field is shaped by the geometry of iron rather than the coil position is the key to low cost. In the former case the claimed savings are related to smaller physical size; in the latter case cost reductions are related to very low manufacturing cost per unit length of the magnet and its housing, and the use of very inexpensive real estate (the "desert"). Neither psoition rests as yet on a solid enough base of data and studies to be persuasive. For this reason we do not know as yet by how large a factor the coefficient of the linear scaling law applying to proton colliders of cost vs energy can be reduced.

Let me give a rough numerical example: Fermilab with its 500 GeV accelerator required $250 M to construct initially, which might be $400 M in today's dollars. Let me assume that had Fermilab been built from "scratch" incorporating the superconducting Saver-Doubler and the Tevatron operating at 1 TeV, it would have cost $500 M. Recently in the speculations about a low magnetic field "desert" machine an estimate of $750 M for a 20 TeV new laboratory was advanced. This would be $40 per MeV, or a decrease by roughly a factor of ten to twenty in the scaling coefficient below current experience. Clearly for this estimate to be valid one would need (a) a steep decline in the cost ratio of ancillary facilities to the machine proper, and (b) a decrease of perhaps an order of magnitude of the linear scaling multiplier for the collider itself. Thus the skeptic might set the projected cost of such a machine at a great deal higher value. Yet in looking at the cost per MeV plot shown in Fig. 3, one is faced with a large amount of scattering of the data points. Should it indeed be possible to achieve a cost as low as $40 per MeV, then the trend of the past could be continued without basically new technology. If, as a result of more detailed design and analysis, the cost remains near $500 per MeV, then it is dubious whether a great leap forward in proton-proton or proton-antiproton energy is affordable if the low field "conventional" approach is used.

In the high field direction we can anticipate that conventional proton machines using superconducting magnets could, if desired, reach field near 10 T and will continue to expand in size. One need not have much imagination to visualize that such a ring eventually will go into the LEP tunnel. Whether anyone will go beyond that size is not clear, although plans are under discussion; I doubt that Fermi's proposal to put such a ring into a Saturn-like orbit around the earth will become a reality!

For all the above reasons the question therefore remains how many "conventional" proton colliders, based on superconducting proton synchrotron storage rings, can be accommodated in the United States and world programs. Considering the high unit cost the number will be small. After that <u>either</u> new technology will take over <u>or</u> the drive towards higher particle collision energy in the laboratory, which has been the basis for much of the advance in physics during this century, will have to come to an end.

4. Linear Colliders: Luminosity, Energy and Power

The above remarks show the need for new technology. Most ideas project that somehow high gradient linear devices accelerating beams economically to high particle energies will be designed and built. The beams of two such devices can then be brought into collision, resulting in a <u>linear collider</u>, either for electrons or protons. For electrons the transition from circular to linear colliders is required at lower energy due to the unfavorable quadratic scaling law of circular electron-positron storage rings and the limit set by the quantum fluctuation in radiation during the beam-beam interaction. Interesting proposals for muon colliders have at times been advanced; these offer the possibility of circular colliders for leptons at lower radiation loss. However luminosity may well be marginal for practical designs.

Let me discuss the scaling laws applying to linear colliders in general, both protons and electrons, with special reference to beam power. Note that today electric power consumption limits the operations of most, if not all, existing accelerator centers.

The center-of-mass collision energy 2E is of course not the only parameter of interest to measure the capability of an accelerator or collider installation. One must also be concerned with interaction rate, that is luminosity, the signal-to-background ratio for the physical events of interest, the time structure of the beam, etc. Let me discuss some of these in turn.

The luminosity produced by two streams of bunches, each containing n_1 and n_2 particles, respectively, and colliding at a frequency f across an area A is given by

$$L = n_1 n_2 f/A \quad .$$

This implies that the average beam power $\bar{P}$ of two colliding beams is given by

$$\bar{P} = 10^{-2} \frac{LE}{(n/A)}$$

where $\bar{P}$ is measured in megawatts, L is measured in units of 10^{32} cm^{-2}sec^{-1}, E is the energy of each beam in GeV, and n/A is the number of particles per unit area, assumed equal for each bunch, measured in units of 10^{10} particles per micron. Choosing an example approximating the SLC parameters ($E = 50$ GeV, $n/A \approx 5$), this implies that a luminosity of 10^{32} cm^{-2}sec^{-1} would require average beam powers near 300 kW.

