THE FUTURE OF HIGH-ENERGY PHYSICS

IS A HIGHER-ENERGY ACCELERATOR NECESSARY?

BY

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We shall be concerned principally with the prospects of high-energy physics. The near and the more remote future of this field was discussed extensively over the last decade, particularly during the planning of the construction of the accelerator in Batavia.

The results of this discussion have been compiled in the collection "Nature of Matter. Purposes of High-Energy Physics" [1], later reprinted in Uspekhi fizicheskikh nauk [2] with the addition of a number of papers by Soviet authors.

Now, however, when the construction of the Batavia accelerator has been successfully completed and the machine has been put into use, it is natural and opportune to consider the next generation of higher-energy accelerators -- natural from the standpoint of predicting the trends in the development of science in general and opportune in view of the character of the object of the prediction in particular. Thus, in a foreword to one of the documents referring to the planning of the European 300 GeV accelerator at CERN, B. Gregory (then the Director of CERN) wrote:

" I should point out one difference between high-energy physics and many other fields of science. The unavoidably large size of the installations requires planning 15 years ahead" [3].

The problem is thus the next generation of accelerators, and essentially for the next generation of physicists.

Nowadays the future of high-energy physics, and the question put in the subtitle of this report, cannot be considered in isolation from the future of physics or even the whole future of science. During the last two decades major advances were made in various branches of physics, astrophysics, and biology.

And the future, sometimes highly promising future, of these fields of science attracts general attention and gives rise to certain anxiety and apprehension about the prospects for their financial support.

In the last few years a number of papers appeared on the future of science, touching also in one way or another on the future of high-energy physics. Thus, Dayson [4] published his paper "The Future of Physics" in 1971. In the same year (1971) "Uspekhi fizicheskikh nauk" published Ginzburg's paper [5], interesting in its extensive problems, and in 1972 Artsimovich's paper [6] appeared in "Priroda", emphasizing the predominant development of astrophysics. The papers [7,8] in "New Scientist" and "Science" of September 1971 also attract attention. One of them, by F.T. Cole [7], is a panegyric on the completion of the construction of the 500 GeV synchrotron in Batavia and the imminent program of scientific investigations, and the other, by F. Anderson ("Are the big machines necessary ?") is to Cole's paper like a particle to an antiparticle: the "signs" of many of the statements made by Cole are here reversed.

In all these papers an attempt is made to analyze the changes in physics and in natural science in general during the past few years, the place of high-energy physics in science of our time, and the prospects of science in the next few decades.

During the decades close to the first half of the twentieth century the greatest achievements are considered the developments of microphysics. The turbulent growth of nuclear physics during this period was accompanied by fundamental discoveries.

The immense influence of these discoveries on economics, politics, and relationships between countries led to a certain feeling of elitism in nuclear physics and elementary-particle physics, accepted by scientists in other disciplines such as solid-state physics, chemistry, and biology, who were struck by the advances in these two fields and by the significance of these advances not merely for science but for the social life of the whole planet [9].

With time important advances were made in other branches of science as well, and with them a change in the accepted belief in the hierarchy of various scientific fields. The situation is not without a purely prosaic aspect either: research in high-energy physics is becoming very expensive, and so is the construction of accelerators.

The future generations of accelerators will be even more expensive, and in this connection the value of the results obtained with their aid is discussed.

Sometimes the question is asked whether the game is worth the candle.

Rather than follow the path of general discussions and argue whether ultrahigh-energy accelerators are needed at all ("at all times and in all nations"), we shall confine ourselves to asking specifically whether we need the next generation of accelerators, i.e. the generation succeeding the accelerators that are now or have just been put into operation. We are here considering both the traditional-type accelerator in Batavia and the CERN crossed-beam proton accelerator.

The discussion will reflect both the optimistic and the pessimistic views expressed in the papers cited above, and will be conducted bearing in mind the development of science in general, with allowance for how this is predicted in particular in these papers, omitting from the papers all that has no direct bearing on our problem, but sometimes provides "background" needlessly complicating the issue.

We shall begin with Dayson's paper [4], the "Future of Science [Sic]", which is interesting in many respects. It begins with recollection of the situation that arose at the Cavendish laboratory after the death of Ernest Rutherford.

"To the dismay of all those who still remained in Cambridge, Bragg (the new Director of the laboratory) made no effort to revive the former fame of Cambridge... he was not very interested in the construction of new accelerators... and was fond of saying 'We have taught the world well how to work in nuclear physics. Now we shall show them how to work in something else' ".

This "something else" did in fact come about in due course, in the form of new trends in radioastronomy and molecular biology, the development of which was accompanied by truly fundamental discoveries. Analyzing the reasons for Bragg's success as the Director of Cavendish laboratory, Dayson formulated the "three don'ts" which in his opinion helped Bragg in the situation that arose in Cambridge at the end of the 'thirties. "I am convinced (writes Dayson) that very important conclusions follow for us from this story" (i.e. for discussing the future of physics).

In the first place we should see whether these conclusions and rules can really be helpful in our situation.

