SECTION I

PERSPECTIVES

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Particle Physics since Lawrence and Yukawa

Yoichiro Nambu
The University of Chicago

Abstract: The present state of particle physics is briefly reviewed and characterized from a historical perspective.

Introduction

I am honored to participate for the first time in a conference on the hardware side of particle physics. I do not know what kind of credentials I can present to you, but as a theorist I have been witnessing the great strides high energy physics have made during the past 40 years. So allow me to talk about particle physics from a historical perspective.

My first serious study of particle physics at the University of Tokyo started with Hans Bethe's famous review articles on nuclear physics, published in the Reviews of Modern Physics in 1936-37. The first two articles dealt with theory, and were written by Bethe and Bacher; the third article dealt with experimental techniques, and was written by Livingston and Bethe.[1] Before coming here, I looked at these articles, the contents of which I had forgotten almost completely. I was surprised to find that, in discussing nuclear forces, Bethe never mentions Yukawa's meson theory. Yukawa's theory was published in 1935, about which Bethe probably did not know anything. The cosmic ray muon was discovered in 1937, but this must have been just after these articles had appeared.

Turning to the Livingston-Bethe article on experimental nuclear physics, they devote just one page to the cyclotron. According to the article, the magnetic resonance accelerator, or "cyclotron" of Lawrence and Livingston, had achieved energies up to 6 Mev and currents up to 20 microamperes.

But of course the cyclotron, invented in the early thirties, was fast becoming the main tool for doing nuclear physics, and eventually for going beyond nuclear physics. I recall that during the wartime Nishina constructed a 60" cyclotron, the world's largest at that time, in his laboratory at the Institute for Physical and Chemical Research, widely known as Riken. Nishina (1890-1951) was instrumental in laying the foundations of nuclear and cosmic ray physics in Japan. Not only did he build up experimental physics, but he also

nurtured theoretical physics at a time when quantum mechanics was still in its infancy. He created an environment which made possible the unique development of elementary particle theory in Japan, as is symbolized by the names of Yukawa and Tomonaga.[2] I sometimes rode a street car to attend the seminars at Riken conducted by Nishina and Tomonaga together, where the latest results in cosmic ray and nuclear physics were discussed. In fact it was there in 1942 that I learned of the two-meson hypothesis of Sakata, a hypothesis that the cosmic ray meson, now called muon, and Yukawa's meson, now called pion, were different particles, and the latter decayed into the former. Around that time, physicists were puzzled by the large meson production cross section in the upper atmosphere, and the small interaction cross section which enabled the produced mesons to reach the ground. Most theoriests of the day, like Heisenberg, Wentzel, Heitler, Pauli, Oppenheimer, and Schwinger, had tried to explain the puzzle in terms of dynamics. They had invented the strong couling meson theory for this purpose. Sakata, on the other hand, was not afraid to postulate yet another particle.

This brings me to a historical charaterization of particle physics in the last 60 years. Some time ago I proposed to call Earnest Lawrence and Hideki Yukawa the two founding fathers of particle physics. [3] They created the basic tools, one experimental and the other theoretical, for doing particle physics research. These tools have turned out to be enormously powerful and successful. They are still very much alive, as we are witnessing it today.

In the past 60 years, the energy of accelerators has steadily grown from 1 Mev to 1 Tev. This exponential growth is usually illustrated by the Livingston plot, which I show here, extrapolated to the year 2090. There have been many technical improvements and innovations that have made this possible, but the basic principle of accelerators is still the same as that of Lawrence's cyclotron.

On the side of theory, Yukawa's principle was to explain or understand phenomena in terms of particles, freely inventing or postulating one when necessary. It was antithetical to the prevailing attitude that beyond electron, proton, and neutron, no new physical entities should exist; rather, the mysteries of nuclear physics called for new departures from the existing quantum mechanics, just as quantum mechanics had to replace classical mechanics to explain atomic phenomena.

