High-energy physics horizons

The prospects for advances in accelerator technology and resulting gains in fundamental knowledge appear as bright as ever--but planning and funding procedures could be improved.

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A horizon is a boundary beyond which we cannot see. But one can speculate on what lies beyond it, and I shall try to do so for three aspects of high-energy physics: its technology, its need for support and planning, and—what makes the other two parts worth caring about—the prospects for significant new discoveries.

Technological advances

A question often raised about high-energy physics is whether its technology is running out. The answer appears to be a definite "no" for the foreseeable future. It is true that the increasing cost of each specific type of accelerator with energy tends to make any one technology noncompetitive in a relatively short time. However, during the past forty years the remarkable growth in the energy of accelerators has been achieved by the successive exploitation of increasingly effective new techniques. As shown in figure 1, the accelerator energies available have grown in a stepwise fashion from less than 1 MeV to the present level of hundreds of GeV, with each new technology providing not only higher energy but also a lower cost per unit of energy. As a result, the growth in energy through more than five orders of magnitude has been achieved with cost variations that span only one or two orders of magnitude. And the technical means are now at hand to extend these gains even farther in the years ahead of us.

These new technologies include storage rings, superconducting components and, possibly, "collective effect" accelerators such as the Electron Ring Accelerator, in which protons are trapped within a ring-shaped cloud of electrons and then accelerated. The important point to keep in mind when considering these new technologies is their potential usefulness for attaining results in high-energy physics. At the risk of grave oversimplification, I have attempted to illustrate the "merit" of the world's high-energy installations by only two parameters: Figure 2 displays the "luminosity" (the number that measures the ratio of attainable data rate in events per sec to the reaction cross section in cm^2) plotted against the "center-of-mass energy" for the world's high-energy accelerators and colliding-beam devices. Note that the luminosities cover an enormous range, roughly ten orders of magnitude, while the energies span three decades.

It is not surprising that the luminosity of conventional machines, in which primary or secondary beams strike high-density targets, greatly exceeds that attained with storage rings. It is also not surprising that the center-of-mass energies now within reach of colliding-beam technology greatly exceed those one could ever dream of attaining with conventional accelerators. Considering this state of affairs, two questions are dominant in trying to forecast the future: What is the minimum luminosity needed for colliding-beam machines at superhigh energies to be productive in high-energy physics? What is the minimum energy advance that would be useful for conventional accelerators, considering the potential advances of storage-ring technology?

The answer for colliding beams clearly depends 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 needed 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^+ and e^- annihilation leading to hadron channels, from the recent Frascati and Cambridge Electron Accelerator (CEA) results. It is also possible that at extremely high energies the inverse will happen; that is, the cross sections will decrease more rapidly. In figure 2 we have assumed that the reactions in which the annihilation of electrons and positrons leads to hadrons will exhibit the same variation of cross sections with energy as does the purely electromagnetic cross section for annihilation into muon pairs, and that this muon production cross section remains pointlike. Therefore useful interaction rates for center-of-mass energies near 100 GeV would require minimum luminosities in the 10^{32} cm^{-2} sec^{-1} regime, a figure

