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WHAT CAN WE EXPECT FROM FUTURE ACCELERATORS?*

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This talk covers a general but highly subjective overview of the expectation for new accelerator development. Let me begin with an updated version of the usual Livingston chart (Fig.1). This demonstrates the exponential growth in time of the equivalent laboratory 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 saturating the energy 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 - have been responsible for this development. The great question before us is whether the type of exponential growth reflected by these two charts can be sustained in the future. I am afraid that for a variety of reasons the answer is probably no.

Of course, motive for attaining large energy growth of accelerators must be expectation of truly meaningful results in elementary particle physics and Bj will talk about such expectations. A problem is that with the exception of Z_0 physics and the expected threshold for production of the t quark, specific predicted energy thresholds are hard to come by. The number of new quarks may not be exhausted. There is expected to be the Higgs particle; there may be the onset of whole new families of hadrons. However apart from structures associated with such specific objects general trends and cross sections tend to have only small variations with energy. Predicted masses for particles leading to grand unified theories obviously 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. Moreover, I would like to remind

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you again that in the past rarely have accelerators been built for the "right" reason: the most important impact of a new accelerator in particle physics has generally been in areas different than those which the designers used in justification. Therefore, the pressure for energy increases, although in my view real and justifiable, has to rest on only general arguments.

Just because the variation with energy of cross sections 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 not in sight through available basic technology.

Let me elaborate on this last, somewhat pessimistic remark. All existing accelerators and storage rings can be divided into those for which the cost to attain an increase in the available center-of-mass energy increased roughly with the square of that energy and those for which the cost variation is approximately linear. In the quadratic categories are all stationary target machines and electron-positron storage rings. In the linear category are proton-proton and proton anti-proton colliding beam machines and linear one-pass colliders for electrons with positrons. Thus cost alone seems to be imposing a serious limit on the growth of all stationary target machines and electron-positron storage ring colliders. There scaling laws do not, of course, define absolute costs; it is always possible to change the coefficient which gives the factor of proportionality. I am reminded of an amusing incident which occured 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 went up with the cube of the energy while the proton linear accelerator exhibited 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 proton machine designers kept changing the rules. They insisted on inventing phase stability, strong focusing etc. Thus any general argument based on scaling assumes an absence of new basic inventions - an assumption which I very much hope will be proven false.

Aside from cost scaling considerations one must look at the maximum gains in energy which are available from those new technologies now in sight. Here again the situation does not look too hopeful. The transition from the use of conventional magnets to superconducting magnets for circular accelerators and proton storage rings permits an increase in practical attainable magnetic field by, perhaps, a factor of 4 or 5, and the reduction factor in cost for a given energy is considerably less than this. The transition from electron-positron storage rings to electron-positron single-pass colliders raises the potentially available collision energy by, perhaps, an order of magnitude, but the large average power consumption of single-pass colliders may constitute a serious obstacle in reaching that goal. Let me remind you that in the past, as shown on the Livingston chart, an order of magnitude increase in laboratory energy was attained every 7 years, or in twice that time for center-of-mass energy. Thus these relatively modest factors in technically attainable growth combined with the relatively long interval between new machine authorization makes it very unlikely that the exponential growth of the past can be maintained much longer. The principal hope of continuing new productivity in this area has to rest on the exploitation of either totally new ideas or the exploitation of technical suggestions which have been made in the past but which had not appeared to be very practical. Unfortunately the list in the latter category is quite short.

