High-energy physics—86[1)]

L. B. Okun'

Institute of Theoretical and Experimental Physics, Moscow
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This paper, which contains the text of the introductory talk at the 13th International Conference on High Energy Accelerators at Novosibirsk in August 1986, presents a review of the contemporary status and prospects of high-energy physics.

The task of this introductory talk, as I understand it, is to give a general review of the contemporary status of elementary-particle physics in order to set the distinctive stage on which various ideas and designs for accelerators will be presented at this conference.

First of all, it is necessary to choose the general tone and color of the whole picture. Should this be light and optimistic, or dark and pessimistic? Many pessimistic arguments can be heard in conversations. It is widely believed that one of the main sources of pessimism is the fact that we now have the beautiful standard $SU(3)\times SU(2)\times U(1)$ gauge theory of the strong and electroweak interactions. During the seventies and eighties, not a single experiment could seriously challenge this theory. There is really little joy in continuing to provide new experimental confirmations of predictions based on a theory whose validity few doubt. Moreover, some computer extremists maintain that experiments with accelerators are not necessary at all for a test of the theory; they say that one can manage with computer experiments.

On the other hand, the most active young theoreticians have gone into superstrings; they work mainly in 2, 10, 26,..., 506 dimensions and prefer to avoid the trivial "everyday" problems of the four-dimensional world in which phenomenologists, experimentalists, and accelerator physicists are doomed to work.

Superstring physicists work with Planck energies, which will never be accessible with our accelerators. Thus, it seems to me that their enthusiasm should not arouse reciprocal enthusiasm in this auditorium. And here we come to the next sources of pessimism: cosmology and astrophysics. We know that in the search for the Ultimate Physical Truth our terrestrial accelerator laboratories cannot compete with the unique Planck laboratory of high energies provided by the early Universe.

A serious challenge to high-energy accelerator physics comes from the underground low-background laboratories in which searches are being conducted for fundamental phenomena such as decay of the proton, double beta decay, nonconservation of electric charge, and detection of neutrinos emitted from the Sun. News is arriving from these same laboratories of mysterious "nasty objects" supposedly emitted from Cygnus X3, and other such sensations.

There are two more old rivals and relatives of accelerator physics: 1) low-energy nuclear physics with its searches for neutrino mass and neutrino oscillations, neutron–antineutron oscillations, and axionlike particles; 2) cosmic-ray physics with its flux of high-energy particles, which is supplied by nature free of charge.

The expression "free of charge" reminds us of governments which are in no hurry to allocate funds for the construction of new colliders, which are by no means cheap, so that many bold projects remain in an embryonic state for a number of years. And to crown all this, recently there appeared in one of the West European countries a report whose authors recommend that a quarter of all the funds allocated to high-energy physics in Western Europe (CERN) should be transferred to our colleagues working in biology, chemistry, solid-state physics, and other branches of science.

Thus, it is said that there are sufficient grounds for pessimism. Nevertheless, I intend to give an optimistic review.

My optimism is based primarily on arguments relating to theoretical physics and, in particular, to its unsolved problems. With regard to theory, we are now sufficiently wise that we are not simply happy that the gauge principles of the standard theory work so beautifully; we also understand that gauge principles are only part of the answer. We are convinced that there must exist a new land of scalar bosons with masses not exceeding several TeV. Theoreticians are importing from this land violation of gauge symmetries, masses of all the discovered (and as yet undiscovered) particles, mixing angles in the weak currents, violation of CP symmetry, and, in the case of certain theoreticians, even a violation of P symmetry. We are absolutely convinced that it is necessary to discover and explore this strange land and that it can be reached by means of accelerators, and only by means of accelerators, since only accelerators can enable us to perform experiments at TeV energies with sufficiently high luminosity and under carefully controlled conditions.

Further, we know that even when scalar particles have been discovered and studied, the end of fundamental accelerator physics will still be very far away, since our understanding of nature in the range of energies accessible to accelerators will still be incomplete.