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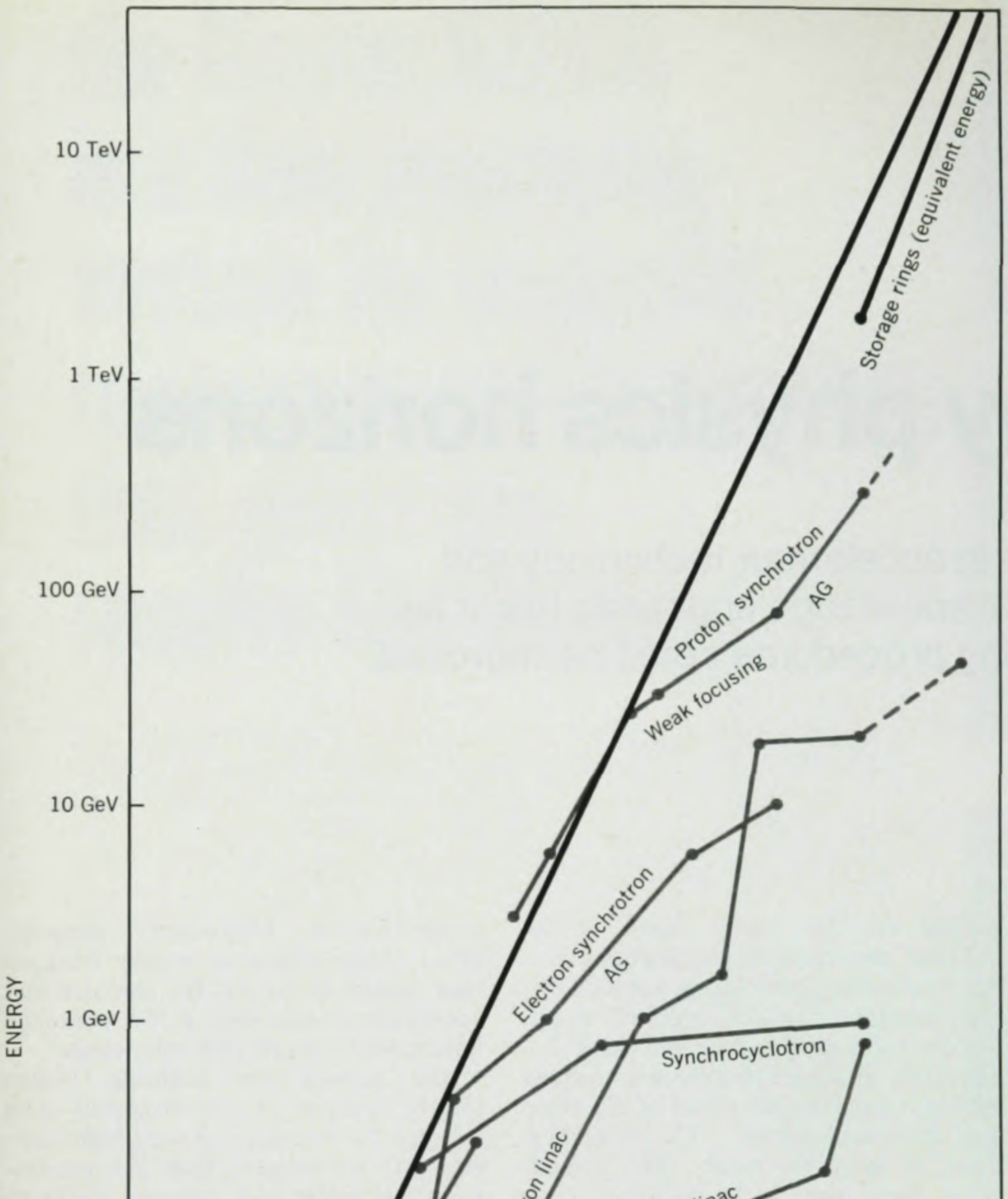


Energy growth of accelerators. The energy available from artificial acceleration of charged particles has increased by at least 10^5 during the past forty years. As the exploitation of a given acceleration technique reached its logical limit, new ideas have emerged to extend the energy frontier. At present, colliding-beam storage rings have already been proved, and developments in superconductivity and other new technologies promise still further advances. This rapid increase in accelerator energy has been accompanied by an almost equally rapid decrease in the cost per GeV. Figure 1

well within reach of current technology.

If we look at the weak interactions the situation is reversed. Weak interactions cannot be studied with storage rings at presently accessible energies, or at least the prospects do not look good. If, however, 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 a value of about 500 GeV. Thus the luminosity needed for useful studies decreases as the square of the energy. As shown in figure 2, the lines for weak and electromagnetic interactions cross each other somewhere near 100 GeV, and luminosities in the 10^{32} cm^{-2} sec^{-1} range should therefore be useful for studying both weak and electromagnetic interactions in this region. The very fact that the strength of the electromagnetic and weak interaction becomes equal in the region near 100-GeV 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 crossover is in itself a major reason to expect confidently that totally new physics will be uncovered if storage rings reaching 100 GeV center-of-mass energy are built.

The strong interactions, of course, yield an adequate cross section for studies at high interaction rates with storage-ring techniques, as has been amply demonstrated at the CERN Intersecting Storage Rings (ISR). Most interest, however, focuses on strong interactions involving very high momentum transfers: here again luminosities of the order 10^{32} cm^{-2} sec^{-1} appear to be needed for studying momentum transfers comparable to those at which the electromagnetic and weak interactions are expected to exhibit new features.

In contrast to the storage-ring situation, where the kind of reasoning I have outlined gives a firm expectation of finding new facts at superhigh energy, it is still too early to predict what new phenomena might become accessible if the energy of conventional proton and electron accelerators were extended beyond those attainable by NAL-CERN II and by SLAC augmented by the Recirculating Linear Accelerator. The answer here depends rather critically on what we find with this generation of machines, and whether and where any new energy thresholds for new phenomena emerge. Note that, historically, proton accelerators have generally uncovered new particles and particle states once new center-of-mass energy regions become accessible, whereas exploration of the structure of

Table 1. Development of CERN Basic Program Budgets

Cost index (%)	Year of council meeting	1965	1966	1967	1968	1969	1970	1971	1972	1973	1974	1975
		Budget Year										
	1964	131	142 (148)	ISR decisions pending								
5.1												
	1965		148	166 (172)	188 (200)	206 (227)						
4.2												
	1966			172	194 (200)	213 (227)	226 (248)					
3.4												
	1967				200	220 (227)	Devaluation prevented decisions					
2.9												
	1968					225	236 (244)	247 (267)				
3.5												
	1969						244	256 (267)	352 (390)	352 (414)	300-GeV decision pending	
4.0												
	1970							267	357 (377)	335 (376)	325	325
4.4												
	1971								371	360 (380)	346	346
6.3												
	1972											
6.0 (est.)												
	1973											

Millions of Swiss francs

"Final" "Firm" "Provisional"

ISR construction excluded | ISR operation and equipment included

1 300-GeV accelerator excluded; budget decisions are quoted in following year's prices
2 Numbers in parentheses indicate forecast plus escalation to reach prices of year in question

such particles required electron machines at such energies.