A dominant problem is that one would expect theoretically that at the very highest energies the required <u>luminosity</u> for a given interaction rate leading to study of a specific new channel of mass (Energy) E would scale as E^2, since the relevant cross sections are expected to go down with the square of the mass scales of interest.

The quantity n/A is, of course, defined by the design of the particular accelerator. For a given beam emittance and for a given β function produced at the interaction region the beam density n/A would increase as E. Thus for a <u>fixed interaction rate</u> for generating "new" events the required <u>average</u> beam power for two colliding very high energy beams would increase as the square of energy! Is the actual situation really this bad?

The only hint we have that more moderate luminosities possibly might be adequate for important new discoveries at the very highest proton energies comes from anomalous cosmic ray events. Here the very fact that what appears to be new physics is showing up in cosmic rays at energies well above 100 GeV center-of-mass energy is in itself an indication that, assuming these new events are truly new, very large cross sections are in fact involved. Thus one should not be too dogmatic about the required luminosity. The matter is, of course, ultimately one of cost. If a higher energy can be reached at a low cost, then totally speculative expectations, assuming high cross sections, may be a sufficient reason to go forward; if the costs of a new installation are so large that they would immobilize the high energy physics program for some time to come, then in general such installation should serve a mixture of the expected and the unexpected.

The situation appears well defined for electron-positron collisions. Here the basic cross section is generally expected to decrease inversely as the square of the energy, multiplied by the celebrated R factor, which in essence measures the sum of the squares of the quark charges contributing to the interaction. Thus the required luminosity must meet certain standards or there will be little to see. The cross section is, of course, dramatically changed if peaks in production occur, as they do when vector meson states are produced, that is states matching the quantum numbers of the virtual photon resulting from electron-positron annihilation. Thus high event rates result at the peaks of the ψ/J and other "onium" states and high counting rates are also expected at the mass of the intermediate vector boson. However at energies above that of the intermediate vector boson, which noncoincidentally is the energy at which electromagnetic and weak interactions are expected to become equal, cross sections would still decrease according to the "standard model." As one goes to even higher energies predictions are difficult to make. Nevertheless, it appears that for extensive and useful physics with high energy electron-positron colliders luminosities well above the now current norm near 10^{31} cm^{-2}sec^{-1} appear essential.

For proton-proton interactions the <u>total</u> cross section is still increasing at the highest energies reached so far. Thus even relatively low luminosity devices at proton-proton or proton-antiproton collision energies above those attained to date will give some basic information of such quantities of interest as total cross section, jet structure, inclusive cross sections for the production of specific particles, particle correlations, etc. However the cross sections for generating genuinely new phenomena, for instance the production of intermediate bosons, are expected to be only a small part of the total cross section, and characteristic signatures by which such new objects can be identified are a further fraction of that. As a result, a luminosity for proton devices increasing with the square of the collision energy is still desirable. Typically, at a collision energy of 1 TeV production rates at a luminosity of 10^{31} cm^{-2}sec^{-1} for the intermediate boson might be one thousand events per year if the muon pair channel is used for detection, but if detection efficiency is otherwise 100%. In contrast, the total cross section yield is about one-half million events per second. Recent experience with the CERN pp collider at 540 GeV collision energy has shown that the majority of events is not greatly collimated forward and backward. Therefore the problem of handling these high rates and those expected at even higher luminosities puts severe strains on detector and data handling capacities. Not only is the absolute event rate a serious issue, but the problem of signal-to-background ratio becomes extreme. This situation is much less severe for electrons and muons than for proton-proton interactions as shown in Fig. 4.

While these generalities appear to give valid constraints, they give only a flavor of the type of question to be asked when weighing the merit of a specific new accelerator or collider proposal. More detailed predictions for specific processes must, of course, be examined. Whenever examining any one process one has to ask whether one will first run out of luminosity, or out of energy, or be dominated by signal to background problems. A classical example is the examination of high momentum transfer events. For those experiments investigating so-called "hard" collisions in which hadron spectra produced at high momentum transfers are to be examined, usually the decrease of cross section with the magnitude of momentum transfer is so steep that intensity or luminosity becomes a limitation much earlier than does the energy of the basic accelerator or collider which sets the kinematic limit for the momentum transfer which can be reached.

