Fermi National Accelerator Laboratory

FERMILAB-Conf-85/133
2000.000

"WILL THE NEXT FIFTEEN YEARS OF HIGH ENERGY PHYSICS MATCH THE LAST FIFTEEN?"*

J. D. Bjorken

September 1985

*Summary talk presented at the Tenth Hawaii Topical Conference in Particle Physics, Honolulu, Hawaii, August 26-31, 1985.

Operated by Universities Research Association Inc. under contract with the United States Department of Energy

"WILL THE NEXT FIFTEEN YEARS OF HIGH ENERGY PHYSICS

MATCH THE LAST FIFTEEN?"*

J. D. Bjorken

Fermi National Accelerator Laboratory

Batavia, Illinois 60510

*Summary talk presented at the Tenth Hawaii Topical Conference in Particle Physics, Honolulu, Hawaii, August 26-31, 1985.

Abstract

I doubt it. But I've been wrong before.

1. THE LAST FIFTEEN YEARS

The splendid series of Hawaii Conferences in High Energy Physics, which we celebrate here, provide ample evidence of the spectacular progress that high energy physics has made in the last 15 years. In the beginning of this period the notion of quark substructure of hadrons was, for optimists, very confusing albeit hopeful and, for pessimists, the idea was self-contradictory and absurd.

The symmetries and general dynamical properties of the strong force were classified. But at a more fundamental level the strong interaction was not understood at all. The weak force was described by a well-organized, elegant phenomenology good at low energies, but clearly inadequate at high energies.

To me, the most profound advance made since then has not been so much the unpeeling of another layer of the onion down to the quark-gluon level, but rather the apparent understanding of the strong and weak force at a level as basic as that of quantum electrodynamics. It is arguable that this level even goes beyond that of general relativity, because one has not yet a theory of quantum gravity - at least none that has been put to the test. In any event, to have added two more inverse-square force laws to those of electromagnetism and gravity within one decade is both remarkable and profound.

These advances did not come easily. They were the product of a long struggle, both theoretical and experimental. On the theoretical side, the battle was fought both at the tactical level and the strategic level. The central strategic advances are the ones by now oft-told: the understanding of spontaneous symmetry breaking and of (renormalizable) non-abelian gauge theories. Understanding these concepts were vital for understanding both strong and weak forces.

But the action at the tactical level was equally important. To me the emergence of dispersion relations and current algebra were vital: they played a role in understanding hadrons similar to what Kramers' dispersion theory and Heisenberg's matrix-mechanics provided in understanding atoms. Much of that formalism got streamlined by use of the Schrödinger equation, just as the dispersion theory and current algebra language has been supplanted by more explicit field-theory descriptions. The use of group theory, with emergence of SU(3) and SU(6), became a central classifying tool.

And the notions of nuclear democracy and duality helped to lead one away from thinking that the nucleon was a privileged hadron relative to, say, the Δ or other N^*'s.

On the experimental side, it was the gradual accretion of a very large data base which was important tactically. For the strong interactions, hadron spectroscopy and its Argand diagrams yielding masses, widths, spins and parities, largely did the job. For the weak interactions it was the large data-base of decay branching ratios and angular distributions that led to the Cabibbo codification. At a grander, more conspicious level, there were the landmark experiments such as discovery of neutral currents, the deep-inelastic scattering program, and the discovery of the new particles such as the J/ψ, τ lepton, charm, Υ, bottom, gluon, W and Z.

During this successful battle to understand strong and weak interactions there was, as in most battles, much confusion. And some of this confusion was profound and still with us, especially the discovery of CP violation, and the discovery and classification of the second and third generations of quarks and leptons. It is this, along with the persistent and stubborn questions of the origins of masses and mixings of quarks, leptons, and intermediate bosons, which remain to be understood in the future.

But in discussing these issues, we arrive at the interface between past and future, and are invited to look forward, rather than continue to reflect on the past.

2. THE TASK FOR THE NEXT FIFTEEN YEARS

The order of business for the future is just as clear as the standard model itself. First of all, for standard reasons underlying any basic science, the standard model itself must be tested as severely as possible. And secondly we must discover and explore the Higgs sector.

