FUTURE DIRECTIONS OF HIGH ENERGY PHYSICS

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Predicting the future is a risky enterprise, as the much-loved comic strip character Pogo once pointed out. Nearly 30 years ago, he confided to one of his friends that he stuck to predicting the past, because it was a lot safer than predicting the future. Pogo's observation was in my mind as I began a few months ago to think about what I would say to you on the subject of the future directions of high-energy physics.

I had thought that I would talk about global collaboration as the path to future high-energy colliders. Since the termination of the SSC in 1993, I and other directors of high-energy physics laboratories have told anyone who would listen that we must convince our government of the necessity of this collaborative approach to building future accelerators. However, just as I had grown comfortable with the idea of speaking to you on this subject, Representative Sensenbrenner, Chairman of the House Science Committee, began to question the terms of United States participation in the Large Hadron Collider at CERN, the highest-profile global collaboration in accelerator physics today. Congressman Sensenbrenner expressed serious concerns about the nature of the US agreement with CERN, and suddenly the prospects for global collaboration began to seem rather dim. It was then that I remembered Pogo's advice. Happily, the CERN Director General and the Secretary of Energy have since clarified the terms of the US involvement in the LHC to the satisfaction of Chairman Sensenbrenner and others, and he has expressed his support for US participation in the new accelerator.

To US physicists, it is very good news that global participation in the LHC is still on track. It is clear that there will be many more bumps in the road, but if we are cooperative and vigilant, we can succeed. While global participation may be the only path to the high-energy accelerators of the future, even Pogo could predict with confidence that it will not be easy.

One measure of the health of a field of science is the depth and significance of its unresolved questions. By this measure, the field of high-energy physics is in excellent health. Further, the nature of the important questions we confront can give us some guidance on the future directions of high-energy physics. I have chosen four questions to talk about, because the accelerators that we will need to answer them have been presented at this meeting.

I doubt that the current generation of accelerators, or even the next generation, will fully answer these questions. But I do believe that they will bring new knowledge and deeper insight into the questions. My four questions are these:

- What is the source of electroweak symmetry breaking?
- Why is there a preponderance of matter in our part of the universe, when the laws of physics that seem to govern the subatomic universe put matter and antimatter on the same footing?
- What is the cause of the unexpected and unexplained deficit of electron neutrinos emerging from the sun, and the cause of the unexplained ratio of electron neutrinos to muon neutrinos produced when cosmic rays slam into the upper atmosphere?
- What is the universe made of?

Before I speak more about the four important questions and their implications for future directions, I want to remind you of the current state of elementary particle physics. All of the familiar forms of matter, such as protons and electrons, and some not-so familiar forms of matter, such as B mesons and top quarks, can be reduced to a set of spin-1/2 particles named quarks and leptons. The quarks, of course, are what make up dull, uninteresting particles such as the proton. The quarks and leptons interact with one another through the exchange of three distinct types of spin-1 particles called gauge bosons. These three types of gauge bosons define the three forces familiar to all those of us who build accelerators. The photon, the boson of electricity and magnetism, is the most familiar to us, because it is what we manipulate to make all of our wonderful accelerators, storage rings, and power sources to create and control beams of charged particles in accelerators. Electricity and magnetism, which Maxwell unified, is the simplest of the forces: the photon is coupled to the charge of a particle, and it is just a simple number. It was the first field for which we understood the elegant properties of gauge interaction.

Of the other two forces, the weak interaction is next in importance, to accelerator builders. When the beams that we so carefully accelerate deviate from the desired path, they typically hit a piece of copper, iron, niobium, or some other piece of a vacuum chamber, RF cavity, or magnet. Sometimes the result is catastrophic, and we get a hole in a vacuum chamber; but most of the time we simply experience a loss of beam. Whenever our beams go astray, however, the objects they touch become radioactive. Thanks to the weak interaction, that radioactivity persists after the beams go away. The weak interactions show up as beta decay, carried by three gauge bosons: the W bosons, which come in positive and negative varieties, and the Z boson, which is neutral. The bosons are nearly a hundred times as massive as the proton, and because they are so massive, the range of the weak force is very small, roughly 2×10^{-16} cm or two thousandths of a fermi. Recall that a proton has a diameter of about one fermi, or 10^{-13} cm. The only way to explore this weak force is in very high energy collisions.

