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The Evolution of High Energy Accelerators*

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In this lecture I would like to trace how high energy particle accelerators have grown from tools used for esoteric small-scale experiments to gigantic projects being hotly debated in Congress as well as in the scientific community.

The first experiment using particle acceleration to explore the forces of nature is supposed to have taken place around 1589 (although many historians, spoilsports that they are, think that it never happened). Galileo dropped a light and a heavy particle, presumably a pebble and a stone, from the LTP (Leaning Tower of Pisa) and noted that they took the same amount of time, thus demonstrating that gravity is a universal force acting the same way on everything.

The particle energy in this experiment was about 5 millionths of an electron-volt per atomic mass unit. Since then we have progressed up to a TeV (10^{12} electron-volts), and expect to top that in the foreseeable future by another factor of 20 or so - so altogether the specific energy will have gone up by close to 10^{20}, that is a hundred billion billion!

How did all this come about?

Accelerators have been devised and built for two reasons: In the first place, by physicists who needed high energy particles in order to have a means to explore the interactions between particles that probe the fundamental elementary forces of nature. And conversely, sometimes accelerator builders produce new machines for higher energy than ever before just because it can be done, and then challenge potential users to make new discoveries with the new means at hand. These two approaches or motivations have gone hand in hand.

In 1930 Cockcroft and Walton found a way to produce high voltages of close to a million volts, and used them to bombard lithium with high energy protons. The result was a reaction where 14 MeV of energy came out - the first nuclear reaction produced by artificially accelerated particles.

Soon Ernest Lawrence and Stanley Livingston built the *cyclotron* or magnetic resonance accelerator. A 1.2 MeV cyclotron was built in 1932, and within a few years the energy limit had gone up to about 5 to 10 MeV. This led to a veritable flood of experiments on nuclear disintegration of light elements, where the Coulomb barrier is not more than about 10 MeV; by the beginning of World War II the maximum attained energy had reached 20 MeV and even the heaviest nuclei were subject to disintegration studies with cyclotrons.

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How does the cyclotron work?

The basic idea is that it is a way of obtaining particles of high energy *without* having to generate the corresponding voltage, by reusing the same moderately high voltage field again and again. This had first been proposed by Ising in Sweden and the Norwegian physicist Wideröe in Berlin: Use a sequence of electrodes with radiofrequency fields going up and down in voltage, and arrange for electric shielding so that the particles are exposed when the voltage is going up and hidden when it is going down. Lawrence saw Wideröe's paper and, as he relates in his 1951 Nobel lecture, "Not being able to read German easily, I merely looked at the diagrams and photographs ... I asked myself the question, ... might it not be possible to use two electrodes over and over again by bending the positive ions back and forth ..."

He noted that a uniform magnetic field makes a particle go in a circle whose radius is just proportional to the speed of the particle. The angular frequency is

$$\omega = \frac{eB}{Mc} \tag{1}$$

(I am using Gaussian units; with SI units you have to insert factors of c or 4π here and there). With two hollow semicircular D-shaped electrodes excited at this frequency, the beam gets accelerated every time it crosses the gap between the two dees, and thus in n revolutions it picks up a total energy of $2neV$ (V the voltage between the dees). The maximum energy is determined by the size of the circular magnetic poles.

But complications arise. The mass in (1) is the total relativistic mass, which increases with energy; so the frequency is essentially constant only as long as the extra energy from acceleration is small compared to the mass energy $E_0 = Mc^2$. Furthermore, to enable the particle to make many revolutions, the orbits have to be focused, and it turns out - as Lawrence and Livingston soon found out - that focusing forces require the magnetic field to *decrease* with increasing radius, while the synchronization condition (1) would require an *increase* of B with radius. Therefore the particle gets out of step after a while, and the number of accelerations it can get is limited. The result was that the cyclotron, while very useful for getting up to 10 or 20 MeV, was limited, at least with the radiofrequency technology of the thirties, to around 50-100 MeV.

In any case, the relativistic limit made the cyclotron completely useless for accelerating *electrons*. But in the same paper that had stimulated Lawrence, Wideröe also suggested a way to accelerate electrons by means of increasing magnetic flux. In 1941, Donald Kerst took up this idea, and, with the help of theoretical orbit dynamic analysis by Robert Serber, he was able to make it work: the beam transformer or *Betatron* requires a field on the circular particle orbit which is just half of the average field inside the orbit; for focusing stability the field has to vary with radius proportional to the inverse n-th power of the radius, where the field index n satisfies

$$0 < n < 1 \qquad (2)$$

Armed with this theory, Kerst built a betatron for 100 MeV in 1945, which then held the world record for artificially produced beam energy; some years later he built one for 300 MeV. These machines served as powerful X-ray sources. and led to extensive work on nuclear photodisintegration.