There is one further problem which puts the continuation of the exponential growth in energy into jeopardy. This is the "how do we get there from here" problem. Let me explain. In the past rarely have nearly full scale operating models to demonstrate new accelerator principles been built which did not in themselves contribute to particle physics. There are exceptions: the conversion of the 37" cyclotron to a synchrocyclotron, the quarter-scale model of the Bevatron (which 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. Thus it would be very difficult in the future to secure funding for the construction of such "non-physics producing" models in competition with the ongoing needs of the particle physics program. It has not been customary in high energy physics to build operating models of non-physics producing machines which cost many millions of dollars, such as is being practiced in connection with the magnetic fusion program. Yet it would also 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. Maybe 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. Let me make a plug for the Stanford Linear Collider (SLC) which through a fortuitous combination of circumstances serves the dual purpose of a pilot project for new technology and as a highly promising physics tool on its own right. However, this opportunity, although very important, appears to be unique. Thus, for all the above reasons, I see no escaping the fact that if the growth of high energy physics opportunities through continuing evolution of the accelerator arts is to be maintained, even if not following the exponential pattern of the past, more funds will have to be dedicated to accelerator technology both for fundamental research and the construction of operating protype devices. As you know this problem was addressed by Maury Tigner's HEPAP subcommittee; they recommended dedicating 4% of total high energy physics funds to advanced accelerator R & D projects. Note that this recommendation does not include funds for the specific R & D necessary to prepare for specific construction activities at the various laboratories. I concur with the Tigner panel's recommendation and in fact in retrospect feel that the numerical estimate for the needed advanced accelerator R & D may be on the low side.

All this means that the talents of the accelerator physicist are needed more than ever, not only for the design and construction and the care and feeding of specific accelerators, but also for an intensified effort to enrich the technology needed to keep the enterprise going, and hopefully to make the great inventions. I recognize that such a drive cannot be force-fed; in fact if you look in the past you find that the great inventions were not made by what would now be called accelerator specialists, but by particle physicists and elevator engineers! Thus the future in respect to new inventions has to remain highly uncertain.

There is a drastic shortage of capable accelerator people, not only to fill the needs for high energy and nuclear physics but also to support the increasing number of emerging applications - synchrotron radiation sources and radiographic machines, to name but a few. Cancer therapy machines using different radiations including photons, electrons, pions and neutrons for specialized therapeutic goals continue to extend their promise.

Now let me turn to the expectation for the future along specific lines of accelerator development. Again Maury Tigner's HEPAP panel has made an attempt to do this and I am afraid that none of the visible avenues look too encouraging. Let us hope that this view will turn out to be myopic.

Clearly conventional proton machines using superconducting magnets will in time reach fields near 10T 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 in size is unclear; I doubt that Fermi's proposal to put such a ring into a Saturn-like orbit around the earth will become a reality! As mentioned before, I have great hopes for the electron-positron linear collider using linear accelerators of improved design, hopefully using high gradients and very large peak powers. However, it is difficult to imagine such machines going much above ½TeV against ½TeV particles because electromagnetic radiation in the beam-beam interaction becomes a very serious obstacle and because average electric power demands become very large, unless beam energy recovery schemes prove feasible.

Then 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 more. Yet practical difficulties look enormous. 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 powers, 3) very small phase volume for acceleration. None of the proposed solutions for these problems at this time look terribly inspiring but 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. In contrast to the first type of laser accelerator this second type produces energy gains proportional to the square of the electric field strength and thus the need for the development of high intensity laser sources is even more critical. 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 other fields.

Beyond these ideas there continue to be proposals for "collective" accelerators, that is those machines in which the degrees of freedom received by the motion of many particles "gang up" on the energy of a single particle to give it very large energy. The work in this field in this country has concentrated primarily on high current, low energy applications. Attempts have been made to apply these ideas to high energy beams with a notable lack of success with the possible exception of acceleration of heavy ions. This may not always remain so, but again experimentation looks arduous and expensive because the predictive power of theory to the complicated collective phenomena involved is limited.

The previous discussion has only dealt with the center-of-mass collision energy as if it were the only parameter of interest to measure the power of an accelerator or collider installation. This is, of course, not true; 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. It is extremely difficult to say anything even remotely intelligent on these other factors in a few minutes.

The fundamental cross section of the proton-proton interaction is given in Fig. 3 . Note that the cross section is still increasing at the highest energies reached so far. Thus even relatively low luminosity devices at higher proton-proton or proton-antiproton collision energies than 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, etc. On the other hand, 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 the intensity requirements for proton devices have to take account of these factors, and moreover the question of how to apply appropriate "cuts" which can unambiguously identify new phenomena on top of the very large volume of less interesting events may become more controlling than the matter of rates. 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.