These rules sound almost like religious commandments and have the character of categorical imperatives*:

Don't try to revive past fame ... This is a question of the specific situation, specific conditions and possibilities. Maybe you shouldn't, and then maybe sometimes you should. Perhaps those who reproached Bragg for not wanting to "recreate the past glory of Cambridge", i.e. to continue work in nuclear physics, were not so wrong after all. This is not to say that new fields such as radioastronomy or molecular biology should not have been tackled, but perhaps it would have been better to do this in the second place, not at the expense of nuclear physics.

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* a) Don't try to revive past fame.

b) Don't be concerned with something only because it is most fashionable.

c) Don't pay attention to mockery and superciliousness from theoreticians.

Happily, a new research center arose at Berkeley, which in a way took over the research on the structure of matter and elementary particles where Cambridge left off.

But suppose that *everybody* decided *everywhere* at that time that there is no point in reviving the past glory of nuclear research ... Bragg's first "don't" is in no way a commandment -- it is merely an alternative proposal when discussing the fate of a scientific organization.

Each scientific institution has its youth, its maturity, and its age of decline. The development cycle of an institution is usually 15-20 years, after which the institution is revived, fades out of existence, or is reborn in completely different form. No, this rule will not be of help to us. And yet, it is quoted constantly, in the literature, adding to the excess "noise background" which should be eliminated.

The problem of "fashion" is not so simple.

Each specific fashion first appears as an "antifashion", countering an existing state of affairs. In science, as a rule the fashion is the direction that seems promising in some way. Who is this commandment aimed at ?

Fashion as a rule attracts people who en masse are not very creative, i.e. people who themselves do not open up new fields. And yet these people are very often energetic and get practical results, and frequently have considerable formal theoretical capabilities. This makes it possible for them to obtain rapid and effective results "within the framework of the fashion", test the fashion "for strength", and perhaps exhaust or close up the field in this way.

In science this variety of investigators *exists*, and it is *needed*, indeed *necessary*, in the ecology -- if we may call it that -- of scientific creativity, just as a variety of living organisms is needed to maintain the ecological equilibrium of life on Earth.

And finally, "Don't pay attention to mockery and superciliousness from theoreticians".

Why only theoreticians "

Remember the quote "Anyone who expects production of energy from transformation of atoms is talking nonsense". This comes from Rutherford's speech at a meeting of the British Association for the Advancement of Science on September 11, 1933.

The problem comes back to the same ecology of scientific creativity or even ecology of the scientific society. Many scientists, and not just theoretical workers, are ready to pour cold water on enthusiastic hotheads.

Very often the critics are highly qualified, with great erudition and wide mental horizons. They can see the difficulties first, and by habit, according to their logic, these difficulties appear insuperable.

Ford seems to have written somewhere that if he wanted to cause trouble for his competitors he would advise them to hire a large number of highly qualified engineers, each of whom would clearly see that any new proposal is impossible. If Ford never said this, the story would have to be invented anyway. But, on the other hand, criticism is necessary in the ecology of science. Wolves do much good by killing off weak stock, though of course some strong and healthy animals are sometimes caught as well. This is the tragedy of individuals.

Ecological equilibrium is established wisely by itself, and not only in the fauna and flora.

Therefore, if Dayson's advice is aimed at the innovators in science, strong in spirit and mind, it is not necessary. If it is aimed at the great majority of "inventors" -- it does no service to science. Scientific progress cannot be fitted to the Procrustean bed of three commandments. However, putting aside the parts of Dayson's paper not directly relevant to the issue, we find in it two points that must be included in the list of specific questions for further discussion.

In the first place, *Dayson's evaluation of the imminent experimental situation at the Batavia accelerator* is of interest. This evaluation is rich in shadings differing "in sign". Dayson writes: "Roughly speaking, the whole result of the great financial expenditure and considerable human effort in Batavia reduces to raising the energy region accessible to physics by one power of ten from the tens of GeV available in 1970.

We all sincerely hope that Nature will present us with new and very important phenomena which we can discover by rising to this particular energy magnitude. If Nature did just this, then the effort expended on the construction of the accelerator will prove fully justified. If, however, it proves that precisely in this new accessible energy interval there are no fundamentally new phenomena, then the newly constructed machine will be simply a monumental knick-knack."

Dayson's attitude to the construction of the record-energy accelerator in Batavia is expressed grammatically by conditional sentences: "if ... ".

From the standpoint of strict logical construction of grammatical phrases we cannot fault this author.

However, the factual material for these phrases is so scant that it not only fails to describe the situation under discussion but distorts it considerably. It is of course also true that other accelerators, like the Bevatron in Berkeley, were in their time constructed with clearer aims in view. The Bevatron, in particular, was built directly to confirm (or disprove) the existence of antiprotons. Fulfilment of this task a priori justified the Bevatron, and resulted in the award of the Nobel prize to the discoveries of the generation of proton-antiproton pairs.

However, the limiting energy of the Batavia machine was not determined by some such fundamental problem. On the other hand, an extensive problem was formulated, which in general is aimed at filling the gaps in the picture of physical phenomena in this energy range.