In the meantime, E M McMillan at Berkeley and Vladimir Veksler in Moscow saw a way to get around the limitation of the cyclotron. We rewrite (1) in the form

$$E \equiv Mc^2 = \frac{eBc}{\omega} \qquad (3)$$

and suppose that we have a field B, an rf system with frequency ω, and a particle with energy E satisfying (3), and that we then slowly change ω and/or B. Then, as Veksler and McMillan showed, the energy E will automatically adjust itself so as to maintain the synchronism condition (1) or (3), as long as the speed at which the field or frequency changes is not too fast, and the rf field has enough voltage to keep up with the rate at which the energy has to change in order to keep (3) satisfied.

This principle of *phase stability* overcame the limitation on the cyclotron: Simply modulate the frequency ω downwards in a cyclotron magnetic field, and the particle will accelerate. In principle one can go as high as one wishes if one makes the radius of the machine big enough. The radius for a given energy is determined by the equation

$$pc = eBR; \; R = pc/eB \qquad (4)$$

where p is the particle momentum. But the whole area encircled by the top energy orbit has to be filled with magnetic field; this can get expensive if the size gets too big. In practice, *synchrocyclotrons* working this way have been built for energies up to about 600 MeV.

Another way to apply the McMillan-Veksler principle is to keep the orbit radius constant, and change the field - and the frequency - with time so as to keep (3) and (4) satisfied. Now magnetic field is only needed in a small annular region surrounding the orbit; for large radii (high energies) this is much more economical than the synchrocyclotron. And electrons can be accelerated this way; the advantage over betatrons is again that the magnetic field is only needed in the vicinity of the orbit rather than throughout its interior.

In the immediate postwar years, the late 1940's, synchrocyclotrons and electron synchrotrons in the 300 MeV range proliferated, because there was exciting new physics to be done: the pion had been discovered in cosmic rays, and these machines led to a thorough exploration of pion physics.

Next came the realization that there seemed to be nothing standing in the way of much higher energies for proton synchrotrons. Several proposals for proton synchrotrons appeared, notably one for a 1 BeV machine at Birmingham, England (Oliphant, Gooden and Hide) and one for a 10 BeV machine by Brobeck at Berkeley. Discussions between Leland Haworth at Brookhaven, Ernest Lawrence at Berkeley, and the AEC authorities led to the decision that both Brookhaven and Berkeley, instead of competing for the 10-Bev prize, would each build a smaller proton synchrotron, one around 3 BeV and one at 6. Haworth chose the smaller size with the hope of getting it finished faster; in later years he often said that this was one of the best decisions he had ever made.

And now we get into some of my personal recollections. An Accelerator Project was set up at Brookhaven under M S Livingston. In the spring of 1947 he, P M Morse (director of Brookhaven) and R A Patterson (personnel director) visited Cornell, where I was a post-doc under H A Bethe, and invited me to Brookhaven for the summer. The next year I joined for good. Among others in the accelerator group were G K Green, John and Hildred Blewett, and a young theorist named Nelson Blachman with whom I worked on several of the theoretical problems of the proposed machine (named the Cosmotron).

One of the important problems we tackled was the dynamics of particle oscillations, both transverse (betatron oscillations) and longitudinal (synchrotron oscillations) as modified by the fact that this machine, unlike the earlier electron synchrotrons and cyclotrons, had straight sections between the circular arcs, i.e. non-circular orbits. (Dennison and Berlin, following a suggestion by H. R. Crane, had tackled a similar problem here at Michigan; I think Serber was also involved). We derived a matrix formalism for handling the spatially periodic force fields seen by the particles, and found that (a) the frequencies of the oscillations are more complicated to calculate than in the circular case; (b) that the amplitudes of oscillations are modulated, and (c) that there might, especially if the straight sections were long, be a "transition energy" at which the stable and metastable phase equilibrium points that give phase stability exchange roles - but we saw that in the Cosmotron, with its rather short straight sections, this problem would be avoided.