The only hint we have that moderate luminosities possibly might be adequate for important new discoveries at the very highest 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 when one considers exploitations of the new technologies outlined above, or considers totally new approaches, one should not be too dogmatic about the required intensity or luminosity. The matter is, of course, to some extent 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 must serve a mixture of the expected and the unexpected.

Let me say again that these generalities give only a flavor of the type of question to be asked when weighing the merit of a specific new accelerator or collider idea. More detailed predictions for specific processes must of course be examined. Whenever examining the merit of any one experimental program using a new accelerator or collider installation one has to ask whether one will first run out of luminosity or out of energy. A clasical 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 luminostiy 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.

The situation is very different for electron-positron collisions. Here the basic cross section, being electromagnetic, is 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 simply be nothing to see. On the other hand, the problem of signal-to-background ratio is much less severe for electrons as is shown in Fig. 4. In addition the small 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 number of the virtual photon resulting from electron-positron annihilation. Thus high event rates result at the peaks of the psi/J and other--onium states and high counting rates are also expected at the mass of the intermediate vector boson. In addition, at energies above that of the intermediate vector boson which non-coincidentally is near the energy at which electromagnetic and weak interactions are expected to become equal, the behavior of cross sections is less definite. If the weak interaction becomes a dominant component of the cross section, and if the Weinberg-Salam rules hold, the cross section will vary inversely as the square of the energy. There could of course be further "bumps" - we don't know. As one goes to even higher energies predictions are difficult to make. For useful physics with high energy electron-positron colliders luminosities well above 10^{30} or 10^{31} cm^{-2} sec^{-1} appear essential.

To summarize, energy remains the primary parameter which must be extended in time if the productivity of the field in high energy physics is to grow. 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 respects.

This is the roughest of outlines as to where we stand and what the expectation for future accelerators may be. I do not see a clear path ahead but this is possibly a sign of old age. It is to counteract this problem that I consider the work of this summer school to be so criticaly important to the future of elementry particle physics.

