

Needs versus means in high-energy physics

Some physicists have recently begun to voice their expectations that the "grand synthesis" of all physics is almost within our grasp. If only we had a little more of this or that, and if only some of the remaining nagging difficulties would go away... Here, however, I would like to turn from this sublime ambition to more down-to-earth considerations. Physics remains a science based on experience. How do we acquire that experience to determine whether or not particle physics is indeed on the right track to the "grand synthesis?"

During the last few decades high-energy physics, which is almost synonymous with elementary-particle physics, has enjoyed a period of extremely high productivity. Approximately once every two to three years something has happened to cause a profound change in Man's view of the fundamental building blocks of inanimate Nature. These new results have been a mixture of the expected and the unexpected. The tools of this evolution have been the combination of accelerators and storage rings attaining ever-increasing energies as measured in the reference frame in which the center-of-mass of the colliding particles is at rest. We will call this energy the "collision energy" for short. This progression has been accompanied by the development of detectors to match the increasing capabilities of the sources of high collision energy.

Two of the latest steps in the march toward ever-higher collision energies are illustrated on the cover of this issue of *PHYSICS TODAY* and on these pages. The cover photograph shows the progress being made to upgrade the Fermilab proton synchrotron (from 500 to 1000 GeV in beam energy) via the Energy Doubler/Saver concept, and figure 1 shows part of the PEP storage-ring tunnel at Stanford.

Figure 2 shows how the energy available in the collision center-of-mass frame has evolved in time. This chart, an updated version of the representation first introduced by M. Stanley Livingston, indicates that since the early 1930's collision energies have increased by a factor of ten approximately every seven years. It is evident from this chart that any one technology exploited for the attainment of high collision energies always reaches a point of saturation, only to be superseded by a new idea that extends the frontier. Thus the spectacular exponential growth in attainable collision energies, paralleled by the steady procession of important results in physics, has been paced by major advances in technology. The question is, of course, whether this pattern will continue in the foreseeable future or will cease as a result of either cost or fundamental physical or technical limitations.

I am reminded of Enrico Fermi's half-joking proposal, made during the late 1940's, that the "ultimate machine" would be a circular accelerator consisting of a line of magnets, in orbit around the entire Earth. When I mentioned this proposal in a casual meeting with Lev Artsimovich, the great Soviet plasma physicist, he asked me whether I had estimated what such a device would cost. I replied that the sum of the Soviet and US defense budgets would pay for it in two years, an estimate that even now is not too far off the mark. The subject of conversation was rapidly changed.

The present program

Considering first those accelerators that collide beams with stationary targets, we find that the highest energies are currently being attained by the Fermilab Proton Synchrotron and the CERN SPS for protons, and by the SLAC Linear Accelerator for electrons. Because the available center-of-mass energy for particles striking sta-

tionary targets increases only as the square root of the incident beam energy, the increasing importance of *colliding-beam* devices is progressively replacing the square-root law with a linear relationship, albeit at a sacrifice of luminosity. This parameter, luminosity, is what the high-energy physicist uses to measure the power of a given installation in terms of potential data rate: It is defined as the number by which to multiply the cross section of interest to obtain the relevant interaction rate. While collision energy will indeed remain the primary param-



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To maintain the steady increase in collision energy achieved during the last fifty years, after the lifespan of currently planned accelerators and storage rings, will take new, still conjectural, ideas.

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eter, in many important investigations in physics one runs out of useful data rates before the full range of energy can be explored. It is for this reason that one must not ignore further development of fixed-target machines, the square-root dependence of collision energy on beam energy notwithstanding. Such machines will continue to be an important tool for high-energy physics. For instance, the dependence of the cross section of various hadron reactions on momentum transfer is so steep that the attainable coverage of transverse momentum, and therefore

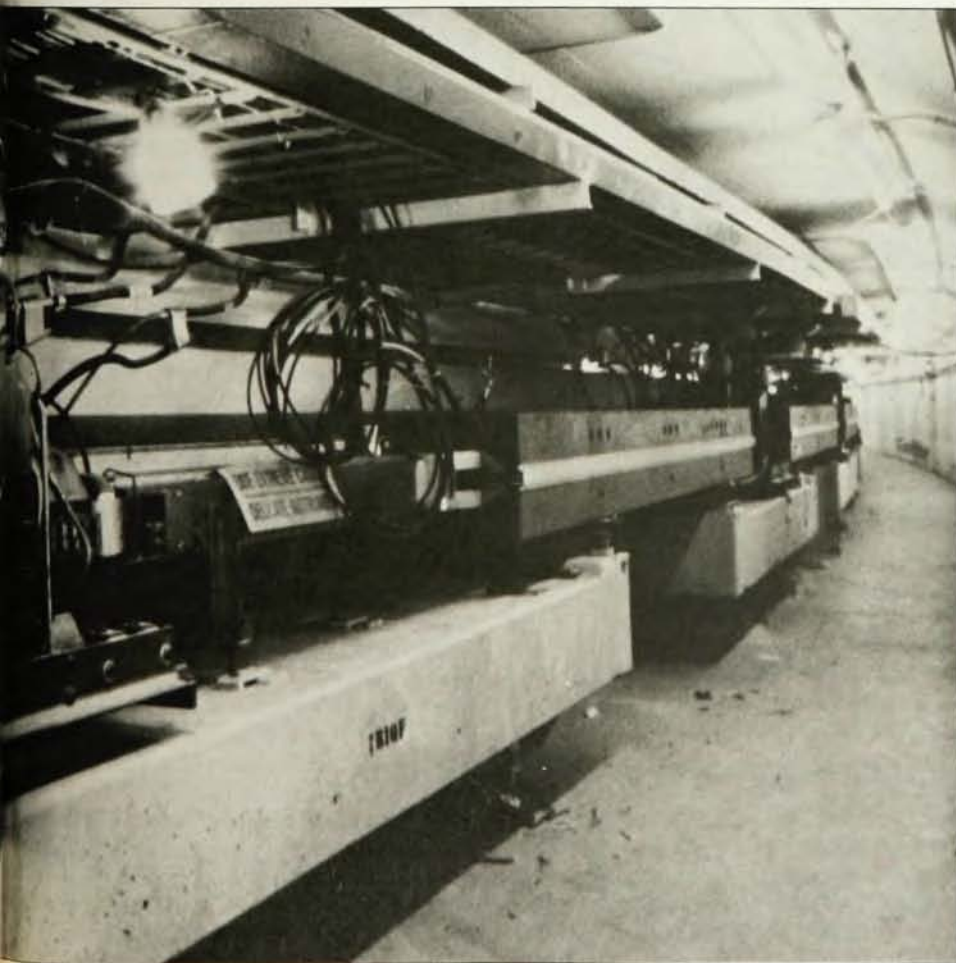
hardness of collision, is much more heavily controlled by available intensity than it is by the energy of the primary accelerator. Also, accelerators employing stationary targets are the only tools that produce secondary beams, in particular neutrinos. Therefore, the spectacular rise in attainable collision energies made possible through colliding-beam devices must not be permitted to blind the planner to the need for selectively increasing the energy of stationary target machines as well.

