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THE FUTURE OF HIGH-ENERGY PHYSICS*

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INTRODUCTION

Mel Month is responsible for the modest title of this talk. Lately I have spent time worrying about machines and experimental physics, and I have not had much time to keep up with theory. But I have tried to fulfill the charge. Given the impossibility of doing that, what you get here is a bland, broad-brushed overview. Sorry about that.

THE STANDARD MODEL AND OUR FUTURE

By now, almost everyone believes in the correctness of the standard model, at least in its broad outlines. The weak and electromagnetic forces are described by the SU(2) X U(1) electroweak theory and QCD underlies the strong forces, and we essentially know the Lagrangian associated with those interactions. Nevertheless, the standard model has basic shortfalls and fundamental problems which we don't know how to handle. There are the three generations of building blocks and the many parameters of the model. The basic mechanism for the origin of W and Z masses of spontaneous symmetry breakdown is most likely right. But what in detail is going on in the Higgs sector really isn't understood — or at least trusted. Maybe the minimal Lagrangian picture of the Higgs sector with its single Higgs particle is correct, but it looks a little artificial and clumsy. The origin of the fermion masses and mixings is believed coupled to that problem. The strong CP problem is awkward and along with it the origin of CP violation. These are believed to be all tied together but we cannot really prove that yet. These are the big problems which provide the background for the whole field, problems that all red-blooded theorists grapple with so unsuccessfully. We all believe that sooner or later the shortfalls of the standard model will yield. But it's still a judgment call on what mass scale is needed in order to crack the problem and begin to resolve these shortfalls. We don't have many clues. The most solid clue is the upper limit on the mass on the Higgs boson of around a TeV that comes from unitarity considerations. The natural scale of the electroweak theory is really not the mass of the W and the Z, but rather the vacuum expectation value of the Higgs field (or the decay constant of the Goldstone modes) which is somewhat higher, or order 250 GeV.

There may even be not much to discover within the TeV mass range except the single Higgs boson, especially if it turns out to have low mass. This is a very minimalist view, which suffers from "hierarchy," "fine tuning," and other technical problems of the theorists. But, albeit unlikely, it is at least thinkable that there is nothing beyond a single Higgs boson until the fantastic GUT scales of 10^{15} GeV. The evidence for that hypothesis really depends, first of all, upon making sure that the parameters at low energies are consistent with simple SU(5) grand unification, and whether or not the decay of the proton occurs on schedule. As of today it seems to be behind schedule.

THE STANDARD MODEL AND OUR PRESENT

The standard model has been so successful that theorists have become rather arrogant and experimentalists have become rather intimidated about possibly getting an answer which is in disagreement with it. The standard model needs more testing. This can be done at all energy scales. And aside from the fundamental tests of the standard model, there are a lot of details and loose ends around. It is the kind of work that usually doesn't get on the front pages of newspapers but nevertheless forms the backbone of our subject. I have tried to make a short listing of loose ends, jobs to be done in the near future.

Everybody believes there's a tau neutrino but it would be nice to find one. Beam dump experiments hopefully can do that. The lifetime of the tau ought to be measured well enough to see whether it behaves like an ordinary heavy lepton should behave.

Spectroscopists might like to see the magnetic moment of the omega minus. Even the QCD lattice people might have a good time calculating it and it may be easier to do than the proton moment.

There is a nagging problem of same-sign dimuons produced in neutrino interactions. The yield is large compared to what theorists can estimate from charm production and known mechanisms. I don't believe that phenomenon is understood. There's still some confusion, I think, in the beam dump experiments looking for prompt neutrinos ν_e and ν_μ from charm production in beam dumps. Some get a result of unity for ν_e/ν_μ and others don't. It has to be done again so that it is absolutely clear what's going on.

All this is related to the problem of hadronic charm production, also a very confusing subject. A lot of leading charm is found at the ISR, and possibly in pion beams at the SPS, both with rather large cross sections, whereas other experiments at Fermilab and SPS energies seem to be more consistent with central charm production, with leading charm being relatively small. The overall level of charm production doesn't seem to jibe very well with the simplest QCD calculations and again is not well understood.

There is a published marginal measurement of the various asymmetry parameters in the beta decay of the Σ^-, where the polarization asymmetry comes out with the wrong sign compared to Cabibbo theory. Were that experiment to hold up, there would be mass suicide among theorists; they will categorically deny the correctness of that experiment. The result needs obviously a follow-up experiment.