The spirit of Lawrence and Yukawa was one of adventure. One wants to do things and see what happens. Like the exploration of the New World, one was guided only by vague expectations. It was heuristic in nature. Sometimes the experiment led theory, sometimes the other way around, but they worked very well together. As the energy of accelerators

grew, one was able to explore physics at higher and higher energies, and thereby discover more and more new and heavier particles. Theories were proposed and tested, and further predictions were made in anticipation of the next accelerator.

Unfortunately, this happy state of affairs seems to have come to a halt in recent years. The last really new surprises were the J/Psi, the tau, and the upsilon of the 70's. By the early eighties, the theoretical predictions about the W and the Z had been basically confirmed.

The problem seems to be two-sided. On the one hand, it has become increasingly more expensive and time-consuming to continue along Lawrence's path to higher and higher energies. We sense that we are approaching, albeit ever so slowly, the practical limits of accelerator technology. On the other hand, the theoretical big bang that occurred in the early 70's has caused a radical change, from Yukawa's mode of operation they had been pursuing for decades, to what I call the Einstein and Dirac modes. [3,4] In the Einstein mode, one first postulates some fundamental principles, and works one's way down to testable predictions. Einstein's theory of gravitation as well as the gauge theories, based on the principles of symmetry and geometry, belong to this category. The Dirac mode is even more extreme. It is largely based on the physicist's esthetic sense, believing that what is mathematically beautiful must also be true. I would count Dirac's original monopole theory, supersymmetry, and the string theory among them.

Indeed the development of gauge and string theories has raised the expectation that we may now have the possibility for a unified description of the fundamental laws of elementary particles. It seemed as if we no longer had to grope our way through, guided by experiment step by step, dealing with each new phenomenon on an ad hoc basis The logarithmically slow change of parameters with energy scale, characteristic of renormalizable theories, meant that the range of energies under our command suddenly jumped exponentially, outdistancing the steady but modest progress of accelerator technology.

This sounded all very exciting. An unfortunate reality, however, is that these unified theories tell us that physics should be very elegant and simple at the Planck energy scale, but so far they have not been able to make concrete predictions about what should happen at our present lowly energy scale. At the historical rate of the Livingston curve, it would take more than a century for the experimentalists to catch up to the Planck energy, whereas the only solid testable prediction at low energy, namely the proton decay, has not been confirmed.

What are then the most important unsolved problems we are facing now, for which the unified theories do not give any answers? They are what may be categorized as the problem of mass. We have as yet no theory to explain the mass spectrum of leptons and quarks; we do not know why there are generations, or how many there are. These questions are intimately related to the properties of the weak interactions. The week interactions have special properties which we do not yet understand, and which seem to be responsible for making the world we live in very complicatied in spite of the apparent simplicity and elegance of the unified gauge theories. The fermion masses look very irregular. The reference frame for the weak interaction is not parallel to those for the other three interactions (strong, electromagnetic and gravitational). This causes the mixing of generations, and makes the heavier generations unstable so that we are made up only of the lightest generation of quarks and leptons.

Einstein once complained that his equation of gravity did not look very beautiful.[5] He compared the equation to a two-winged building; the left-hand side of the equation representing gravity was a wing made of marble, whereas the right-hand side representing the matter was made of cheap wood. The reason for this ugliness is precisely the problem of mass.

At any rate, I have the feeling that Nature had been unusually kind to us until ten years ago, but since then she has turned mean, or perhaps we have gotten too arrogant. It may not be unreasonable to say that particle theory is at present in the Baroque period. Theories have become exceedingly refined and sophisticated, but it looks as though they are being pursued just for their own sake. The theorists might retort: "What else can we do? Give us new data, then we will do more practical things."

I am sure they will, and that time may be coming soon. Recently the accelerators at KEK, Fermilab, and SLAC have been reporting fresh data, and the LEP has just gone critical, so to speak. We have to find some clues, some handles, that will enable us to understand the mysteries of the fermion masses. The top quark must exist, the Higgs boson almost certainly does. Otherwise we would not be able to explain the successes of the quark model and the electroweak theory. If we do not find them with the upcoming group of accelerators, the next generation of accelerators will. We might find even more.