This energy interval had to be gone through in physics research -- this is the same historical necessity in scientific progress as once was the filling of "blanks" on maps of the Earth; the investigation should also have its enthusiasts, heroes, and perhaps even martyrs.

Dayson completely ignores this extensive and thematically important research program, formulated by a large group of physicists, program that is being gradually expanded and is becoming even more interesting and significant. I am thinking here in particular of the situation with multiple particle generation, scale invariance, and in general a circle of problems that arose when the construction of the accelerator was nearing completion -- cycle of problems that did not enter into the number of arguments justifying the need for the new machine.

Next Dayson carried out a comparison of the possibilities of experiments with cosmic rays and experiments on accelerators. We shall discuss these questions further on among other questions appearing in these papers.

The very essence of Anderson's paper [8] lies in the phrase "Scientists have begun to understand that the pie is finite" and that all that is "pro" high-energy physics is "con" something else. ... "Any discussion of the subject must start from the fact that high-energy physics is terribly expensive ...". There is indeed no doubt that high energy physics is expensive. Moreover, all science is becoming more expensive, and this we shall discuss in the list of questions under the *growing cost of science*. But we should like to "sieve off" from Anderson's paper the statements which, as in Dayson's article, cloud the issue around the real problems under discussion.

To this category we must refer the author's discussion of scientific strategy. Anderson tries to convince the reader that it is not the accumulation of new facts but new concepts and new viewpoints that achieve revolutions in science. Starting from purely philosophical premises, he seemingly recommends for high-energy physics not the accumulation of new data (too expensive) but more conceptual thinking.

There is no need to persuade and prove to the reader that scientific revolutions are usually associated with new points of view, but could the author indicate the precise moment at which there are sufficient facts for the new concepts and new viewpoints to appear ?

Another point is Anderson's specific statement that the rate of appearance of new discoveries in high-energy physics is radically slowed down with increasing energy of new accelerators. Anderson even uses the term "crisis situation" in high-energy physics. And this statement cannot be allowed to go unchallenged.

We thus come to the *"crisis situation" in high-energy physics, or the "law of diminishing returns" and high-energy accelerators.*

Anderson's final advice is to slow down not only experimental but also theoretical research in the field of high-energy physics.

This advice to clamp down on theoretical research sounds particularly strange. In order not to distort Anderson's view and make it even more unacceptable, it is best to quote it verbatim in English, just as he put it down:

"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 "genuine problems". Any comments from me at this point would only weaken the impression made by this thought. However, we shall come back to this remarkable advice.

Anderson seeks a logical justification for his recommendations, in particular in his discussion of "the absence of hierarchy in science" and his thesis that "the sciences are autonomous". Developing this thesis, Anderson argues against W. Weisskopf's idea of the fundamental character ("intensive" in Weisskopf's terminology) of subnuclear physics. Further on it will be useful to examine in greater detail Anderson's thesis about the autonomous nature of sciences, in view of the remarkable role played by high-energy physics in the family of sciences by its direct and indirect influence. Next it is necessary to supplement the list of questions for discussion by the questions that arise in reading of Ginzburg's paper [5].

Following Ginzburg, we shall now speak of microphysics, as a field including problems of high-energy physics and even more -- problems of "subnuclear" physics.

In microphysics research use is made not only of the high-energy technology. In physics and microphysics there are two directions of development which supplement each other -- high-energy physics on the one hand and physics of particle beams having relatively low energy but high intensity on the other hand.

These directions in research compete against each other in a certain region. Thus, the specific aspects of effects characteristic for the high-energy region are manifested weakly at low energies. However, weak manifestations of these effects (small cross sections) may be detected in particle beams of very high intensity.

Specific laws of phenomena are also manifested in high-intensity physics. In the limiting energies of the individual particles high-intensity physics may differ from high-energy physics by many orders of magnitude. Typical representatives of high-intensity physics are the physics of laser beams, physics of high-intensity electron accelerators in MeV energy ranges, and the so-called meson factories.

Laser beams of photons constitute an interesting example of high-intensity physics. Laser beams appeared not in accelerator technology, not in high-energy physics, and not for the needs of nuclear physics and elementary particles.

However, raising the intensity in a laser beam extends the application of the beam to problems of controlling thermonuclear reactions and even of elementary-particle physics.

At high intensities the laser beam and a beam of accelerated electrons are analogous in many respects. These beams, so different in their nature, can compete with each other in possibilities of various applications*. Competition is possible, in particular, in connection with problems of controlled thermonuclear reactions. Both a powerful laser beam and a focused beam say of electrons can act as a source of secondary high-intensity beams, e.g. of neutrons or sources of high-energy charged particles -- here I am thinking of the generation of particle-antiparticle pairs in intense laser beams [10] or the acceleration e.g. of protons by beams of electrons, in particular in installations of the type of smoke-ring accelerator. In contrast to high-energy physics, laser beams, high-current electron and proton accelerators -- so-called meson factories -- have almost limitless possibilities of practical application in technology, medicine, and national economy in general.