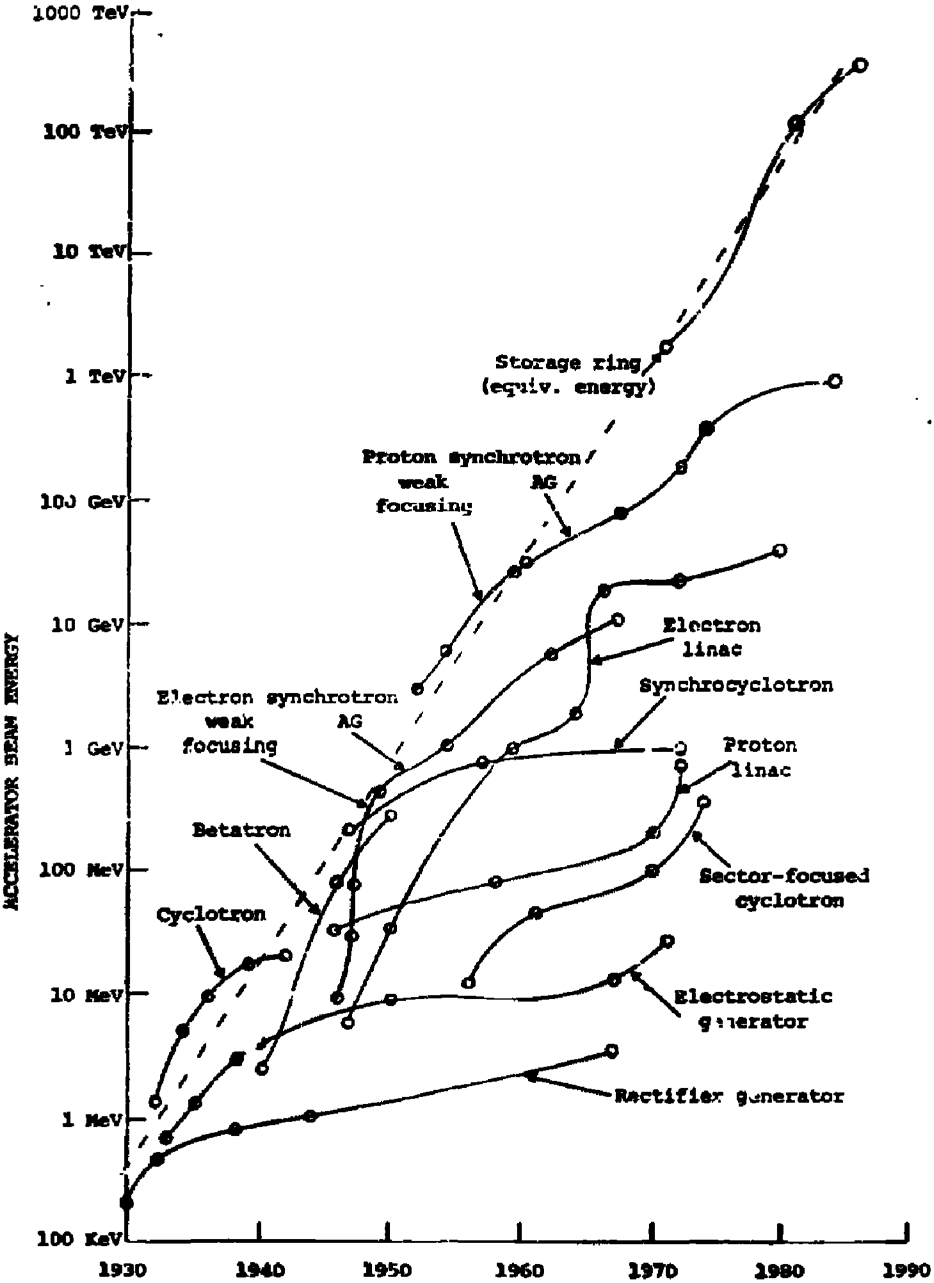
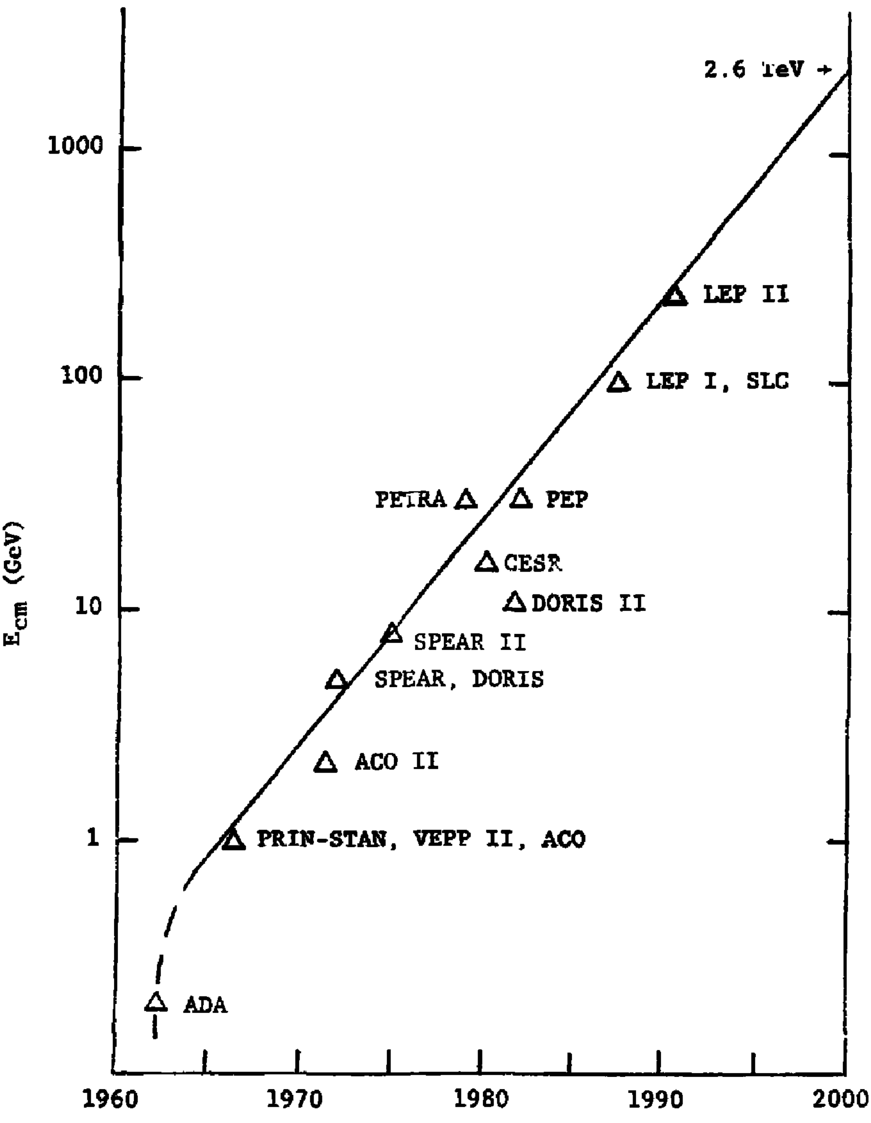


