HIGH ENERGY PHYSIC - 86

L.B. Okun

Institute of Theoretical and Experimental Physics
117259 Moscow

The purpose of the talk

The purpose of this introductory talk, as I understand it, is to give an overview of particle physics which would serve as a kind of a backdrop for the subsequent stage performance of a variety of accelerator ideas and projects at this Conference.

First of all I have to choose the general tone, the colors of the backdrop. Should it be mainly bright and optimistic, or mainly gloomy and pessimistic?

Why gloomy?

In the discussions one can hear a lot of pessimistic arguments.

One of these gloomy arguments is that we have such a beautiful standard SU(3) x SU(2) x U(1) gauge theory of strong and electroweak interactions. Not a single experiment during the seventies and eighties was able to find even a minor flaw in this theory. And it is not much fun to repeatedly confirm and substantiate the correctness of a theory which is generally believed to be correct.

Moreover, some computational extremists claim that to test a theory you don't need accelerator experiments at all : you can manage by performing only computer experiments.

The most active young theorists have deserted the field of accelerator physics. Entangled in superstrings, they work in 2, 10, 26, ... 506 dimensions, and avoid the trivial everyday problems of the four--dimensional world where humble phenomenologists, experimentalists and acceleratists are doomed to work.

The superstringers operate with energies measured by the Planck mass, which will never be reached by any man-made accelerator. So it seems their excitement would not find any positive response in this audience.

Our earthbound accelerator laboratories will never be able to compete with the Superhigh Energy Laboratory of the Early Universe. Cosmology and astrophysics provide us with unique fundamental data.

A serious challenge to accelerator physics comes from underground laboratories looking for proton decay, double-beta decay, electric charge nonconservation, neutrinos from the Sun, and reporting on such mysterious creatures as ugly cygnets from Cygnus X3.

Now think about two old but healthy relatives and rivals of accelerator physics : low energy nuclear physics and cosmic ray physics. The former promises such discoveries as the neutrino mass and neutrino oscillations, neutron - antineutron oscillations and axion-like particles. The latter uses a flux of high energy particles which is provided free of charge.

The words "free of charge" remind us of governments which are reluctant to give money for the construction of new accelerators, which are very expensive. As a result, some bold projects are kept in an embrionic state for quite a few years. And on top of that we have a certain report which recommends allocating a quarter of the present high energy physics budget to our colleagues working in biology, chemistry, solid state physics etc.

So it seems there are enough reasons for pessimism. And nevertheless I'm going to give an optimistic overview. My colors will be bright.

Why bright?

For me the main source of optimism is the "loose ends" of theoretical physics. First of all we are now clever enough to understand that the gauge principles of the standard theory are only part of the truth.

We know that below a few TeV there must

exist Scalarland - the land of scalar bosons where gauge symmetries are violated and masses of all known (and still unknown) particles are produced. Theorists import not only masses, but also weak mixing angles, CP-violation and sometimes even P-violation from Scalarland. We are absolutely sure that this terra incognita can be reached and explored by accelerators and by accelerators only.

We know that even when the scalars are found this will not mark the end of fundamental accelerator physics; our understanding of Nature will still be incomplete. There exists a widespread belief that to be self-consistent the theory has to be supersymmetric at high energies. Sparticles - superpartners of our ordinary particles - are expected to be not heavier than 1 TeV. This upper limit is determined by the Fermi scale

$(m_F = G_F^{-1/2} \simeq 0.3 \text{ TeV.})$

The only road to Superland goes through accelerator labs.

There are arguments (based on superstrings) that there may exist numerous additional particles - remnants of higher symmetries :

$E_8 \times E_8 \supset E_6 \supset \cdots \supset SU(3) \times SU(2) \times U(1).$

Among these remnants with masses in the vicinity of 1 TeV : the second Z-boson, some new leptons and quarks of the three 27-plets of E_6, extra higgses, and of course spartners of all of them.

No supertheorist can predict in detail the properties of these particles. Only experimentalists working on colliders can discover and explore them.

I don't agree with those who say that multidimensional superstrings are just a pandemic theoretical vogue. I think we are witnessing a very important event in the history of physics comparable in its greatness to the creation of the quantum field theory.

