The future of physics (future generations of particle accelerators)

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In the light of the predictions for the development of high-energy physics, it becomes clear that a special and fundamental significance for the further study of the properties of matter will be possessed by an accelerator with an energy $E \geq 300$ GeV in the center of mass of the colliding particles—the so-called unitary-limit accelerator.

INTRODUCTION

In this article we will discuss mainly the future of high-energy physics. The immediate and remote future of this field has been widely discussed during the last ten years; discussion was carried on particularly intensively when the construction of the Batavia accelerator was being planned. Summaries of this discussion have been given in the collection "Nature of Matter. Purposes of High-Energy Physics."[1] This material has been translated and published in Uspekhi Fizicheskikh Nauk[2] with addition of a number of articles by Soviet authors.

Now, however, when the construction of the Batavia accelerator has been successfully completed and its use is beginning, the problem naturally and opportunely arises of the next generation of accelerators of higher energy. The problem is natural from the point of view of forecasting the trends in development of science in general, and opportune in the nature of the object of the forecast in particular. In the foreward to one of the documents relating to planning of the European 300-GeV accelerator at CERN, B. P. Gregory (at that time director of CERN) wrote: "I must point out one difference between high-energy physics and many fields of science. The unavoidably large size of the equipment compels us to plan about fifteen years ahead"[3]. Therefore we will be discussing the next generation of accelerators and, in essence, the next generation of physicists. At the present time the future of high-energy physics cannot be discussed except in connection with the future of physics and, even more broadly, the future of science in general. During the past two decades great advances have been made in the various divisions of physics, astrophysics, biology, and other sciences. The future of these branches of science, often very promising, attracts the attention of scientific society and produces a certain anxiety and fear regarding the outlook for the material security of science.

In the last few years a number of articles have appeared on the future of science which in one way or another have touched on the future of high-energy physics. Thus, the well known article by Dyson[4] in 1970 was entitled "The Future of Physics." In 1971 there was an article by V. L. Ginzburg[5] in Uspekhi Fizicheskikh Nauk which was interesting for its broad factual content and for the questions raised, and in 1972 there appeared in the journal Priroda an article by L. A. Artsimovich[6] in which attention is drawn to the predominant development of astrophysics. Articles by Cole[7] and Anderson[8] is the journal New Scientist in 1971 attracted attention. One of these[7] is a eulogy on the nearly completed construction of the 500-GeV synchrotron at Batavia and the forthcoming program of research, and the other[8]— "Are the Big Machines Necessary?"—is related to the preceding article (if you will pardon the comparison) as an antiparticle is related to a particle. Here the signs of many assertions of the previous article are reversed.

In all of these articles an attempt is made to examine what changes have occurred in physics and in the natural sciences in general in recent years, what place is occupied in science at the present time by high-energy physics, and what can be said on the outlook for science in the coming decade.

In the decades near the end of the first half of the present century, the development of microphysics was noted by very great achievements. The violent development of atomic and nuclear physics of those decades was accompanied by fundamental discoveries. The tremendous influence of these discoveries on economics, politics, and intergovernmental relations led to a unique elitism of nuclear physics and elementary-particle physics. The representatives of other scientific fields—solid-state physics, chemistry, biology, and so forth—, struck by the advances in this field of science and the value of these advances not only to science but to society[9], meekly acknowledged this elitism.

With time, important advances appeared also in other fields of science. The opinion of scientific society regarding the hierarchy of the various scientific endeavors is changing. The situation also has a purely prosaic aspect. Research in high-energy physics is becoming very expensive, and the accelerators being built are turning out to be expensive. Future generations of accelerators will turn out to be still more expensive. Accordingly, the value of the results obtained with accelerators is being discussed. Sometimes between the lines we can perceive the question: Is it worth it?

We do not wish to engage in a general discussion of whether or not there is a need for ultrahigh-energy accelerators in general. We would like to limit ourselves to discussion of the specific question of the need for the next generation of accelerators, following those which have recently or just now come into operation. Here we have in mind both the Batavia accelerator of the traditional type and the colliding-beam proton accelerator at CERN.

In the discussion we will take into account both the optimistic and the pessimistic considerations in the articles mentioned above; we will have in view also the development of science in general, paying attention to the particular predictions in the articles cited and discarding those parts which are not directly related to our problem but sometimes create a background which unnecessarily complicates the problem.

We will begin with Dyson's article.[4] This article, "The Future of Physics," is interesting in many respects. The article begins with recollections of the situation which arose in the Cavendish Laboratory after the death of Ernest Rutherford:

"To the consternation of those who remained in Cambridge, Bragg (the new director of the laboratory—M. M.) made no effort to rebuild. He was not seriously interested in plans for a new accelerator. He sat smugly in his office at the Cavendish and said: "We have taught the world very successfully how to do nuclear physics. Now let us teach them how to do something else."

This "something else" arose in the form of research in radio astronomy and molecular biology, whose development was accompanied by really fundamental discoveries. Analyzing the causes of Bragg's success as director of the Cavendish Laboratory, Dyson formulates three rules which in his opinion aided Bragg in this situation, which had arisen at Cambridge at the end of the thirties. "I think," writes Dyson, "that this story has important lessons for us today" (for discussion of the future of physics.—M. M.).

These rules sound almost like religious commandments, and are in the nature of a categorical imperative.[1)]

First of all we would like to understand whether these rules can actually be useful in our situation.

"Don't try to revive past glories." This is a question relating to a specific installation, and to specific conditions and possibilities. Perhaps we should follow this rule, but perhaps sometimes we should not. Perhaps the things which Bragg forbade in the absence of desire to "revive past glories" of Cambridge, to continue research in nuclear physics, are not so wrong. This does not mean that it was not necessary to develop new directions of research—radio astronomy and molecular biology. However, perhaps this would better have been done elsewhere, not to the prejudice of nuclear physics.

It is fortunate that there arose at Berkeley a research center which in a sense took the baton of the Cambridge research on the structure of matter and elementary particles.

"Don't try to revive past glories." This is in no way a commandment, but only one of the alternatives in discussion of the fate of a scientific institute. Each institute has its youth, maturity, and old age. The development cycle of an institute usually occupies 15—20 years. Then an institute is either revitalized, or disappears, or arises in a completely different form. No, this rule will not help us. However, this rule is cited in the literature and produces that excessive background noise which it is desirable to avoid.

The question of fashion is not at all that simple. Each individual fashion arises first, so to speak, as an antifashion—a new fashion in opposition to an existing one. As a rule, a direction of science which appears in some way promising becomes fashionable. To whom is this second commandment addressed?

Fashion, as a rule, attracts people who as a whole are not always very creative, i.e., people who, as a rule, do not themselves create the fashion. However, these people are often very energetic and productive in practical matters and frequently possess considerable formal skill in theory, which allows them to rapidly and efficiently obtain results in a fashionable area, to test the fashion for its strength, and perhaps in this way to exhaust or even close out the fashion.

This variety of investigators exists in the scientific world and is necessary and unavoidable in the ecology, if we can use the term, of scientific creativity, in the same way as various living organisms are necessary in the ecological equilibrium which exists on Earth.

"Don't be afraid of the scorn of theoreticians." Why only of theoreticians: we recall the well known quotation: "Anyone who expects to obtain energy as the result of transformation of atoms is speaking nonsense." This is of course from the speech of Ernest Rutherford at the meeting of the British Association for the Advancement of Science on September 11, 1933.

This question refers again to the same ecology of scientific creativity or even to the ecology of the scientific environment. Many persons, not only theoreticians, can throw cold water on ardent enthusiasts. Very often these are well qualified persons with great erudition and wisdom. They are able to see first of all the difficulties which appear unsurmountable when viewed with the customary logic.

It seems that Ford somewhere wrote that if he wanted to make trouble for his competitors he would advise them to collect a large number of highly qualified engineers, to each of whom it would be clear from the beginning that it was impossible to suggest anything new. If Ford didn't actually say this, we would have to invent it. On the other hand, criticism is necessary in the ecology of science. It is well known that wolves are of great use in destroying weak animals. Of course, the strong and healthy sometimes fall into misfortune. This is a tragedy of a specific individual. Ecological equilibrium wisely establishes itself not only among flora and fauna.