Tests of the standard model have their analogue in history. From 1950- 1970, the standard model was QED. Many tests were devised to probe QED at shorter and shorter distances to see whether it "broke down." Well, it finally has broken down. But few would have anticipated that its successor was so satisfying.

So the electroweak and QCD tests must (and will) proceed, despite the low odds per experiment (if done well !!) that something unexpected will be seen. With respect to these standard model tests, I would venture here an opinion: Having now had experience with how QED breaks down, most people anticipate that if the SU(3) x SU(2) x U(1) gauge theory were to break down, it would most likely do so in a way similar to QED, being synthesized into a larger non-abelian gauge group. An alternative is a phenomenological-Lagrangian approximation (something like treating ρ and A^1 as strong-interaction gauge bosons) which, unlike that case, would happen to have a large dynamic range of applicability (several orders of magnitude). A large dynamical range of applicability implies, by necessity, ("Veltman's theorem"?) that the couplings dynamically adjust themselves to essentially the gauge theory values. For no particularly good reason, this viewpoint seems to me less probable, and the synthesis of existing forces within larger gauge groups the most sensible.

The problem of the Higgs sector has its analogue in the status of strong interactions in the post-World War II period. There it was the "Yukawa sector" which was in some general sense inevitable. But even setting aside the confusion created by the muon, it would have been hard to anticipate the richness of even the nonstrange meson spectrum eventually found.

I think that the Higgs sector may well be similar, and that the austere, isolated, yes sterile single elementary Higgs boson of the standard Lagrangian theory is unlikely to exist with the properties ascribed to it. There must, I believe, be <u>something</u> like that Higgs particle, but I expect a system to exist which is perhaps as rich as the quark or lepton systems, and which is as full of surprises as those systems have been.

That there should be such a system at not too high an energy scale, no higher that the "TeV mass scale" touted for the SSC, can be seen especially clearly from the perspective of the "big bang." If the gauge theory description survives at distances short compared to those of the TeV mass scale, then the big-bang plasma in that very early epoch will not be sensitive to mass at all. W's, Z's, and γ's happily behave as massless gauge quanta, as do all the known fermion species. But the Higgs mechanism is, by definition, a kind of Meissner effect for the gauge-quanta, and the short range of the electroweak force emerges at low temperatures, i.e. late big-bang times, in the same way as the electromagnetic fields get screened beyond the London penetration depth. For this to happen there must occur at least one phase transition very analogous to the transition from normal metal to superconductor. The critical temperature is probably ~ 400 GeV. And the emergent superconducting or superfluid "condensate" has degrees of freedom not listed in the standard model periodic table. This defines in some sense the Higgs sector.

Assuming the standard model gauge couplings are well understood, another useful way of viewing the Higgs sector is "full exposure." Simply turn off α, α_W, and

α_s; set them to zero and see what remains. In the orthodox Lagrangian standard model, the Higgs self-couplings and Yukawa couplings do not vanish in this limit. While not obviously true, suppose this is the case and ask what the world is like. There will be a massless trio of Nambu- Goldstone bosons $N^{\pm}$, N°, which occur because of the spontaneous symmetry breaking, in addition to at least one massive Higgs boson. The N's couple to each other and to the Higgs by forces of obscure origin. The couplings of these particles to quarks and leptons are in proportion to their mass. And the N's <u>destabilize</u> matter. For example, the electron becomes unstable

$$e^- \rightarrow N^- + \nu_e$$

with a lifetime of a few nanoseconds. The other leptons decay more rapidly (as mass ratio cubed). Quarks also decay rapidly

$$\begin{aligned} t &\rightarrow b + N^+ \\ c &\rightarrow s + N^+ \\ d &\rightarrow u + N^- \end{aligned}$$

although the d-decay takes a little longer (10^{-12}sec). The K-M matrix survives in the coupling of charged N's to quarks; hence we have, with somewhat suppressed rates,

$$b \rightarrow c + N^-$$
$$c \rightarrow d + N^+$$
$$s \rightarrow u + N^+$$

This leaves the relics of the big bang to be the N's, u quarks, ν_e, ν_μ, and ν_τ (if it has mass less than m_e and/or there is no mixing matrix). There must be an excess of N^- in the big bang relic plasma to balance the net positive charge of the u quarks. Since the N's interact with each other, the properties of the N-plasma at low energy may be delicate, and something interesting might conceivably happen to the u quarks and neutrinos as well. (It could be fun to work this through).