It was within the framework of quantum field theory that our standard model and all its grand unification extrapolations were created. Such strange phenomena as the confinement of quarks and the decay of protons are naturally described by the language of quantum field theory.

Quantum field theory was born six decades ago, the child of quantum mechanics and special relativity.

Superstring theory, when finally born, will be a child of quantum field theory and general relativity. Superstring theory will restructure the most profound notions of physics, such as space, time, and field.

A new fundamental theory needs a new mathematical language. Superstringers have already enriched theoretical physics with new mathematical tools provided by topology and algebraic geometry. Some of the recent superstring constructs are really beautiful.

It seems to me however that superstringers will not succeed in constructing their lofty tower - the theory of everything - until the multi-TeV accelerators form a broad enough base for such a tower, by uncovering strata of new fundamental facts. (Just think of the narrow base of the Kaluza - Klein - Einstein electrogravitational unification project.)

Let us now turn to cosmology and astrophysics. It is evident that without knowledge of the TeV spectra of such fundamental elements of matter as scalars and sparticles, it will be impossible to find the unique cosmological scenario of the first three picoseconds, which determined the subsequent evolution of the Universe. The accelerators are needed to ascertain the nature of the "dark matter" (which constitutes bulk of the matter in the Universe), to choose between numerous dark matter candidates : photinos, gravitinos, axions etc. As never before, cosmology needs the knowledge which is produced in the accelerator laboratories.

There is a deep and growing interdependence between high energy physics and the physics of the heavens. And we must be grateful to cosmology and astrophysics for giving us such bright guiding stars as, for example, the (almost?) vanishing cosmological term and the necessity of inflation and baryon nonconservation.

It is symptomatic that many high energy experimentalists nowadays are involved in various astrophysical projects (some of them underground).

Similar creative interdependence connects high energy accelerator physics with low energy nuclear physics and even with atomic physics (consider, e.g., the recent discussions of enigmatic electron and positron lines seen at Darmstadt or the numerous nuclear and atomic experiments on P-violation).

Even gravimetry, nowadays, is becoming a part of particle physics. Recent sensational publications reporting the discovery of the so-called "fifth force", with a range of the order of 1 km, turned out to be more enthusiastic than reliable. Nevertheless, the search for this force at a higher level of accuracy is very interesting. Just think about the potential users of the "fifth force" when and if such a long-range force is discovered.

There is a growing understanding of the importance of interdisciplinary synthesis of the fundamental sciences. The spirit of creative interdependence calls for multiplication and summation of efforts, not for division and subtraction. I'm sure this spirit will govern the relations of particle physics with solid state physics, chemistry, biology and other natural sciences. Fundamental knowledge is a source of fundamental benefits for the mankind. Extra money should not be searched for by trying to subtract it from the budget of a neighbour's laboratory. There is a lot of money being maliciously wasted outside pure science.

With these naive remarks, let us conclude the discussion of the general prospects of high energy accelerator physics and look at our basic particles.

Our 17 elements

The physical world of 1986 is based on 17 fundamental elements :

6 leptons (e, μ, τ, ν_e, ν_μ, ν_τ);

6 quarks (d, s, b, u, c, t);

4 vector bosons (photon γ, gluon g and wions W, Z);

1 graviton.

Note that I don't count antiparticles and color degrees of freedom, that the word wion (pronounced "why-on") is proposed for weak intermediate boson, that the t-quark has not yet been definitely found, and that an individual free graviton - a single quantum of the gravitational field - will never be observed.

It is remarkable that a majority of the 17 fundamental particles were discovered in accelerator experiments :

3 leptons (τ, ν_μ, ν_τ);

all of the quarks (the quark structure of the light hadrons, consisting of u, d, s quarks, many of these light hadrons themselves, and the heavy hadrons, containing heavy flavors of quarks : c and b);

3 vector bosons (gluon and wions).