The more practical people worked hard on the magnets, vacuum systems, rf etc, and by the spring of 1952 the machine was finished. On May 20, 1952 a beam was injected into the Cosmotron and accelerated to 1.3 BeV - by far the highest energy in the world ever attained by artificial acceleration. Soon we surpassed that record and got to 2.3 BeV in June, and to 3 GeV (the design energy) the next year.

Soon important physics was done with this new machine. Foremost was the exploration of the "strange particles" previously found only in cosmic rays, and in particular the verification of the Pais - Gell-Mann hypothesis of associated production of hyperons and strange mesons, which was accomplished by Fowler, Shutt, Thorndike and Whittemore in 1953.

In the meantime, CERN was being formed in Europe: an international laboratory, a joint venture of a dozen European countries, to be devoted to high energy physics. A delegation of Europeans was expected to visit us to see whether they could pick up some good pointers from us - they were planning, as the centerpiece of their new international laboratory, to build a proton synchrotron even bigger than the Bevatron, around 10 GeV.

As I recall, Livingston set up a study group especially to enable us to tell them not only what we had done but also what one might do better. One problem that bothered him was: The magnets of the Cosmotron all face outward; therefore negative secondary beams are easily obtained, but positive secondaries will tend to hit the inside wall of the machine. In addition, magnet saturation effects tend to reduce the usable "good-field" region at the fields corresponding to top energy. Therefore it would be better to alternate the magnet sectors, with some having the back legs on the inside and others on the outside.

I pointed out that this might have a drawback: The focusing gradients might easily be different in the inward and outward sectors, especially in the fringing fields. Because of my earlier work with Blachman on straight sections, I knew how to attack this problem mathematically: Set up matrices for the focusing action of each sector, and multiply them together.

Almost at once I saw that the alternating gradients could *enhance* stability rather than weaken it! With the right parameters the stability could be made stronger than in the conventional case. Livingston saw at once that this was really something fundamentally new, and that the focusing could be pushed to make it much stronger so as to make it possible to make the magnet aperture really small. That, in turn, makes the magnets - and other components - much cheaper, and so one can go to higher energies than without "strong" focusing. We published a design with 1 inch aperture for 30 GeV. Snyder explained the new results in terms of optical principles, and we and Blewett saw that the same principle can be used without bending magnets - e.g. with just quadrupoles - for focusing of beam lines, and to replace the grids that were then though necessary in proton linear accelerators. So we had something to tell our European visitors when they came!

About a week or two later the Europeans arrived. They were: Odd Dahl, who had worked with high-voltage machines in Washington before the war; Frank Goward, one of the people who had first made a synchrotron work, and Rolf Wideröe, the Norwegian who had first devised a scheme to use radio frequency repeatedly to produce more energy than the corresponding voltage (in fact, his 1928 paper had set Lawrence on the track that led to the invention of the cyclotron). They were duly impressed, and recommended to the nascent CERN organization that they should use the new method to build an accelerator for 30 GeV rather than the 10 that they had counted on.

While all this excitement was going on, some red-faced people at Berkeley dug up and sent us what they had thought was a crank letter from Greece which they had received a couple of years earlier. It turned out that an engineer named Nicholas Christofilos in Athens had thought up essentially the same scheme after reading about plans for the Bevatron. We soon saw that he deserved full credit - and we hired him at Brookhaven. Later he moved to Livermore to work on fusion and on weapons ideas.

Actually strong focusing had also been anticipated by L H Thomas in 1938. He had devised a modification of the cyclotron which would have strictly constant orbit frequency, and achieve the necessary orbit stability by means of azimuthal field variation - indeed a (weak) version of AG focusing. Furthermore, unknown to the open physics community at that time, a project was underway at Berkeley and Livermore to make a Thomas cyclotron as a spallation neutron source, in order to bombard uranium and thus produce plutonium, as an alternative to reactor production for weapons use. This project was classified, and there were those in the AEC who wanted to put the lid on our work too because it was related. Leland Haworth lobbied vigorously - and successfully - with the AEC people to keep us in the open; this may have been helped by the fact that we had already discussed it with the Europeans.

Incidentally, the fact that many of the best people at Berkeley were occupied with this project may have contributed to the fact that the Bevatron was not completed until about two years after the Cosmotron. But I think part of the reason was that the Berkeley people were more conservative in their design, and initially planned the Bevatron with a huge aperture (four feet) - when the Cosmotron worked they changed it to the more modest one foot!