In closing, I would like to make two wishes as a concerned non-expert on accelerator technology. First, it would be nice if radically new technology could be found. I know people are trying very hard, but much will also depends on serendipity. My second wish is more international coollaboration. Of course we have been collaborating for years, and we know that more cooperation is both necessary and inevitable if the scale of particle physics

has to grow. It is by no means an easy thing to realize, though, the nature of the real world being what it is. But the year 1992 is approaching soon. We have to endeaver to uphold the spirit of 1992, which will be both the year of the birth of EC and the 500th anniversary of the discovery of the New World.

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2. For a historical study of the development of particle physics in Japan since the 30's, see:

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"Proceedings of the Japan-USA Collaborative Workshops on the History of Particle Theory in Japan, 1935-1960", edited by L. M. Brown, R. Kawabe, M. Konuma and Z. Maki (published by the Progress of Theoretical Physics, Kyoto University, 1988).

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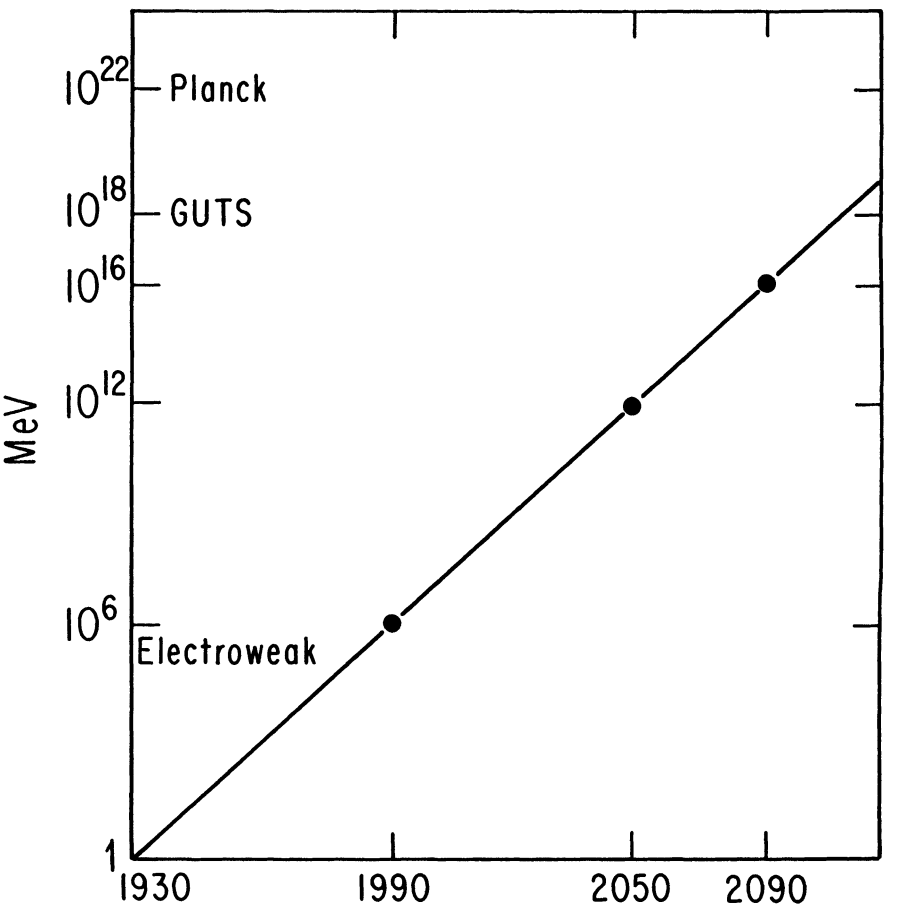
4. Y. Nambu, "1988 International Workshop on New Trends in Strong Coupling Gauge Theories", edited by M. Bando et al. (World Scientific, Singapore, 1989), p.339.

5. A. Einstein, "Out of My Later Years" (Philosophical Library, New York, 1950), p. 83.


10^{22}
Planck
10^{18}
GUTS
10^{16}
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Electroweak
1
1930
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2050
2090