The phenomenology of semi-hard hadron collisions ($5<p_T<10$ GeV) is something of an embarrassment for theorists. It isn't very clear how to handle the median p_T range before the cleanliness of the hard QCD collisions that we see at the SPS collider emerges. There should be better understanding of that whole question.

Even in the relatively clean deep-inelastic phenomena of lepton -nucleon scattering, much is not understood. For example, can QCD describe the Regge behavior of the structure functions, especially the nonsinglet parts, where the Regge trajectory carries nontrivial quantum numbers? As one goes to the small values of x, theorists working within the modern QCD context are notably silent on predicting what goes on, although in the old days everybody knew how to talk about it.

Another major area is non-perturbative strong-interaction phenomena. It's not so many years ago that statements were made that one of the most important discoveries in 20th century physics was the measurement of the rise in the total pp cross section with energy. It _is_ important and I don't think we yet understand why it rises. Does QCD predict the rise in the total cross section? I don't think that has been shown. In particular the whole relationship of Pomeron phenomena to QCD is something which very few people even work on. But I would guess in the future more people will. As easy problems in QCD like the mass of a proton get solved, people will go on to more difficult ones like nonsinglet Regge trajectores. Finally the ultimate challenge will be the Pomeron. It's a tough problem and sooner or later it will get attacked with more energy than now.

There is another whole class of phenomena associated with high multiplicity or high transverse energy which seems to get more important as energy goes up. KNO scaling is one manifestation of it. There are events at the SPS collider where enormous numbers of particles (say 50 GeV of energy) emerge isotropically into a central calorimeter. I don't think these events are well understood. They may or may not be related to the question of whether or not high energy ion-ion and/or hadron-hadron collisions can make quark-gluon plasma. There may be ephemeral plasma production even in a $p\bar{p}$ collision.

Of course production of quark-gluon plasma is itself an interesting field, no matter what the projectiles. Some people think it's a dirty business to slap a couple of ions together and watch thousands or tens of thousands of particles be produced. What can you learn from that? The skeptics may be right. It may be very hard to learn anything from it. On the other hand, it may in fact impact in very fundamental ways on QCD. For example, people work very hard to measure Λ, the scale parameter of QCD. They feel quite happy if they measure it to 50%. Now suppose you can convince yourself that there's a first order phase transition between ordinary hadronic matter and quark-gluon plasma. Then the transition temperature is proportional to the QCD scale parameter, with perhaps small corrections due to presence of the fermions and finite quark masses. This transition temperature is one of the easier things for nonperturbative QCD theory to try to calculate, and the lattice calculators already give us the constant of proportionality.

If there's any truth to this idea of quark-gluon plasma and a phase transition, then the transition temperature between normal hadronic matter to the plasma is somewhere between 100 and 300 MeV, because at 100 Mev one clearly has dilute pion gas and at 300 MeV there is such a high energy density that it's hopeless to imagine that there's still hadrons swimming around instead of quarks and gluons. Also, common sense says that this a good scale just from estimating the characteristic momenta of the constituents, whether they be hadrons or quarks and gluons. Therefore, just by waving of the hands, I can tell you that the transition temperature is 200 $\pm$ 100 MeV. Given that lattice theorists can tell me what the proportionately coefficient is that connects it to Λ, I can tell you what Λ is to 50%. Now if I can do this here without any calculation at all, then with a lot of hard work — including some measurements that convince us that this idea of plasma isn't just a figment of our imagination — it may be that measurement of the transition temperature could become the most accurate measurement of Λ that we can get.

Of course there are other features of heavy ion collisions which may be even more fun than quark-gluon plasma. Some people think that fractional charge is more easily liberated if you put it in a lot of boiling colored soup. There may be other exotic objects, such as high density metastable hadronic matter. Those are very speculative ideas. It is very hard to know how to weigh their importance. But I think they're not completely out of the question and therefore one should factor those in when thinking about how relativistic heavy ion physics might be.

Another interesting area concerns phenomena having to do with large longitudinal distances, such as A-dependence of high energy collision processes. This tells us about the nature of the evolution of processes in space-time, i.e. how the initially formed hadronic matter evolves. Its interaction with nuclear matter during the evolution tells something about the early stages of collision processes.

Also there is still a lot of spectroscopy to do. Heavy quark spectroscopy is relatively clean from the viewpoint of QCD. But it's good to have the light quark spectroscopy there to compare it with and give some idea of continuity or discontinuity between light quark properties and those of heavy quarks. And of course glueballs need a lot of attention.