There is a widespread opinion that if the theory is to be self-consistent, it requires the existence of so-called "sparticles"—superpartners of our ordinary particles. Sparticles, just like scalar particles, must be not heavier than 1 TeV. This upper limit is determined by the Fermi scale: $m_F = G_F^{-1/2} \approx 0.3$ TeV. The only road to the land of sparticles runs through accelerator laboratories.

Superstring models indicate that there may exist numerous particles—the "remnants" of higher symmetries:

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

Among these particles with masses of the order of 1 TeV, there are a second Z boson, new leptons and quarks from the three 27-plets of the group E_6, additional Higgs particles (some of them are electrically charged, while certain neutral ones may be very light), and, finally, "spartners" of all these particles.

There is no supertheoretician who could predict in detail the properties of these particles. Only experimentalists working with colliders will be able to discover and investigate them. I do not share the opinion of those who think that the theory of superstrings and additional spatial dimensions is a transitory fashion. I believe that we are witnessing very important events in the history of physics, which in their significance can be compared with the development of quantum field theory.

It is in the framework of quantum field theory that our standard model and all its extrapolations known as grand unified models have been constructed. Unusual phenomena such as quark confinement and proton decay have a natural explanation and are predicted in the framework of quantum field theory.

Quantum field theory is a child of quantum mechanics and the special theory of relativity. (This child was born six decades ago.) The theory of superstrings is a child of quantum field theory and the general theory of relativity. When it is finally established, this theory will give a new, deeper meaning to the basic concepts of physics such as space, time, and field.

A new fundamental theory requires a new mathematical language. Studies of superstrings have already enriched physics with new mathematical tools created by topology and algebraic geometry. Some of the superstring constructions are very beautiful. Nevertheless, it seems to me that the builders of the grandiose superstring tower—the "theory of everything"—will not be able to realize their plans until multi-TeV colliders uncover new layers of fundamental facts and thereby establish a sufficiently broad foundation for this tower. (Just think about what a narrow foundation was used by Kaluza, Klein, and Einstein in their work on the scheme of electrogravitational unification.)

We turn now to astrophysics and cosmology. It seems obvious to me that without a knowledge of the properties of the fundamental elements of matter such as the scalar particles and sparticles, whose mass scale is 1 TeV, it is impossible to find a unique cosmological scenario for the first few picoseconds, which determined the entire future development of the Universe. Or another example: the celebrated dark matter which apparently forms the bulk of the mass of the Universe. To clarify its nature, it is extremely important to know the spectrum and other properties of the as yet undiscovered neutral stable particles [photinos (?), gravitinos (?), axions (?), etc.].

Cosmology, as never before, requires the knowledge obtained in accelerator laboratories, and this should be a matter of special pride for accelerator physicists. There is a deep and ever deepening interdependence between particle physics and celestial physics. And we experience a sense of deep gratitude to astrophysics and cosmology for the fact that they have given us guiding stars of the first magnitude, such as the (practically?) zero cosmological term or the need for an inflationary stage and nonconservation of baryons in the very early Universe.

It is symptomatic that the same physicists who work with colliders now also participate in large-scale astrophysics projects, including underground experiments.

A creative interdependence connects high-energy physics to both low-energy physics and atomic physics (recall, for example, the numerous nuclear and atomic experiments to investigate the nonconservation of parity, or the active concern caused by the sensational reports from Darmstadt on the observation of inexplicable lines in the spectra of electrons and positrons emitted in collisions of heavy ions). Even gravimetry has recently become a part of elementary-particle physics. I have in mind here the search for the so-called "fifth force" with an effective range of the order of a kilometer. Recent reports that such a force has been discovered turned out to be a false alarm. Nevertheless, this question merits further investigation at a higher level of accuracy. Obviously, if and when such a long-range force is discovered, it will find the widest application.