Therefore, if Dyson's advice refers to the innovators of science, to the vigorous spirit and thought, it is not necessary. If it is directed at the great majority of self-styled "investors", it is not to the advantage of science. However, setting aside those pages of Dyson's article which are not directly related to the question, we find in it two statements which must be placed in the list of specific questions subject to further discussion.

First, we are interested in Dyson's evaluation of the experimental situation which may arise at the Batavia accelerator. This estimate has many nuances, of different sign. Dyson writes: "Roughly speaking, the effect of the future investment of money and talent at

Batavia is to push the energy range of physics up by one power of ten, from the tens of GeV that we shall have in the 1970.

We all devoutly hope that Nature has put important new phenomena that we can discover within this one power of ten. If it turns out that she has done so, the effort we put into building the machine will be worthwhile. If there are no basically new things to be found in this particular energy range, the machine will be a monumental flop.''[4] Dyson's attitude toward the construction of the highest-energy accelerator at Batavia is expressed grammatically by the arbitrary assumptions ''if ...''. We cannot refute Dyson in the strictly logical construction of his grammatical sentences. However, the structural material for these sentences has been used so sparingly that they not only do not characterize the situation being discussed but substantially distort it, Of course, it is also true that other accelerators such as the Berkeley Bevatron were built for a particular purpose; the Bevatron, in particular, was built directly to confirm (or disprove) the existence of antiprotons. The resolution of this question a priori justified the construction of the Bevatron and the awarding of the Nobel Prize to the discoverers of the creation of proton-antiproton pairs.

The maximum energy of the Batavia machine was not determined by any single fundamental problem of this type. A broad program was formulated which was generally directed toward filling the ''blank areas'' in the map of physical phenomena in this energy region.

This range of energies in physics research must be traversed—this is the same historical need in the development of science as existed in the past for investigation of the white areas in the geographical map of the Earth; it must have and will have its enthusiasts, heroes, and perhaps also martyrs.

Dyson completely ignores the extensive and thematically substantive program of research, which was developed by a large group of physicists. It is to the point that this program with the passage of time is gradually being broadened and is becoming more and more interesting and significant. Here we have in mind in particular the situation with multiple production of particles, scale invariance, and generally the group of questions which had arisen at the time when the construction of the accelerator was almost complete; this group of questions naturally did not enter into the arguments on which the need for construction of the accelerator was based.

Dyson then sets off the possibilities of cosmic-ray experiments against experiments with accelerators. We will continue the discussion of these questions subsequently, among other questions raised in the articles mentioned above.

The essence of the article by P. W. Anderson[8] is in the following sentence: ''Scientists have begun to realize that the pie is finite and what is ''pro'' high-energy physics is ''con'' to something else... Every discussion of this subject must start from the fact that high-energy physics is terribly expensive.'' Of the fact that high-energy physics is expensive, there is no doubt. Furthermore, any science becomes more expensive, and we will discuss this separately under the heading ''the rise in the cost of science.'' However, first we would like to ''wring'' out of the Anderson article the statements which, as in the Dyson article, form a background cloud around the real problems which require discussion.

We must place in this category the author's discussion about the stategy of science. The author attempts to convince the reader that revolutionary changes in science are completed not by accumulation of new facts, but by new ideas and new points of view. The author, proceeding from general ideas, apparently would recomend to high-energy physics not to accumulate facts (moreover, this is expensive), but to think more conceptually. It is quite unnecessary to prove that revolutionary changes in science are usually associated with new ideas. However, can the author point out the moment when enough facts have been accumulated for the appearance of new ideas and new points of view?

Another point is the specific statement by the author that the rate of appearance of new discoveries in high-energy physics is slowing down as the energy of accelerators coming into operation rises. The author even uses the term ''crisis situation'' in high-energy physics. This statement of the author already requires discussion, for example, of the ''crisis situation'' in high-energy physics or of the ''law of diminishing returns'' or, perhaps, of high-energy accelerators.

Finally, Anderson's advice is to slow down not only experimental but also theoretical research in the field of high-energy physics. There is a particularly strange sound to Anderson's advice to slow down theoretical research. To avoid distorting the author's meaning and giving his words a less attractive connotation, it is better to quote him directly: ''I do not advocate abandoning high-energy theory, just slowing it down in favor of a broader attack on the genuine problems we already have.'' In other words, the problems of high-energy physics are not ''the genuine problems,'' i.e., are not real or true problems. Here commentaries can only weaken the impression from the thought expressed by the author. We will return again to this original advice.

Anderson seeks a logical basis for his recommendations, in particular, in the postulate discussed by him of the absence of a hierarchy in science, and in his statement on the autonomy of science (''the sciences are autonomous''). Developing this thesis, Anderson argues against the statements of Weisskopf on the fundamental nature (in Weisskopf's terminology the ''intensive'' nature) of elementary-particle physics. Later we will draw in more detail on Anderson's thesis of the autonomy of the sciences in the light of the unique role played by high-energy physics in the family of sciences as the result of its direct and indirect influence.

We must further add to the list of questions to be discussed those questions which arise on reading the article by V. L. Ginzburg.[5] Following Ginzburg, we will speak of microphysics as a field including the problems of high-energy physics and even, somewhat more broadly, the problems of ''subnuclear'' physics.

Microphysics utilizes in its research not only the techniques of high energies. In physics and microphysics there are two directions of development which supplement each other. These are, on the one hand, high-energy physics and, on the other hand, the physics of beams of particles of relatively low energy but high intensity. These directions of research compete with each other in a certain region. Thus, the specific features of effects which are characteristic of the high-energy region appear weakly at low energies. However, the weak manifestations of these effects (small cross sections) can be detected in very high-intensity particle beams.

In high-intensity physics, specific regularities of phenomena also appear. High-intensity physics can lag behind high-energy physics by many orders of magnitude in the maximum energy of individual particles. Typical examples of high-intensity physics are the physics of laser beams, the physics of high-intensity electron accelerators in the MeV region, and the so-called meson factories.

Laser photon beams are interesting examples of high-intensity physics. Laser beams arose not in accelerator technology nor in high-energy physics nor as the result of the needs of nuclear physics and elementary-particle physics. However, the increase of the intensity in a laser beam extends its application to problems of controlled thermonuclear reactions and even to elementary-particle physics. At high intensities a laser beam and an accelerated electron beam, for example, are similar in many respects. These beams, so different in nature, turn out to be capable of competing with each other in the possibility of various applications.[2)] Competition is possible, in particular, in application to the problems of controlled thermonuclear reactions. Both a high-power laser beam and a focused beam, for example, of electrons can serve as sources of secondary beams of high intensity, for example, of neutrons, and as sources of high-energy charged particles—here we have in mind the production of particle-antiparticle pairs in intense laser beams[10] and the acceleration, for example, of protons by electron fluxes, in particular, in apparatus of the smokatron type. In contrast to high-energy physics, laser beams and high-current electron and proton accelerators—meson factories—have almost unlimited possibilities of practical application in technology, medicine, and the national economy.

Therefore it is expedient for various ministries of the government to play an important part in the financing of high-intensity physics. High-energy physics also returns with a high percentage to the national economy the money expended on it, but (as we will see below) not always directly and, as a rule, not rapidly. High-energy physics needs a long-term "loan" but at a high interest rate.

One of the rules here is that accelerator installations built for research in nuclear physics and elementary-particle physics begin to be used more and more for the needs of neighboring sciences: solid-state physics, chemistry, biology, geology, ecology, and so forth, and to a lesser degree for the needs of elementary-particle physics itself. At the same time, the next accelerator of higher energy is built for the needs of elementary-particle physics. The news is, for example, that one of the world's largest cyclic electron accelerators (the CEA in Cambridge) is converted entirely to use of its synchrotron radiation in various applications. It is well known that at the DESY accelerator in Germany the synchrotron-radiation channels are also used extensively.

These remarks make an important correction to Anderson's thesis that everything that is pro high-energy physics is con to something else.