The points in figure 3 represent the

attainable collision energies of the highest energy accelerators and storage rings in the world, plotted against effective luminosity. For colliding-beam machines, luminosity is a primary parameter of the device; for stationary target machines, the luminosity depends on the target configuration. Target assumptions incorporated in figure 3 are generally set by the interaction of the incident beam.

It is technologically simpler and therefore less costly to accelerate protons rather than electrons to a given energy. In consequence, both the highest energies of beams striking stationary targets and the highest center-of-mass energies with colliding beams are reached by proton machines. The ISR at CERN generates proton-proton collisions of above 60 GeV collision energy, while ISABELLE at Brookhaven is designed to reach 800 GeV center-of-mass energy by 1986. For proton-antiproton colliding-beam devices, CERN hopes to obtain a collision energy above 500 GeV sometime next year, with Fermilab aspiring to 2000 GeV after several more years. At the same time the electron-positron machines are scheduled to increase in energy more slowly: PETRA has reached a collision energy of 36 GeV, with PEP to follow soon, and plans for higher energy electron-positron machines of otherwise similar design are progressing in Europe.

Although electron-positron machines have lagged in energy behind proton machines (the CERN ISR has a



The underground beam tunnel of the PEP storage ring at the Stanford Linear Accelerator Center. This recently completed device, now undergoing trials, stores electrons and positrons from the linac at 18 GeV per beam. PEP represents one of the relatively few projects currently in planning or building stages that will extend collision energies of leptons or protons beyond current limits.

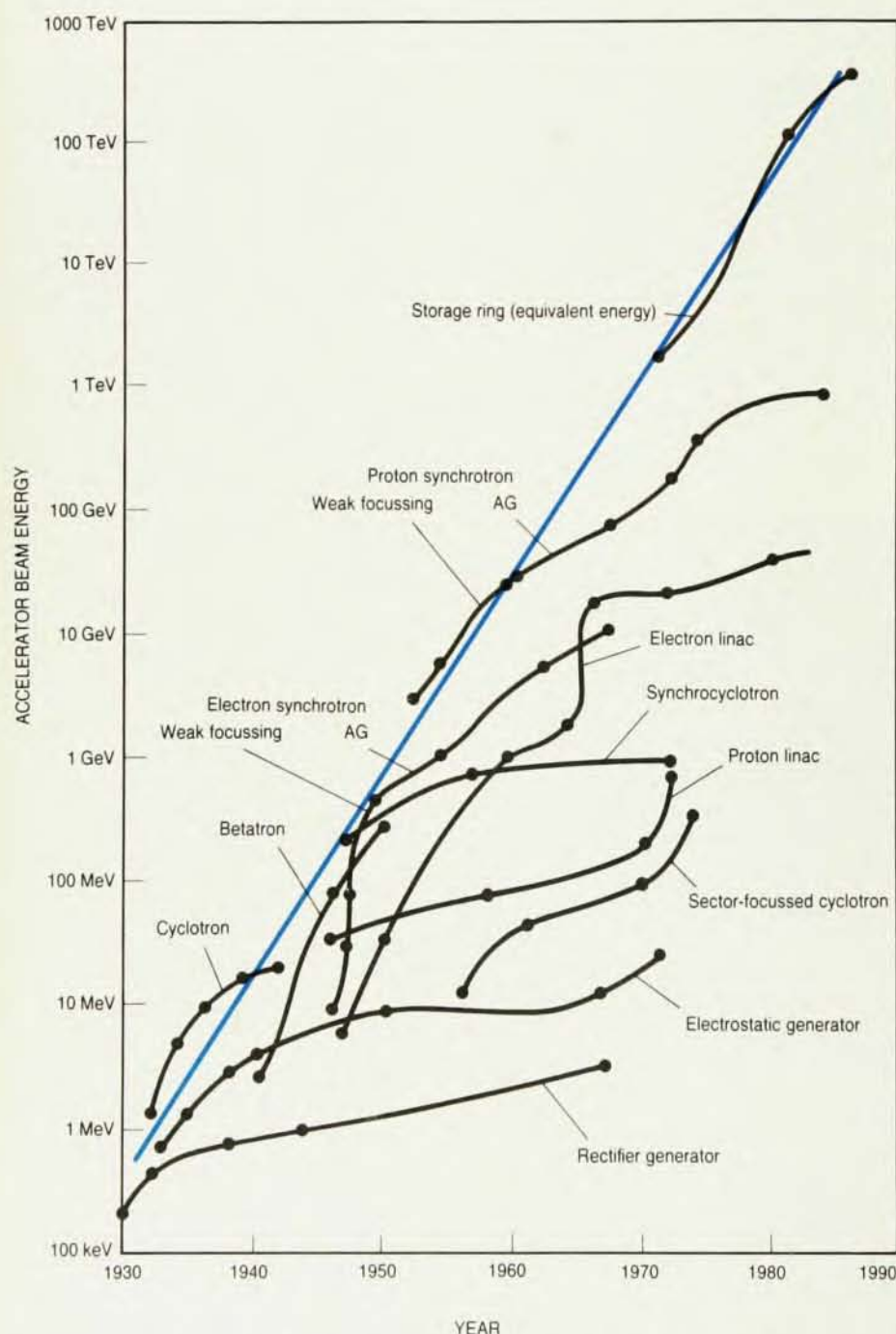
Figure 1

higher center-of-mass energy than either PETRA or PEP), the scientific productivity of electron-positron machines has been superb. There are many basic reasons for this fact. The fundamental process induced in electron-positron collisions is shown in figure 4. Electrons and positrons annihilate to form a virtual photon, which then rematerializes into all the possible combinations of particles that conserve the quantum numbers of the virtual photon and the available energy. As a result, electron-positron rings give data unencumbered by extraneous phenomena, and the threshold for the on-