It is therefore desirable for various ministries to take on a substantial proportion of responsibility for financing high-intensity physics. High-energy physics also pays back to the national economy the sums expended upon it, and with a tidy interest, but (as we shall see below) not always directly, and as a rule only after a time. High-

* Very intense fluxes of any particles (photons, neutrons, electrons, protons, etc.) have the common feature that they can transfer immense amounts of energy to a small volume of matter (in the limiting case to a pair of particles or even to a single particle).

energy physics requires a longer "loan", but at a higher "interest".

One of the facts of life in this context is that accelerators built for research in nuclear physics and elementary particles eventually come to be used more and more for the needs of related sciences: solid state science, chemistry, biology, geology, ecology, etc. and less for the needs of elementary-particle physics itself. For these latter needs the next accelerator is built -- one of higher energy. For example, one of the largest circular electron accelerators in the world (C.E.A., United States) has gone over entirely to the use of its synchrotron radiation in various applications. As is well known, at DESY as well wide use is made of the channels of synchrotron radiation.

These last comments make a substantial correction to Anderson's thesis that all that is "pro" high-energy physics is "con" something else.

In Ginzburg's paper there is an attempt to answer the question "Which problems in physics and astrophysics are today particularly important and interesting ?" Ginzburg lists about 20 problems from various branches of physics, which are indeed of considerable interest. However, a section of special interest is the one entitled "Microphysics of yesterday, today, and tomorrow". The author repeatedly apologizes for the unavoidable subjectivity of statements, makes many reservations, softening the formulations, afraid of appearing as an "enemy" of microphysics. He emphasizes in every possible way the avantgarde role of microphysics in science, and wishes microphysics every success, particularly in the construction of new accelerators.

However, for the purposes of our discussion it is desirable to reject the half-tones to reveal more clearly the outline of problems arising from Ginzburg's paper, though we shall then pass outside the framework of the paper in question. The point is that these problems really exist, they figure in public opinion, and that is in fact why they are not spoken of openly "at home" -- in physics journals. "Today" in comparison with "yesterday", according to the opinion defended here (writes the author),"the position of microphysics in physics and in all natural science has been radically changed".

The author sees these changes both in the decreased specific weight of microphysics problems in physics periodicals and in the cooling off of interest in microphysics on the part of the new generation just entering science. According to him the reasons are that up to the 1950's questions of microphysics "had a decisive significance for the development of the entire natural science". The subjects studied by microphysics (the atom, atomic nucleus) "were the daily bread" ... to resolve the structure of the atom, to understand the laws operating inside it (for this it was necessary to discover quantum mechanics !) was to give a powerful impetus to many branches of physics, astronomy, chemistry, and biology. Much the same could be said about the atomic nucleus -- its study led to the possibility of using nuclear (atomic) energy and even gave a certain justification to calling the 20th century the Atomic Age".

This was yesterday. But what about today ? Today the object of microphysics has changed: "The particles investigated by microphysics either live for an insignificant fraction of a second or, as in the case of the neutrino, penetrate the Earth almost freely and are captured only with immense difficulty". In general, the new objects of microphysics are "exotic and rare plants".

The object of microphysics has changed, and with it the significance of this object for other sciences and the so to speak "social position" and authority of microphysics among the younger generation. Thus, among the questions raised by Ginzburg's paper we must discuss the *"Exotic nature of the object of microphysics and its significance today for other sciences".* And what about "tomorrow" ? Tomorrow the "postulate" (which I am not afraid to make) that in a way the most brilliant period in microphysics is already behind us. Nothing goes on forever, and not everybody has to believe that it does.

Thus, in our list we have a new question: is there any *real reason to suppose* that the golden era of microphysics has already passed and that the innermost door has been opened.

It is true that:

"The character of the problems facing microphysics today is in no way less mysterious and difficult than it was yesterday. In other words, microphysics is still ... the ultimate outpost of physics, its leading and most profound part". Pity that this thesis was not developed further in the paper. One of the principal objectives of our discussion is to find out, as fully as possible, the contents of this thesis. In other words, how and why, after all the criticism aimed at this field, we can consider that microphysics has remained *"the ultimate outpost of physics, its leading and most profound part".*

After this rather protracted introduction it is now time to turn to examining the situation in microphysics directly.

The yesterday, today, and tomorrow of microphysics

It is very instructive to consider the historical development of physics in very broad outlines -- a bird's eye view of the historical picture. One curious feature clearly appears: the remarkable hierarchy of laws prevailing in the world of physical phenomena as research probes into ever smaller and smaller regions of space-time in which the processes in question proceed. In this historical process the physicists faced each time whole new worlds of phenomena with their own specific laws as research entered regions 2-3 orders smaller in magnitude.

a) Hierarchy of lengths and hierarchy of laws

Physics has a historically justified tendency to study phenomena in regions of ever smaller dimensions: worlds of physical phenomena opened up at various length boundaries in the interval of 10^{-5} to 10^{-15} cm studied so far.

Thus, in regions measuring 10^{-5} - 10^{-7} cm physicists found the world of molecular physics, giving rise to the kinetic theory of matter.