The low energy interactions of N's with each other and with quarks and leptons are <u>determined</u> within the standard model just from the lore of spontaneous symmetry breaking. The only parameters needed are the vacuum expectation value F_N of the Higgs field ($F_N \sim 240$ GeV) and the fermion masses. Therefore <u>we know, within the standard model, there must exist an additional force not described</u> (at the present level of understanding) <u>by gauge principles.</u>

This low-energy picture has its strong-interaction analogy in the nonlinear chiral effective lagrangian which summarizes all the soft-pion theorems of current algebra. And it is just as incomplete. We do not know what will happen as the energy scale goes up any better than we knew about strong interactions at short distances in the 1950's. The N's epitomize a new force and set of quanta which remain to be understood, with a starting point which may be just as remote from final understanding as Yukawa's was from QCD.

This way of looking at the Higgs sector may be wrong. Maybe, as we turn off the gauge couplings, the Higgs couplings vanish too, leaving behind well organized, highly symmetric (but big) multiplets- as happens in supersymmetry, extended technicolor, etc. But perhaps the most likely situation is that everything we talk about for the Higgs sector at high energy may be wrong, and new ideas as imaginative as fractionally charged quarks, color, and confinement via non-abelian gauge theories will be needed. And I mean new, not a rewarmed version of these ideas applied to the Higgs sector.

Thus, there seems to be little room for questioning the existence of the Higgs sector within the framework of electroweak gauge theory. And although it is possible to go outside that framework (even today - although I no longer try hard to do so), the alternatives usually lead to composite W's and Z's; hence to some other new degrees of freedom we might as well call, for sake of economy of labels, "Higgs" as well.

How do we explore this Higgs sector? The best is to reach the natural energy scale. Only so much could be done with meson theory via low-energy nucleon-nucleon phase-shifts, etc. Attaining the natural 1 GeV energy scale was necessary and, for QCD, not even then sufficient. But careful and accurate study of phenomena below the natural threshold can help a great deal. This was especially true in the case of weak interactions and the V-A Cabibbo description. For the Higgs sector, the analogous phenomenlogy is that of CP violation, K-M parameters, fermion mass values, and searches for rare flavor changing decays and/or mixings all the way from ν and μ to K, to charm and bottom, and to nucleons. Searches for axion-like, light spinless mesons is another area of opportunity which often has rather exquisite sensitivity. But alas, while there are many limits, one wishes there were some positive indicators.

To summarize this discussion of Higgs, I urge that, from the practical side

a) Higgs searchers should <u>not</u> rely on standard parameterizations but simply search in all ways possible.

b) The variants of the standard models such as the left-right symmetric version should be taken quite seriously.

c) Continue to push hard on neutrino masses and mixings.

d) Don't trust standard model estimates for CP and mixing effects in charm and bottom systems.

e) Keep an eye out (and an agnostic one) for light axion-like scalar particles.

3. AVAILABLE THEORETICAL TOOLS

Understanding the Higgs sector cannot be done without close interplay of theory and experiment. What are the theoretical tools available? To venture opinions here is hazardous in the extreme. Had I been asked to do this twenty years ago, I am sure I would have dead wrong and missed the methods which turned out to be most important. I don't know why I am so crazy to try this now. But anyway, despite the fact that I will omit something crucial, here goes:

A. Go-for-Broke

This is the approach embodied in GUTs, supersymmetric or otherwise, and especially the superstring. Its most distinguished heritage is general relativity, which was "freely invented," and "so beautiful it had to be right." What that took was full and tasteful use of the existing knowledge, profound and creative insights, and at least some contact with data. Can it be done again? Probably comparable intellectual power is available, if not in a single young Einstein, at least in the totality of talent around - especially if more time is allowed to figure things out than would be needed by an Einstein.