There are many of the fundamental parameters of the standard model that need to be known from low energy experiments. It is very important to get an accurate measurement of the weak mixing angle at energy scales very small compared to the electroweak mass scale of 100 GeV. One wants to compare the angle at low energies with what one gets at the electroweak scale via direct measurements of properties of the W and the Z. The comparison of the two classes of measurements may provide information on the radiative corrections involving higher orders of the weak interactions. These may be sensitive to what is going on at a mass scale much higher than even the natural electroweak mass scale, just as accurate radiative-correction work in QED leads to information about very short distances. This kind of precise, careful work may in fact have a lot of leverage.

The mixing parameters of the quarks, i.e. the generalized Cabibbo angles which in the six-quark world become the elements of the K-M matrix, are obviously very fundamental parameters. They may be hard to measure accurately, and will probably take a lot of work. For that, one needs copious charm and bottom production, perhaps not only at the e^+e^- machines, but also in the fixed target machines. Tevatron II, where hopefully one can learn how to handle hadronic production of charm and bottom in the presence of a very large background, is especially attractive.

CP violation parameters are vital: is the phenomenon milliweak or is it superweak? There is healthy activity now on the neutral kaon system. It is very deserving and has to go on.

Are there neutrino masses and mixings? Are the Russians correct in their tritium beta decay endpoint experiments? Of course, it is a very important topic right here at Brookhaven.

Proton decay will tell us whether or not "naive" SU(5) is alive or dead. Little more has to be said on the significance of that kind of experiment, except to say that the big proton-decay detectors may learn something about high energy cosmic rays too.

There are many ways in which data may show phenomena which deviates from the standard model. This can happen even with the lowest energy experiments. Rare decays such as $K \to \mu e$, $K \to eee$, etc. or rare charm and bottom decays may occur. The history of the kaon is germane; the more intense the kaon beams were made, the more we learned, and that continues to this day. I believe it could be the same with charm and bottom decays. We need the most intense pure charm and bottom beams we can get, whether they come from e^+e^- machines or hadron machines. Maybe we will be surprised by mixings of the neutral D's and neutral B's as we were with the neutral kaons.

There may be axions. There may be extra neutrinos or other neutral fermions which can be produced in beam dump experiments. There may be low mass supersymmetric particles, e.g. photinos, which likewise could be produced in beam dump experiments or otherwise.

The question of right handed currents comes up in everything from low energy experiments to ep colliders. There may be a right handed fermi constant which is smaller than a left handed one by the square of the mass ratios of right handed to left handed gauge bosons. This would give electroweak and milliweak interactions involving right-handed currents; again there's a very broad scope for searches for such things.

So, to summarize, it's clear there's much to do at existing energies, and it must be done. It is important, and new directions of study are there. Ion-ion and maybe even electron-ion collisions should be thought about as well. I think it is a very exciting direction in which to explore and would hope that it will become a very serious Brookhaven future option during the coming months and year. For charm and bottom physics, what is needed is as high luminosity an e^+e^- machine at the psi and upsilon regions as possible. Neutrino experiments need to be done at all energies, for example for the long base-line physics of neutrino oscillations as well as for short base-line dump experiments. I think this part of our future we always have to remember to emphasize. In many ways it's the life blood of the field, even though the bulk of the results don't end up on the front pages of newspapers.

THE INTERMEDIATE ENERGIES: 100 GeV

The intermediate-energy range of center of mass energies, of the order of 100 GeV for the processes of interest, will be the natural habitat of TeV 1, SLC, LEP, HERA, as well as the $S\bar{P}PS$. This is familiar territory, covered well already in the lectures given at this school. Such broad attention is focussed on it these days that I will not belabor it much here. The next step after W and Z seems to be to find the top quark. Maybe that one will come soon but a fourth generation may have to wait awhile — if there is one. If a fourth generation is "typical" in the sense of having a light neutrino and its lepton, along with a pair of quarks which aren't degenerate in mass, then it really belongs in the intermediate range category. There is a fairly convincing theoretical argument for a ceiling on the masses that those fermions could have. They contribute vacuum-polarization radiative corrections to the mass of the Z and W, so that above a mass scale of ~300 GeV these fermions cannot exist unless they become degenerate in mass or decouple from the electroweak interaction.