On going down an order or two further, to regions of 10^{-8} cm ($h^2/m_e c^2$), a world of atomic phenomena appeared, giving rise to quantum theory. Work in regions of 10^{-11} cm ($h/m_e c$) led to a new and unexpected circle of phenomena connected with the possibility of generating electron-positron pairs, and phenomena were discovered that are described by Dirac's relativistic quantum theory.

The regions of 10^{-13} cm revealed the world of physics of the atomic nucleus, and those of 10^{-14} cm the world of hadrons, strange particles, and excited hadron states.

Today we are moving toward lengths smaller than 10^{-15} cm. This hierarchy of lengths and discoveries is schematized in Table 1.

Table 1. Hierarchy of lengths and hierarchy of laws

l, cm	World of physical phenomena	Energy of accelerated particles
10^{-6} - 10^{-7} cm	World of molecular physics	1 eV
10^{-8} cm	World of atomic phenomena. Atomic spectra	10 eV
10^{-11} cm	Discovery of e^+e^- pair generation - Dirac's quantum theory	1-10 MeV
10^{-13} cm	Physics of the atomic nucleus	100-1000 MeV
10^{-14}-10^{-15} cm	World of strange particles	10-100 MeV
10^{-17} cm	(Discovery of the nature of weak interactions) ? ? ? ?	10^5 GeV in the LAB system 10 GeV in the C.M. system
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10^{-33} cm	--	10^{19} GeV

As can be seen from this table, so far it has indeed been true that descending into regions 2-3 magnitudes smaller led to the discovery of new worlds of physical phenomena. So far behind every door opened there has been another. We can of course ask how many doors there are in reality. Is the chain of discoveries infinite ?

To this general form of the question there is no answer*, but it is

* More detailed discussion of the problem will be found in the author's paper "On the concept of primary matter" in Voprosy filozofii, April 1970.

quite appropriate to ask whether we can truly say that physicists have already reached the inner sanctum of matter. More specifically, can we still expect essentially new physics, when lengths (shock parameters) 1-2 orders of magnitude smaller become attainable ?

In other words, what awaits the physicist at lengths of 10^{-17} cm ? In the language of energy, this means accelerators with energies of 300 GeV in the center-of-mass sytem.

If we analyze the table of the succession of lengths and laws, we must come to the conclusion that the most important and interesting results are the unexpected and unpredicted findings made at these new stages of physics research. Reality as a rule exceeds the wildest imagination.

In this striving toward physics of smaller and smaller lengths -- and higher and higher energies -- we should not underestimate the powerful attraction of the so far historically justified hope of meeting the unknown. However, we shall not emphasize this perhaps purely psychological factor, though we cannot ignore it either. In the present situation, at the boundary of imminent lengths, namely lengths of 10^{-17} cm we can *confidently expect* a major advance of our knowledge. The point is that this boundary, the boundary of lengths having fundamental significance, is already organically contained in the modern theory of weak interactions. The dimensional constant determining weak interactions is characterized by the square of the length (ℓ^2), where ℓ is about 10^{-17} cm.

In any event, we can say *with confidence* that at these lengths we shall find the answer to one of the most intriguing questions in modern physics: *what is the nature of the weak interactions ?*

What is the essence of the still undiscovered secret of weak interactions ?

As is well known, the weak interaction cross sections increase with increasing energy of the interacting particles.

The constant of weak interactions is small, and therefore in weak interactions we use the apparatus of perturbation theory -- expansion in the weak parameter of interaction. Since the cross sections increase with increasing energy of the interacting particles, we find that at large energies, despite the smallness of the interaction constant, the next higher approximations of the theory become comparable with, or even exceed, the lower approximations. So far we have no other apparatus (other than that of perturbation theory) at our disposal. Attempts to construct an improved formalism for calculating the cross sections of weak interactions meet with the fundamental difficulty of the theory of weak interactions, due to the presence of diverging quantities that cannot be eliminated by methods (renormalizations) that are effective in electrodynamics.

In general, *we do not know the behavior of weak interactions at collision parameters close to the length characterizing weak interactions,* the very length of 10^{-17} cm mentioned above, or at energies close to 300 GeV in the center-of-gravity* system. *In other words, we have a real fundamental problem for accelerators with energies of ~ 100 GeV* in the C.M. system. Such an accelerator (300 [?] GeV, C.M. system) we

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* Translator's note: center-of-mass ?

shall call simply a unitary-limit accelerator*. There are indications that the problem under discussion may be connected with another problem of weak interactions.

Ever from Fermi's time (1934) the theory of weak interactions has been formulated as a four-particle interaction: in β-decay a neutron becomes a proton, an electron, and an antineutrino. All other interactions known in nature are exclusively three-particle interactions. Thus, a neutron emitting a π-meson turns by strong interactions into a proton, and so on.

For about 30 years there has been a tendency to reduce the four-particle interaction to a three-particle one. This may be done by assuming that the observed weak interaction in fact occurs in two stages. First the neutron turns into a proton, emitting a hypothetical W-meson (three-particle interaction) and then this intermediate W-meson decays into an electron and an antineutrino (another three-particle interaction).