The model I have described is a great picture. It explains almost everything that we have been able to do with accelerators. But there are a few flaws. First of all,

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the Standard Model has a glaring mathematical weakness. It would be just fine mathematically if all of the particles in the theory, including the gauge bosons, were massless. However, this is not the case— all the quarks and the charged leptons do have masses, and so do the bosons, except for the photon. The W and Z are as massive as a silver atom.

If the particles were massless, then electricity, magnetism and the weak interaction would constitute a beautiful, unified, elegant theory. But the existence of massive particles leads to another question: Why are the W and Z different? The Standard Model gives mathematically inconsistent results for energies

$$E_{cm} > \sqrt{\frac{1}{2G_F}}$$

Peter Higgs proposed a way around the problem by introducing a spin-0, scalar "particle" that could give the W and Z a mass, retain the electroweak unification, and give the quarks and leptons masses. This particle, the Higgs, is named for him.

There is an aspect of the theory that is contrived. It explains the nine very different masses of the six quarks and three charged leptons by nine different coupling constants—not much of an explanation. Only one of these couplings, the coupling of the top quark to the Higgs, appears reasonable, because the mass of the top is 175 GeV, a value that may be an accident or may have deep significance.

The model that adds one spin-0 Higgs particle to what we already know is the minimum Standard Model. It has been highly successful for many years. LEP I, and now LEP II, have mounted challenge after challenge to the Standard Model, through incredibly precise experiments, and the theory has stood firm.

However, the Minimum Standard Model is not the only way to provide a consistent description of matter. There are at least two other ways: supersymmetry and dynamical symmetry breaking, both very interesting theories. Supersymmetry requires that for every spin-1/2 fermion, there must be a super partner with spin 0. The spin 1 gauge bosons should also have super partners with spin 1/2. Supersymmetry would automatically double the number of elementary particles and gauge bosons. In order for this theory to represent nature, the superparticles would have to be very massive, of order 246 GeV, although the lightest could be as light as 70 GeV. If it does turn out to be that light, experimenters will find it at LEP II in the not-too-distant future.

An alternative theory is that the quarks and leptons are made up of still smaller but more massive fermions—let me call them technifermions. The technifermions have very strong interactions, just as the quarks do. Again, this model would give us many new particles. If this were the correct theory, we would not expect technicolor to produce observable effects until collision energies exceed 1 TeV.

It really doesn't matter to accelerator builders whether nature chooses to break electroweak symmetry by supersymmetry or technicolor, because, either way, to find out one has to build much higher-energy accelerators than we have today.

The Tevatron and LEP II may get glimpses of the Higgs and perhaps even of the lightest supersymmetric particles. The LHC offers the best hope to explain the nature of electroweak symmetry breaking, but even that cannot be the end of the story. The pursuit of ever-higher energies will surely be one of the future directions of elementary particle physics. The course it takes will depend on whether we can continue to contain the cost of the great colliders. The SSC was touted as the most expensive scientific instrument ever to be started. In the end, it was too expensive for an era of deficit reduction.

As we pursue ever higher collision energies, lepton colliders offer different possibilities from hadron colliders. Leptons are fundamental particles with no internal structure, unlike hadrons, with their complex internal structure of quarks and gluons—"colliding garbage pails," in the late Luis Alvarez's well-known metaphor. When hadrons collide, among the banana peels and coffee grounds, every now and then they produce a pearl. The possibility of finding a pearl among the coffee grounds gives hadron colliders the name "discovery machines." Because of the complex hadron structure, it rarely happens that one gets as much mass in new particles from hadron collisions as goes in through the energy of particle acceleration, although occasionally one comes close.

In contrast, lepton collisions are clean; they don't produce the "garbage" of hadron collisions. All of the collision energy is available for new particles, at specified levels of energy. Thus a lepton collider can "sit on" the energy of a postulated "pearl" and go and look for it, rather than sending experimenters poking thought the coffee grounds and banana peels of hadron collisions. Electron accelerators take advantage of this property to create clean, high-energy collisions. However, at high energy, electrons lose energy to bremsstrahlung and synchrotron radiation; and the higher the energy the more of it they lose, effectively limiting the achievable energy of electron colliders. Muons, however, offer an interesting possibility for lepton colliders, because, with a mass 207 times the mass of the electron, accelerated muons do not suffer such large energy losses.