set of a new phenomenon produces a significant increment in total yield. Let me be more specific: Until very recently it has been possible to interpret all phenomena produced in electron-positron collisions as proceeding via the rematerialization of the virtual photon, either into quark-antiquark pairs or lepton-antilepton pairs. In contrast, a proton-proton collision constitutes the interaction among six quarks and the available energy is shared among them. Even this description is in oversimplification, since the quark structure of the objects in question encompasses not only the "valence" quarks

but also the "sea" of quark pairs virtually available for interaction.

As we go to higher and higher energies the greater simplicity achieved in studies of the electron-positron interaction may become less distinct from that exhibited in proton-proton collisions. The presumed reason for this assertion is that not only the quarks and leptons, but also the gluons (which are the carriers binding the quarks) will become directly created by the virtual photon. Thus as we go to higher energies the pendulum that has been swinging back and forth in terms of relative importance of electron versus proton accelerators may again swing back to proton machines.



Energy growth of accelerators and storage rings. This plot, an extended version of M. Stanley Livingston's original, shows an energy increase by a factor of ten every seven years. Note how a new technology for acceleration has, so far, always appeared whenever the previous technology has reached its saturation energy.

Figure 2

Planned future machines—the risks

The previous broad outline has indicated that no one has the wisdom to predict uniquely which instrumental path should be taken in the future to extend the spectacular achievements in elementary-particle physics from the past. Thus, as has always been the case, planned construction projects for the future necessarily involve substantial risks, both of a scientific and technological nature. Figure 5 shows the overall plans for the US and of Western Europe both for stationary target machines and for colliding-beam devices. Let me now examine these plans from the point of view of their promise and risks.

I have already mentioned the chief scientific dilemma: electron-positron collisions promise results of greatest simplicity (although this may decrease in the future), while proton-proton collisions are the only open path to highest energies. It is clear that, as presently configured, US plans concentrate on proton machines, while European plans emphasize a sequence of expensive e^+e^- machines. Currently the US program is lagging in plans for further steps in e^+e^- physics, despite the US pioneering role in this field. Although in the global sense the tightening pattern of US-Western Europe collaboration mitigates this polarization of programs, critical voices have been raised on both sides of the Atlantic. In Europe pressures are developing to plan for a proton (superconducting) ring in the same tunnel as the electron-positron colliders planned for CERN, and DESY is planning a "mixed" electron-proton colliding-beam device. In the US there is a strong incentive to find a less costly alternative to providing high-energy e^+e^- collisions. I will discuss these possibilities later.

On the technological side there are indeed substantial risks that the future plans may run into problems. This is nothing new. In fact, high-energy particle physics in maintaining the spectacular exponential growth shown in

figure 2 has always proceeded through a series of relatively high-risk endeavors, and it has been this risk-taking that has generated both the needed tools for particle physics and the technological by-products for which the field justly deserves credit. One might note that Ernest O. Lawrence built the first cyclotron in the absence of any real analysis of the focusing conditions pertaining to circulating beams. Similarly, storage rings were built without a full understanding of all the stability conditions pertaining to circulating and interacting beams. The technologies employed in attaining higher energies have generally exceeded the state of the art at the time it was decided to proceed. The increasing investment in individual installations demands greater caution, of course, but this caution should not become so excessive that it stints innovation.

What are the risks? In the US program above all we have a great reliance on as yet unproven large-scale superconducting-magnet technology. Between the Fermilab Energy-Saver/Doubler program and the Brookhaven ISABELLE storage rings, the US is planning to construct some seven kilometers of superconducting magnets, while the Europeans are building none. Conversely, the Europeans are hoping to attain proton-antiproton collisions next year, even though there continue to be some unsolved problems on the rate of decay of the attainable luminosity resulting from the collisions of bunched particles, such as protons and antiprotons, whose radial motions are not damped through the emission of synchrotron radiation, as is the case for electrons. Moreover, the European community is betting its longer-range future on an evolution of electron-positron storage rings to higher energies through the ambitious LEP program at CERN, and a possible intermediate step at DESY in Hamburg. These increases will result in collision energies considerably below those designed into the US proton machine program, but they are still expensive because the cost of electron-positron colliding-beam rings scales approximately as the square of the energy. Moreover, the attainable luminosity in the highest energy of electron-positron storage rings operating to date has fallen somewhat short of design expectations, and this limit may prove to be fairly fundamental. Thus the future programs to extend the energies of the available tools in the world are far from risk-free, but the nature of the risks is quite different in the different continents.