The idea of unifying the types of interactions is so attractive that in all new energy ranges, in all new accelerators, physicists repeatedly search for the intermediate W-meson. So far, with existing accelerators, the W-meson has not been found.

The lower limit of mass of this W-meson still lies in the region of 2-5 GeV. 300 GeV in the center-of-gravity system (unitary-limit accelerator) is the limiting energy to which the concept of the intermediate meson makes sense and there is point in looking for it, and in

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* In perturbation theory it is assumed that because of the weakness of the interaction the initial state of the system does not change, i.e. unitary nature is allowed for with accuracy to the following approximation. In the limit unitary nature is disturbed if the following approximation is equal to the preceding one or is greater.

this sense the theory of weak interactions will undergo its decisive test. On the following pages we shall extend the discussion of various aspects of the forthcoming stage in microphysics into the epoch of appearance of experimental possibilities of the unitary-limit accelerator.

b) Are there any grounds for saying that the "golden era of microphysics has already passed " ?

The avalanche of striking discoveries of new laws in the microcosm in the 'twenties and 'thirties, the multiplicity of elementary particles and their properties in the next few decades, all this has in a way "distorted" our perception and evaluation of the rate of scientific progress. We await new discoveries with some impatience. There have been reproaches, and even a certain dissatisfaction with the rate of advance. An attempt is made to establish almost a "law of nature", according to which with the introduction of higher energy accelerators "the rates of new discoveries in these new energy ranges are radically slowed down...".

The point is, that the hierarchy of lengths discussed above, and the corresponding energies, should be calculated in the C.M. system. The energy in the center-of-gravity system is connected with energy in the LAB system by a square dependence:

$$E_{lab.} = \frac{E^2_{c.i}}{2\ Mp}$$

where M_p is the mass of the proton.

Beginning at the time of the first cosmotron for 3 GeV (1953) and ending with the operation of the Serpukhov 75 GeV accelerator at the present time (1973), the energy in the center-of-gravity system has increased, or the corresponding lengths have decreased, by a factor of $\sqrt{3/75} \approx 1/5$, i.e. only fivefold.

Thus, from the standpoint of the hierarchies of lengths and laws, *for the last 20 years we have been experimenting in roughly the same region of physical laws.* It would be well to keep this fundamental point in mind in any analysis of the situation in high-energy physics.

We should rather wonder how much has in fact been, and still is being, discovered in this relatively narrow energy interval.

Perhaps one of the fundamental experimental achievements of recent high-energy physics is the investigation of the cross sections of deeply inelastic interactions during scattering, in particular of high-energy leptons (neutrinos, electrons) on nucleons.

"65 years ago Ernest Rutherford, observing how α-particles are scattered on a thin metallic foil, concluded that atoms are not homogeneous but consist of negative electrons circling small, positively charged nuclei. Recent experiments with electrons accelerated to 21 GeV on the two-mile Stanford linear accelerator indicate that history is repeating itself at distances 100,000 times smaller than atomic distances. It was found that electrons of ultrahigh energy are scattered on protons and neutrons in a totally unexpected way, and it was concluded that nuclear particles have a complex internal structure and consist of point components called partons [11]".

On the other hand, the interpretation of these experiments is not at all as unambiguous as it is made out by Kendall and Panofsky*. Nevertheless, a new idea of nucleons made up of particles new to science (partons) has been born, and it will live and be checked in further experiments.

And it may actually be confirmed experimentally in the future.

But in our case we are concerned with "opening the next door", almost in the literal sense.

It is true that, in contrast to the situation with weak interactions, modern theory does not contain a specific length with which the existence of new structural units could be associated. If such particles do exist, we cannot say which generation of accelerators will be needed for their discovery. Perhaps direct or important indirect evidence in favor of these particles will come from existing accelerators or ones of the next generation.

It should be emphasized that a radically new concept of the structure of material particles appeared in the last few decades. While until that time it was generally held, roughly speaking that particles of greater mass

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* Such behavior (increase of the effect of lepton scattering on neucleons accompanied by multiple generation of particles was postulated a few years before the performance of the SLAC experiments [13].

The following theorem was postulated (and some proofs were adduced in its favor):

$$\sigma_{tot}^{form},\ E_0 \to \infty \geq \sigma_0$$

where σ_{tot}^{form} is the total cross section of deeply inelastic scattering with allowance for form factors in each of its channels and σ_0 is the cross section for elastic scattering on a point particle.

Not so long ago it was shown in [12] that the existence of this theorem is compatible with the formalism of modern theory with not very strong limitations.

However, it should be noted that the observed effects are very far from asymptotic. It is, therefore, not impossible that they go beyond the boundaries of traditional theory. In other words, the possibility of interpreting the effect with the aid of existence of some kind of subparticles (partons, quarks) is still not excluded).

are made up from ones of smaller mass, in these last few decades this seemingly self-evident idea was turned upside down. The new approach is to build particles of *smaller* masses from particles of *greater* masses, strong interaction between which leads to a corresponding mass defect in these systems. I am here thinking of the attempt to make π-mesons from μ-mesons (Wentzel*), π- mesons from nucleons and antinucleons (Fermi and Young), hadrons from aces, quarks (Zweig*, Gelman), and finally

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* Translator's note: Spelling uncertain.