In Ginzburg's article[3)] an attempt is made to answer the question: "What problems in physics and astrophysics are particularly important and interesting at the present time?" The author presents from various fields of physics about twenty problems, which actually present significant interest. However, our attention is particularly drawn to that section of the article headed "The Microphysics of Yesterday, Today, and Tomorrow." The author repeatedly excuses himself for the unavoidable subjectivity of his expressions and makes a great many stipulations, softening his formulation and not wishing to appear as an enemy of microphysics; he repeatedly emphasizes the avante garde role of microphysics in science and wishes it every kind of success, particularly in construction of new accelerators.

However, for the purposes of our discussion it is desirable to discard halftones so that the outlines of the questions raised by Ginzburg's article will show through more clearly, even though in so doing we go beyond the framework of the article cited. The fact is that these questions really exist and are a matter of common discussion. In fact, why is it not customary to discuss them openly in our own house—in the pages of physics journals? "Today, in comparison with yesterday, according to the opinion defended here," writes the author, "the place of microphysics both in physics and in the whole of natural science has radically changed."

These changes the author sees both in the reduction of the density of microphysics problems in physics journals and in the reduction of interest in microphysics on the part of the new generation coming into physics. The cause of these changes the author finds in the fact that up to the middle of this century the problems of microphysics "had a decisive importance, essentially, for the development of all of natural science." The objects studied by microphysics (the atom and the nucleus) were the "daily bread." To untangle the structure of the atom and to understand the laws acting in it (for this it was necessary to discover quantum mechanics!) meant to give a powerful impetus to many fields of physics, astronomy, chemistry, and biology. Roughly the same thing can be said about the nucleus—its study created the possibility of using nuclear energy and even provided the well known basis for calling the twentieth century the atomic age."

This was yesterday. But what about today? Today the object of microphysics has changed: "The particles studied by microphysics either live a negligible fraction of a second or, as in the case of the neutrino, penetrate the Earth with almost complete freedom and are caught only with colossal difficulty." In general the new objects of microphysics are "exotic and rare plants."

The object of microphysics has changed, and the value of the object of microphysics for other sciences has changed; the "social" position of microphysics, so to speak, has also changed, as well as its authority among the younger generation. Thus, among the questions posed in Ginzburg's article, it is desirable to discuss the question also in this formulation: "Exoticity" of the object of microphysics and its significance today for other sciences. And tomorrow? "The suggestion (which I do not hesitate to make) that, in a sense, the most brilliant period in microphysics is already behind us. Not everyone is obligated to believe in the existence of an infinite set of nested dolls (matryoshka)—when one doll is opened, another is inside, and thus indefinitely."

Thus, a new question appears in our list: Is there really a basis for the suggestion that the most brilliant period[4] in the life to microphysics is already behind us and that we have opened the last matryoshka?

It is true that "the problems confronting microphysics today are in no way inferior in their burning mystery and difficulty to the problems of an earlier day. In other words, microphysics has of course remained the forefront of physics, its most advanced and deepest part." It is unfortunate that the last thesis remains undeveloped in the article. One of the main problems of the subsequent discussion is to uncover as completely as possible the possible content of this thesis. Just why and in what sense can we consider that microphysics has remained the "forefront of physics, its most advanced and deepest part?"

Now, after a somewhat extended introduction, the time has come to turn directly to discussion of the situation in microphysics.

YESTERDAY, TODAY, AND TOMORROW OF MICROPHYSICS

It is very instructive to discuss the historical process of development of physics, a process drawn with very large strokes—a picture of a historical process, so to speak, from a bird's-eye view. In such a discussion there appears clearly an interesting feature of this process, the unique hierarchy of the laws which govern the world of physical phenomena as physical research is advanced to smaller and smaller regions of space-time in which the processes considered are occurring. In this historical process, each time when physicists have turned to study of phenomena in regions smaller by two to three orders of magnitude, there have been unmasked before them new worlds of physical phenomena with their specific laws.

A. The hierarchy of lengths – the hierarchy of laws. In physics there is a historically justified tendency to study phenomena in regions of smaller and smaller size. At the various boundaries of regions of length in the interval studied so far from 10^{-5}–10^{-15} cm, previously unknown new worlds of physical phenomena have been discovered. Thus, in the region from 10^{-5}–10^{-7} cm the world of molecular physics was discovered and the kinetic theory of matter was developed. In the region two orders of magnitude smaller, 10^{-8} cm ($\hbar^2/m_ec^2$), the world of atomic phenomena was discovered and the quantum theory was developed. Investigations in the region $\sim 10^{-11}$ cm ($\hbar/m_ec$) led to a new and unexpected group of phenomena associated with the possibility of creating electron-positron pairs, and the group of phenomena described by Dirac's relativistic quantum theory were uncovered. In the region $\sim 10^{-13}$ cm the world of nuclear physics was discovered. In the region $\sim 10^{-14}$ cm there were discovered the physics of hadrons and strange particles and the world of excited states of hadrons.

At the present time physics research is advancing into the region of lengths shorter than 10^{-15} cm. This hierarchy of lengths and the worlds of physical phenomena discovered in these lengths are clear from the table.

As can be seen from the table, the historical law is so far actually such that penetration into a region of physical phenomena two to three orders of magnitude smaller in size has led to discovery of a new world of

TABLE. Hierarchy of lengths – hierarchy of laws

l, cm	World of physical phenomena	Energy of accelerated particles
10^{-5}–10^{-7}	World of molecular physics	~ 1 eV
10^{-8}	World of atomic phenomena, atomic spectra	~ 10 eV
10^{-11}	Discovery of e^+e^- pair production; Dirac's quantum theory	~ 1–10 MeV
10^{-13}	Nuclear physics	
10^{-14}–10^{-15}	World of strange particles	$\sim 10\,000$ GeV lab $\sim$ ~ 100 GeV cms
10^{-17}	(Unfolding of the nature of weak interactions)	10,000 GeV lab $\sim$ 100 GeV cms
... 10^{-33}		... 10^{19} GeV

physical phenomena. So far the structure of matter, figuratively speaking, is actually illustrated by the well known toy—the matryoshka (nested dolls). We may of course raise the question how many layers does the real toy—matter—contain? Or is this process of opening the matryoshka infinite?

Of course, in such a general form the question will remain without an answer.[5] However, it is quite appropriate to ask whether we can actually state that physics has already uncovered the last matryoshka or, more specifically, can we expect a substantially new physics when lengths (impact parameters) two orders of magnitude smaller become available to us? In other words, what awaits physicists at lengths of the order 10^{-17} cm? Energetically this means that we are discussing accelerators with energies of ~ 300 GeV in the center of mass.

If we analyze the table of the succession of lengths and laws, we would have to say that the most important and interesting results in new stages of physics research turn out to be the unexpected and unpredicted results. Reality, as a rule, turns out to be more fantastic than any unrestrained fantasy.

In this striving toward physics of shorter lengths—to higher-energy physics—it is impossible to underestimate the great allure (so far historically justified) of the hope for meeting something new. However, we will not concentrate our attention on this perhaps purely psychological factor, although divorcing ourselves from it is also impossible. In the present case at the boundary of the lengths which are forthcoming, namely lengths of the order 10^{-17} cm, we can confidently speak of great expected progress in our knowledge. The fact is that just this length boundary is already organically contained in the current theory of weak interactions as a length boundary having fundamental significance. The dimensional constant determining weak interactions is characterized by the square of a length l^2, where l is close to 10^{-17} cm. In any case we can say with confidence that at these lengths we will obtain an answer to one of the most intriguing questions of contemporary physics, namely: What is the nature of weak interactions? What is the so far undiscovered secret of weak interactions?

It is well known that the cross sections for weak interactions increase with increasing energy of the interacting particles. The coupling constant for weak interactions is small and therefore perturbation theory is used in weak interactions—the expansion in the weak-interaction parameter. Since the cross sections increase with increasing energy of the interacting particles, it turns out that at high energies, in spite of the smallness of the coupling constant, the next higher approximations of the theory become comparable with or even greater than the lower approximations. There is no other method, different from perturbation theory, which is available to us so far. Attempts to construct a more refined formalism for calculation of weak-interaction cross sections encounter a fundamental difficulty in weak-interaction theory due to the existence of diverging quantities which cannot be removed by the means (renormalization) which turned out to be effective in electrodynamics.