Figure 1

Energy growth of accelerators and storage rings



GROWTH OF ELECTRON COLLIDERS

Figure 2

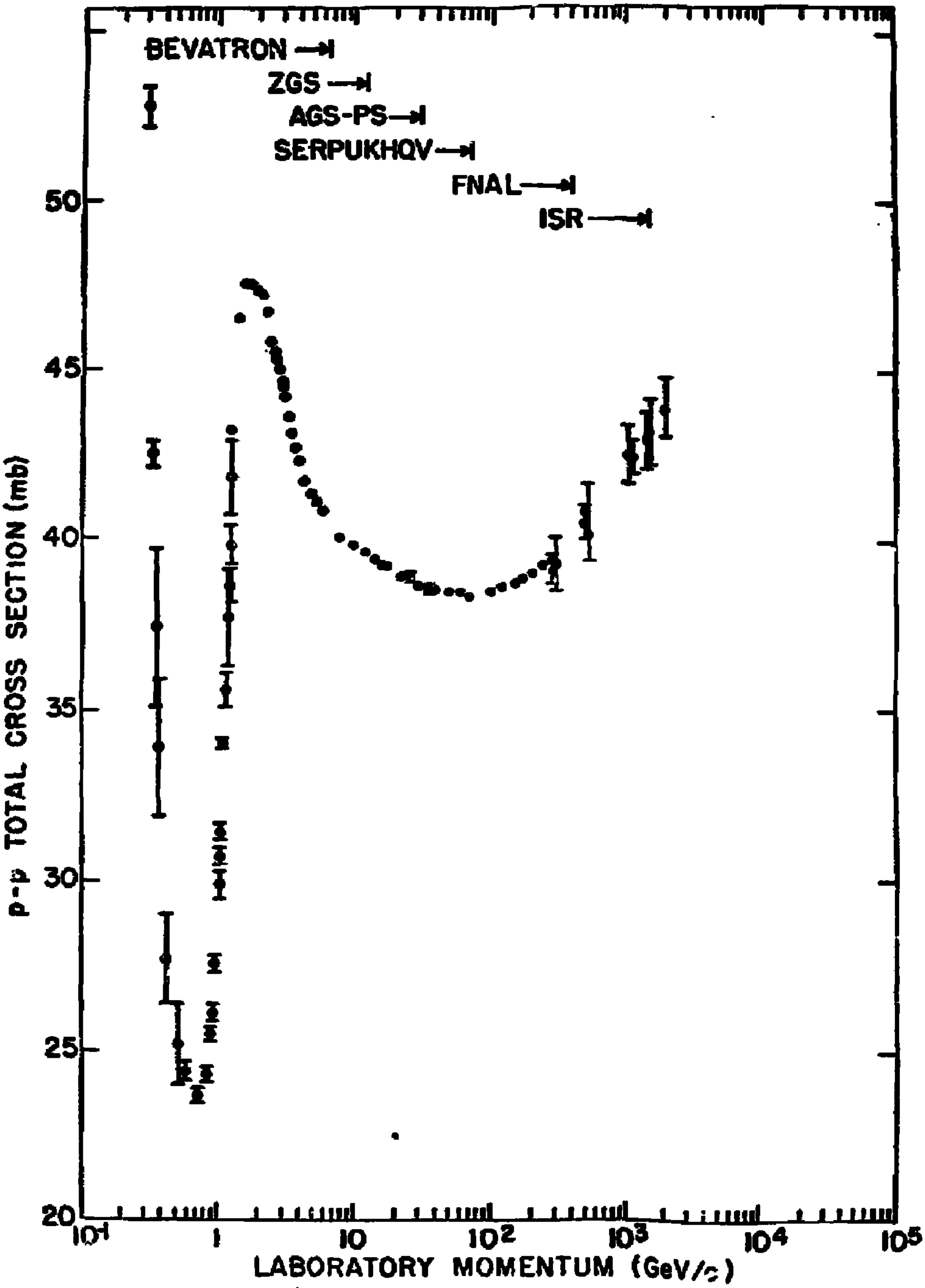
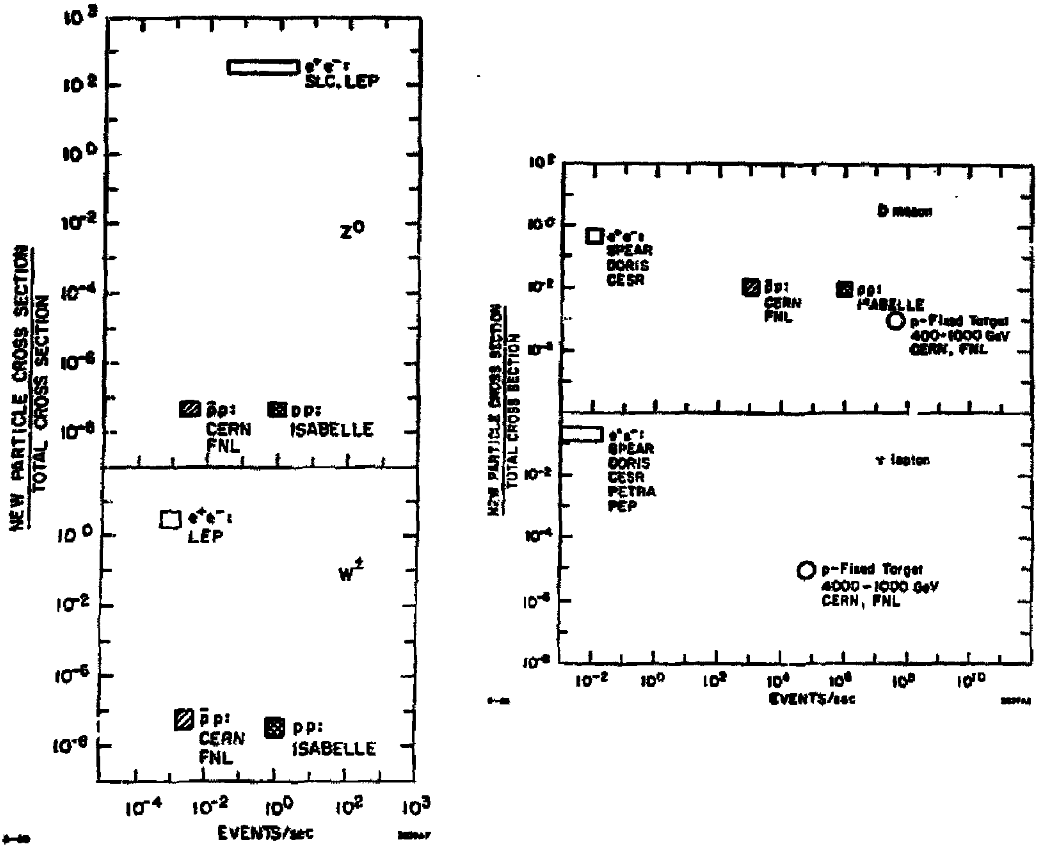

BEVATRON
ZGS
AGS-PS
SERPUKHOV
FNAL
ISR
p-p TOTAL CROSS SECTION (mb)
50
45
40
35
30
25
20
10^{-1}
1
10
10^2
10^3
10^4
10^5
LABORATORY MOMENTUM (GeV/c)

Figure 3



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.

Figure 4