Let us hope that "go-for-broke" will work. But what are the odds? I place them much lower, mainly because the gap between the grand synthesis and, say, understanding K-M mixing angles and muon mass seems vastly greater than going from the equivalence principle and field equations to the perihelion of Mercury. In addition, the problem is less precisely set than for general relativity ("Find the correct relativistic theory of gravitation.").

B. Gauge Principles at a More Modest Energy Scale

The existing pattern in the standard model would seem to strongly imply the idea of using the gauge principle for all forces. There are plenty of theoretical approaches this side of the GUT scale that try this; for example, technicolor and extended technicolor for the Higgs sector and left-right symmetric theories for the electroweak sector. But while these do have promise, they don't seem to work smoothly. Models successful with the phenomenology tend to be quite complicated and models possessing simplicity and elegance tend to be unsuccessful with the phenomenology.

C. Compositeness

History forces us not to ignore this option. The presence of three generations of building blocks reinforces this idea well. Nevertheless, the same general problem as we discussed above occurs. In fact the number of building-blocks needed to successfully interpret the standard model periodic table often turns out to be comparable to, and sometimes in excess of, the number of degrees of freedom now extant. Using compositeness as a way of economization is hard to implement.

D. Supersymmetry

The arguments here are strong and elegant. SUSY helps to alleviate the fine-tuning problem of the Higgs sector: why its mass scale remains low in the presence of spinless degrees of freedom. Beyond this phenomenological argument lies a powerful esthetic one: we have witnessed the application of ever-broadening symmetry principles as the hallmark of progress. As seen from today's perspective, the next obvious step is bose-fermi symmetry, both global and eventually (at the go-for-broke level) local. But again the problem is one of applicability. With so many candidate SUSY particles which should exist, why have we not seen as much as a glimpse of their presence? The SUSY optimist can, quite correctly, point out how hard it has been to discover what from today's point of view are gross features of phenomenology; e.g. parity violation and the V-A structure of the electroweak force, neutral currents, and charm. The SUSY phenomena may be much more subtle, and more difficult to initially detect.

E. Some opinions:

1) The main concern I have about the technicolor and compositeness approaches is that they do not go far enough: they rely on an imitation of history at a higher mass scale. In general terms, this may well occur. But it is arguable that these "repetitions" are synthesized with something quite novel, not anticipated by imitation. This has been the pattern in the past.

2) The other approaches -- SUSY, supergravity, superstrings - clearly go deeper. In particular, the superstring presumes a solution to physics beyond the Planck scale. The guiding principles are smooth short-distance behavior and uniqueness: the candidate theory is very special. I find the underlying principles conservative: should we expect, beyond the Planck scale, a flat space-time continuum - albeit in 10 dimensions - where conventional quantum-mechanical rules

continue to hold? The Planck scale has always been a natural scale for these concepts to break down. Do we consider this 10-dimensional world of not very strongly interacting strings to hold to smaller distances *ad infinitum?* If not, and something radical intervenes at some sub-Planck distance scale, does this not erode the initial motivation for considering the superstring theory? I do not mean by these dour comments to disparage the present efforts. They are both impressive and exciting. But I think such questions should influence the odds-makers.

4. AVAILABLE EXPERIMENTAL TOOLS

The name of the game is high energy, and higher energy machines will be an absolutely essential tool. In the next decade we are blessed with a splendid mix of new facilities which will come on line -- the Fermilab collider, the SLC, LEP, and HERA. This, together with the existing fixed-target hadron beams at BNL, Fermilab, and CERN, with the lower energy e^+e^- machines new and old, and with the underground facilities, provide an impressive degree of discovery potential.