The conclusion, then, is that the rapid gains in accelerator technology show no immediate indications of slowing down, provided, of course, that the work is well planned, supported and executed. This leads us to our discussion of the management and support of this field of science.

Management of high-energy physics

High-energy physics involves the construction of facilities and equipment that make use of technology generally exceeding the "state of the art." Nevertheless, the record of the high-energy physics labs in accomplishing new construction within estimated costs and in meeting schedule and performance goals has been excellent. This pattern contrasts favorably with advanced technology undertakings in the defense and space program, and has been achieved with relatively small administrative overhead costs at the laboratories.

An important feature of the successful exploitation of these facilities is the pattern of *decentralized initiative* that determines the research program, despite the fact that the actual experiments must be carried out at relatively few large centers. Proposals for high-energy physics experiments are initiated by investigators throughout the US, with the work supported by nearly one hundred different grants or contracts, principally at universities. These proposals are reviewed by diverse committees advising the laboratories and are then translated into action within the program authority delegated to the laboratories under government contract. If the laboratories themselves initiate new proposals for major equipment or other general use construction, such proposals are reviewed by a representative community of "user" physicists from other institutions.

These arrangements constitute a quite successful pattern: Central funding and statutory control originate from Washington, but detailed program management, and—even more important—program initiative, is subject to a highly critical evaluation process among competing program elements. In the past the US program in high-energy physics research has been highly productive in comparison with those of other countries. This productivity is attributable in no small measure to the decentralization and competition that characterize the US program.

Funding and planning

However effective this pattern may have been in the past we now see serious shortcomings developing; some but not all of these are a consequence of a sharply limiting budget; others are connected with managerial problems.

Figure 3 shows the total funding for high-energy physics in the US and in Western Europe, corrected for inflation. These graphs represent total moneys for all needs—new construction, operating costs of existing facilities and experimental equipment. The growing gap between European and US funding is striking. Figure 3 also shows the time at which new major facilities came into operation—clearly activation of new accelerators is not reflected in increased support. We note that support of high-energy physics in Western Europe is still increasing at a substantial rate, in contrast to the US support in real dollars, which has decreased for several years. As a result it was reported for the first time at the most recent biennial International Conference on High Energy Physics (sponsored by the International Union of Pure and Applied Physics) that "It was noticeable that the contributions from Europe, and particularly CERN, were dominant," in the words of the *CERN Courier*.[1]

Much has been written (see, for example, reference 2) and spoken about the "criteria of scientific choice" among subfields of physics, among the sciences, and between science and other needs of society. I will not enter into that arena here beyond stating the obvious: At whatever level of support high-energy physics operates, the funds should be spent in the most productive

Table 2. High-Energy Physics Forecasts and Actual Budgets for AEC-Supported Research

Joint Committee on Atomic Energy hearings for fiscal year	Budget year 1968	1969	1970	1971	1972	1973	1974	1975	1976	1977
1969	129.1	143.1	201.4	249.9	284.4	286.8				
1970		140.5	136.5	198.1	220.3	249.3	274.5			
1971			134.2	143.3	213.5	231.9	244.3	267.6		
1972				142.5	130.0	207.2	211.5	213.0	215.6	
1973	Millions of dollars				132.0[1]	151.6[2]	168.0[3]	180.3[3]	191.0[3]	197.8[3]

1 Actual appropriations
2 President's budget
3 Prior year's five-year forecast

manner. But figure 3 clearly demonstrates that here we are at best deficient: the funding pattern appears to show little sensitivity to the activation of new facilities, and this means that available funds are spread very thin. Clearly this situation indicates that when these new facilities were started more liberal funding was anticipated. Thus the problem is one of planning and longer-range commitments. It is in this respect that the US program does have a major shortcoming, particularly compared with the situation in other countries.

This problem is not only serious over the multi-year interval between beginning and end of construction, but it is a matter of year-by-year, and sometimes month-by-month, uncertainty. The directors of high-energy laboratories in the US usually do not know their yearly operating budgets until four or more months of the budget year have already gone by. This difficulty is compounded because regulations do not permit a reserve of operating money to be maintained (operating funds cannot be "carried over" from one fiscal year to the next). Accordingly, any last-minute changes in expected operating funds tend to have a disproportionately large effect during the latter part of the budget year. This problem was well illustrated by the events of this past January, when a 2% impoundment of already appropriated operating funds for fiscal 1973 resulted in a significant loss of productivity in all US high-energy laboratories. At SLAC, for example, two months of accelerator operations for research had to be cancelled, an action that will reduce the utilization of SLAC's facilities to about 50% for the fiscal year as a whole.