In general we do not know how weak interactions behave for collision parameters close to the length which characterizes weak interactions, a length of $\sim 10^{-17}$ cm, which we discussed above, or at energies close to 300 GeV in the center of mass. In other words, we have a real fundamental problem for accelerators with energy ~300 GeV in the c.m.s. Such an accelerator (~300 GeV in the c.m.s.) we will designate briefly in what follows as the unitary-limit accelerator.[6)] There are reasons to suppose that this problem may be associated with another problem of weak interactions. Specifically, from the time of Fermi (1934), weak interaction theory has been formulated as a four-particle interaction: in β decay the neutron decays into a proton, an electron, and an antineutrino. All other interactions known in nature are exclusively three-particle interactions. Thus, a neutron, on emitting a π meson, is converted by strong interactions into a proton, and so forth.

For about thirty years there has been a tendency to reduce four-particle weak interactions to three-particle. This can be achieved if we assume that the observed weak interaction occurs in fact in two steps. First the neutron is converted to a proton, emitting some hypothetical particle the W meson (a three-particle interaction), and this so-called intermediate W meson then decays into an electron and an antineutrino (a second three-particle interaction).

The idea of unification of the types of interactions is so attractive that, in all new accelerators and in all new energy ranges, again and again experiments are set up to search for the intermediate W meson. At the present time the W meson has not yet been observed in the operating accelerators.

The lower limit for the mass of the intermediate W meson still lies in the region 2–5 GeV. The energy of 300 GeV in the center of mass (the unitary-limit accelerator) is the limiting energy up to which the idea of the intermediate meson and the experimental search for it make sense. In this sense, weak interaction theory is subjected to a decisive test in the unitary-limit accelerator. Below we will extend the discussion of various aspects of the forthcoming stage of microphysics to the epoch of realization of the experimental possibilities of the unitary-limit accelerator.

B. Are there reasons to suppose that "the most brilliant period in the life of microphysics is behind us?" The rapid sequence of striking discoveries in the twenties and thirties of new laws of the microworld and of the diversity of elementary particles and their properties in the subsequent decades have in a sense corrupted our perception and estimation of the rapidity of scientific progress. Our expectation of new scientific discoveries has become somewhat impatient. There have been reproofs and even a certain discontent with the rate of new discoveries. The attempt has been made to establish almost a law of nature according to which, with the appearance of higher energy accelerators, the rate of new discoveries in these new energy regions radically slows down.[8]

The fact is that the hierarchy of lengths which we discussed above and the energies corresponding to them must be calculated in the center of mass. The energy in the center-of-mass system E_{cms} is related to the energy in the laboratory system E_{lab} quadratically:

$$E_{lab} = \frac{E^2_{cms}}{2M_p c^2},$$

where M_p is the proton mass.

Beginning with the time of construction of the first cosmotron with an energy of 3 GeV (1953) and ending with the operation of the Serpukhov accelerator with an energy of 75 GeV, at the present time (1973) the center-of-mass energy has increased and the corresponding lengths have decreased in the ratio $\sqrt{3/75} = 1/5$, i.e., only by a factor of five. Thus, from the point of view of the hierarchy of lengths and laws discussed above, during the last twenty years we have been experimenting roughly in the same region of physical laws. It would be desirable for this important fact always to be kept in mind when the situation in high-energy physics is analyzed.

It is more worthy of astonishment how many new things have been discovered and are being discovered in this comparatively narrow region. It is not excluded that one of the important experimental achievements of high-energy physics of recent years is the study of the cross sections for deep inelastic interactions in scattering, in particular of high-energy leptons (neutrinos and electrons) by nucleons.

Kendall and Panofsky have written "Sixty-five years ago Ernest Rutherford observed how alpha particles are scattered by thin metal foils and concluded that the atom is not a homogeneous body but consists of negatively charged electrons surrounding a small, massive, positively charged nucleus ... Recent investigations with electrons brought to an energy of 21 billion electron volts by the two-mile accelerator at the Stanford Linear Accelerator Center (SLAC) strongly suggest that history may be repeating itself on a scale 100,000 times smaller than that of the atom. It turns out that ultrahigh-energy electrons are scattered by protons and neutrons in ways that no one had predicted. The tentative conclusion is that the nuclear particles have a complex internal structure consisting of pointlike entities now called partons."[11] Although the interpretation of these experiments is in no way so unique as set forth by Kendall and Panofsky[7)], nevertheless the new idea that nucleons are constructed of particles new to science (partons) arose, and will survive and be tested in future experiments. Of course, it is not excluded that it will be experimentally confirmed.

In the present case we are discussing the opening of the next matryoshka almost in the literal sense of this figure. It is true that, in contrast to the situation with weak interactions, contemporary theory does not contain a specific length with which the existence of the new structural units would be associated. If such particles exist, we cannot say which generation of accelerators will be required for their observation. It is still not excluded that direct data or substantial indirect data favoring the existence of such particles can be obtained in accelerators now in existence or in the accelerators of the next generation.

It must be emphasized that in recent decades a fundamentally new idea has appeared regarding the structure of material particles. While prior to the recent decades the history of human culture was dominated by the idea that, crudely speaking, particles of large mass were constructed of particles of smaller mass, in the last decades an idea has arisen which in some sense opposite to the old idea and, as it has turned out, is an obvious idea. The idea has appeared that particles of smaller masses are constructed of particles of larger mass, between which the strong interaction leads to a corresponding mass defect of these systems. Here we have in mind the attempts to construct π mesons from μ mesons (Wentzel), π mesons from nucleons and antinucleons (Fermi-Yang), hadrons from aces and quarks (Zweig and Gell Mann) and, finally, Feynman's partons.

In connection with this modification of the fundamental conception of the very nature of the structure of matter, which in fact can be considered perhaps as one of the most radical revolutions in our representations of the structure of matter in the entire history of physics, the question arises: Can there be any reasons for the existence in nature of a heaviest possible particle, which would be a structural material of limiting mass for all particles? It is interesting that we can construct from the universal constants the masses of the entire group of particles which are neighbors in mass and which might assume this role. From the constants e (electric charge), g (mesonic charge), $\hbar$ (Planck's constant), c (the velocity of light), and κ (the gravitational constant) we can construct the following quantities which have the dimensions of mass:

$$M=\frac{e}{\sqrt{\varkappa}}\sim 10^{-6}\,\mathrm{g},\quad \sqrt{\frac{\hbar c}{\varkappa}}\sim 10^{-5}\,\mathrm{g},\quad \frac{g}{\sqrt{\varkappa}}\sim 10^{-5}\,\mathrm{g}.$$

It is curious that the masses of this group of particles of the maximum possible mass (shall we say maximons) which can be constructed from the universal constants all lie in the narrow interval: 10^{-5}–10^{-6} g. The corresponding lengths ($\hbar/Mc$) lie in the region 10^{-32}–10^{-33} cm. From the point of view of the hierarchy of lengths, these lengths must be in the lowest line of the table. Evidently 10^{-33} cm is the very last length in the list of fundamental lengths. At this limiting length the very concept of distance loses its meaning in general, as the result of quantum fluctuations of the metric. From this point of view these lengths and the particle masses corresponding to them must actually be considered as limiting.

However, the interest in maximons as a possible structural element lies in the fact that with these masses and distances, as it turns out, only gravitational forces are sufficient for formation of systems with the desired mass defect. It is possible that between the "weak" length (10^{-17} cm) and, say, the gravitational length (10^{-33}–10^{-32} cm) there exist a series of hierarchies of lengths which control their specific worlds of physical phenomena. However, in terms of existing physical ideas and known universal constants there is not yet any place for any other lengths. It is quite possible that these hypothetical particles are not stable in a free state.[8]

Let us turn now to the statement that the most brilliant period, in some sense, in the life of microphysics is already in the past. This assertion does not follow logically from any substantiated premises; it is simply one of the pointedly formulated controversial (and, we must say, often discussed) themes. Incidentally, questions of this type have already arisen repeatedly in the history of science, and it is instructive to recall them.