Anyway, Brookhaven and CERN went on with parallel projects to build another world energy record holder. CERN got there first; the CERN PS had a beam in 1959, a year or so before the AGS in 1960. There was always a friendly and collaborative rivalry between Brookhaven and CERN - and remains so to this day.

Much great physics has been done with both the Brookhaven and CERN machines. A few highlights: The systematization of strange-particle dynamics (the Ξ, the Ω, the experimental basis of SU3); CP nonconservation, the existence of two neutrinos; the discovery of the J/ψ particle, neutral currents. And the Bevatron at Berkeley, which was finished two years after the Cosmotron but of course long before the AGS, found the antiproton, and more than held its own in the exploration of the many particle states and resonances that still warrant an annual data booklet on high-energy particles.

In January 1954 Enrico Fermi delivered the "Retiring Presidential Address" at the annual meeting of the American Physical Society. In that lecture he projected some of the possible advances of physics in the future. In particular he noted what has become known as the Livingston Curve: The energy produced in particle accelerators had, since about 1930, increased by a factor of 10 every six years or so, from the 700 keV of Cockcroft and Walton to the 3 GeV of the Cosmotron. With the new strong focusing method it appeared likely that this advance would continue for some time more, to something in the range 30-100 GeV.

He made the bold prediction that the advance would continue another forty years, to 1994 (ten years beyond Orwell's 1984), at which time there would be an accelerator in an orbit encircling the earth above the equator, with energy of 3×10^{15} electron volts - a million times the highest energy reached at that time. But he also predicted that this machine would accelerate only a few particles every minute.

No doubt he thought he was being facetious. But today we are in a position to attain and even surpass the energy Fermi's machine would have made available, and with very high beam intensities.

This is primarily due to the concept of colliding beams. With colliding beams *all* the energy of the accelerated particles is available, rather than just a small fraction as was the case in 1954. Therefore the equivalent energy of Fermi's accelerator can be had from machines of more modest size. The realization, by Kerst, Symon, and O'Neill, that colliding beams could be used was, at least in part, made possible by the high beam intensities the new accelerator techniques provided.

Even before the AGS and PS were finished, some people started to explore the possibility of going even higher in energy, that is, starting to realize Fermi's program. A workshop at Madison, Wisconsin in 1959 explored the possibility of going to 300 GeV. Matt Sands introduced the idea of a cascaded sequence of rings, with beams transferred from small accelerators to successively larger ones. By 1966 projects for 200-300 GeV machines were started in the US and at CERN; again two essentially similar machines were being undertaken on opposite sides of the Atlantic. This time the US machine was done first, while CERN started theirs later. The Fermilab accelerator, initially aimed at a nominal 200 GeV, went as high as 400 GeV (and even 500 at times), starting in 1972.

The beam intensity of the AGS and the PS was 10^9 at first, and soon went up by a factor of 10. The next factor of 10 - to 10^{11} - took a bit longer, but in a couple of years it was up to 10^{12}, and now we routinely get 10^{13} - ten thousand times as much as at first.

The high intensity that the AGS and other machines produced made it possible to think in terms of collisions between two beams going in opposite directions. This greatly increases the energy available for particle reactions - with a fixed target and highly relativistic beams most of the energy is "wasted" in getting everything to move forward. Colliding proton beams were first suggested seriously by Kerst and Symon at MURA, and by O'Neill at Princeton. The MURA group, led by Kerst, undertook the design of a 15 GeV FFAG colliding-beam machine, but eventually the Government decided against funding it. But in the process of planning and designing the machine, MURA physicists including Kerst, Symon, Terwilliger, Sessler and many others made many fundamental contributions to our understanding of the behavior of accelerated and stored beams.

The reason the Fermilab machine came much sooner than its CERN counterpart was that CERN decided about 1964 to build a colliding-beam complex called the Intersecting Storage Rings (ISR). Protons from the PS were fed into two rings, circulating in opposite directions and colliding in eight places. The energy was about 30 GeV per beam. Because of the inherent inefficiency of collisions of a high-energy beam with a fixed target, a single beam aimed at a target would have needed 2000 GeV to make the same energy available for particle reactions - thus the ISR in effect raised the world's record energy by an enormous amount.

Colliding beams have also been used extensively with electrons and positrons. Electrons can be accelerated in synchrotron rings just as well as protons, as we have seen. Positrons (positive electrons) can be produced quite copiously by bombarding targets with high energy electrons, and then they can be accelerated too. In a given magnetic ring positrons will go in just the same orbit as electrons, but in the opposite direction; therefore only one ring is needed for beam collisions instead of two.