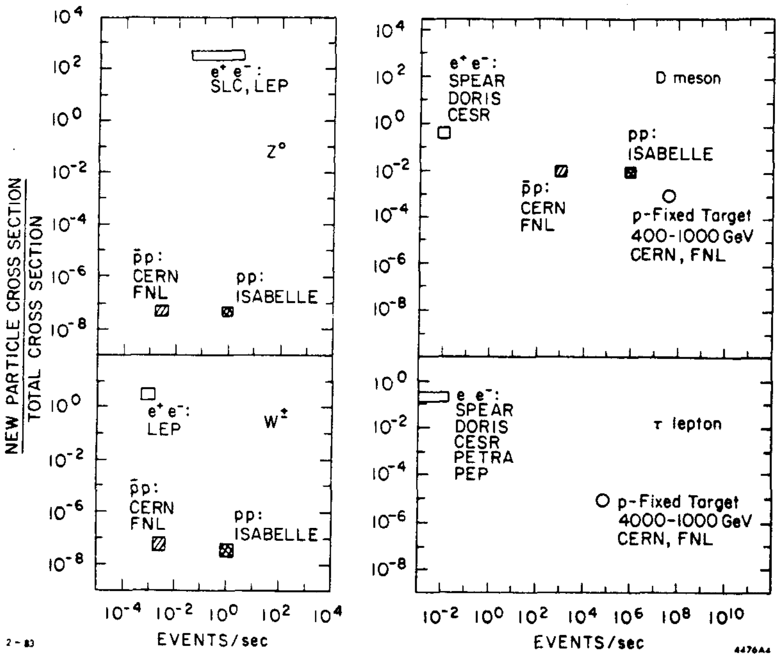


Fig. 4. A comparison of the production of new particles (Z^0, $W^{\pm}$, D, τ) in e^+e^- and hadron machines. The ratio of the new particle production cross section to the total cross section is a measure of the ease with which the new particle can be isolated from the background and thus studied in detail. The events/second is the rate at which the new particle is produced at the design luminosity for new machines, or at the maximum average luminosity for old machines.

Yet energy remains the primary parameter which must be extended in time if the productivity of the field of high energy physics is to continue. Luminosity or intensity, and signal-to-noise ratio are essential factors, but history has shown that the ingenuity of. the experimenters has generally managed to retain some rate of progress even if the installation is marginal in these latter aspects.

5. Fundamentally New Accelerating Methods

The above simplistic but apparently very general calculations would apply whether the colliding particles are electrons or protons, and whether the accelerator producing them is a conventional radiofrequency linear accelerator or some more esoteric device. Thus a linear collider of any kind would demand large average beam powers, even if substantial improvements in beam quality appear attainable. A total power consumption of perhaps one gigawatt for the entire laboratory might be viewed as an upper limit even for the ultimate "world machine." In consequence the efficiency of converting the primary electric power into beam power becomes paramount. Note that this argument is independent of the duty cycle, that is, the situation does not change as the ratio of peak power to average power becomes large, as may be needed to attain the high beam energy to start with.

Being mindful of these general considerations let us examine some of the expectations of frequently mentioned new technologies which might have bearing on the problem of providing ultrahigh energy collisions.

There are laser accelerators, hopefully capitalizing on the very large electromagnetic fields in laser light which in time will become available.

The expected gradient G in GeV/meter given by the equation $G = 2\sqrt{U/p}$ where U is the linear laser beam density in joules/cm and p the pulse length in picoseconds, looks very challenging. Existing lasers produce gigawatts of peak power and lead to fields predicted by this formula of a large fraction of a GeV per meter. Expected future lasers predict even higher gradients. Yet practical difficulties look enormous. A basic problem is the power efficiency of the lasers themselves. As the above considerations indicate, this efficiency sets a limit on the economic feasibility of any high energy collider. Current carbon dioxide lasers have efficiencies near 5% while other high peak power devices have only about 10^{-3}. This remains a serious problem for the prospect of any laser accelerator, apart from any specific design issues.

Ideas for laser accelerators lead in two directions. The first is to tailor the field pattern of a laser beam in such a way that the phase velocity matches that of the particle, while the electromagnetic field pattern has a longitudinal component of the electric vector. All such devices require a material interface next to the laser beam in order to obtain the desired field configuration. Such devices face three practical problems: (1) provision of an adequate laser source; (2) a practical solution to keep this physical interface from burning up under the high incident power; (3) very small phase volume for acceleration. Ingenious solutions for these problems have been proposed, but only the future will tell.