Leptons

The most interesting and mysterious leptons are neutrinos. There remain several important questions pertaining to neutrinos:

1. Are neutrinos massless or massive, and if they are massive, what are the values of their masses ?
2. Are neutrinos different from the corresponding antineutrinos or are they genuinely neutral (self-conjugate) ?
3. Are neutrinos loyal to their charged partners and if not how do they oscillate and what are the mixing angles in the charged leptonic currents ?
4. Do neutrinos have electromagnetic dipole moments, diagonal and/or off-diagonal ?
5. Do neutrinos have some new anomalous interactions ?
6. Are there other neutrinos in addition to ν_e, ν_μ, ν_τ ? What is the total number of neutrino flavors ?

The experimental upper limits for muon and tau neutrino masses have been improved recently. But they are still very high compared to theorists' guesses. As for the mass of the electron neutrino, the ITEP group still insists on a non-vanishing value between 45 eV and 20 eV, while the new results from SIN are compatible with zero mass, the upper limit being 18 eV. A number of very accurate measurements are under way. If m_{ν_e} is larger than 10 eV, we will soon know its value with great certainty. But it may take another decade to go from 10 eV to 1 eV in the accuracy of measurements.

It is well known that the discovery of neutrinoless double-beta decay would prove the neutrality of the neutrino. Unfortunately, until now only lower limits exist for the corresponding lifetimes, the best of them being in the vicinity of $10^{22} \div 10^{23}$ years.

There has been a kind of "backward progress" in reactor experiments searching for neutrino oscillations : some favorable indications seen at Bugey have been almost completely ruled out at Gösgen and Rovno.

On the other hand some very important "forward progress" has been made in the theory of solar neutrino oscillations. It was theoretically discovered that in the Sun, with its density gradually decreasing from the centre to periphery, even a small mixing angle may lead to an almost complete transformation of ν_e into ν_μ or ν_τ . This phenomenon of resonant neutrino oscillations caused by the crossing of neutrino levels at the appropriate matter density, makes the predictions for the future solar neutrino detectors (especially for the gallium detector) less unambigous and the corresponding measurements even more interesting.

Future solar neutrino detectors (especially the liquid argon detector) may also shed some light on the problem of neutrino electromagnetic dipole moments. If these moments are of the order of 10^{-10} Bohr magnetons and if the magnetic field in the convective zone of the Sun is strong enough, solar neutrino flux variations with 11-year and semiannual periods will appear. (Some hints of such variations may be seen in the famous Homestake Mine data.)

As for some possible new anomalous interactions involving only neutrinos (and some new neutral bosons) it turns out that such interactions are very difficult to detect even if they are rather strong. Special high statistics studies of the electron and muon spectra from W-decays may be intersting from this point of view.

On the other hand, neutrino - electron scattering is a very clean process for the quantitative test of radiative corrections predicted by the electroweak theory. This is the aim of a new CHARM-2 experiment at CERN. In this experiment the ratio of neutrino to antineutrino electron scattering will be measured with an accuracy of the order of 2 %. Unfortunately such an accuracy allows one to test mainly the electro rather than the weak contributions to radiative electroweak corrections. The non-trivial electroweak terms may become observable in this experiment if new heavy fermions exist. Note in this connection that a fourth generation of quarks and leptons (with a light or even massless neutrino) is still not excluded by laboratory experiments (the Z-boson's width) nor by cosmological interpretation of the ^{4}He abundance.

I would like to conclude the neutrino part of the talk by a semi-rhetorical question : wouldn't it still be easier for theorists to find a principle according to which neutrinos are particles without masses, oscillations and crazy interactions, than for experimentalists to find all these things ?

Weak interactions of quarks

Our knowledge of two of the three quark mixing angles is still not adequate. But what is really tantalizing is the nature of CP-violation. Up to now the effects of CP--violation were observed only in four decay channels of one particle - the neutral long-lived kaon K_L° : $K_L^\circ \to \pi^+\pi^-$, $K_L^\circ \to \pi^\circ\pi^\circ$, $K_L^\circ \to e^\pm \nu \pi^\mp$, $K_L^\circ \to \mu^\pm \nu \pi^\mp$. Of special interest are the amplitudes of two-pion decays. Their ratios to the amplitudes of the corresponding K_S°-decays are denoted by η_{+-} and $\eta_{\circ\circ}$. Let us consider the ratio $\eta_{\circ\circ}/\eta_{+-}$. The value $|\eta_{\circ\circ}/\eta_{+-}| - 1$ is a measure of the direct CP-violating transition of the CP-odd kaon state K_2° into two pions. Recent measurements of this value, although not conclusive, are somewhat at variance with the standard mode. Further experiments are under way.