We are witnessing and participating in a very important process—interdisciplinary synthesis. It is becoming better understood that the spirit of creative interdependence requires summation and multiplication of efforts, and not subtraction and division. I am sure that this same spirit will determine the interrelation between elementary-particle physics and solid-state physics, chemistry, biology, and other natural sciences. Fundamental knowledge is a potential source of fundamental service to mankind. Extra funds should be sought, not by curtailing the budget of the neighboring laboratory. A lot of money is wasted by expenditure on evil beyond the realm of pure science.

With these remarks we conclude the description of the general picture of high-energy accelerator physics and turn to our fundamental particles.

The 1986 model of the physical world is based on 17 "elements": six leptons ($e, \mu, \tau, \nu_e, \nu_\mu, \nu_\tau$), six quarks (d, s, b, u, c, t), four vector bosons (the photon γ, the gluon g, and the wions W, Z), and one graviton.

Note that I am not considering here the antiparticles and color degrees of freedom, that the word "wion" (= *w*eak *i*ntermediate bos*on*) is pronounced in English like "pion," that the t quark has as yet not been definitively discovered, and that gravitons—the individual quanta of the gravitational field—will apparently never become accessible to experimental observation.

It should be emphasized that most of the 17 fundamental particles have been discovered in accelerator experiments: three leptons (τ, ν_μ, ν_τ), all the quarks (the quark structure of the light hadrons consisting of u, d, and s quarks, and many of the lightest hadrons and heavy hadrons containing the heavy quarks c and b), and three vector bosons (the gluon and the wions).

In the lepton sector, the most interesting and puzzling particles are, of course, the neutrinos. We list a number of important questions relating to the neutrinos:

1. Are they massive or massless? If they are massive, what are their masses?

2. Are the neutrinos different from the corresponding antineutrinos, or are they truly neutral?

3. Is each of the three neutrinos faithful to its charged partner, and, if not, how do they oscillate and what are the

mixing angles in the leptonic charged currents?

4. What are the values of the diagonal and/or nondiagonal electromagnetic dipole moments of the neutrinos?

5. Do the neutrinos not have any anomalous interactions?

6. Do there exist other neutrinos besides ν_e, ν_μ, and ν_τ?

Note that in the simplest variants of the theory the second and third questions are physically meaningful only if the neutrinos have nonzero masses.

The upper limits for the masses of the muon and tau neutrinos have recently been substantially improved, but they are still inconceivably large in comparison with what theoreticians expect for the neutrino masses.

With regard to the mass of the electron (anti)neutrino, a mass greater than 20 eV, which is still indicated by the group at the Institute of Theoretical and Experimental Physics at Moscow, does not agree with recent data from an experiment at SIN, which can be interpreted in a natural way for $m_{\nu_e} = 0$ and does not allow $m_{\nu_e} > 18$ eV. A number of accurate measurements of the mass of the electron neutrino will be completed in the near future. Thus if it is greater than 10 eV, we will know this with great confidence. However, it will apparently require at least a decade to go from 10 eV to 1 eV.

As is well known, the discovery of neutrinoless double beta decay would be a direct signal of the neutrality of the neutrinos. Unfortunately, we know at present only lower limits for the corresponding half-lives, the best of which are close to 10^{22}–10^{23} yr.

The experimental achievements in the search for neutrino oscillations tend to have a negative character; certain positive indications obtained with the reactor at Bugey are almost completely ruled out by subsequent experiments with the reactors at Goesgen and Rovno. However, special mention should be made of a very interesting theoretical prediction by Mikheev and Smirnov, according to which in the Sun, with its slowly decreasing density from the center to the periphery, even a very small mixing angle can lead to almost complete resonance conversion of the electron neutrino into the muon or tau neutrino. This new resonance mechanism renders the predictions for future solar detectors (particularly gallium detectors) less certain, and makes the corresponding experiments even more interesting.

Future solar-neutrino detectors (particularly liquid-argon detectors) may shed light on the problem of the electromagnetic dipole moments of the neutrinos. If these dipole moments are of the order of 10^{-10} Bohr magnetons and if the magnetic field in the convective zone of the Sun is sufficiently strong, there should be 11-year and half-year variations in the flux of solar neutrinos. (Some hints of such variations can be seen in the well-known data of Davis's group.)