In fact, muons would appear to be the perfect particles for a collider—they give clean lepton collisions and they don't radiate energy—were it not for one flaw: they don't live very long. Muons are only muons for two microseconds before they decay into electrons and neutrinos. However, accelerating them to high energies can extend their lifetimes to 40 milliseconds, long enough to take 1,000 turns around a collider ring, and perhaps long enough to make themselves useful. The trick in building a successful muon collider is to find a way to take advantage of the muon's useful qualities within its short lifetime, and to deal with the products of its decay.

It's quite a trick, but here are the basics for creating muon collisions: Send an intense beam of protons to a target, producing pions. Capture the pions in a magnetic field, where they decay into positive and negative muons. Cool the muons into intense, coherent beams and quickly accelerate them to collision energy. Collide. Repeat as necessary.

Muon colliders are one avenue of exploration for future colliders at the energy frontier beyond the LHC. Another is the VLHC, or Very Large Hadron Collider, which would be a successor to the LHC, at higher energy and—of necessity—lower cost per TeV. It will take a great R&D effort to bring this cost down. To be successful, a new accelerator will need to probe physics at a scale at least on order of magnitude beyond the LHC, for an energy of 100-200 TeV in the center of mass. The VLHC could be built with either high-field or low-field magnets; the higher the field, the smaller the size of the collider.

About 150 physicists interested in a future VLHC held a conference at Fermilab in March. The conference poster showed Fermi's famous "Accelerator In Space" transparency, which Fermi used during his retiring presidential address to the American Physical Society at Columbia University on January 29, 1954. The title of his talk was "What can we learn with high energy accelerators?"

At the time, many more elementary particles were turning up than anyone had suspected. Physicists were barely making out the outlines of elementary particle physics. Fermi asked then: What should we do? and answered that question: Accumulate more data with higher and higher energy accelerators.

Fermi made a plot of energy and cost of accelerators as a function of time. He used that plot to extrapolate 40 years to 1994: he got a \$5 $\backslash 10^{15}$ eV, for a 5 PeV accelerator, at a cost of \$170B (probably in 1954 dollars). Its energy was comparable to the highest-energy cosmic-ray proton known at the time. The design on the workshop poster is a fanciful "preliminary" design of such a gargantuan machine: around the earth with a radius of 8000 km and a magnetic field of 20,000 Gauss—above the orbit occupied by the space shuttle. The accelerator he was talking about was a fixed target one. If one converts the 5 PeV to a collider energy one obtains 3 TeV. So the energy in the center of mass of Fermi's "accelerator in space" is comparable to the Tevatron's energy today.

This is very interesting. Clearly, at the time of the talk, this extrapolation must have seemed completely impossible to reach, both technologically and financially. Yet Fermi's dream of reaching a center of mass energy of the order of the TeV has been achieved, not only for a much smaller price but also long before 1994. His extrapolation in energy by three orders of magnitude to the end of the century is actually amazingly accurate when compared to the SPS, Tevatron, and LHC. The TeV center of mass energy that accelerator physicists have now achieved should be compared to the few GeV the Bevatron would provide later in 1954. Fermi's machine did not take account of technical innovations, and no one in 1954 could have predicted all the milestones that would enable us to achieve that energy. To cite a few: discovery of the antiproton, of superconductivity that can achieve high fields and enormous savings in electric bills, the invention of stochastic cooling, advances in cryogenics and controls.

We can hope that other brilliant discoveries and ideas will allow us to reach the 100 TeV frontier in the lifetime of at least some of us.

Of course, the question of electroweak symmetry breaking is not the only question in particle physics, just as ever-higher energy colliders are not the only answer. There are other important questions, and other ways of answering them.

Color change does not appear to change the mass of the quarks, so irrespective of color, all quarks have the same mass. Up and down quarks have different masses because the weak force appears to distinguish between the weak isospin 1/2 quarks and the weak isospin -1/2 quarks. So we have six different masses for the quarks, and perhaps six different masses for the leptons. Although the theoretical structure is perfectly consistent with zero-mass neutrinos, as I will note later, certain astrophysical observations may not be.