Feynman's partons.

In connection with this modification of the fundamental concept of the very nature of structure of matter, which may perhaps be regarded as one of the most radical revolutions in our views on the structure of matter in all history, the question arises whether there are many arguments for the existence in nature of a *maximally heavy particle* representing the mass-limit of the structural material for all particles, Interestingly enough, from the universal constants we can construct a whole group of particles of similar mass that could pretend to this title. From the constants e (electronic charge), g (meson charge), $\hbar$ (Planck's constant), c (the velocity of light) and κ (the gravitational constant) we can construct the following quantities having dimensions of mass:

$$M - \frac{e}{\sqrt{\varkappa}} \sim 10^{-6} \text{gr} ; \sqrt{\frac{\hbar c}{\varkappa}} \sim 10^{-5} \text{gr} ; \frac{g}{\sqrt{\varkappa}} \sim 10^{-5} \text{gr}.$$

It is interesting to note that this group of particles with maximal mass ("group of maxima"), which can be constructed from universal constants, lies in the narrow mass interval of 10^{-5} to 10^{-6} g.

The corresponding lengths are in the region of 10^{-32} - 10^{-33} cm. From the standpoint of our hierarchy of lengths, these lengths should be at the very bottom of Table 1. Evidently 10^{-33} cm is the last length in the list of fundamental lengths. Beyond this point the very concept of distance loses meaning owing to the quantum fluctuations of metrics. Thus, from this point of view these lengths and corresponding particle masses should really be regarded as limiting.

The interest of maxima as possible structural elements is that with these masses and dimensions only gravitational forces suffice for the formation of systems with the required mass defect. Perhaps between the weak length (10^{-17} cm) and the "gravitational" length (10^{-32} - 10^{-33} cm) there is a series of hierarchic lengths with their own worlds of physical phenomena. However, within the framework of existing physical representations and known universal constants there is still no room for any other lengths.

It is very possible that these hypothetical particles are unstable in the free state*.

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* The statement that nowadays microphysics deals with "rare and exotic plants", short-lived particles which have no direct relationship to the "daily bread", the form of stable substance in which we exist, is not altogether true. In the first place, the stable particles -- protons, electrons, photons, neutrons, and complex atomic nuclei -- are, as before, investigated in all ranges of energy, and the most impressive results of high energy physics (deeply inelastic processes) are connected with the interaction of precisely such particles (protons - electrons). Moreover, such a short-lived particle as a neutron in the free state becomes stable in the bound state and enters structurally into stable nuclear matter.

It appears that such short-lived particles as hyperons are the structural elements of collapsing stars in their post-neutron stage. Furthermore, if it is found that partons or other, similar, hypothetical particles are indeed the structural units of matter, then perhaps it is precisely these "exotic" short-lived particles, unstable in the free state, that are the "daily bread" mentioned in Ginzburg's paper. We cannot say that this will be so in fact, but neither can we exclude such a possibility.

As regards the statement that the "golden era of microphysics is already behind it", though it sounds very definite in the context, it is accompanied by such reservations and admissions of directly opposing possibilities that it does not follow logically from any justified premises -- it is simply one of the sharply formulated discussion themes (and we must say, often discussed).

As a matter of interest, questions of this kind had often arisen in science in the past, and it is highly instructive to recall them.

At the end of the last century, according to Millikan the great discoveries in physics had already been made and further progress would consist not of the discovery of qualitatively new phenomena but rather of more exact quantitative measurement of phenomena that were already known. This generally held opinion of the time is formulated by Planck's teacher Philip Holli*, somewhat more picturesquely but essentially in the same words: "Of course, we can still note or remove a speck of dust in some nook, but the system as a whole is strong, and theoretical physics is noticeably approaching the degree of perfection achieved centuries ago by geometry" [14].

These words "all the great discoveries in physics have already been made", voiced only decades before the advent of relativity and quantum mechanics, are now taken as a scientific anecdote.

Of course, this historical diversion does not in any way prove that the most illustrious period in microphysics has not passed. We can only recommend greater caution in dogmatic statements. But in a certain

* Translator's note: Spelling uncertain.

sense a real, and it seems fairly convincing, answer to this question may be obtained from an analysis of still unresolved problems that are facing microphysics. Obviously, in the event of discovering more fundamental structural elements of the type of quarks or partons we would indeed be going into a new and in a certain sense illustrous scientific period. However, this will not necessarily come about. There are, on the other hand, problems whose solution will undoubtedly create a new, and perhaps really most brilliant, epoch in science.

And these problems, as it now appears, may be directly connected with the experimental possibilities of the accelerator generation under discussion.

Strangely enough, speaking very roughly, our understanding of physics has not advanced very far from the time of the ancients. This may sound like a paradox but, in very broad outlines, it is true. Thus, while the ancient Greeks thought in terms of the four basic elements of earth, air, fire, and water, without understanding, as we would now say, the fundamental properties of these elements, modern physics tries to interpret the real world as a complex interaction of various "fields". We have once more the four elements of the ancients, strong, electromagnetic, weak, and gravitational fields. Like the ancients, we are still far from understanding the fundamental properties of these elements of the 20th century. In other words, our attempts to describe the properties of these fields individually -- electrodynamics on its own, weak interactions on their own, etc., are basically unsound*.