The physics prospects

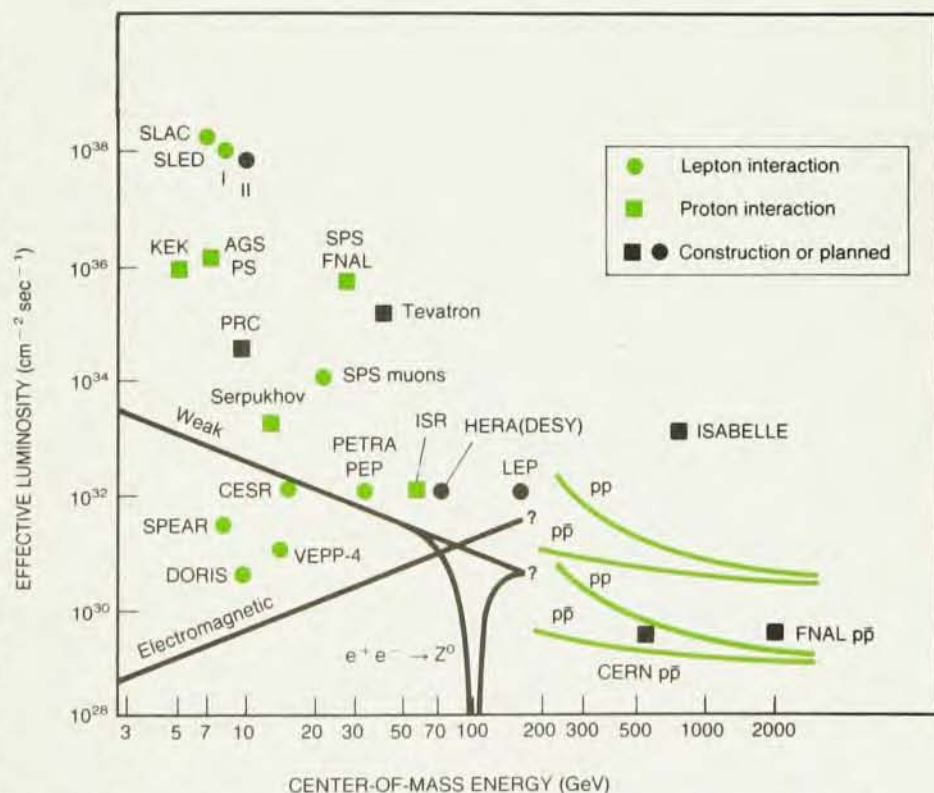
Note that if the plans now projected become reality, an exponential growth shown in figure 2 will continue for at

least the next decade. Interestingly enough, the opinion of the scientific community as to the adequacy of this rate of growth in collision energy is divided. The optimists point to the spectacular past decades to predict that continuation of that exponential growth will surely lead to a scientific productivity that will at least equal if not exceed that of the past. The hadron spectroscopy initiated in the 1950's, which is now understood to involve only the three "ordinary" quarks, has been extended to the "charmed" spectroscopy that began with the spectacular discoveries in 1974, and now has been amplified by the further discovery of a fifth (and presumably sixth) quark seen first at Fermilab. At the same time the lepton family has been extended from the electron and muon through the discovery of the tau heavy lepton by Martin Perl and his collaborators at SLAC. In parallel with these discoveries has come an increased understanding of the pattern of unification of the electromagnetic interaction with the weak interaction, and perhaps also the strong interaction among hadrons, through the evolution of gauge theories. In turn, these predictions have so far been consistent with experiments. This past experience projects that fur-

ther spectacular discoveries will be made at center-of-mass energies near 70-100 GeV. At that energy the strengths of the electromagnetic and weak interactions are expected to become comparable and, noncoincidentally, the projected carrier of the weak interactions, the neutral, and at higher energies the charged, intermediate vector bosons should materialize in the laboratory. The signatures of these particles should be totally unambiguous in electron-positron annihilation experiments, while sorting out the evidence for these new objects from proton-proton and proton-antiproton collisions will be considerably more difficult. In fact, if the intermediate vector bosons are *not* seen in electron-positron interactions, this in itself would be a spectacular discovery.

Quite apart from these specific predictions I should point out that on a linear scale the energy increase projected for the next decade is greatly in excess of the total collision energy accessible in the laboratory in the past. If the progression of new spectroscopies continues, then there may be many surprises beyond the already predicted richness of the energy region to be opened up in the 1980's.

This is the optimistic view. The more pessimistic view is that, yes, in-



Effective luminosity versus center-of-mass energy for the largest accelerators and storage rings now operating, under construction or planned. Accelerator targets are assumed to be one meter of liquid hydrogen (except for "SPS muons," which assumes 50 meters of LH_2). The black lines indicate the luminosities needed to reach a one-event-per-hour counting rate (for 100% detection efficiency) for the electromagnetic and weak interactions. Colored curves show the luminosities required to achieve a one-event-per-hour counting rate for Z^0 in pp and $p\bar{p}$ collisions. The lower pair of curves includes all Z^0 decay modes; the upper pair is for the channel $Z^0 \rightarrow \mu^+\mu^-$ (branching fraction assumed to be 4%).

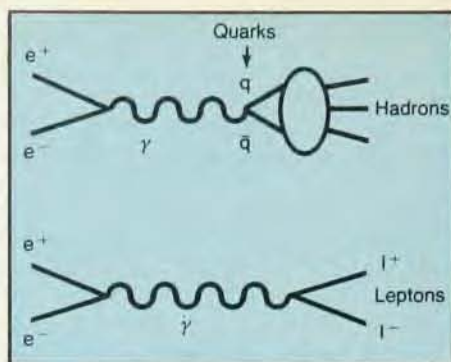
Figure 3

deed, we will confirm the specific predictions of the gauge theories such as the discovery of the intermediate vector boson, but we really know most of this already, considering the good quantitative agreement of parameters predicted by these theories with experience at lower energies. Moreover, the pessimists would conjecture that no more discoveries of new structures and spectroscopies will be made at energies above those currently accessible. If you start from this pessimistic vantage point, then the biggest unknown problem is the detailed exploration of the quantitative features of the theory, quantum chromodynamics, which is currently the leading candidate for correctly describing the strong interactions. The nature of chromodynamics is such that the energy dependence of the basic interaction cross sections depends only logarithmically on the critical quantities, and that therefore only a "great leap forward" in energy will really verify the theory quantitatively.

Again some history might be instructive: At each successive deliberation about future accelerators some highly meritorious arguments were put forward to show that the "next" energy region above 3 GeV... 6 GeV... 25 GeV, and so on would be dull and unproductive. Some of the most distinguished theorists at each epoch shared that view. Today we are more fortunate: There is little question that important known energy thresholds are still ahead of us. Yet we should not be blinded by the existence of pending "known" thresholds that we think we already know the answers—rarely has a new accelerator or storage ring had its expected goal turn out to be the eventual area of its most important impact. This point is illustrated for some US machines in table 1.