HIGH ENERGIES

High energy is the name of the game. We can't just stop in the intermediate range and it is undoubtedly essential to go to higher energy to really get at the fundamental problems of the standard model. A lot can be done without the high energies, but I doubt that it is sufficient. There is an interesting analogy from history. Compare electroweak interactions now with strong interactions in the early sixties. We understood a lot about the symmetries. Pions were exchanged to give the basic long range force. Looking retrospectively, all the machinery which we needed to understand QCD and the nature of the strong interaction, such as the gauge principles, the ideas of chiral symmetry and chiral symmetry breakdown, spontaneous symmetry breakdown, were in place quite early on in the sixties. What wasn't there, at least until late in the sixties, was the quarks. What people did with the gauge principles was to try to gauge the flavor symmetry. So people went off on the wrong track because essentially we had to get down to the constituent picture. We had to understand the need for color and the inevitability of the quarks before the next step could be made.

Now comes the question: could we have found QCD from observations only at low energy? For that matter would, say, the AGS, SLAC, the Bevatron, and SPEAR have been a sufficient number of facilities to have given us <u>all</u> of the standard model? We got a lot of the standard model from them, but I really doubt that there was enough to put it all together in a convincing way. The high energy experiments were extremely supportive and essential in helping guide the way. It would have been much more confusing without them.

And what about the converse: would <u>only</u> the highest energy machines been sufficient? The same is true: with only the high energy experiments available we would have had a very hard time. The low energy ones were vital. I think in particular of the baryon spectroscopy that was done in the sixties. A series of individually uneventful and tedious experiments came together to provide a very powerful piece of evidence in favor of quarks.

When going to high energies, again and again one comes back to the question of the Higgs sector. I think the existence of a Higgs boson of something akin to it is almost inevitable. No matter how elaborately one imagines the final explanation of the Higgs phenomenon to be, there are usually scalar particles of one sort or another within the family of objects envisaged as necessary to give the real world.

The argument for the limit of 1 TeV mass scale for this Higgs phenomenon is as follows. Look at W W scattering. Consider the amplitudes one gets from gauge couplings alone. Add them all up and look at the amplitude in the $J = 1$ angular momentum channel. At very high energies the amplitude grows in strength to a point, about a TeV in the center of mass, where unitarity is violated. If one takes the full electroweak theory with its Higgs particle and adds the exchange from the Higgs in the various channels, everything is smoothed out and unitarity is not violated. This is true provided the mass of the Higgs is not too high. Otherwise it doesn't have enough clout and doesn't avert the catastrophe that would otherwise be there. Upon going to the supercollider energy scale of, say, 20 TeV on 20 TeV (at a decent luminosity), the cross-sections of quark-antiquark annihilation to W pairs in the $J = 1$ channel are big enough to be observed out to the TeV mass scale. If there are large W W scattering phase shifts or crazy things going on, one may be able to see them.

When one searches for the standard Higgs, which will clearly be a central problem in the years to come, there are various mass scales which have to be considered. For a Higgs mass of less than 40 GeV we may hope to count on SLC or LEP I to find it through the Z decay into the Higgs and lepton pair. It is a good signature, with decent branching ratio if the mass is no larger than that. If the mass is somewhat larger but under 100 GeV the process is slightly different. One first has e^+e^- annihilation into a real Higgs and a real Z. Again the Z can decay into dilepton or maybe a dijet and there is a good signature. Then there is an embarrassing mass region between 100 GeV and W pair threshold where everybody has a hard time. For a Higgs mass above W pair threshold, there is good hope that a sufficiently high energy hadron collider with sufficiently high luminosity will make the Higgs in an observable way. For this, the hadron beams should not be regarded even as quark beams, but as gluon beams. [The gluon is already a commonplace, and we will in the future be into gluon-gluon collision processes in a big way.] The process is resonant annihilation of a gluon pair to a Higgs particle, which then decays with large branching ratio into a pair of intermediate bosons. This is a fairly good signature. The basic Feynman diagram goes through a virtual process involving the famous triangle diagram with a top quark loop (or, if there's a heavier quark, the heavier one). The calculation is done in the Snowmass summer-study proceedings, and the rates look good. But it is not clear how high a mass one can reach even with a very high energy collider. As the Higgs mass approaches one TeV, the width of the Higgs boson grows rapidly. (It must, because unitarity is about to be violated. Somewhere around a TeV the width of the Higgs is comparable to its mass.) If it is so broad, then the resonant peak in the mass distribution of W pairs starts sinking into the background and there becomes a detection problem. Exactly where that occurs is not yet very clear.

So much for the standard model Higgs boson. There may be indirect manifestations of the Higgs phenomenon such as technicolor. In the technicolor scenario, the Higgs boson is a complicated object, a bound state of fermion pairs, not an "elementary" particle. The analogy is the pion and 0^+ sigma meson, which are bound states of quarks. There would be many family members of the Higgs and new strong interactions. Again, arguably this whole family should be below the TeV mass scale.