The brevity of the government's fiscal commitments to the accelerator laboratories contrasts sharply with the commitments that the labs themselves must make to their scientific users. When a new high-energy physics experiment is proposed to one of the laboratories, it is subjected to the complex scientific review we have noted, which includes appraisals of both scientific merit and technical feasibility. If the proposal is approved, it is placed on the experimental schedule with a typical lead time of a year. When the experiment itself is completed, there follows a substantial period of time devoted to analysis and interpretation of the data, and to writing up the results for publication. The total time interval between initial submittal and eventual publication averages about three years. The situation is similar in regard to the construction of new scientific tools: The time interval between the decision to develop and build a new scientific tool for SLAC's experimental program and the completion of that tool averages 30 months. These long commitments, combined with lack of meaningful longer-range indications as to future support, lead to substantial risks for the laboratories in times of uncertain funding.

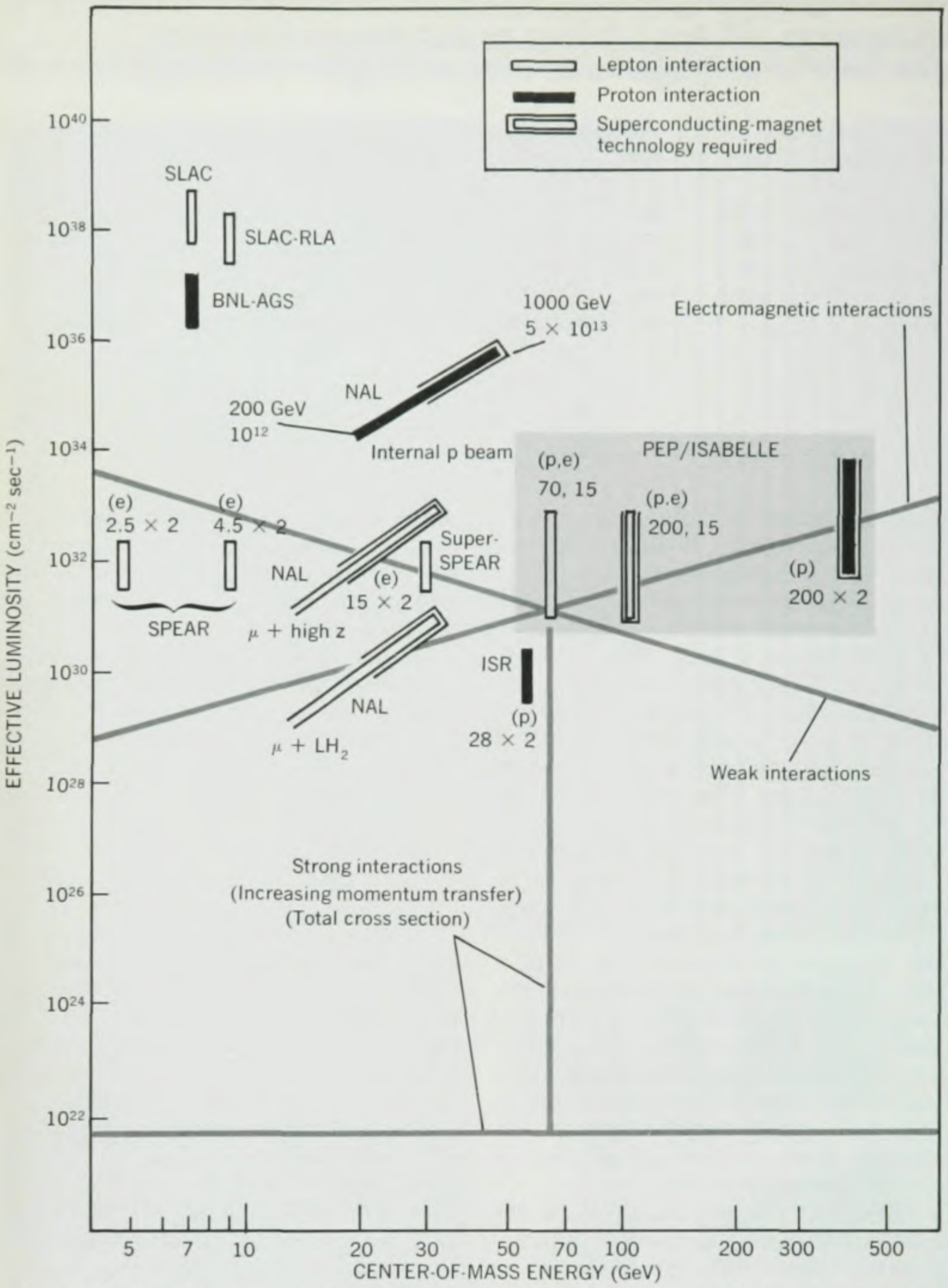
These criticisms focus less on the level of funding for high-energy physics research than on the predictability of that level. In this respect let me compare our situation with that of Western Europe, where the planning cycle permits substantially firmer commitments than it does in this country. Table 1 illustrates the budgetary cycle at CERN, which is the central European laboratory for high-energy physics research. The cycle spans four years, with the first two years being "provisional determinations," the third year "firm budgets" and the final year "actual funding." Allowance is made for inflation during this cycle, and we see that the actual funding adheres extremely well to the provisional determinations—particularly remarkable because the funds come from twelve independent nations. In contrast Table 2 illustrates the history of the US "five-year plans," which are prepared annually by the Atomic Energy Commission and submitted to the Congress. Such plans are compiled from submittals by the individual laboratories and imply no future commitments whatever; in the past there has been no meaningful feedback to the laboratories to indicate to them the degree of realism or lack of realism in these five-year plans.