At the end of the last century, as witnessed by Millikan, it appeared that all of the great discoveries in physics had already been made and that further progress would consist not in discovery of qualitatively new phenomena but rather in the more accurate quantitative measurement of already known phenomena. This dominant social opinion of the epoch is formulated by Max Planck's teacher Phillipp von Jolly somewhat more figuratively but essentially in the same words: "Of course, in some little corner or other we may yet notice or extract a speck of dust, but the system as a whole stands firm, and theoretical physics is clearly approaching that degree of perfection which characterized geometry a century ago" (see ref. 14). We now take as a historical anecdote the words: "All great discoveries of physics have already been made," which were spoken only ten years before the discovery of the theory of relativity and quantum mechanics.

Of course, this historical excursion is in no way any proof that the most brilliant period of microphysics is not behind us. The purpose of this excursion is only to argue in favor of greater caution in our statements. However, a real (in a certain sense) and, it appears, rather convincing answer to this question can be obtained from analysis of the as yet unsolved problems which confront microphysics. Of course, in the case of observation of more fundamental structural elements of the type of quarks or partons, there would arise before us a really new and, in a definite sense, brilliant period of science. However, this hypothesis may not be realized. There are, nevertheless, problems whose solution will unconditionally comprise a new and possibly very brilliant epoch in science. These problems, as it appears at the present time, may be directly related to the experimental possibilities of the generation of accelerators being discussed.

Strange as it may seem, in the very large view our understanding of physics has not progressed very far from that of the ancients. This statement sounds like a paradox, but it is just in the large view that it is valid. In fact, while the ancient Greeks considered four elements as fundamental entities: earth, water, air, and fire, not understanding, as would now say, the fundamental properties of these elements, on the other hand contemporary physics attempts unmask the entire content of the real world as a complex interaction of various "fields." These are the same four elements of the ancients: strong, electromagnetic, weak, and gravitational fields. And like the ancients, we are still far from understanding the fundamental properties of these elements of physics of the twentieth century. In other words, our attempts to describe the properties of these fields in their individuality—electrodynamics, weak interactions, and so forth, each in their own right—turn out to be unsubstantial.[9]

We do not understand and cannot quantitatively describe the spectrum of so-called elementary particles arising in the interactions of these "elements." We long ago reached the practical conclusion that investigation "to the end" of each of these interactions in its individuality is impossible. In high-energy physics there is always a point at which other interactions also begin to play a part in the behavior of a given effect; this means that it is impossible to extract from nature as a whole one of its elements without destroying all of the remaining elements. We are accustomed to the idea that nature is built without architectural excesses. In essence, we long ago arrived at the conclusion that there is a unity of nature. However, we cannot describe the unity of the four "elements," although we are attempting to do so by various means. Faraday was able to establish a fundamental connection between magnetic and electronic phenomena. However, Einstein was unable to unite the gravitational and electromagnetic interactions into a single picture. Heisenberg, on the basis of some fundamental Ψ field, was unsuccessful in understanding any aspects of this unity. However, we are attempting and will attempt to understand the fundamental unity of the "elements." At the present time the idea of "symmetry violations" has arisen. In this idea there is a glimmer of the possibility of creating a unified theory of weak and electromagnetic interactions.

We are still talking not about a definite and concrete theory, but rather about some strategy in the attempts to construct such a theory in terms of the universalization of three-particle interactions. In this idea a natural place is found for the idea of the intermediate boson, and its mass is given by numerical values not very far from the energy value of the same unitary limit.[10] In the next generation of accelerators we will be dealing with just such energies. The ideas being developed in the theory of weak and electromagnetic interactions are also one of the strong arguments in favor of the high-energy accelerators which have been built and for building the next generation of accelerators. It should also be recalled that not only leptons but also hadrons possess weak interactions; therefore it has already become clear at the present time (various specific versions of the theory show this) that a systematic scheme of this type should include a unified theory of weak, electromagnetic, and strong interactions. There are also serious reasons to suppose that the regularizing role of the gravitational field may be one of the most important aspects of this idea. Everything that has been said is an important argument in favor of the opinion that the "brilliant period" of microphysics is likely to be still ahead of us.

In what follows we will again repeatedly return to the justification of this thesis, in discussing certain problems of the physics of the future.

C. The place of microphysics in the hierarchy of sciences. The influence of microphysics on other sciences and on technological progress. We hardly speak seriously of any hierarchy of sciences in the formal meaning of this word in general. There is hardly any meaning to the question: What is the hierarchical relation of microbiology and microphysics or of microphysics and sociology? However, in terms of physics or, more accurately, in terms of the science, say, of nonliving nature, we cannot but agree with Ginzburg that microphysics is "the forefront of physics, and its most advanced and fundamental part." The relation of microphysics to other sciences is characterized by many specific features organically present just in microphysics and by the uniqueness of its development.

Ginzburg's article enumerates a large number of important problems confronting physics and astrophysics. However, as a rule, these problems are particular problems. One of the characteristic features of these problems is that almost every one of them, while very significant and interesting at the present moment, can with further study lose a major part or all of its significance and interest and be crossed off the list of "titled" problems. At the present time, for example, the problem of heavy water has been crossed off this list. Metallic hydrogen cannot possess properties which would be convenient, for example, for use in high-temperature superconducting technology.

The very tempting idea of searching for and creating high-temperature superconductors may turn out, for example, to be unrealizable for fundamental physical reasons. It may happen that in nature there are no relatively stable transuranium elements. The most promising method for achieving thermonuclear reactions may turn out, for example, not to be the laser method but, as has been suggested by some, the electronic or even, in a certain sense, the traditional thermonuclear method.

This does not mean that the problems enumerated do not present interest. If high-temperature superconductors were found, a genuine revolution in technology would occur.

Here it is desirable to emphasize the difference between particular physical problems and the general problem of microphysics—investigation of physical phenomena in smaller and smaller regions of space-time. This is a general problem of a universal nature; it has an absolute value, independent of the result of the investigation: it is necessary to know the nature of the world of physical phenomena in these regions, and this thirst for knowledge presents for mankind a goal toward which it will always strive. This is the same natural struggle which leads us to study the ultramicroscopic depths of the Universe, to astronomy and astrophysics.

However, disregarding these general statements and returning to the specific theme of our article, we can only repeat that in the accelerators of the present and next generations, in any case at lengths of $\sim 10^{-17}$ cm, we will unmask the secret of the nature of weak interactions. And whatever it is, i.e., whether it turns out that the true interaction is similar to electrodynamics (three-particle) or that the true interaction is the weak interaction (four-fermion interaction), either result will be fundamental and will be accompanied by a fundamental change in the level of our knowledge of nature.

We have already mentioned that from the very moment of origin of the four-fermion formulation of the theory of weak interactions it seemed attractive to reduce the four-fermion interaction to a three-particle interaction similar to electrodynamics and the other known interactions. The fact that weak interactions in this theory turned out to be interactions of a special type, so to speak, a white crow (rara avis) in the family of all interactions, made the idea of unification of all interactions natural and very attractive. If it is found in the experiments being discussed that weak interactions retain their four-fermion uniqueness, then the directly opposite idea of the structure of interactions[11] will be attractive. From the very moment of appearance in physics of such quantities as spinors, it was known that from spinors it is possible to construct objects with different transformation properties—vector, tensor, scalar, and so forth. In its time the idea of the neutrino theory of light arose in this way: to construct the electromagnetic field vector from two spinors describing neutrinos. From the first appearance in physics of spinors there arose and has remained with us the idea of the fundamental nature of spinor fields, which perhaps determine structurally all other fields. Thus, experiments in the energy region discussed, in case of confirmation of the truth of the four-fermion interaction, will unconditionally bring to life this idea, which is also extraordinarily attractive in its own right. We see, therefore, how important a threshold for the further development of the science of nature will be crossed with experiments in this specific energy region under discussion.