Pessimists and optimists agree on the likelihood that at "very high" energies, that is at collision energies well above 1000 GeV, some totally new phenomena may exist, and that such new phenomena might have cross sections sufficiently large to permit investigation. One indication in support of such a conjecture is a series of isolated cosmic-ray events that defy explanation through conventional ideas. The occurrence of these events in the cosmic radiation, where the flux density is very low, indicates that these new phenomena have relatively large cross sections.

I stated previously that the next generation of accelerators and storage rings involves some technological risks, but that this should not in itself be discouraging. Nevertheless, should one wish or need to go much beyond collision energies of several TeV for protons, or perhaps 150 GeV per beam for electrons, then new ideas in accel-



Electron-positron annihilation into hadrons and into lepton pairs. The simplicity of these processes (compared with proton-proton collisions) accounts for the high scientific productivity of electron-positron machines. Figure 4

eration beyond those now discussed will have to be reduced to practice. During the past few years most discussions about future accelerators or storage rings have had the disconcerting feature of being based only on well-established ideas. The goal of the participants in most discussions has been to optimize parameters to conform to their prejudices about possible energy thresholds for phenomena of interest, on the one hand, and sources of available funds, on the other. Clearly this process cannot lead to very much higher collision energies. What new technologies are in view? I will mention here only three: collective accelerators, laser accelerators, and single-pass colliders.

Collective accelerators

In essence all existing accelerators and storage rings are based on *single-particle* orbit dynamics for charged particles guided and accelerated by external electric and magnetic fields. As a result the performance of such machines is controlled by such properties of materials as the largest electric fields that can be sustained and the largest magnetic fields that can be produced. The alternative idea of *collective* accelerators is an old one. In the most fundamental sense, high single-particle energies can be produced by the principle of equipartition of energy. If one arranges for elastic collisions between a single particle and a single "collective" degree of freedom of a system consisting of many particles, then after a large number of such interactions the mean energy of the single particle should approach that of the multiparticle system. Because a macroscopic system can have enormous energies measured on a single-particle scale, such a sequence of collisions could lead to huge energies for a single particle.

The classical mechanism to illustrate such a process is the one invented by

Fermi, which he suggested as a possible source of high-energy cosmic-ray particles. Fermi proposed that elastic collisions in space between a charged particle and the geomagnetic fields of celestial objects would eventually lead to very high energies. If particles from space enter the magnetic field of such a celestial body at high magnetic latitudes, that is near the poles, then their straight-line trajectories will become helical as they approach closer, and the pitch of the helix will tighten up and will ultimately be reversed as the magnetic dipole is more closely approached. In short, the particles will be reflected by a "magnetic-mirror" mechanism and will be re-emitted back into space. This whole process thus constitutes an *elastic* collision between the particle and the celestial body, as shown in figure 6. If this process is repeated often enough, involving, presumably, diverse bodies moving randomly in space carrying magnetic fields, then the energy of the particle will continuously increase. Although calculation of the rate of this process shows that it is insufficient to account for a substantial fraction of cosmic-ray energies and fluxes, the Fermi mechanism does provide a classical example of a collective mode of acceleration.

On a terrestrial scale most mechanisms proposed for collective accelerators utilize the capture of protons in a bunch of high-energy electrons. Namely, one might imagine that if, say, one proton were captured in a cloud of electrons produced by a low- or medium-energy accelerator, then the proton would get accelerated to an energy larger by its mass ratio to that of the electron, that is by a factor of about 1840. In other words, electrons from a 1 GeV accelerator could "entrain" 1840-GeV protons. Unfortunately, this naive picture breaks down on simple reasoning. If the space-charge forces produced by the electron cloud (that is, the negative potential well produced by the electrons) are so large that the proton will remain captured, then conversely these forces would also be so large that the electron cloud would blow apart. Extremely high energy gains of a proton in a short distance require very high electric fields, and the space charge of the electron bunch is ordinarily insufficient to produce these. This fundamental conflict notwithstanding, several inventions have been made to ensure that the same forces that capture the proton in the electron cloud will in principle not adversely affect the stability of the electrons.

All familiar collective-accelerator types are designed to exploit such ideas. The best known of these devices is the electron ring accelerator or "Smokatron." The proposal, originat-

ed by the late Soviet physicist Vladimir Vexler, is to consider a ring of electrons moving at highly relativistic velocity. Such a ring is accelerated in a conventional machine and produces a space-charge potential minimum at the center of the ring originating from its electrostatic field. At the ring itself the magnetic force of the circulating electron current opposes this electric force so that the overall space-charge forces tending to blow the electron ring apart are reduced in the ratio v^2/c^2 , where v is the velocity of circulation of the electrons. The most intensive effort to exploit this idea is being conducted by the Soviets in Dubna, with relatively smaller efforts being pursued in the US and in Germany. Miscellaneous stability difficulties are encountered in maintaining the circulating electron ring. Moreover, a large ratio of electrons to protons (or heavy ions) is required for a practical device. The large number of electrons that will therefore have to be accelerated (albeit to energies lower than that of the proton) detracts seriously from the overall energy efficiency of the device. Thus far the electron-ring accelerator appears practical only for acceleration of heavy ions to moderate energy and does not extend much hope for a more economical approach to high proton energies.

Other collective accelerators of a more sophisticated nature consist of protons trapped in electron beams confined by external magnetic fields. Under these conditions several modes of dynamic behavior of the electron stream can be postulated, some of which are singularly suited for the trapping and acceleration of protons. Again the limitation is stability: Is it possible to control the electron stream in such a way that it propagates the benign modes suitable for proton acceleration but not the disruptive modes? We do not really know. This work is also being carried out in many countries but not on a scale nor with sufficient promise to make any confident prognostication possible.