Electron-positron colliding beam rings have been built in many places, starting in the sixties, with energies up to 30 GeV per beam, and just this summer the "Large Electron-Positron Ring" (LEP) came into operation at CERN with 47 GeV per beam (and eventually it may go as high as 100 GeV). That machine is enormously big - its circumference is 27 km or 17 miles, and it is in a deep underground tunnel that crosses the Swiss-French border near Geneva and goes right under the Jura mountains.

The reason LEP has to be so much bigger than a proton machine for the same energy is that electrons (and positrons) radiate light (synchrotron radiation) very copiously when they are forced to move in curved orbits. This causes a loss of energy which has to be made up by the accelerating system, and therefore the accelerating process becomes very inefficient. The radiation is less intense if the curvature is more gradual, hence the effect is less severe in a large circle than in a small one. But it is strong enough to make it very unlikely that anyone will ever build a circular electron accelerator with more energy than LEP.

An important technical development was the discovery of "type II" superconductors, with which one can make magnets at much higher fields and with much less energy dissipation than before. The high fields again make it possible to build the rings smaller for a given energy, or more energetic for a given size, than otherwise. Superconducting magnets were added to the proton synchrotron at Fermilab, converting it into what is now known as the Tevatron (because it gets close to a TeV), in which protons go as high as 900 GeV - the current world's record.

Some years ago - actually already in the seventies - it became clear to particle physicists that there should be phenomena beyond the energies available with machines like the AGS or even the Fermilab proton synchrotron and the ISR. In particular, what came to be called the "Standard Model" of particle interactions postulated the existence of the intermediate vector bosons W and Z, the "Higgs boson", and new generations of quarks including

Bottom (or Beauty) and Top (or Truth). This called for more energy. How much?

The W and Z were predicted to have masses around 85-100 GeV. Therefore proton machines needed at least this much in the center of mass (available energy) system, preferably several times more (since protons are thought to be composites of three quarks plus gluons, so that the energy of each component is only a fraction of the proton's energy). At CERN it was proposed to go for proton-antiproton colliding beams in the SPS (300 GeV per beam). This was made possible by the development of the concept of beam cooling by Budker in Novosibirsk and by van der Meer at CERN. It is a technique which compresses the beams - especially antiproton beams - to small dimensions and high density, so that colliders can work with sufficiently high reaction rates. The payoff was the proton-antiproton collider at CERN, which indeed led to the spectacular discovery of the W and Z particles; now the Tevatron at Fermilab uses cooling to get collisions at 900+900 GeV.

At Brookhaven a superconducting proton-proton collider for 400 GeV (named ISABELLE) was proposed; the advantage being that with proton beams one could get much higher luminosity (reaction rates) than with antiprotons, and therefore do a more thorough job of exploring the detailed behavior of the new particles beyond just demonstrating their existence. Unfortunately a comittee of "wise men and women" forced abandanmont of that scheme in 1984, when it was halfway done, because they thought one could then proceed more expeditiously with the next step in energy, the Supercollider. (But it has taken from 1983 to now to get started on the SSC; if ISABELLE had gone ahead it would now have several years of copious W and Z production behind it.

Instead Brookhaven is now working on a heavy ion collider, RHIC, which will use the tunnel and other facilities initially intended for ISABELLE, and in which heavy ions - at least up to gold, maybe uranium - will be accelerated to more that 100 GeV per atomic mass unit. This should inaugurate a new field of study of bulk nuclear matter - the "quark-gluon plasma" - under conditions simulating the state of affairs just after the "big bang" creation of the Universe.

An alternative approach is to use electrons and positrons. To make the Z, a total energy of around 90 GeV was needed, i.e. 45 per beam. The LEP machine just mentioned was designed to accomplish this, and it has indeed done so - and in a few years it may be upgraded in energy so that it can make a pair of W's.

A more radical approach was followed by SLAC (Stanford Linear Accelerator Center). They had an existing linear accelerator for 20 GeV electrons. They (a) upgraded its energy to 50 GeV, (b) added a positron generator to enable it to accelerate positrons as well, and, (c) attached two magnetic bending arcs at the end, one of them to bend electrons first to the right and then to the left and the other to do the opposite with positrons, so that they can finally collide where the two arcs meet. This requires fantastically concentrated and well-focused beams, just a few microns in diameter, and of course very well aimed. It took almost a year of growing pains after it was all built, but Z's were indeed produced early this year.