The phase arg ($\eta_{\circ\circ}/\eta_{+-}$) is a measure of CPT-violation. According to the Review of Particle Properties this phase is $9.8^\circ \pm 5.4^\circ$. Two new experimental proposals to measure this phase with an accuracy of $\pm 1°$ have been accepted at CERN and FNAL.

The services done by kaons to particle physics are really great. In 1956 kaons triggered the discovery of P- and C-violation. In 1964 they revealed CP-violation. In 1970 the small K_L-K_S mass difference inspired theorists to argue that charm is responsible for this smallness and that the mass of the c-quark is close to 1 GeV. Apropos, the famous box diagram of Fig.1 which describes $K° \leftrightarrow \bar{K}°$ transitions ($K° = d\bar{s} \leftrightarrow \bar{d}s = \bar{K}°$) is the only manifestation of second order weak process observed up to 1986. The real

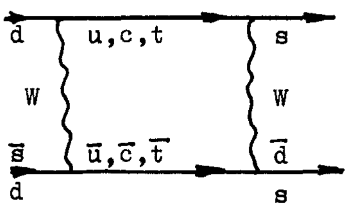


Fig. 1. Box diagram describing $d\bar{s} \leftrightarrow \bar{d}s$ transitions.

part of this box diagram is responsible for the K_L - K_S mass difference and the imaginary part for the CP-violating transition $K_2° \leftrightarrow K_1°$.

I'm sure that future experiments with kaons, especially the search for and the study of their rare decays, will give new insights into the most profound problems of particle physics.

Meanwhile, a new family of mesons has begun to contribute to our study of weak interactions. I'm referring here to B-mesons, or beons. The transitions $B_s° = \bar{b}s \leftrightarrow \bar{s}b = \bar{B}_s°$ are described by the box diagram of Fig.2, which is similar to that of Fig.1. $B° \leftrightarrow \bar{B}°$ transitions are sensitive to a

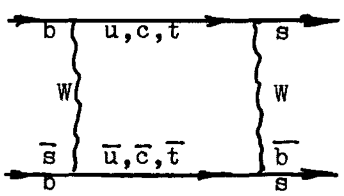


Fig. 2. Box diagram describing $b\bar{s} \leftrightarrow s\bar{b}$ transitions.

possible contribution from the fourth generation quark t'.

Recently, the UA1-collaboration has reported seeing a set of same-sign dimuon events. The natural explanation for these events is the strong production of $B_s°\bar{B}_s°$ pairs with subsequent vacuum transitions $B_s° \leftrightarrow \bar{B}_s°$ and semileptonic decays :

$$B_s°\bar{B}_s° \to \bar{B}_s°\bar{B}_s° \to \mu^-\mu^- + \dots$$

or

$$B_s°\bar{B}_s° \to B_s°B_s° \to \mu^+\mu^+ + \dots$$

There exists an interesting proposal to use tagged $B_s°$-mesons produced by high energy hyperons to study CP-violating effects in beon decays. This experiment would end the CP-monopoly of kaons.

The nonleptonic decays of charmed par c es ave puzz e t eor s s w o a believed that the c-quark, being so heavy, should decay without paying any attention to its environment. But the experiments have proved this picture to be an over-
-ım-'ifı-d -ne. E--erım-nt-''-- th- lıf-t'm- of $D°$ turned out to be a factor of two shorter than that of D^+. And the lifetime of D_s^+ and Λ_c^+ are even shorter. Theorists have found a posteriori that weak interactic with spectator quarks (see Fig.3) works in the right direction. But quantitative

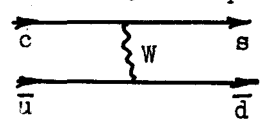


Fig. 3. Weak interaction of c-quark and ū-quark contributing to the decay of D°-meson.

predictions need a better understanding of virtual strong processes. And so we come to our next topic : strong interactions of quarks. But before proceeding to it, let us formulate a conclusion which is suggestec y e a ove scuss on o e wea interactions of quarks : in order to solve a number of important problems concerning the weak interactions of quarks we need factories of strangeness, charm and beauty.