Laser accelerators

Then there are lasers. Maximum fluxes of available lasers correspond to electric fields of the general order of 10^{15} volts per meter. These are tantalizingly large numbers, and many speculations have been made about how these fields might be tapped for particle acceleration. So far these proposals have not looked promising for two reasons. First, the phase volume occupied by the particles that could possibly be accelerated is quite small, the laser wavelengths involved being in the micron range. Second, we know that the electromagnetic field of a plane wave will not produce a continuing accelera-

Table1. Original goals and eventual impacts		
Machine	Goal	Most important impact
Bevatron	Antiproton	Hadron resonances
Cosmotron	Multiple pions	Strange particles; neutral kaon
CEA	Photoproduction	Large e^+e^- cross sections
SLAC	Elastic electron scattering	Deep inelastic electron scattering; SPEAR discoveries

tion of a charged particle, and special characteristics are therefore required for an accelerating field. It would have to be generated from the original laser beam by reflection from suitable devices such as gratings or by interaction with gaseous discharges. Thus the problem of properties of material media, which one is trying to avoid, reenters the picture: Will the modifying boundaries be destroyed by the laser fluxes? If so, can they be continuously replaced? Can plasma properties be adequately controlled? These objections have as yet not been overcome, although many interesting ideas remain in the running. One ray of light in this otherwise dim situation is the realization that electron linear accelerators can produce beams of exceedingly small emittance—in other words, large particle numbers fit into a small volume of phase space. However, as I will now show, this realization has led primarily to the consideration of the so-called single-pass collider in addition to a revival of analyses of laser accelerators.

Single-pass colliders

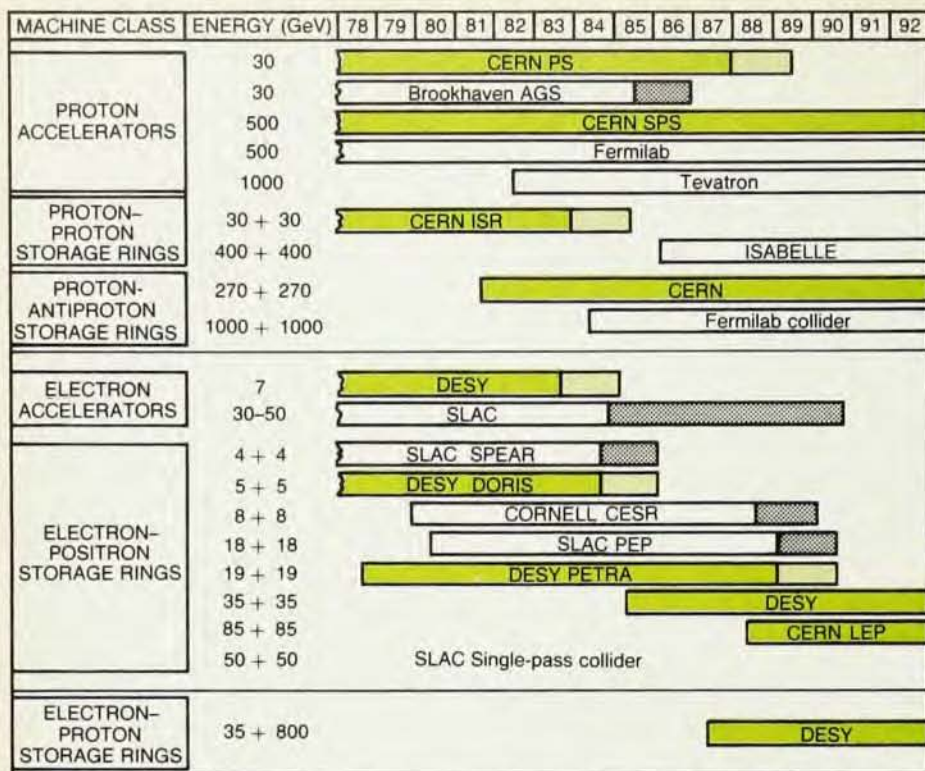
Before describing the single-pass collider let me digress to a discussion of the basic limitations of "conventional" storage-ring colliding beam devices. When electrons and positrons are stored in circular machines they radiate energy through synchrotron radiation, and this energy loss must be compensated for by a radiofrequency power system. The energy loss per turn varies as the fourth power of the energy divided by the bending radius, and therefore the power required for the radiofrequency system increases rapidly as a function of energy because of the energy loss in the accelerating

cavities as well as the energy radiated by the particles themselves.

An electron-positron storage ring thus consists of an rf system and a suitable guide field designed to maintain electrons and positrons in stable orbits and to produce as high as possible a density of particles at the interaction region or regions. The limitations of such machines are in two categories: single-beam instabilities, and effects associated with the beam-beam interaction. Let me discuss here the latter, which determines the practical limit to the luminosity.

If one beam passes through another, both the electric and magnetic fields of each beam produce disturbances in the other one; the electric and magnetic effects are approximately equal and are mutually reinforcing. A characteristic parameter of this beam-beam interaction is a quantity called the "tune shift," which measures the shift in focusing wavelength, caused by the beam-beam interaction, of the radial focusing oscillations in the storage ring. Note that in principle the tune shift is a linear phenomenon, which could be compensated for by appropriate readjustments of the focusing elements of the ring. Unfortunately, when this linear detuning effect occurs, the beam-beam effect also introduces nonlinear terms in the particle motion. The disturbing effects of these nonlinear effects have not been fully analyzed, and therefore the tune shift as a practical matter has become an empirical constant that controls the maximum luminosity which can be obtained without leading to beam loss. The maximum luminosity attainable for a given tune shift turns out to be directly proportional to the permissible tune shift and inversely proportional to

Table 2. Storage-ring and collider parameters		
Parameter	Storage rings	Single-pass collider
Collision frequency f	$\approx 10^5$ /sec	$\approx 10^2$ /sec
Particles per bunch $N_+ = N_-$	$\approx 10^{12}$	$\approx 10^{10}\text{--}10^{11}$
Beam radius σ	≈ 100 microns	≈ 1 micron
Luminosity $L = f(N_+ N_-)/(4\pi\sigma^2)$		



Present and future major facilities of the US and western Europe. Open bars represent the US program; colored bars show western European machines. Lighter-color and gray bars indicate assumed shutdown dates. Proton devices are grouped in the upper half of the figure, electron accelerators and storage rings in the lower half. Figure 5

the practically attainable radial extension of the beam in the interaction region. The radial beam excursions, in turn, are defined by the equilibrium between the radial damping effect of the emitted synchrotron radiation, which abstracts extra energy from particles oscillating with high amplitude, and the driving force on these oscillations generated by the quantum fluctuations of the emitted synchrotron radiation.