In recent years there have been great advances in astrophysics: observation of the residual radiation, new astrophysical objects such as quasars, pulsars, neutron stars, and perhaps black holes—these achievements have attracted great attention in the scientific community. We can even find the statement that astrophysics should be given preference over other physical sciences at the present time.[6] There is no question that the progress in astrophysics has been great and that not enough material means and attention have been actually devoted to it. At the same time, however, astrophysics has become an experimental science to a greater degree than before. This is due to the fact that, as Ginzburg correctly states, astrophysics is being investigated at all wavelengths. While astrophysical studies were previously carried out only in the optical region, the development of radio astronomy, on the one hand, and x-ray and γ-ray astronomy, on the other hand, extraordinarily extends the experimental possibilities of astrophysics and the obtaining of new information from the cosmos. And ahead of us is the extremely promising neutrino astronomy and the astrophysics of gravitational waves.

Unquestionably in recent years important results have been obtained and major discoveries made in astrophysics. However, the most brilliant discovery in astrophysics is nevertheless, perhaps, not a discovery of recent years. Rather it is apparently the discovery which led almost half a century ago to the model of a nonstationary expanding Universe with a metric, perhaps, of the Friedman type.

In comparison with this discovery, the discoveries of quasars, neutron stars, and black holes, of course, are less impressive discoveries. If we dare to show a certain lack of caution in our statements, can we express the opinion that the most brilliant period in development of astrophysics is perhaps already behind us? However, we do not wish to take upon ourselves the role of a prophet. In addition, it is not without interest to note that astrophysics is in a certain sense becoming closer to microphysics. Neutron stars are essentially grandiose atomic nuclei and in a certain stage even hypernuclei. Neutron stars are a macroscopic form of nuclear matter.

On the other hand, the global properties of black holes are widely discussed at the present time, and it looks very much as if this state of matter must be taken into account in construction of a systematic theory of elementary particles.[12] Astrophysics, or more accurately the general theory of relativity, permits in principle the existence of such objects with an almost closed internal metric as friedmons.[13] This possibility makes the very concepts of macro and micro relative. There are reasons to suppose that the final stage of collapse of stars is a problem of microphysics. In fact, while the long-range Coulomb forces can in principle stop the collapse, the forces due to exchange of heavy vector mesons play the same role at a distance of $\sim 10^{-13}$ cm as long-range repulsive forces. The density of a collapsing star in such a small volume is of the order 10^{72} g/cm^3, i.e., twenty orders of magnitude smaller than the so-called quantum density (10^{94} g/cm^3) where, it is suggested by some, the collapse could be stopped by some as yet unknown peculiarity of quantum phenomena.

Finally, what can be said about the Universe in the initial moment of its development, when it was localized, let us assume, in a region $\sim 10^{-13}$ cm?[14] It is quite unclear whether such an object belongs to the regime of macrophysics or microphysics.

What mystery still surrounds this moment of the initial explosion! What kind of unexpected things can arise as our representations of physical laws change when we come to understand the physics of this event—can this be the most brilliant stage in the history of astrophysics, and perhaps of microphysics? These remarks represent important corrections to Anderson's postulate of the autonomy of the sciences.

Of course, high-energy physics or, generally, microphysics is not the hierarchical basis of all sciences. In fact, any isolated result of high-energy physics cannot have any relation to biology, chemistry, sociology, or philosophy. Nevertheless, the direct and indirect influence of the entire developing field of microphysics on all of science as a whole is greater than that of any other specific scientific field. It is very important to note also the indirect effect of current fundamental studies in high-energy physics on science and technology in general. The fact is that this field of research is accompanied by appearance of fundamentally new and highly refined physical apparatus, and often of fundamentally new technology which finds application in many fields of science and industry and in the national economy as a whole and exerts substantial influence on technological progress. The scales of this influence have not been adequately studied, and it is awaiting investigation. Let us recall the major role of accelerators in the various fields of science, medicine, and national economy.

Multichannel analyzers were developed as experimental equipment for microphysics, and what broad application they have found now in various fields of science! How can we take into account the gain to the world's economy from those refinements introduced by experimental microphysics in use of electronic computers? At the present time synchrotron radiation is beginning to be widely used in various wavelength regions in chemistry, solid-state physics, and biology. It is unknown what discoveries in biology may result from the new wealth of possibilities in study of short time intervals in cells by means of synchrotron radiation in the angstrom region. Still earlier, the electron microscope was a "gift" to biology. Solid-state physics in the synchrotron-wavelength range 10–2000 Å is still awaiting development.

We can state, for example, that the next generation of accelerators will be built with superconducting technology. The last in the course of development of accelerators of this generation will receive further development which will exert a substantial effect on the applicability of superconducting technology in many fields of the national economy. We mentioned above the indirect influence of experimental high-energy physics on other sciences and on technical progress. No less important and worthy of attention, however, is the fact that the theoretical apparatus and formalisms which have been developed, it would appear, in terms of the requirements of elementary-particle physics, have found and will find, as is well known, brilliant and effective application to other fields of physics, in particular to solid-state physics. Can we once again recall Anderson's suggestion that theoretical research in high-energy physics be slowed down? Very unenviable is the role of that personage from a fable who, for reasons, as we would say, of the narrowness of his outlook, uttered the well known sentence: "So long as these are acorns! The statement that the wide practical application of the effects of microphysics itself is already exhausted is not a logical conclusion from any indisputable premises.

When the practical application of nuclear physics in the past is discussed, one usually speaks of the utilization of the energy released in nuclear reactions. However, this is of course only about 1% of the entire energy contained in matter. From the time when the relation $E = Mc^2$ appeared in science, utilization of the total energy of matter has been a tempting problem. I already hear "the cries of the Boeotians" and a sentence like that in the Rutherford speech mentioned earlier. Well, we know history, we know how unsuccessful has been the prediction of the most authoritative scientist. We do not yet actually know the means of using this energy, but does this lack of knowledge mean the impossibility of its use even in the future? In any event an unused store of energy exists in nature. In the future, probably, means will be found to preserve for a long period appreciable quantities of antimatter—the fuel with the maximum possible available heat.

According to current ideas, the energy released by the Sun arises as a result of nuclear reactions with emission simultaneously of an intense neutrino flux. This neutrino flux has not yet been observed. If a tenfold improvement in the experiment does not lead to observation of solar neutrinos, we will be forced to search for nontrivial explanations of this phenomenon. Opportunely, theoreticians have already prepared one of the nontrivial possibilities for this case. It arose from the attempt to explain the so far still hypothetical effect of violation of so-called combined parity in the decay of the K_L^0 *meson into two* π *mesons*. In one version of the theory developed at Serpukhov, violation of energy conservation occurs. It turns out that this violation of the law of conservation of energy is sufficient for the observed energy release of the Sun without emission of the expected number of neutrinos. Of course, proceeding from a healthy scientific conservatism, we should "morally" resist this extraordinary possibility ("the ecology ..."); well, but if...

Unfortunately, by definition, we cannot say much about the future possibilities of science. We cannot speak about that which we do not yet know. Usually that which is most important and significant in a new field of research (as history teaches us) is that which is unexpected and unforseen. An important argument in favor of microphysics lies in the fact that just here is the unexpected the most probable. It is impossible to foresee what practical applications will arise on the basis of future research in microphysics. Any negative judgements would be still more unfounded.

Those phenomena which do not find explanation in terms of established ideas usually have far reaching consequences. We actually do not know to what we will be led by understanding the still not understood situation with violation of combined parity in the K_L^0-meson decay. We do not yet know what is hidden in the not yet understood situation of the absence of the expected flux of solar neutrinos. We are still continuing in these cases to think in terms of our customary concepts. It is not excluded that we will again enter the microworld with macroscopic impoliteness, "in our overcoat and galoshes".

"At each historical stage some one of the scientific disciplines belonging to the broad field of natural science will step forward on the stage and become the symbol of scientific progress."[6] As follows from what has been said above, there are definite reasons to expect that at lengths close to the unitary limit (300 GeV in the center of mass), high-energy physics will again take this step forward on the stage. We would like to emphasize once again that this article in no way has as its purpose to prove the necessity of building high-energy accelerators in general. We are discussing accelerators of a definite energy, namely ~300 GeV in the center of mass[15] with complete defined problems[16], and here the cost is certainly justified.