As to possible anomalous interactions involving only neutrinos (and hypothetical neutral bosons), it turns out to be very difficult to detect such interactions experimentally, even if they are strong.

Purely leptonic weak processes are very clean and can therefore be used to test the predictions of the higher-order corrections of the electroweak theory. This is the purpose of a new neutrino experiment at CERN, which will measure the ratio of the cross sections for scattering of the muon neutrino and antineutrino by the electron with 2% accuracy. Unfortunately, the leading radiative corrections have here a trivial electromagnetic origin; nontrivial electroweak corrections can become observable in this experiment only if there exist new heavy fermions. In this connection it makes sense to note that a fourth generation of quarks and leptons (with a light or even massless neutrino) is still not ruled out by either laboratory measurements of the width of the Z boson or the cosmological theory of nucleosynthesis in conjunction with observational data on the abundance of helium.

I would like to conclude the neutrino section of my talk with a semirhetorical question: Could it still be easier for the theoreticians to discover a principle according to which neutrinos are particles without mass, oscillations, and abnormal interactions than for the experimentalists to discover all this experimentally?

We turn now to the quarks. We begin with their weak interactions. Our knowledge of two of the three mixing angles is still completely inadequate. However, the strongest feeling of inadequacy is caused by CP violation. Until now, the effects of CP violation have been observed only in four decay channels of the long-lived neutral kaons:

$$K_L^0 \to \pi^+\pi^-, \quad K_L^0 \to \pi^0\pi^0, \quad K_L^0 \to e^\pm \nu \pi^\mp, \quad K_L^0 \to \mu^\pm \nu \pi^\mp.$$

The amplitudes for the decays into two charged pions and into two neutral pions are of the greatest interest. When divided by the amplitudes for the corresponding decays of K_S^0 mesons, they are denoted by η_{+-} and η_{00}. Consider the ratio η_{+-}/η_{00}. The phase of this quantity must be equal to zero (with accuracy of the order of 1°) by virtue of CPT symmetry. (Experimentally, this phase is $9 \pm 5°$.) The deviation of the modulus of this quantity from unity is a measure of the direct CP-violating transitions of the CP-odd component of the neutral kaon into two pions. Recent measurements of this modulus are not in complete agreement with the predictions of the standard theory, in which the entire violation of CP symmetry is described by a single phase in the matrix of the charged weak quark currents. New, more accurate measurements of the modulus are now being made, and there have appeared two proposals to measure the phase with accuracy up to $\pm 1°$ at CERN and Fermilab.

The role of kaons in elementary-particle physics is unique. Thirty years ago it was their decays that made it necessary to raise the question of violation of the P and C symmetries; in 1964 they revealed the violation of CP symmetry. Several years later, the small mass difference between the long-lived and short-lived neutral kaons led theoreticians to the idea that this small difference is due to charm and made it possible to predict that the mass of the c quark is close to 1 GeV. Incidentally, the celebrated box diagram (Fig. 1) which describes the transitions $K^0 \leftrightarrow \bar{K}^0$

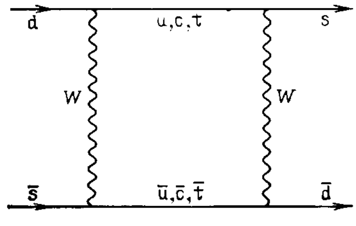

FIG. 1. The box diagram, which describes the transitions $d\bar{s} \leftrightarrow \bar{d}s$.

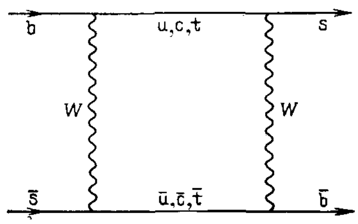

FIG. 2. The box diagram, which describes the transitions $b\bar{s}\leftrightarrow s\bar{b}$.