Another possibility is supersymmetry. If supersymmetry is connected with the underlying problems of the standard model, there could be many partners of all the known particles. But the masses are unknown. There are many schemes for what the spectrum should look like, and some of the particles are arguably below the 1 TeV mass scale.

But I think it's fair to say that these ideas and others that I haven't talked about are in trouble. You only hear about generalized discussions of these notions because specific models don't work. Were there good specific models, then you would have certainly heard much more. So these ideas are beautiful "in principle" ideas. Maybe they're even right and the right combination just hasn't yet been found. Maybe supersymmetry is right in the same sense that the gauge principle in 1960 was right, but that it's not being applied in the right way. Maybe new ideas are needed, preons or constituents or something else to get us out of the present impasse. Everyone will agree that, if one could get experiments on the TeV mass scale, it would help a great deal.

NONACCELERATOR PHYSICS

Before finishing, I should mention the non-accelerator experiments. These are sensitive to all energies. I've always believed that with the big new instruments underground built to look for proton decay that in fact, when the dust settles, they may well come up with cosmic ray phenomena comparable in excitement with their original goal. This hasn't happened yet, but I keep waiting for it. Also, the Fly's Eye in Utah which looks at very high energy air showers is also a nice direction to go. The Utah cosmic-ray group was forced upwards, both in energy and altitude, by the rapid increase in collider energy scales. They are now looking at scintillation light from air showers at 10^{17-19} electron volts as they propagate through the atmosphere. Recently they have become interested in the possibility of detecting upward going showers caused by neutrinos interacting in the surface of the earth. The neutrino comes through the earth, interacts near the surface, and sends a shower up through the atmosphere. There are even rumors of candidate events. But, irrespective of candidates, interesting calculations have been done on rates. It one take the standard model and standard neutrino cross sections, it turns out that the mean free path of a neutrino at these energies is reasonably well matched to the diameter of the earth. Furthermore for an electron neutrino the effective thickness of the surface of the earth which can be used as a target is much larger than one would naively think at low energies. This occurs because of what is called the Landau-Pomeranchuk-Migdal effect, which is an increase of the effective radiion length with increasing energy scale. There are a couple of orders of magnitude of gain from this mechanism, with the net result that detection of such neutrino events at these colossal energies may be within reach. I think that's a most happy development and I hope it bears fruit.

Carlo Rubbia taught me that searches for other stable heavy relic particles is the ultimate in high energy physics. If one ever found a monopole and could bottle it, and then found an antimonopole and bottled it, and then brought them together — and their mass were at the grand unified scale or anything like it — you'd really have high energy physics. A single event is a radiation hazard. If a monopole-antimonopole pair annihilates, say, into an X or Y boson of the grand unified theory (plus other particles), with mass of 10^{14} GeV, and the X or Y decays into an electron, and that electron is headed your way, watch out. It's a rather deadly particle and — again because of the Landau-Pomeranchuk-Migdal effect — it penetrates.

ACCELERATORS

Finally, a few words on the future of accelerators. First I simply want to congratulate Mel Month on organizing these schools as an excellent way of stimulating what is really needed. If the SSC takes off and we have 40 TeV in the center of mass in the foreseeable future, where do we go from there? That machine is hard to beat with conventional technology. To go beyond it we need high gradient linacs (or maybe monopoles). The urgency of doing something radically new becomes greater if we do increase the slope of the Livingston curve and get to high energy faster than we thought before. My own prejudice is that the specifications for e^+e^- colliding linacs should be center of mass energies of at least 2 TeV, to compete with the physics done in the SSC. For proton linacs my specifications are enormous: 10 GeV per meter gradients, i.e., one electron volt per Angstrom. That is enough to destroy the accelerator every time you pulse it. But why not? Bob Palmer has already proposed that with his laser grating accelerator. I don't think we should be _ab initio_ afraid of destroying an accelerator every time it's pulsed. But if you do that, you are making plasma out of it and so you have to know about plasma physics. A laser will probably be used to do the destruction, and so you have to know about laser physics. So it is likely that a much closer liason between our community and plasma and laser physicists will be needed.

SUMMARY

In summary, I have emphasized the importance of low and medium energies. Diversity exists at present in the program. It is very much needed and must be protected, even as we push aggressively to the highest possible energy. But the name of the game is energy and the push to higher energies must continue. The mandate which this year's Woods Hole Subpanel has given us is a very exciting one, one very much worth uniting behind. We must do our very best to make our hopes for such a high energy machine a reality.