Thus, for the second time in a quarter century there arises a mission-oriented accelerator project whose construction is justified beforehand by the expected results.

Will it be necessary then to build accelerators of still higher energy? This remains an open question; at the present time there are no specific arguments supporting such a statement. It is not excluded that accelerators of this generation will turn out to be the last (of the highest energy) in the history of accelerator technology. In this article we do not discuss what type of accelerator is to be considered preferable for this energy: traditional or colliding-beam, and in the latter case—proton-proton or proton-antiproton, electron-proton or electron-positron. It is true that electron-proton colliding-beam accelerators are very tempting for a number of reasons. However, discussion of the specific form of an accelerator of this generation is a special question. It requires extensive and painstaking work.

THE INCREASE IN COST OF SCIENCE

One often hears the statement that high-energy physics has become expensive. This is true, However, a more general statement can be made, unfortunately, namely that all sciences are becoming more expensive. The point is that we have gradually entered an era, if we can use the expression, of the industrialization of science. Nuclear physics was the initiator and the first object of serious industrialization of its experimental base, and this was a precedent for creation of large-scale installations in essentially all fields of science. Atomic physics was the first to overcome the purely psychological barrier of the customary modesty of experimental appratus intended for scientific research. The practice developed of building apparatus on a scale which simply did not exist previously. The main point is that the real possibility and desirability of such industrialization has been demonstrated. Of course, we are actually discussing here not the psychological effect but the real progress in development of science. Science in the broad sense of the word has become more than ever before an essential element of technological progress, and high technological progress industrializes science. Space science long ago and substantially surpassed high-energy physics in its cost. In many other fields of science the need for large expenditures is also rising rapidly. It has now become obvious that creation in a country of an accelerator with maximum parameters at the present time is not simply the organization of one more institute, but is the appearance of a new national and in many ways international center in high-energy physics.[17] Such a center coordinates the scientific activities of the numerous scientific institutes which take part in the work of the center.

The organization of such a center in almost any field of science requires substantial expenditures which tend to produce a substantial qualitative rapprochement of the various fields of science. Thus, in the August 1972 issue of Physics Today a program is published for the proposed financing of astrophysics research in the coming decade. This program was developed by a special committee headed by J. Greenstein. The total cost of this program is estimated as something like eight hundred million dollars. The value of a single radio telescope which it is proposed to complete in 1980 is expressed as close to eighty million dollars. As another example, a national center in solid-state physics which is to assume a leading scientific role must include a rather large research reactor, various types of accelerators including an accelerator providing useful synchrotron radiation, and a bank of modern computers; this equipment, together with the initial construction of the center, will require an expenditure of around one hundred million rubles.

A national center for thermonuclear research with the several approaches—traditional, electronic, laser—will require at least one hundred to two hundred million rubles just for the first stage of its development.

A well equipped national biological center (the latest type of centrifuges, electron microscopes, and appropriate accelerators including those providing synchrotron radiation in the necessary range, a bank of computers, and so forth) in the course of several years will require a budget of the order of one hundred million rubles.

Construction of a contemporary national center, for example, for cancer research, equipped with the most advanced means of π-meson therapy and computer diagnostics, will require an expenditure of the same order. An accelerator of the next generation—the unitary-limit accelerator, will require up to 1990 an expenditure of no more than is planned for development of astrophysics in the U.S.A.

We sometimes hear the statement that accelerators of the next generation must be built not by expending large sums of money but, figuratively speaking, by using "gray matter," finding new, nontraditional possibilities in accelerator technology. Of course, it is necessary to search for new possibilities in accelerator technology, and this is being done. However, the experience in high-energy physics demonstrates that what is built around an accelerator, including the technology necessary for the experiments, requires expenditures greater than the cost of the accelerator itself. Thus, if in the future we have the luck to find a means of reducing the cost of the accelerator itself to zero, the cost of creating the entire center will still not be cut in half. Evidently we are actually talking about reducing the total cost of building a center by a rather small percentage, which hardly has basic significance for the problem being discussed.

Dyson's advice[4] to replace research using accelerators by research in cosmic-ray physics is not useful advice, and the arguments presented by Dyson, although valid in many ways, are for the most part unsound.

Cosmic-ray research has produced many valuable and important results for high-energy physics. Dyson correctly emphasizes this merit of cosmic-ray physics. His advice to increase the activity in cosmic-ray physics research is also valid. The fact is that the representatives of accelerator high-energy physics have underestimated and continue to underestimate the results and possibilities of cosmic-ray physics. This underestimation and sometimes disregard of cosmic-ray data is often the result of a lack of acquaintance with them. It also must be said that it is due in part to the qualitative thinking of cosmic-ray specialists, which is unfamiliar and strange to accelerator technology: It is often necessary to draw a conclusion on the basis of an incomplete set of rather inaccurate data, although in many cases these conclusions have turned out to correspond to reality.

On the other hand, it is just the strictly quantitative nature of the data obtained in accelerators which has "demoralized" the cosmic-ray physicists, who previously worked in the same energy region. As a result a peculiar inferiority complex arose among cosmic-ray physicists; this complex has delayed the industrialization of this field of research.

The cheapness of cosmic-ray physics emphasized by Dyson belongs to the past. If we seriously occupy ourselves with cosmic-ray physics (with the intention of obtaining results), it will cease to be cheap. Extra-atmospheric cosmic-ray physics will require construction of well oriented orbiting stations. A certain modest proportion of such a program was planned in Greenstein's report, together with extra-atmospheric x-ray and γ-ray astronomy, to require a total sum of three hundred and eighty million dollars. However, an even more refined and more expensive program of extra-atmospheric cosmic-ray physics cannot carry out the same program of research which is expected to be within the possibilities of the next generation of accelerators. Individual subject areas of cosmic-ray physics performed on the Earth's surface, of course, present great interest in this energy region from the point of view of those problems which do not overlap accelerator high-energy physics in the present decade. They could play a role of qualitative and semiqualitative indicators for accelerator physics of the next generation.[18] However, in this case the cost of an experiment increases significantly as the result of the need to carry out a substantial industrialization and a corresponding increase in the scale of the experiment.

Thus, the myth of the cheapness of contemporary experiments in scientific fields not associated with high-energy physics is gradually disappearing. The rise in the cost of a contemporary experiment has the same cause as the rise in cost of an airliner in comparison with the cost of the most luxurious passenger coach at the end of the eighteenth century.

However, the possibility of such expenditures in various regions of the national economy rests on the national income, whose continuous rise is due to technological progress and, in the last analysis, to science itself. At the same time it must be recalled that the total amount of the world's expenditure on science amounts to only a small part of the world's budget.

The availability of funds for development of science is still to a major degree somewhat like the patronage of art—it is not always determined rationally by the intrinsic requirements of science, but by how much it is "possible" to expend on science, among other expenditures. The term "possible" contains much uncertainty and chance.

We are not discussing the arbitrary division of a previously specified and, realistically speaking, not very large "pie" (to use Anderson's words), but of what is natural, expedient, rational, and therefore in the last analysis advantageous for the national economy, i.e., that the appropriation of funds for development of science should be in reasonable correspondence with the intrinsic and natural requirements of the various scientific fields.

If this does not occur, the main fault of us, the scientists, will be that we could not further convince the contemporary world and society of the need, not of patronage, but of a rational and truly planned approach to financing of science.

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1)"a) Don't try to revive past glories. b) Don't do things just because they are fashionable. c) Don't be afraid of the scorn of theoreticians."[4]

2)At very high intensities of any particles (photons, neutrons, electrons, protons, and so forth) a common property is the ability to transfer to a small volume of material (in the limit to a pair or even a single particle) huge amounts of energy.

3)Note that this article[5] has recently been revised and extended by the author (it is printed as a separate brochure under the title "On Physics and Astrophysics" by the Nauka publishing house) we cite the article[5], since it is known to the reader in this form.