Manpower fluctuations are induced by the uncertain financial outlook. High-energy physics, in common with most research and development, is a "labor intensive" undertaking: A large fraction (about 65% for SLAC) of the money spent at the high-energy physics laboratories is payroll-related. Thus any changes in financial support of the program tend to dislocate individuals who have trained for many years in a given speciality. In the absence of valid funding projections for the future, laboratory managers will tend to resist such dislocations; this factor has further aggravated the impact on efficiency caused by funding fluctuations.

The recent decreasing funding pattern has resulted in a sharp loss in manpower to the field; over 20% have been lost between fiscal 1970 and fiscal 1972, and the drop is continuing. Although some of this drop may not be totally unwelcome, because the growth of high-energy physics has not met earlier expectations, it is clear that this loss of personnel, particularly of young scientists, has now overshot a reasonable adjustment process. Graduate-student enrollments in high-energy physics have now dropped drastically, and in consequence the problem of finding first jobs, either within the field or outside it, for high-energy physics PhD's and for the best young post-PhD scientists, has all but disappeared; yet permanent jobs are still extremely difficult to come by. This prospective loss of young talent bodes serious problems for the future, and so does the apparent lack of opportunity for new participants to stay permanently in the field.

Exploitation versus innovation

The budget pressures of the past five years have decreased the level of exploitation of the operating accelerator laboratories (as measured by beam



Effective luminosity versus center-of-mass energy for the largest accelerators and storage rings now operating, in construction or under study. For accelerators, attainable data rates are described here by an "effective luminosity" instead of the usual intensity figures. The reaction in question is assumed to be observed at 100% efficiency, and the detector solid angle is assumed to collect all the events of interest. To apply this concept to an accelerator, it is here assumed that (unless otherwise indicated) a one-meter-long liquid-hydrogen target is used; center-of-mass energies are plotted assuming a stationary proton target. For NAL, performance is shown at assumptions ranging from an energy of 200 GeV at 10^{12} protons per pulse all the way up to an (optimistic) intensity of 5×10^{13} protons per pulse at 1000 GeV. Gray lines indicate the luminosities needed 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 line points out the increasing luminosities for events of increasing momentum transfer. Figure 2

hours delivered) to about 60% of the practical maximum; the fiscal year 1973 operating fund withholding and the decreased fiscal year 1974 support level have both acted to decrease this figure even further, by a factor much larger than the magnitude of the cut. This general situation has drawn criticism in Congressional hearings and a General Accounting Office report.[3] Yet all existing accelerator centers (other than CEA, which is scheduled to close) support a productive and competitive program and all have fairly recently acquired significant new capabilities. The Brookhaven Alternating Gradient Synchrotron (AGS) is completing its "Conversion Program;" SLAC is initiating its SPEAR colliding beam facility; the Zero Gradient Synchrotron (ZGS) has recently resumed operation with improved performance after installing a new vacuum chamber and pole-face windings; the Bevatron has initiated its heavy-ion beams; NAL is still in its "shakedown" phase but many initial experiments are in progress.

The actual drop in research output is less severe than the quoted figures of beam-hour loss indicate: Various capital-improvement projects have increased efficiency of beam operation in terms of numbers of simultaneous experiments, operating reliability and other qualitative factors. Regrettable as the low utilization may be, the worst solution to the present budget crisis would be to make a blanket "exploit what you have" priority decision, irrespective of the performance of the existing facilities.

An accelerator that is "fully utilized" in terms of beam time but which yields results of little physical interest is ultimately the least cost-effective from the point of view of optimum research productivity. Conversely, the best physical tools cannot serve efficiently if our most able scientists cannot gain access to them for a time adequate for discovery and measurement. What is the appropriate fraction of total funds to be dedicated to innovation, whatever the total funding level may be?

In the past high-energy physics has been a field of rapidly moving frontiers and of high productivity. To a large extent we attribute this productivity to the ability to generate new facilities at a pace compatible with the worldwide evolution of the science. In figure 1 we saw how the most important accelerator parameter—energy—has evolved over time. This dramatic advance was achieved by a remarkable sequence of new inventions and developments. We should also recall that the cost per MeV has dropped drastically during this evolution. Thus an arbitrary "freeze" on accelerator technology would be a highly uneconomical decision.