Matter dominates our corner of the universe, the galaxy. Other than the paltry amount of antimatter created when high-energy cosmic rays collide with particles in our atmosphere, there is no evidence for antimatter in our galaxy. On the other hand, the interactions of our friendly quarks and leptons produce matter and antimatter in equal amounts. With one exception, there is no way to tell matter from antimatter. The exception comes in the decay of the long-lived neutral K meson, K^{o}_{L} . The K^{o}_{L} meson is made of nearly equal amounts of an anti-K^{0} and a K^{0}. There is a small difference of two parts in 10^{-3}, and that is all that we know. The decay of K^{o}_{L} is said to violate the symmetry obtained by changing all particles into their antiparticles and changing their intrinsic parity. The electromagnetic and strong interactions are invariant separately under charge conjugation and parity, and the weak interaction. There is little room to incorporate the CP violation into the Standard Model.

The search for an understanding of the difference between matter and antimatter is for the moment focused on searching for a deeper understanding of CP violation in the decays of K^{o}_{L} and searching for CP violation in the decays of neutral and charged B mesons. There is an immense amount of activity in this area. The KEK synchrotron, the Brookhaven AGS, the CERN SPS, and the Fermilab Tevatron all support fixed-target experiments designed to look for further evidence of CP violation in K decays. The holy grail is direct CP violation. What we have found so far is CP violation that occurs as a consequence of the fact that the state vector that is a K^{o}_{L} has slightly unequal amounts of K^{0} and anti-K^{0}. This is CP violation through mixing. By comparing very precise measurements of the relative rates of the K^{o}_{L} into two charged pions and two neutral pions with equally precise measurements of the relative rates of K^{o}_{S} decaying into two charged pions and two neutral pions, it may be possible to detect direct CP violation in K decays.

But nature could be unkind. This comparison of measurements could give a null result even though direct CP violation does exist. At a specific value of the top quark mass close to or equal to 175 GeV, the experiment could give a null result. Alternatively, the search for very rare decays of K mesons has been underway for more than a decade. The branching ratios of the K_L^o into two neutrinos and a neutral pion is only 10^{-11}, and yet it is a primary constituent. At this moment, the only way to observe such small branching ratios is with intense neutral kaon beams, and these can only be produced by proton collisions. In spite of the fact that these decays have gone undetected, two medium-energy proton synchrotrons—one, the Fermilab Main Injector, under construction and one, the KEK Japanese Hadron Collider, just proposed—could be used to produce kaon beams with enough intensity to reach these limits.

Because one expects a similar mixing to occur in neutral B decays, there is a strong expectation to observe CP violation in neutral B decays. There are three e^+e^- B factories nearing the end of construction, all designed to produce the Υ (4S), which decays into a $B\bar{B}$. The PEP II B factory and the KEK B factory use beams of unequal energies. Both should be in operation with complete detectors in 1999. Both B factories expect to reach and then exceed luminosity of $3\text{x}10^{33}$ $\text{cm}^{-2}\text{sec}^{-1}$. The luminosity record for colliders of any sort is held by CESR at Cornell. At these meeting, they have reported that CESR has reached a luminosity of $4\text{x}10^{32}$, well on their way to reaching their luminosity goal of $6\text{x}10^{32}$.

Certainly the exploitation of these factories by increasing the luminosity will be one of the future directions of high-energy physics. But this is a very hard way to make a living. There may be a quicker path to the goal of measuring everything that can be measured about B decays. B's are made of quarks, and quarks, even b quarks, can be more readily pair-produced in hadron collisions. Already, more B's decaying into the preferred decay mode of $B \to J/\psi\, K_S^o$ have been reconstructed by the CDF collaboration than anywhere else. At the Tevatron, the cross section for all $B\bar{B}$ is $3\text{x}10^4$ times larger than the cross section for e^+e^- production of $B\bar{B}$ pairs. The CDF detector is not particularly efficient at detecting B decays. When CDF and DZero begin running again in the year 2000, they will certainly contribute to the understanding of B decays, particularly B_s decays, which are not easily detected at e^+e^- B factories. When LHC-B begins operation sometime after the year 2005, it will bring an even more powerful detector to bear on the search for CP violation in the B system. Given all of that activity, CP violation will surely define one of the future directions of our field.

However, it is unlikely that K decays and B decays will reveal the whole story. In fact, they won't. There must be another set of interactions that transform quarks into leptons. These interactions will be characterized by a mass scale somewhere between the electroweak scale and the Planck scale. These interactions should lead to proton decay, and they must distinguish between matter and antimatter. The current view in particle physics is that they favor the production of quarks over antiquarks, and leptons over antileptons. The reaction rates must ultimately lead to the microwave background photon-to-baryon ratio of 10^9.