The second type of laser accelerator is based on proposals to use laser light to induce a traveling wave in a plasma and for the resulting electromagnetic field in the plasma in turn to accelerate the particles. Here the problem of maintaining the integrity of a material interface does not have to be solved but before such a scheme can be evaluated one needs the type of time-consuming and expensive plasma experimentation with which we have become only too familiar in the magnetic fusion programs.

The laser accelerator program has recently been intensified. The prospects to attain very high gradients look good but in view of the above general considerations an economically viable and practical system is still only a hope, not an expectation.

RF linear accelerators continue to be very much in competition with more esoteric accelerator concepts. There are hopes for the electron-positron linear collider using linear accelerators of improved design, hopefully using high gradients and very large peak powers. Much work is needed to develop suitable components and to couple the power efficiently into the beam. However, above one-half TeV per particle electromagnetic radiation in the beam-beam interaction becomes a serious obstacle. This problem - "Beamstrahlung" - can be substantially ameliorated if a narrow energy spectrum of the interacting particles is not required.

Of particular interest are the recent explorations of "two beam" machines. In such devices a structure is employed in which a low energy, high current, electron beam is coupled directly into the structure such as to produce a high gradient field for a high energy, lower current beam. In other words, such structures are designed to act directly as a voltage transformer. RF structures offer at this time the greatest hope of tight coupling between the beam and the primary source of power. Unless the presence of the beams to be brought into collision substantially "loads" the primary power line, the good power efficiency so necessary for the economical operation of a high energy linear collider cannot be attained.

There arises a nontechnical issue; this is the "how do we get there from here" problem. Let me explain. In the past nearly full-scale operating <u>models</u> to demonstrate new accelerator principles have rarely been built other than those which themselves became direct tools for particle physics. There are exceptions: the conversion of the 37" cyclotron to a synchrocyclotron, the quarter-scale model of the Bevatron (which, however, later became a productive electron synchrotron), some of the early MURA models are examples. ESCAR at LBL was cancelled before completion. However, such operating models, if built to a meaningful scale, would be very expensive for future anticipated developments. Yet it would be difficult to secure financial support in the billion dollar category for a future machine only on the basis of "table top" experiments and theory. Past practice will have to be changed and construction of operating accelerator prototypes will become another contender for the already scarce high energy physics dollar. The SLAC Linear Collider serves the dual purpose of a pilot project for a new technology and as a highly promising physics tool in its own right. However, this opportunity, although very important, appears to be fortuitous and possibly unique. I see no escaping the fact if the growth of high energy physics opportunities through continuing evolution of the accelerator arts is to be maintained, more funds will have to be dedicated to accelerator technology both for fundamental research and the construction of prototype devices.

6. Outlook

What developments could occur which might invalidate the apparently pessimistic assessment given here of the long-range hope of future accelerator or collider technologies meeting the needs of the field at affordable cost? It is, of course, possible that these considerations are somehow simply wrong and that some other factors, not identified here, might modify the whole picture. It may be true that as we go to higher energies cross sections will not go down as the inverse square of the relevant masses. As mentioned above, there is some speculation that for some of the unexplained cosmic ray events the production cross section must somehow or other have been large.

On the technical side there might be the emergence of superconducting materials which operate at temperatures much higher than those of liquid helium. Some hope has been extended that metallic superconducting materials operating at temperatures as high as that of liquid hydrogen might be developed, and recently the old hope that organic molecules can be synthesized which would eventually lead to room temperature superconductors has been revived. There is a wide gap from current results to useful application, but active work is proceeding. Should room temperature superconductors be developed this would substantially reduce the cost of circular proton accelerators and storage rings, but it would not change the basic scaling laws.

I have identified the basic considerations controlling the luminosity of linear colliders. Thus far the emittance attainable at a specific beam intensity appears to be controlled by practical considerations such as the initial emittance produced by the initial injector, the practically attainable accuracy of accelerator alignment, and the noise level which limits the ultimate performance of stochastic cooling devices used to improve emittance in proton storage rings. To the best of my knowledge the ultimate limit of such cooling processes is not well understood and this is a subject worthy of careful investigation.

Let me remind you that the rather pessimistic assessments of this paper apply to the "generation after next" of accelerators and colliders. Candidates for the "next" generation - large proton-antiproton circular colliders and large RF-supplied linear colliders for several particle combinations - look both practical and promising. Although the cost of proton-antiproton or proton-proton colliders in the multi-TeV range is still quite uncertain, the time scale leading through conceptual design to construction could be roughly a decade. For large RF-fed linear colliders some basic component development must proceed before a meaningful time scale can be projected.