4)In speaking of the brilliant period in the life of microphysics, Ginzburg has in mind mainly its applications, atomic energy, and its social significance. Here we must excuse ourselves for some inaccuracy in presenting the author's point of view. When we meet with such expressions, we must discuss them appropriately.

5)A more detailed discussion of the problem is contained in the author's article "On the Concept of Primitive Matter", Voprosy filosofii (Problems of Philosophy), No. 4, 66 (1970).

6)In perturbation theory it is assumed that because of the weakness of the interaction the initial state of the system is not changed, in other words, unitarity is taken into account with an accuracy to the next approximation. Unitarity is violated in the strongest possible way if the next approximation turns out to be equal to or greater than the preceding one.

7)The fact is that this behavior (an increase) in the scattering of leptons by nucleons, accompanied by multiple production of particles, was actually suggested several years before the performance of the SLAC experiments. The suggestion was made[12] and several arguments were presented in favor of the existence of the following theorem: $\sigma^{\text{form}}_{\text{tot},\ E\to\infty} \geqslant \sigma_0$, where $\sigma^{\text{form}}_{\text{tot}}$ is the total cross section for deep inelastic scattering with inclusion of the form factors in each of the channels, and σ_0 is the cross section for elastic scattering by a point particle. It was shown not so long ago by Bogolyubov, Vladimirov, and Tavkhelidze[13] that the existence of this theorem is compatible with the formalism of contemporary theory with rather mild stipulations. However, we are struck by the fact that the observed effects are far from asymptotic. It is therefore not excluded that they lie outside the framework of traditional theory. In other words, the possibility of interpreting the effect with the aid of existence of some kind of sub-particles (partons or quarks) is still not excluded.

8)The statement that microphysics at the present time has as its object "rare exotic plants" or short-lived particles, which have no direct relation to the "daily bread" or to that form of stable matter in which we exist, is not completely accurate. In the first place, the stable particles – protons, electrons, photons; neutrons and complex nuclei – remain as before the objects of study in all energy regions. The most impressive results of high-energy physics (deep inelastic processes) are related just to the interaction of such particles (protons and electrons). Furthermore, a particle with a short life in the free state, like a neutron, in a bound state is stable and enters into the structure of stable nuclear matter. Evidently such short-lived particles as hyperons are structural elements of collapsing stars in their post-neutron stage. In addition, if it turns out that partons or other similar hypothetical particles actually are structural units of matter, then it is possible that just short-lived particles unstable in the free states (exotic particles) will be the "daily bread" which is discussed in Ginzburg's article. We cannot state that such will in fact be the case, but this possibility cannot be excluded.

9)At the present time there have appeared arguments that the intrinsic mass of the electron apparently cannot be of electromagnetic origin. The point is that in a number of investigations a more accurate expression has been found for the electromagnetic mass of the Dirac electron. This solution of the "superconducting" type[15] is not expanded in powers of the fine-structure constant. In contrast to the well known logarithmic dependence, it returns the expression to the classical linear divergence, but with a very small factor characterizing the vacuum polarization:

$$\Delta m \sim (e^2/rc^2)\, e^{-3\pi\hbar c/2e^2}.$$

A mass of the order of the electron mass arises only with a cutoff in the lengths $r \sim (m^2/m_e c^2)e^{-650}$. This length is many orders of magnitude smaller than the length at which the concept of length loses its meaning as the result of quantum fluctuations of the metric. Further, the expression for Δm automatically is made finite when the gravitational defect of a given mass, concentrated in a given region, is taken into account. The finite electromagnetic mass of the electron turns out to be of the order $\sim\sqrt{\hbar c/\kappa}\, \exp(-3\pi\hbar c/2e^2)$, i.e., the electron mass cannot be of electromagnetic origin (more accurately, of electrodynamic origin) if these calculations are correct. It is true that the expression for Δm changes substantially if we take into account the existence of μ mesons, protons, and possibly other charged fermions, but in that case we are going beyond the framework of electrodynamics.

10)In one version of this theory, for example, a value of ~ 40 GeV is given for the mass of the neutral intermediate boson, and ~ 80 GeV for the charged intermediate boson.

11)That is, the idea that *all* interactions have a four-fermion structure.

12)The last remark is due to the fact that the current theory of elementary particles permits intermediate states with arbitrarily high energy. The total mass of an intermediate state can be greater than the mass of any cosmic object or even system. However, at the same time, in violation of any logic, current theory ignores the gravitational interaction of these masses in these states. We are struck by the fact that, if there turns out to be a mass of the order $M \sim \sqrt{\hbar c/\kappa} \sim 10^{-5}$ g in the intermediate state, then the gravitational radius of the mass $r_{grav} = 2\kappa M/c^2 = 2\sqrt{\hbar c}\sqrt{\kappa}/c^2$ coincides with the region of localization of this mass permitted by the Heisenberg uncertainty principle $l \sim \hbar/Mc = \sqrt{\hbar c}\,\sqrt{\kappa}/c^2$. With further increase of the energy of the intermediate state (E) the gravitational radius would increase correspondingly. On the other hand, however, the region of localization of the intermediate state according to the uncertainty principle should decrease correspondingly and for $M > \sqrt{\hbar c/\kappa}$ would become less than the gravitational radius. If such a situation were to arise in the region of applicability of classical physics, we would say that we were discussing a system whose mass is under the Schwarzschild gravitational sphere, i.e., a system of the black hole type.

13)It is well known that the total mass of a closed Friedman world is equal to zero: the gravitational defect of the masses completely cancels the bare mass (the mass of the atoms of the matter). The total mass of a closed world is given by the expression $M_{tot} \sim$ const $\times \sin^3 \chi$ $(0 < \chi < \pi)$. For $\chi = \pi$ the total mass is zero. If the world is almost closed: $\chi = \pi - \delta$, where δ is very small, then $M_{tot} \sim$ const $\times \sin^3 \delta$ also is arbitrarily small. To an external observer an entire given Universe (with its galaxies, say, and civilizations) will be considered as a particle of arbitrarily small mass (let us assume to strengthen the illustration – of the order of the mass of any elementary particle) and arbitrarily small size. The sphere (in which the material system is included) is seen by an external observer also in the form of a microscopically small object: $s^2 \sim$ const $\times \sin^2 \delta$. If the matter is electrically charged with a charge (ϵ), then the metric of the world turns out to be half-closed. For ϵ equal to the charge of one electron, the total mass of the system turns out to be equal to one of the "maximons", namely $M = e/\sqrt{\kappa}$. A friedmon is a maximon which is realized in such an almost closed Friedman metric.[16]

14)The quantum density of matter is reached in the Universe at roughly this size.

15)That is, of the order of 300 GeV; however, we are really talking about 300 GeV and above, for example, 300–400 GeV.

16)In justifying the desirability of building such an accelerator we have limited ourselves to those problems which clearly have solutions (alternatives), either of which justifies the construction. We have not touched at all on the general program of physics research (asymptotic problems, and so forth) which this accelerator will inherit as a continuation of the work of the accelerators at CERN, Serpukhov, and Batavia. We have done this consciously in order to be able to say: "In addition, there is an extremely broad program of research with this accelerator which we will not mention here." A research program of this type is discussed in the article by D. I. Blokhintsev et al., Usp. Fiz. Nauk 109, 259 (1973)[Sov. Phys. Uspekhi 16, No. 1 (1974)].

17)These tendencies for conversion of national centers into international centers can greatly facilitate also the problem of financing.

18)Of course, the Batavia accelerator, even if the planned energy of 500 GeV is achieved, will be equivalent in the center of mass to a colliding-beam accelerator of 2 × 16 GeV. From the point of view of the hierarchy of lengths (see the table) it would appear that there is little basis to predict the appearance at these lengths of a fundamentally new physics, which Dyson used to justify the construction of the Batavia accelerator. Since nature is as generous as it is, it may be that a substantial part of what we say with assurance will be uncovered in unitary-limit accelerators will turn out to be possible study in the Batavia accelerator. This will make certain corrections to the planning of the next generation of accelerators.

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