($K^0 = d\bar{s}\leftrightarrow\bar{d}s = \bar{K}^0$ was until this year the only diagram of second order in the weak interaction which certainly has a bearing on actually observed phenomena. The real part of this diagram is responsible for the mass difference between the K_L^0 and K_S^0 mesons, and the imaginary part is responsible for the CP-forbidden transition $K_2^0\leftrightarrow K_1^0$. I am sure that further experiments with kaons, particularly the search for and quantitative study of their rare decays, will make it possible to touch upon the most profound problems of elementary-particle physics.

Meanwhile, a new family of mesons has apparently begun to provide information about the weak interactions. I have in mind here the B mesons or, in abbreviated form, beons. The transitions $\bar{B}_S^0 = \bar{b}s\leftrightarrow\bar{s}b = B_S^0$ are described by the box diagram of Fig. 2, which is analogous to the diagram of Fig. 1. The transitions $B^0\leftrightarrow\bar{B}^0$ are sensitive to a possible contribution from a fourth-generation quark t′.

Recently, the UA1 collaboration reported the observation of about 200 like-charged dimuon events. A natural explanation of these events is a process of strong production of $B_S^0\bar{B}_S^0$ pairs with subsequent transitions $B_S^0\leftrightarrow\bar{B}_S^0$ in the vacuum and semileptonic decays of the B_S^0 mesons:

$$B_S^0\bar{B}_S^0\to B_S^0B_S^0\to\mu^-\mu^-+\dots$$

or

$$B_S^0\bar{B}_S^0\to\bar{B}_S^0\bar{B}_S^0\to\mu^+\mu^++\dots .$$

There exist interesting proposals to make use of energetic hyperons for the production of slow B_S^0 mesons and to observe oscillations and CP-odd effects in the decays of the latter. It is likely that, after the K^0 mesons, the neutral B_S^0 mesons will be the next particles in whose decays the violation of CP invariance will be measured.

The nonleptonic decays of charmed hadrons have proved to be a hard nut for theoreticians, who expected at first that the heavy c quark would decay, taking no notice of which light quarks occur together with it. But this picture turned out to be too simple. Experimentally, the lifetime of the D^0 meson was found to be approximately half of the lifetime of the D^+ meson, and the D_S meson and the Λ_c hyperon decay even more rapidly. Afterwards, the theoreticians showed that the interactions with the neighboring light quarks explain qualitatively these deviations from the naive expectations, but for quantitative predictions we require a more complete understanding of the virtual strong interactions (the single diagram of Fig. 3 is insufficient).

This brings us to our next topic—the strong interactions of the quarks. However, before proceeding to it, we draw the natural conclusion from the foregoing survey of the

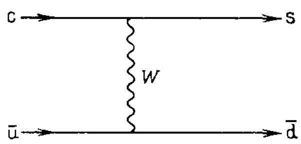

FIG. 3. The weak interaction of c-quark with ū-quark, which contributes to the D^0-meson decay.

weak interactions: in order to solve a large number of important problems concerning the weak interactions, we require high-current accelerators—factories of strangeness, charm, and beauty.

The situation with regard to the strong interactions is unusual. For over a decade we have been convinced that we know the most important aspect of them—the Lagrangian of quantum chromodynamics (QCD). In this sense, the strong interactions are not inferior to the electromagnetic interactions. However, the apparatus of perturbation theory, which has been brought to perfection in QED and permits experimentally verifiable predictions with the greatest accuracy (in certain cases, up to nine significant figures), works in the case of QCD only at small distances, and even then only with an accuracy of the order of ten percent. At the same time, there is not a single process involving hadrons in which large distances play no role. From the theoretical point of view, the fundamental distinction between QCD and QED is not only the difference between the magnitudes of the coupling constants, but also the strong direct gluon–gluon interaction, whereas there is no direct photon–photon interaction. Attempts to take into account the contribution of large distances are very diverse, often witty, less frequently refined, even less frequently reliable, and, if we are not speaking of high accuracy, almost always successful. (I nearly said "unfortunately.") On the whole, the picture of contemporary QCD is reminiscent of a colorful oriental bazaar.