How has this spectacular growth been supported quantitatively in the past? Since 1961 the ratio of construction obligations to total costs incurred has been around 24%. Although this spending ratio for new construction has permitted rapid innovation of facilities and prevented their obsolescence, it has also been blamed as the main driving force toward high operating costs. This contention is not supported by the actual financial picture once inflationary factors have been taken into account. Figure 3 documents that the total real operating costs have actually *decreased* since 1967, the year SLAC started operation. This decrease has been bought admittedly at the expense of fuller exploitation of the existing facilities, but there is no question that during the past decade the research productivity of the US in high-energy physics has been second to none in the rest of the world.

How much innovation?

The purpose of new construction in support of US high-energy research is to ensure that the competitive nature of such research is not limited by the tools available. However well supported an operating program at a facility may be, exciting results breaking new ground cannot be obtained if that facility is essentially obsolete and limits the qualitative nature of the obtainable data. *Exploitation of existing facilities is expensive whether such facilities are operating near the frontiers of existing knowledge or not.* Therefore a certain fraction of the total costs dedicated by the nation to high-energy physics must be dedicated to new construction; this number has been about one-fourth in the past and the question is what guideline to adopt for the future.

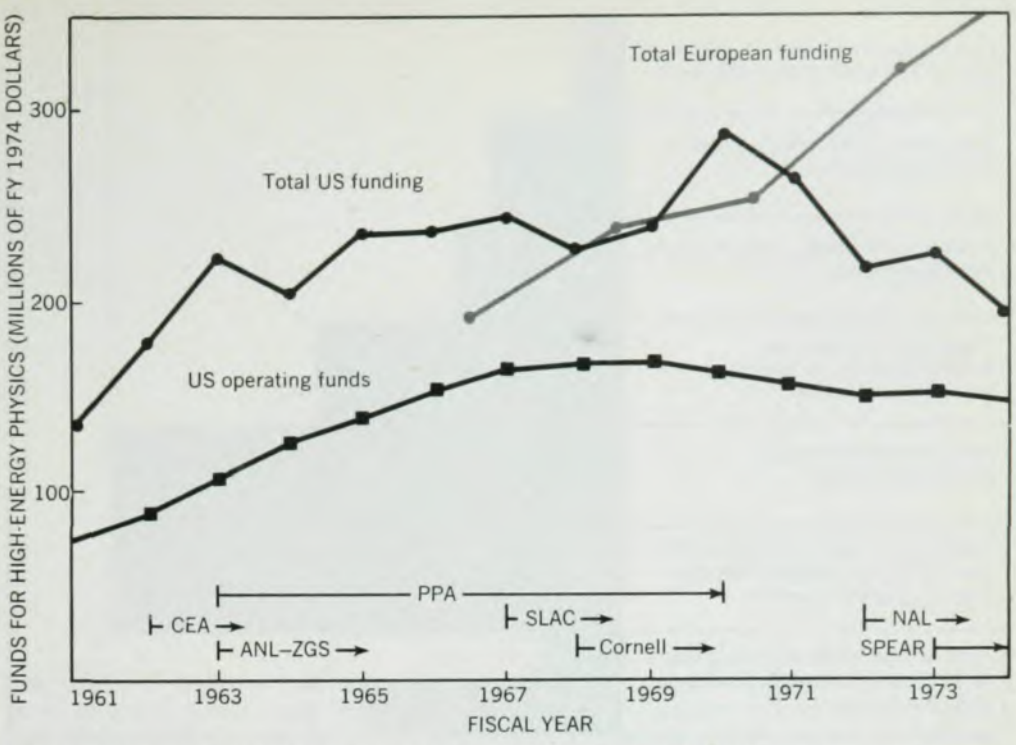
In trying to optimize this fraction the following time-scales are relevant:

- The "obsolescence time" T_0, which is the time during which a given accelerator can be usefully exploited, assuming it does not undergo a major improvement program.
- The "doubling time" d, which is the time required for the operating and capital equipment costs of the laboratory to equal its original construction cost.

The obsolescence time has been roughly ten years in the past, although this has, of course, differed for various installations. We also note that the lead time between submission of a formal proposal for a new installation or a major improvement program is also roughly ten years; the decision time, including that for reviews by outside scientific bodies, the Executive Branch and the Congress, generally takes about five years, and actual construction time approximates five years also. Therefore, to maintain the viability of the high-energy physics program, scientists must concern themselves with future planning for new construction at the same time as they undertake exploitation of existing facilities. Figure 4 summarizes some of the time scales discussed that illustrate the current problems.

Total funds available to high-energy physics are presumably limited at some level and we are here concerned with the fraction that should be dedicated to new construction to maintain viability of the program. An elementary calculation defines the fraction that should be dedicated to new construction in order to have innovation keep up with obsolescence.

Figure 5 shows the result of such a calculation. If the fraction dedicated to new construction is less than this amount, the number of viable installations will shrink in time; if it is larger then growth is possible. A number of simplifying assumptions have been made in this calculation. For instance, the cost of each new construction project designed either to revitalize or replace the original installation is assumed to be roughly equal to that of the original machine; actually the cost of a new installation has been varying substantially and improvement programs that extend the obsolescence time greatly are much lower in cost than that of the original machine.