So now what do people want? More. Theorists speculate that the next qualitatively new phenomena will happen at elementary interaction energies of a TeV (trillion electron volt) or two. And since protons are composite, to get that much per component it is safer to aim at 10 to 20 TeV per proton, probably the more the better. So a 20 TeV proton collider, the Superconducting Supercollider (SSC) is now being designed, to be built near Dallas, Texas.

The SSC is to be about 54 miles (87 kilometers) around, in a tunnel underneath the rolling countryside of Texas south of Dallas, surrounding the small city of Waxahachie. The tunnel will be mostly underground, so that the surface is not disturbed except in the "campus" area where the experiments are done, and in occasional access buildings spaced several miles apart. As the name implies, the machine will be made of superconducting magnets, composed of very fine filamentary niobium-titanium wire and producing fields of 6.5 Tesla or 65 kilogauss (as compared to 1 to 2 Tesla in conventional magnets). This high field is necessary to keep the overall size down to the (almost) reasonable 54 miles. - The injector for this machine, i.e. the last stage of acceleration before the final one, is now envisaged at 2 TeV, i.e. twice as much as the biggest accelerator in existence today; it in turn will be fed by a chain of smaller accelerators.

And, to keep up the tradition of transatlantic competition, the CERN people are proposing the LHC (Large Hadron Collider) to be built in the same tunnel that already houses LEP, but with very strong superconducting magnets (10 Tesla or 100 kilogauss), so as to get to about 8 TeV per proton - a lot less than the SSC, but maybe enough to get into what, according to current speculation, is the next interesting energy range - and undoubtedly less expensive.

These beasts are expensive. The SSC is estimated at six billion dollars - far more than any other scientific machine in the past - but still not so much when compared to some of the things proposed in the space program, in particular the space platform, and almost chickenfeed next to some defense projects. So *if we really want it, we can afford it.* But that opinion is not universally shared.

Since, as mentioned before, electrons and positrons are really more elementary than protons, the energy needed to get into the desired TeV range is less for electrons than for protons. Furthermore it is believed that the reactions will be simpler, and the interesting results (needles) would be easier to disentangle from the huge background (haystack) of other things that happen when particles collide. So maybe electron-positron colliders of 1 or 2 TeV will be just as good (or even better) for physics as 20 TeV protons - and significantly cheaper - ???

Because of the synchrotron radiation energy loss, circular electron-positron colliders are limited to around 100 GeV - definitely not enough. The only hope is to have two *linear* accelerators shooting their beams at each other. But this means that each electron gets only one chance to collide with a positron, while in a circular machine it gets a chance on every revolution, hundreds of thousands or millions of times altogether. So to get interesting collision rates in a linear collider system one needs beams that come at a very high repetition rate (and therefore need a lot of power), and have to be focused and aimed to exquisite accuracy, preferably a fraction of a micron. The Stanford Linear Collider now operating at 50 GeV is, of course, a prototype and existence proof for this technique, but to extrapolate it to 1 or 2 TeV is certainly far from straightforward - and surely nowhere as near to proven feasibility as the circular proton colliders.

So far all the really high-energy machines built and planned in the world - except the SLC - have been ring accelerators and storage rings using the strong-focusing method. But this method has not removed the energy limit, it has only pushed it higher. It seems unlikely that we can go beyond the 20+20 TeV SSC - if indeed we get that far. Other acceleration and beam-forming methods are now being discussed - collective fields, laser acceleration, wakefield accelerators etc., all aimed primarily at making linear colliders possible and more attractive than with present radiofrequency methods. So far it is not entirely clear which of these schemes will dominate particle physics in the future - maybe something that has not been thought of as yet.

Why are we doing all this? High energy accelerators are often described as super microscopes, which enable us to explore smaller and smaller distances - and there is every reason to believe that there will indeed be new things to be seen with microscopes finer than any now available. Another way to look at the accelerators is as "time machines" - they can duplicate some of the conditions of the first instants of the "Big Bang" in which the whole Universe got its start. The Heavy Ion Collider now being designed at Brookhaven can simulate a drop of the hot primordial "soup" that existed for a moment; the SSC can make an even hotter but smaller drop. Thus Enrico Fermi's dream of 1954 can be realized.