It looks as if we have a comparatively reliable theory of hard (deep inelastic) inclusive or semi-inclusive processes, in particular, quark and gluon jets, in which, if we do not strive for high accuracy, we can assume that the fragmentation of hard quarks and gluons into hadrons does not alter the probability of a process determined by the contribution of small distances.

Turning to the calculation of the static properties of hadrons, and, in particular, their masses, decay widths, and magnetic moments, the greatest success and consistency from the theoretical point of view have so far been achieved here by quantum-chromodynamical sum rules, in which a bridge between small and large distances is provided by dispersion relations. A very important feature of this approach is the so-called quark and gluon vacuum condensates, which have a nonperturbative character, i.e., are not described by perturbation theory. These condensates are vacuum expectation values of the quark and gluon fields, the simplest of which are bilinear in these fields: $\langle\bar{q}q\rangle$, $\langle GG\rangle$, etc. Using the values of these condensates as parameters, it is possible to describe a very wide range of experimental data relating to both hadrons containing heavy quarks and hadrons consisting solely of light quarks, as well as those containing no quarks at all, the so-called glueballs.

Besides the success of the quantum-chromodynamical sum rules, the experimental data are described very successfully by more naive simplified models which are not supported by such a firm theoretical basis: potential models of heavy quarkonia, the nonrelativistic quark model, and various modifications of the bag model.

The very fact that these models coexist peacefully with quantum chromodynamics indicates that the latter is immature as a quantitative theory. The lack of development of the computational apperatus of QCD is particularly apparent when one considers, for example, the impossibility of excluding theoretically the existence of an exotic object such as strange quark matter, speculations about whose existence are based on the bag model.

I would particularly like to dwell on so-called computer experiments: QCD calculations in which the space-time continuum is replaced by a four-dimensional lattice. In the most recent calculations, the number of lattice sites exceeds 10^5, and the step length is of the order of 10^{-14} cm. Computer calculations have been performed, in particular, in the framework of quantum gluodynamics, i.e., QCD without quarks, in order to estimate the expected masses of glueballs. Calculations are also being carried out with inclusion of quarks, in particular, calculations of weak nonleptonic amplitudes.

The greatest interest has been aroused by computer investigations of QCD for large values of the density and temperature. These investigations indicate that at a temperature of the order of 200 MeV nuclear matter should undergo a transition to the state known as the quark–gluon plasma. The intensity of the signals which will indicate that such a phase transition actually occurs is as yet not completely clear. As a first step in the search for the quark–gluon plasma, an experiment is being started at the CERN supersynchrotron to bombard a stationary target with a high-energy beam of oxygen ions.

The experimental prospects for investigating the strong interactions are extremely favorable. From the point of view of quantum chromodynamics, there is great interest in experiments with the most diverse levels of difficulty over a very wide interval of energies: from very low energies to the highest possible energies. This means that it will be possible to obtain valuable information not only with future superaccelerators, but also with the existing ordinary machines, and even with machines which no longer exist. This last remark was prompted by beautiful measurements of the masses and widths of two levels of charmonium, χ_1 and χ_2, which were produced in a resonance manner in $p\bar{p}$ annihilation at the now dismantled ISR collider. A preprint containing the results of an analysis of this experiment appeared in April 1986. This is like a flash of light from a long-extinct star.

We have discussed leptons and quarks, and we turn now to vector bosons. We have already said a little about them in discussing the electroweak theory and the strong interactions. When SLC and LEP come into operation, we will acquire unique factories for the production of Z bosons, which will permit quantitative tests of many aspects of the electroweak theory. However, even now we must look into the more distant future: it is clear that the most interesting feature of the gauge bosons is their self-interaction. The experimental study of the self-interaction of ions requires LEP2 and VLÉPP. As to the gluon-gluon self-interaction, it is of special interest here to study pairs of gluon jets at the large hadron colliders.