US and Western European funding for high-energy physics research. Note the start-up dates for several US accelerators; the level of operating funds in the US program seems to bear little relation to the operation of new facilities. Figures are adjusted to the purchasing power of the dollar in fiscal year 1974. (The relatively small Princeton-Penn accelerator, PPA, was closed down in 1970.) Figure 3

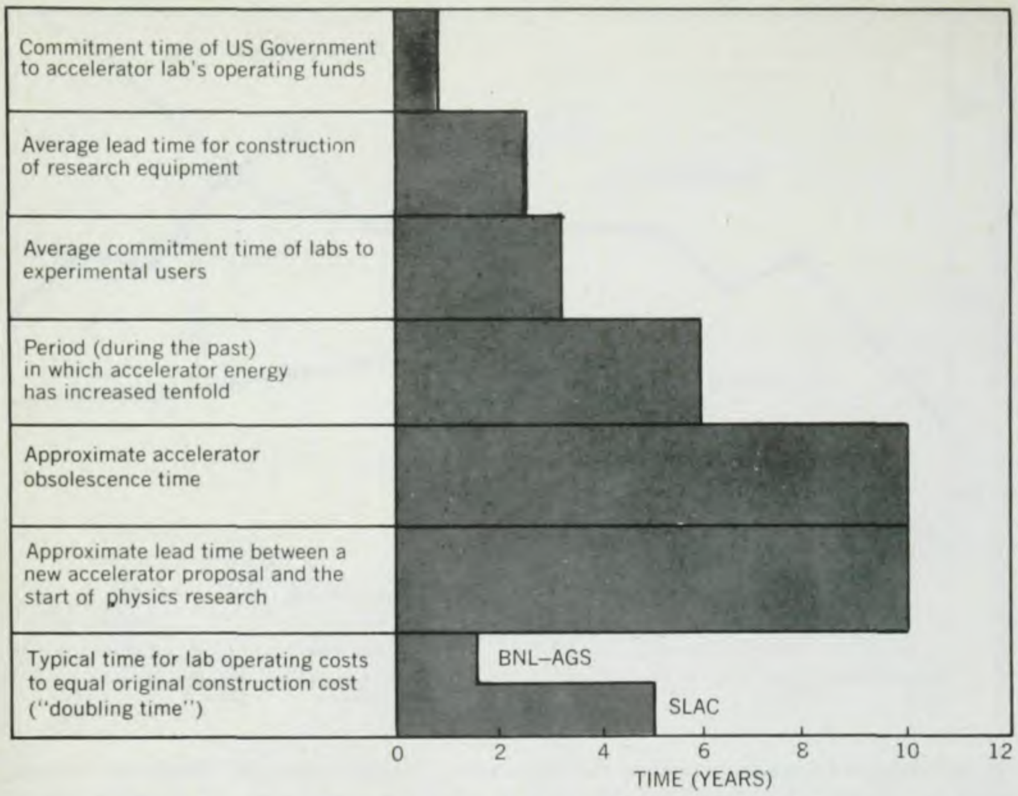
The graph indicates that the fraction that should be dedicated to construction is relatively *insensitive* to the ratio d/T_0. The "doubling time" d is approximately five years for SLAC (SLAC's operating costs at its present limited rate of exploitation equal its original construction costs in five years) and is about $1\frac{1}{2}$ years for the AGS at Brookhaven. These numbers are even smaller if the costs in the university program associated with using these accelerators are added to the direct operating cost. For an average d of three years and T_0 of ten years a fraction f of one-fourth should be dedicated to new construction, corresponding to the historical practice of using fractions of this magnitude for new construction.

Naturally this kind of calculation is only a policy guide, and each project must be judged on individual technical merit. However, these very general considerations should not be ignored in such decisions. Once the viability of the program has been permitted to suffer it takes a long time to recover competitiveness again.

Let us suppose that we do get sufficient support, and that meaningful long-range commitments can be made. Will the rate of discoveries in high-energy physics then keep up with the promise set by the machines? Any such assessment is of course subjective: I remember many times during the evolution of the accelerator art when "wise men" assembled in committees have said that the field was saturated and that future installations would only fill in details of previous work, with nothing genuinely new left to be uncovered. Subsequent experience has always contradicted such gloomy forecasts in the past.

Table 3 lists those discoveries in elementary-particle physics that have profoundly shaken our concept of Nature; again, such a list is subjective. The conclusion, however, appears sustained that there is no real indication that the rate of truly profound discoveries in elementary-particle physics has been slowing down in the postwar period. Thus we again face the question whether this will remain 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 possible both future systematic measurements *and* basic new discoveries. My earlier comments about the expectations for



Some important lead times characterizing US high-energy physics research. The commitment time of the government is only a year or so, compared with the ten-year lead time between a new accelerator proposal and the start of actual research. Figure 4

ultrahigh-energy storage rings as they reach the center-of-mass energies where electromagnetic and weak interactions become equal certainly indicate strongly that profound new revelations will come about once such machines are built. To predict specific additional discoveries is of course speculative, but it might be useful here to list those questions that should prove answerable and that, if answered, would lead to very profound conclusions indeed. I again divide this subjective list by the kind of interaction—strong, weak or electromagnetic—and finish with some general questions.