But I share the majority view that within the decade we will need to go to still higher energies and push much further into the natural energy scale associated with the Higgs sector. This is the business of supercolliders such as SSC or LHC and of e^+e^- colliders beyond LEP. I will not speculate on when or how many such facilities will be available within the next fifteen years. But for the sake of argument here, let us suppose that they <u>are</u> there - in fact <u>both</u> a very large hadron- hadron collider and an e^+e^- machine. What will be found? Almost certainly, I think, there will be discoveries pushing beyond the standard model, with a new phenomenology opening up. The challenge to experiment will be to discern clearly and incisively the properties of what is seen. Initial evidence for the new physics will probably be indirect: things will be seen which do not fit any hypothesis consistent with the standard model. Examples of such things nowadays are the same-sign dileptons seen in neutrino reactions, as well as the same-sign dileptons seen by UA1, monojets, etc. The UA1 top-quark evidence is another example; only by knowing well the anticipated top quark properties does the interpretation gain crispness.

Such indirect evidence for the new physics will not lead to unique conclusions, but will lead to a host of theoretical hypotheses which will demand new measurements. The detection apparatus, concentrated in a few collision regions, may or may not be flexible enough to meet the challenge, and one may anticipate the turnaround times required to make the next step to be relatively long. I think observation of jets, leptons and missing E_T will be a very powerful tool. This multijet spectroscopy at the TeV scale may look very much like multiparticle spectroscopy at the GeV scale. Nevertheless, at the GeV scale one also had the option of particle identification as well as flexibility in choice of reaction energy and of incident beam. And there were more independent experiments per year performed. All this may be harder to come by at the TeV scale. It could be of help to have information on the interior structure of each jet in the multijet events. But that is a big order.

In summary, there will be a great challenge to provide detectors with greater information density, and with enough flexibility to smoothly respond to what will very likely be many sharp changes in physics emphasis as the new phenomena unfold.

5. THE QUESTION

It is now time to face the question posed by the title of this talk. It can be put in a more precise form: will we learn enough in the next 15 years to probe the Higgs sector down to the same level of understanding as what was done in the last fifteen years for the strong and electroweak forces?

The eventual answer to the Higgs mystery may not, of course, lend itself to such a neat solution as for the strong and electroweak forces. But suppose it is indeed so. Will we be capable of finding out?

My guess is that in the long run, and perhaps with energies no higher than the supercollider scale now discussed, the answer may well be yes. But in the shorter 15-year run, my answer is no. The reasoning is as much sociological and technological as it is theoretical. Most of the points have been covered in the previous sections. In particular

1) The Higgs sector is probably no less complicated than the strong- interaction "Yukawa" sector. We "see" in our existing low energy phenomenology - in the sense of the example in section III - only indirect evidence for the analogs of the pions and the 0^+ sigma.

2) Experiments using multijet spectroscopy at the TeV scale may be comparable in resolution to the experiments which used multiparticle spectroscopy in the GeV range to establish hadron properties. That is the good news. The bad news is that signal/background is probably smaller and rates low. Even worse is the fact that the number of independent experiments available will be much lower, and the complexity much greater. The time span from one generation of experiments to the next will be much longer than what was the case at the GeV scale.

Thus we will be faced with a situation where the initial, relatively indirect evidence for phenomena beyond the standard model will spawn a plethora of theoretical explanations. (This assertion, unlike all the others in this talk, is absolutely incontrovertible.) To winnow down the alternatives will require many measurements. But the number available at any point may be relatively small. Steady progress can be expected, but it may require good luck and/or exceptional insight to break through fast. This should not be regarded as discouragement, but rather a mandate to be sure to explore all the available avenues at high and low energy, and to try to maintain as much flexibility as possible in the face of this difficulty.

Before concluding, I feel obliged to present my credentials as a forecaster. If in 1970 I had been asked my opinion on whether the strong and weak forces would be understood at the level of quantum electrodynamics within 15 years, I would have answered "impossible!" That I was proven wrong was the product not only of my own fallibilities, but also from the beautiful theory and beautiful experiments which I, for one, simply could not have anticipated. And while the revolution was going forward, I played a role of skeptic and conservative, one of the very last to jump on board the bandwagon. That is the way I am, and that may be the way it will go in the next fifteen years. I hope very much that it will be the case and I am proven wrong.

6. MAHALO AND ALOHA

In closing this final session of this series of Hawaii Conferences, I know I speak for all participants in thanking the organizers of all these meetings for making them such special occasions. While this has been the work of many people, two stand out: San Fu Tuan and Caroline Chong. A special mahalo and aloha to you both.