Strong interactions of quarks

The situation with strong interactions is rather unusual. Since the early seventie we have been convinced that we have in hand the heart of the matter - the lagrangian of quantum chromodynamics, QCD. In that sense, strong interactions are not inferior to electromagnetic ones. There is however a great difference between the two theories, QED and QCD. In QED the apparatus of perturbation theory is brought to perfection. It allows one to make experimentally testable predictions with the highest accuracy (sometimes up to one part per billion). But with QCD, perturbation theory is valid only at short distances and even then, only with an accuracy of about ten percent. Moreover there is not a single process involving hadrons in which large distance effects can be neglected. Attempts to take into account the contributions of large distances have been diverse, often witty, less often refined, even less often reliable, and (unfortunately ?) almost always successful (of course, if high accuracy is not needed). The scene as a whole reminds one of an eastern bazaar.

What appears to be quite reliable is the theory of deep inelastic, hard inclusive and semi-inclusive processes; more specifically, the theory of quark and gluon jets.

One can assume, in a crude approximation, that the fragmentation of hard quarks and gluons into hadrons does not change the probability of a process which is determined by short distance contributions.

As for calculations of the static properties of hadrons (their masses, decay widths, and magnetic moments) the most succesful and theoretically consistent approach was demonstrated by the so-called QCD Sum Rules. In the framework of this approach, the bridge between short and large distances is provided by dispersion relations.

The essential ingredients of the QCDSR are the quark and gluon vacuum condensates. These condensates represent nonperturbative vacuum mean values of quark and gluon fields, the simplest of them being bilinear in these fields : $\langle q\bar{q} \rangle$, $\langle GG \rangle$ etc. By using the values of these condensates as free parameters, it is possible to describe the properties of various hadrons : those which contain heavy quarks, those which contain light quarks and those which contain no quarks at all, the so-called glueballs.

It would be unfair to ignore the fact that the same hadronic properties are also successfully described by a number of simpler and more naive models which are not as theoretically sound as the QCDSR. I'm referring here to the nonrelativistic quark model, to the potential models of heavy quarkonia and to the various modifications of the bag model. The very fact that these models coexist with QCD indicates the immaturity of QCD as a quantitative theory.

(It is proper to mention here that the bag model serves as a basis for farfetched speculations about the existence of strange quark matter. And we cannot at present theoretically exclude this possibility by a rigorous application of QCD.)

Special attention should be paid to computer-based QCD calculations. In these computer experiments, the space - time continuum is replaced by a four-dimensional lattice. In recent computer experiments the number of lattice vertices has exceeded 10^5, and the length of a link has been about 10^{-14} cm. Computer experiments were carried out in the framework of quantum gluodynamics, i.e., QCD without quarks. They were used to estimate the expected masses of glueballs. There have also been some calculations with quarks, e.g. of the weak non-leptonic amplitudes.

Of particular interest are the computer studies of QCD at high density and temperature, which indicate that, at temperatures in the vicinity of 200 MeV, nuclear matter should experience a phase transition into a state which is called the quark - gluon plasma. At present it is not quite clear how definite the signatures of such a phase transition will be. As a first step, a beam of high energy oxygen ions is being prepared at the CERN SPS machine to bombard a fixed target, so that we can see whether quark - gluon plasma is formed in this way.

Experimental prospects in strong interaction physics are exceptionally favorable. Of great interest are experiments in a broad range of energies : from very low to extremely high. This means that not only future superaccelerators, but the existing ordinary machines as well, may bring valuable information. And even machines no longer existent can contribute. I refer here to a beautiful measurement of the masses and widths of two charmonium levels χ_1 , and χ_2 which were resonantly produced in $p\bar{p}$ annihilation at the ISR. The preprint containing the analysis of this data appeared last April. It's like a burst of light from a perished star.