Strong and weak interactions

What is the behavior of cross sections at ultrahigh energies? Will the so-called Pomeranchuk theorem be satisfied, the theorem that predicts that particle and antiparticle cross sections become equal for all species? Will more detailed structure disappear from the curves describing 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 ultrahigh energies in terms of either the fragmentation of the target or the bombarding particle, retain quantitative validity? Will new qualitative features emerge in ultrahigh-energy reactions, features that 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 that 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 that 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.

The dominant question in weak interactions remains the one we identified in relation to the required technical characteristics of ultrahigh-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, the "intermediate boson W"? If so, what are its properties? Is it possibly an already existing hadron? Previous 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.

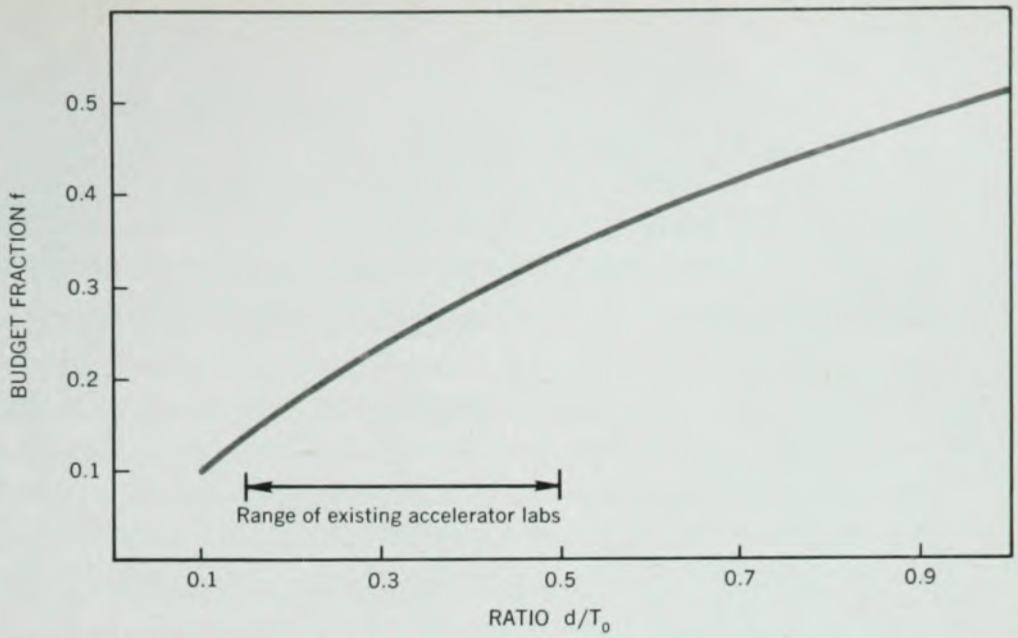
Another important question is the relation between the structure of the hadrons and the description of the weak interactions in which such hadrons are involved. With respect to electromagnetic interactions this question is illustrated by the electric and magnetic form factors that have been measured extensively with electron machines. In regard to weak interactions the corresponding form factors are more numerous, and the high-intensity neutrino beams we hope to have 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 over-all 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?

Electromagnetic interactions

A dominant question is 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. Quantum electrodynamics is now the only quantitative physical theory that 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 sufficiently refined experiments have confirmed this identity. All this means that the question of the muon's role in nature remains as obscure as ever, or to put it in I. I. Rabi's words: "Who ordered that?" 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, remains to be seen.

Then there is 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 are unexpectedly large and that the cross section exhibits "scaling" properties; this means that aside from kinematic factors these cross sections can 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 Richard Feynmann, and the 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, or do these new phenomena indicate something more "ultimate?" This question is equivalent to asking 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?



The fraction *f* of annual high-energy physics budget to be spent on new construction in order to keep up with obsolescence. The independent variable here is the ratio of doubling time *d* (time for operating and equipment costs put into a given accelerator program to equal its original construction cost) to obsolescence time T_0. Figure 5

More generally, we still do not understand why all charges are exact multiples of the electronic charge or whether magnetic monopoles exist. Also are there some totally new phenomena, at center-of-mass energies well above 100 GeV, that should be accessible to the new generation of super storage rings?

Many of the questions raised about 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, particularly because the cross sections governed by these different forces will tend to converge in magnitude at the highest energies 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.

To return to my title: A horizon is that boundary beyond which we cannot see, and I hope that I have demonstrated that there is indeed a great deal of a truly profound but unknown part of Nature beyond. What may of course be true is that high-energy physics exhibits another property of a horizon: As we march on in high-energy physics we do indeed uncover much that is new and far-reaching 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.

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Table 3. Milestones of the Past 25 Years in High-Energy Physics Research

Year	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	Antiproton	Matter–antimatter 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

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