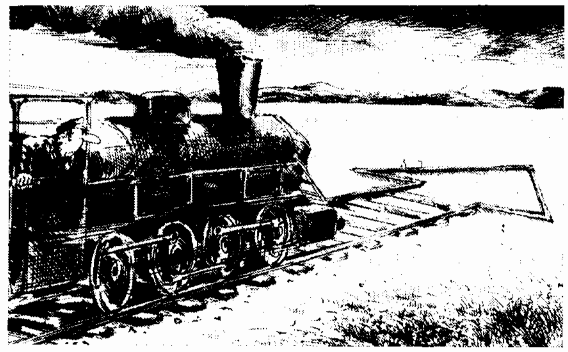

FIG. 4.

Despite the simplicity and beauty of non-Abelian gauge theories, we should not lose sight of the fact that at least some of them may turn out to be merely a phenomenological description of deeper physics. From this point of view, wions might prove to be no more fundamental than the lightest vector mesons (ρ,ω), to which attempts were also made in the sixties to apply non-Abelian gauge symmetry (though with much less success). The quarks and leptons may also turn out to be composite. It is true that not a single elegant preon theory has so far been proposed. However, the final word here should belong not to the theoreticians, but to the experimentalists. If experiments reveal preons—constituent elements of the contemporary fundamental particles—I am sure that there will be no shortage of elegant schemes.

I have said practically nothing about the graviton. I have left it for the end of my talk, since the graviton occupies an exceptional position in modern physics. This is the case because the gravitational interaction, according to contemporary theoretical ideas, plays a major role not only at the largest scales, but also at the smallest. Namely, in order to construct a consistent theory of the gravitational interaction at energies of the order of, and greater than, the Planck energies, where it becomes strong, theoreticians are resorting to additional spatial dimensions and are replacing point particles by multidimensional superstrings having Planck dimensions. It is the attempts to construct a consistent theory of superstrings that have recently led to hopes that it will be possible to guess the higher symmetry group, to find the mechanism of its violation, and to explain the empirical regularities characterizing the masses of the particles and the mixing angles in the weak currents.

One of the basic ideas of this talk is that there are numerous fundamentally interesting experiments which can be performed using the most diverse accelerators.

Nevertheless, we are still most interested in phenomena which occur at ever higher energies. Unfortunately, the higher is the energy E, the smaller is the cross section of the most interesting processes ($\sim E^{-2}$) and the larger is the multiplicity of the background processes.

The strategic triad of high-energy physics consists of accelerators, detectors, and computers. We strive for ever higher energies, luminosities, accuracies, and rates of analysis of the data in order to scrupulously test our theories, solve their unsolved problems, and, above all, seek phenomena which are not predicted by any theories. We simply have a strong desire to know what lies ahead.

For the successful development of physics, it would be desirable to achieve by the beginning of the next century an increase by three orders of magnitude in the rate of acceleration and luminosity of the planned linear electron colliders and in the rate of collection and analysis of data at the hadron colliders.

While preparing this talk, I happened to come across a newspaper cartoon by V. Peskov (Fig. 4),[2)] which seemed to me to have some bearing on this talk. After some reflection, I decided that the drawing can be interpreted as follows. The locomotive is the symbol of high-energy physics. As to the theoreticians, they cannot be seen in the drawing, but it is understood that their job is to construct the railroad. However, some of them sometimes use their own and others' time, and also rails, to construct not a railroad, but rail arrows, which they think should indicate the direction of future progress.

With this self-critical remark, I conclude the talk. As homework, you can seek other interpretations.

I wish you success. Thank you.

[1)]A review talk on the status and prospects of high-energy physics at the 13th International Conference on Accelerators of High Energy Charged Particles, Novosibirsk, 7-11 August 1986.

[2)]I am grateful to V. Peskov, who kindly provided for publication in Uspekhi Fizicheskikh Nauk the original drawing, which received the Gold Medal at the International Competition at Ljubljana in 1969.

Translated by N. M. Queen