As in the case of electroweak symmetry breaking, we may learn a lot about CP violation from low-energy experiments, but a full explanation will require higher energy colliders, probably with energies that are beyond our dreams.

Let me now discuss a third future direction. As in the case of matter-antimatter asymmetry, there are also astrophysical results involving neutrinos that cannot be explained by the minimum Standard Model. The flux of electron neutrinos from the sun is a factor of 2 to 2.5 smaller than predicted by a well-developed model of the sun. Four experiments have measured the solar electron neutrino flux, and all give a consistently low result.

A second astrophysical neutrino experiment has also yielded unexpected and unexplained results. Neutrinos are produced in the upper atmosphere when cosmic rays slam into it and produce pions and kaons, some of which decay before they have a chance to interact again. The pion decays produce a muon neutrino and a muon. The kaon decays yield a muon and a muon neutrino most of the time, and some of the time and electron neutrino and an electron. Typically, the muons decay before they reach the ground to yield a muon neutrino, electron antineutrino, and an electron. If one builds a massive neutrino detector deep underground, where the only particles that can penetrate the earth and reach the detector are neutrinos, one expects to detect roughly twice as many muon neutrinos as electron neutrinos. But, in fact, the fraction of electron neutrinos is larger than expected. Five years ago, when experimenters first announced this puzzling result, the statistical accuracy and systematic errors were too large to allow a definitive statement. Five years make a very big difference, however, and now the errors are smaller and understood. Within the minimum Standard Model there is no explanation for this result. The precision of astrophysical measurements can and will be improved.

There is a plausible explanation for both the solar and atmospheric neutrino observations. If neutrinos have a small mass, less than a few electron volts, and if the mass eigenstates are just a little different from production eigenstates, then mixing can occur; and one kind of neutrino can change into another. For example, if an electron neutrino emerged from the sun and changed into a muon neutrino for part of the time as it traveled 150 million kilometers to the earth, this could account for the solar neutrino deficit. If the muon neutrinos produced as a consequence of collisions of cosmic rays with particles in the upper atmosphere changed into tau neutrinos, one could explain the relative paucity of atmospheric muon neutrinos. These proposals are plausible, but unproved. The only way to truly understand what is going on will be to build very intense neutrino beams with well-defined composition and momentum spectra. This will require intense proton beams. Several laboratories with proton beams are currently considering proposals for long-baseline neutrino experiments

Although the Standard Model tells us that the fourth force, the strong force, transmitted by the gauge bosons known as gluons, gives rise to most of the mass of the matter that we can see, the stuff that makes up most of the universe remains a complete mystery. Neither particle physicists nor astrophysicists know the nature of 80 to 90 percent of the matter in the universe. For decades, since Zwicky's observations of galactic rotation curves in the 1930s, physicists have recognized that there must be more matter in the universe than meets the eye. During the past 60 years, still stronger evidence for unseen mass has come from observing the motions of clusters of galaxies and from examining the large-scale structure of the universe.

What is this unseen mass, this dark matter? Conjectures range from ordinary matter that takes the form of huge Jupiter-like objects that give off too little light to be observed, to small black holes, to fundamental particles such as neutrinos, or more speculative wisps of the fundamental fabric. Among the candidates is the class of weakly interacting massive particles, or WIMPSs, also known as cold dark matter. Such particles would have been created in the energetic early universe and have "frozen out" or stopped annihilating one another as the universe cooled and their energy decreased. According to this scheme, a large number of WIMPS could be left in the present universe. If so, the weak force provides the only means of detecting them. Experimenters have designed underground crystal detectors that they hope will be sensitive enough to detect the few WIMP scattering events expected to occur as the earth and sun move in their galactic orbit though the hypothesized sea of cold dark matter. Experiments such as these, together with neutrino experiments and other future high-energy physics experiments, and advances in astrophysics will all help to answer the last of our four questions, What is the universe made of?

The future of high-energy physics lies in the questions that are being posed in particle physics and astrophysics. For the last 50 years, particle physics and high-energy physics have been almost synonymous. We have made progress in understanding the structure of matter and energy by building accelerators with ever higher energy. Our future progress along this path toward answering the great questions that confront us will depend on our ability to collaborate with our colleagues in the global community that constitutes high energy physics.