As the energy of the stored electrons increases, the cost of the machine to produce interactions at a given luminosity goes up rapidly. The radius of the machine has to be increased to reduce the expense of the rf power system to match that associated with physical size. In addition, the quantum fluctuations themselves produce a fundamental energy limit: because these fluctuations increase the energy spread of the beam, the focusing action of the ring must correct this "chromaticity." However, when the quantum fluctuations become too large, such corrections become impossible, and a practical limit is generated. Precise estimates are difficult, but probably an energy of about 150 GeV per beam is the limit (if we temporarily ignore the fact that the cost of electron-positron machines at such an energy becomes clearly excessive). There should be a better way and maybe there is.

An alternative approach is not to attempt to store the electrons and posi-

trons but to collide them only once. We can do that, in principle, by building two linear accelerators that shoot beams at one another. The tune-shift limit and the quantum-fluctuation effects on "conventional" storage rings would therefore be bypassed. But we must instead face the problem that, as a practical matter, the repetition rate of very-high-energy electron accelerators is much lower than the frequency of passage of the stored bunches through one another in a conventional storage ring. The repetition rate of each accelerator is limited, of course, by electric power consumption and cost. However, in this tradeoff there is an additional element—the density of interaction. Much higher interaction-flux densities for the beams accelerated by the opposing machines, if they could be achieved, might compensate for the loss in luminosity brought about by the lower repetition rate.

These considerations are not new; in fact, it is difficult to pinpoint a date of "invention" of single-pass colliding machines; earlier discussions of the subject can be found in the accelerator literature. The problem is not a matter of invention but of practical execution. A boost in the hope that single-pass colliders might indeed be practical has been generated by the recent measurements at the SLAC linear accelerator showing that the radial phase space (the product of radial dimension and radial momentum) occupied by elec-

trons accelerated by that machine is sufficiently small so that respectable luminosities at reasonable repetition rates of two opposing accelerators might be attainable. Therefore the single-pass collider offers a hope that the energy limit for electron-positron collisions, both technical and financial, pertaining to conventional storage rings, might be exceeded. It is clear that the cost of a single-pass collider system would scale roughly linearly with energy as opposed to the quadratic relationship of electron-positron rings.

The argument comparing the linear and quadratic cost relationships must be used with some caution. I recall that when, back in about 1944, plans for the post-war generation of accelerators were being discussed, Luis Alvarez argued persuasively that the proton linear accelerator, with its linear cost-energy relation, would clearly at some energy prove superior to the cyclotron, whose cost scaled roughly with the cube of the particle energy. Yet new inventions (phase stability, strong focusing, and others) kept changing the parameters for circular accelerators so that the proton linear accelerator never attained supremacy in any high-energy region. Therefore, although I strongly believe that the single-pass collider is by far the most promising path to high-energy e^+e^- collisions, one must not lose sight of the fact that new competing techniques may be invented at some time in the future.

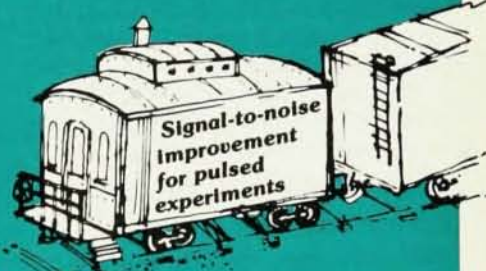
It is, of course, very rare that a new technique for particle acceleration extends the energy frontier in its first application. Rather, it appears generally advantageous to exercise a new method first in a less ambitious device. Fortunately, a possibility seems to be at hand to do just that for the single-pass collider by splitting an electron and positron beam from a single electron linear accelerator and bending each of the two separated beams in a semicircle to bring them into collision. (The way this would be done at SLAC was described and illustrated in a recent "Search and Discovery" story in *PHYSICS TODAY*, January, page 19.) Such a device would, of course, not work at energies so high that the phase-space growth through fluctuation in the semicircular bends would destroy the utility of the single-pass collider. In addition the energy loss in such bends would reduce the economic value of the device. However, at collision energies in the 100–140 GeV region this method looks exceedingly promising, both as a physics tool in its own right, and also as a pioneering model to test the single-pass collider approach. Fortunately this 100–140 GeV-center-of-mass energy is above any reasonable projection for the mass of the neutral intermediate vector boson, the Z^0 particle.



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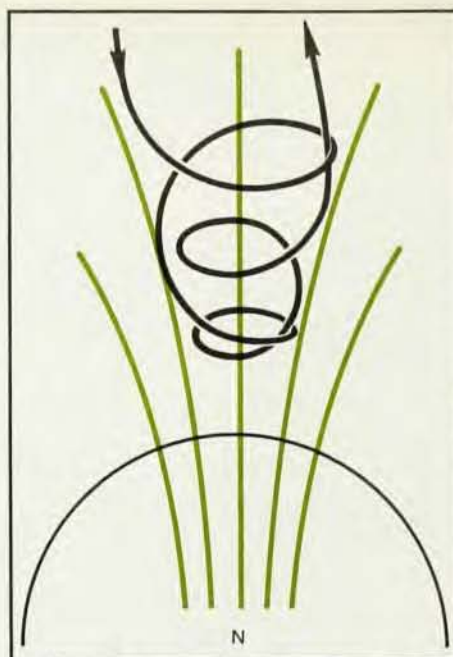
Table 2 compares the operating parameters of colliding beams in storage rings with the single-pass collider as proposed for the initial SLAC installation. Fortunately the parameters for the installation are a good match to the characteristics of the SLAC linear accelerator. In particular, upgrading the energy of the linac to 50 GeV has been shown to be feasible by means of a special technique involving storing the rf energy from the power sources and then delivering that stored energy in a short time at a high peak rate to the accelerator.

The bending system to bring the split beams into collision appears possible, and the generation of positrons of the required emittance can be done by conventional techniques. In short, it is hoped that such a single-pass collider facility can become both a critical test for this technique and can also be a relatively inexpensive Z^0 factory, since the expected cross section at the Z^0 resonance is very large.