Vector bosons

SLC and especially LEP-1 with its mass production of Z-bosons will allow us to test quantitatively many aspects of electroweak theory. But perhaps the most interesting features of the non-abelian gauge bosons are their self-interactions. The self-interactions of gluons will be explored by studying pairs of gluon jets at large hadron colliders. The study of nonlinear wion vertices is a task for LEP-2 and for future linear electron colliders, such as VLEPP.

In spite of the simplicity and beauty of the non-abelian gauge theories, at least some of them may turn out to be only phenomenological manifestations of some more profound physics. From this point of view, wions may be only slightly more fundamental than the lightest vector mesons (ρ , ω) which were considered (although much less successfully) as gauge bosons of a non-abelian symmetry 25 years ago.

It is quite possible that quarks and leptons are also built of more elementary particles - preons. Unfortunately, I don't know any attractive preonic scheme. But the absence of an attractive scheme should not prevent experimentalists from searching for preons. There will be no lack of beautiful schemes as soon as preons are discovered.

Graviton

It has been known for centuries that gravity is the dominant force at the largest, astronomical distances. The idea that gravity is dominant at the shortest distances as well, is one of the main theoretical achievements of this century. It is in order to build a selfconsistent gravitational theory for distances of the order of the Planck length and even shorter that theorists address multidimensional superstrings 10^{-33} cm in length. They hope that by constructing such a theory they would be able to unify all interactions, to guess the highest symmetry group, to find the mechanism of its breaking, and to explain the mysterious pattern of particle masses and mixing angles which have been observed experimentally.

ADC - the strategic triad

The main message of this talk is that there is a lot of fundamentally interesting experiments which can be done at various types of accelerators. Still the most interesting are phenomena at the highest energies. Unfortunately, the larger is the energy E, the smaller are the cross-sections of interesting processes ($\sim E^{-2}$), and the larger is the background multiplicity.

The strategic triad of particle physics is formed by accelerators, detectors and computers. We strive for higher and higher energies, luminosities, accuracies, data handling rates in order to test our theories, to solve their unsolved problems and - what is even more important - to find something totally unexpected.

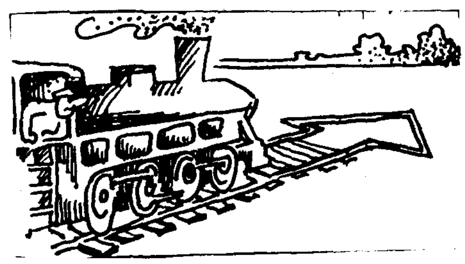
To meet these needs of physics, it is desirable to raise by three orders of magnitude the acceleration gradient and luminosity of linear electron colliders and the data acquisition and handling rate at hadron colliders at the beginning of the next century.

On the direction of progress

When preparing this talk I came across a newspaper with a cartoon by V.Peskov, which I felt had something to do with the talk. After some thought I decided that the meaning of the cartoon may be interpreted as follows. The steam engine is the symbol of High Energy Physics. As for the theorists, you cannot see them, but they are expected to be busy building the railroad. However sometimes some of them spend their (and others') time and railing to build not the railroad but the rail-arrows, which are supposed to indicate the direction of future progress. With this selfcritical remark I'm concluding the talk. For your homework you may try other interpretations.

I wish you every success.

Thank you.



Discussion

Вопрос из зала. В конце доклада Вы призвали к получению высоких энергий на ускорителях, но в самом докладе Вы называли только две энергии: 1 ТэВ и 10^{19} ТэВ.

Л.Б. Окунь. Когда говорится об энергиях порядка 1 ТэВ, то это означает следующее. Для электронных коллайдеров энергия частиц в точности соответствует энергии взаимодействия. Для адронных коллайдеров необходимо приблизительно в 6—10 раз увеличивать энергию частиц по сравнению с энергией взаимодействия. Это связано с тем, что каждый адрон состоит из трех кварков, на которые приходится примерно половина энергии летящего адрона. Поэтому на каждый кварк приходится $\sim 1/6$ всей энергии. Таким образом, если речь идет об адронных ускорителях, то их энергии должны составлять десятки ТэВ. Далее. Для того, чтобы не просто открыть частицу, а исследовать те теории, о которых шла речь, посмотреть сечения, разобраться, нет ли каких-либо взаимодействий, нужны, конечно, энергии в несколько ТэВ.

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