Future prospects

We have seen where present plans for future accelerators and colliding beam devices stand and how the more conjectural expectations for future technologies in new tools for elementary-particle physics now look. Let me emphasize again that fulfillment of the long-range hopes will depend critically sooner or later on the fruits of such new technologies. The inherent question, of course, remains as to how well the actual performance of such planned and conjectured accelerators will match physics needs. Any answer to this question must be speculative by its very nature. In addition to collision energy and attainable luminosity, many other parameters can limit the utility of accelerators and colliding-beam devices and of detection instruments in elementary-particle physics. For example, such matters as background conditions, the time structure of the beam, and other factors define the ease with which such devices can be used. Let me nevertheless be forgiven for concentrating on luminosity and collision energy only.

The black lines on figure 3 indicate for e^+e^- colliders the luminosities required to produce a counting rate of one event per hour for both the electromagnetic and weak interactions. As is expected, the required luminosities for reactions driven by the electromagnetic interaction increase with energy, while observation of weak-interaction effects requires progressively less luminosity. Naturally, this presentation is a gross—and I emphasize *gross*—oversimplification, because, for instance, interference effects between electromagnetic and neutral weak interactions are not represented, and also



Collective acceleration of a cosmic-ray particle by the magnetic field of a celestial body, as proposed by Enrico Fermi. Figure 6

because I do not consider details of the strong interaction.

It is clear from this representation that for electron-positron colliders to be effective at high energies, the threshold for useful luminosities depends crucially on the observability of weak-interaction effects; this in turn depends on the presence of resonances that are expected to occur at the position of the masses of the intermediate bosons in the energy spectrum. In addition, hadron yields may again exhibit unpredicted structures, depending on quark spectroscopy and their underlying dynamics.

The colored lines in figure 3 are also one-event-hour counting rates, this time for p - p and p - $\bar{p}$ colliders.

For proton-proton and proton-antiproton machines the absolute values of total interaction rates are exceedingly high, so high in fact that they pose a large challenge to the on-line computation facilities associated with the detectors. The problem is not the absolute rate but rather the isolation of those channels that carry the essential information. Total cross sections are a substantial fraction of a barn at the upper end of the collision energy shown on the figure, and the angular distributions are strongly peaked along the direction of the colliding beams. Note that for detection of the Z^0 particle through its muon-pair decay mode—the most promising channel—a factor of about 25 loss in rate must be anticipated. Thus detection of these particles in proton collisions will be difficult. On the other hand, hadron production will be copious, and study of detailed hadron jet structures will shed

further light on the new quark-gluon dynamics. And then we must always expect the unexpected, considering that new collision energy regions will be penetrated.

Administration and economics

Let me now digress to some administrative matters. Unfortunately one cannot discuss the means-versus-needs topic as it pertains to elementary-particle physics solely on its technical and scientific merits, while disregarding the question of level of support, the size of the community, and the nature of the participating institutions.

Currently the US program is based on a mutually supporting relationship among three major laboratories sponsored by the Department of Energy (Fermilab, Brookhaven and SLAC), one laboratory supported by the National Science Foundation (Cornell), and the approximately 80 academic institutions that participate to varying degrees in the work of the four laboratories where the high-energy machines are maintained and operated. This pattern has been eminently successful and has in the past produced the lion's share of the results that have made the US competitive with Western Europe—in spite of the fact that the US level of support is considerably lower.

In high-energy physics the explosive increase in available collision energy has been a consequence of a succession of new technologies, and the cost per GeV of attainable reaction energies has dropped drastically. As a matter of fact, during the period from 1936 to the present, the collision energy has gone up by some seven orders of magnitude, the cost per unit energy has decreased by roughly six orders of magnitude. In other words, the cost per installation, once corrected for inflation, has not dramatically increased from the early electrostatic machines of the 1930's to the colliding-beam devices of today.

Even more important is that, despite tight funding of the field, the constructive interaction between performers and supporters has generally achieved the spectacular progression of results with no increase in overall cost of the high-energy physics program. The life-and-death cycle of the various accelerators in the US, for example, has occurred in such a way as to make the envelope of overall costs fit the support pattern, which is not too dissimilar to that pertaining to other physical sciences. However this cycle has now shrunk the number of centers operating accelerator and colliding-beam devices to its present low value.

The question remains whether these centers can continue to operate in such a way as to continue the dramatic pattern of results of the past. If we assume that this pattern *should* be

continued, we must examine what assumptions have to be made to ensure an overall healthy situation. I hope that my discussion here has been persuasive for continuing technological innovation as an absolute must for the trends of the past to continue. A bit of arithmetic based on the relevant time cycles of creating and exploiting machines shows that something like one-fourth of total resources should be dedicated on the average to new construction.

More detailed examination shows that the successful operation of the past has been achieved to some extent by living on borrowed time; laboratories have deferred many things that sooner or later have to be done, such as badly needed modernizing and maintenance activities. In addition the increasing costs of many things, electric power in particular, have forced each of the laboratories to reduce operating hours to such an extent that all machines are seriously underutilized. Each laboratory is running for only a fraction of the time possible. This underutilization, in turn, sharply increases the unit cost of operation, since many fixed costs have to be met. I would conclude that if the pattern of numbers and types of institutions that has been so successful in the past is to be preserved and is not to shrink further, and if the utilization and maintenance of the existing laboratories is to be restored to a reasonable level, then the overall level of support must increase by, perhaps, 20-30% in real terms. I recognize, of course, that this is just one of many pressures these days based on the real needs of Science.

No one can give a *logical* ceiling for what justifiable costs should be to attain the answers to very fundamental questions. It has never been possible to specify an absolute fraction of the Gross National Product as the "right" amount to dedicate to basic research, nor to establish a quantitative index for the economic linkage between money spent on research and then development, and our material well-being. One can refer costs to corresponding expenditures for high-energy physics in Western Europe (about twice as large as those in the US). Or one can reply to critics that the present annual cost of the US particle physics program (about \$350 million) "is not chickenfeed" by pointing out that the cost of chickenfeed in the US is about ten times that amount. Ultimately the correct support level for the most basic sciences must be defined by the very nature of society and its creative spirit that our current generation wishes to maintain.

* * *

This article is an adaptation of a talk presented at the APS meeting in Chicago, January 1980. □

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