* Arguments have now appeared that the intrinsic mass of the electron evidently cannot be of electromagnetic origin. The point is that in a number of works [15] a more exact expression has been found for the electromagnetic mass of the Dirac electron. This is a "superconducting type" solution -- it is not expanded in powers of the fine structure constant. In contrast to the known logarithmic dependence, it brings the expression back to classical linear divergence, but with a very small factor, characterizing the polarization of vacuum:

$$\Delta m_e \sim \frac{e^2}{rc^2} e^{-\frac{3\pi}{2}\frac{\hbar c}{e^2}}$$

A mass of the order of the electron mass appears only on truncation at lengths:

$$r \sim \frac{e^2}{m_e c^2} e^{-650}$$

This length is many orders of magnitude smaller than the length at which the very concept of length loses meaning owing to the quantum fluctuations of metrics. Moreover, the expression for Δm is automatically made finite when we consider the gravitational defect of given mass concentrated in a small region. The finite electromagnetic mass of the electron is :

$$\sim \sqrt{\frac{\hbar c}{\varkappa}}\, e^{-\frac{3\pi}{2}\frac{\hbar c}{e^2}}$$

i.e. the mass of the electron cannot be of electromagnetic origin if these calculations are correct.

We do not understand, and cannot quantitatively describe, the spectrum of so-called elementary particles arising in the interaction of these "elements". It has long been realized that none of the interactions can be studied in isolation "to the end". There always comes a time in high-energy physics when all the other interactions begin to participate in a given effect. We cannot take from Nature one of its elements in its entirety without disturbing all others. It appears that nothing is superfluous in Nature's architecture. We have long got accustomed to the unity of Nature. However, we are unable to describe the unity of the four "elements", though we travel toward this goal by various roads. Faraday was able to establish the profound connection between electric and magnetic phenomena.

Einstein failed to combine into a single picture gravitational and electromagnetic interactions. Heisenberg could not achieve understanding of various aspects of this unity on the basis of his fundamental ψ-field. But we are still trying, and will continue to try, to understand the underlying unity of the "elements".

Ideas of "symmetry violation" have now appeared, and perhaps in them lies the key to a unified theory of weak and electromagnetic interactions.

At the moment we are not thinking of a definite specific theory, but rather of some strategy in attempts at constructing such theory, within the framework of universalization of three-particle interactions. In this concept the idea of an intermediate meson finds its natural position, and the mass of this meson is given by numerical values not very remote from the energy value of the same unitary limit*. And these precisely are the energies that will become available in the next generation of accelerators. The developing concepts of a unified theory of weak and electromagnetic interactions are also a strong argument for the high-energy accelerators already in existence and for building the next generation of accelerators. It should further be mentioned that not only leptons but also hadrons exhibit weak interactions, and therefore it is already clear (this is shown by various specific variants of theory) that such subsequent concept should include the unified theory of weak electromagnetic and strong interactions. There are also serious reasons for thinking that the regulating role of the gravitational field may prove one of the most important features of this concept. All this constitutes an important argument in favor of a motion that the golden age of microphysics is still ahead of us.

In what follows we shall return more than once to a justification of this thesis, discussing further problems of physics of the future.

c). <u>The place of microphysics in scientific hierarchy.</u>

<u>Influence of microphysics on other sciences and on technological progress</u>

One can hardly speak seriously of any hierarchy in science in the

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* For example, in a variant of this theory a value of ~ 40 GeV is given for a neutral intermediate boson, and ~ 80 GeV for a charged one.

formal sense of the word, and there is little point in asking how microphysics ranks, say, against microbiology or sociology. But within the framework of physics, or rather the "science of nonliving things", we must agree with Ginzburg that microphysics is the "ultimate outpost of physics, its leading and most profound part". The relationship of microphysics to other sciences has many specific features intrinsic to microphysics and the special manner of its development.

Ginzburg listed many important problems facing physics and microphysics. However, these are as a rule special problems, and are also found in microphysics. One of the characteristic features of these problems is that nearly every one of them, though at the moment appearing highly significant and interesting, may on further investigation loose most or all of its significance and interest and be struck off the list of major problems. At present, for example, we are downgrading in this way the problem of heavy water. Metallic hydrogen may fail to exhibit the properties that would be useful e.g. in a high-temperature superconductivity technology.

The very enticing idea of finding and producing high-temperature superconductors may, for example, prove physically impossible in principle. It may be found that there are no relatively stable transuranic elements in nature. Perhaps it will not be the laser variant that will prove most promising for realizing thermonuclear reactions but rather, as some believe, an electronic variant or even a "traditionally thermonuclear" variant.

None of this is to say that these problems are of little interest. Should high-temperature superconductors be found, there would be a genuine revolution in technology. Here it may be useful to emphasize the difference between special problems in physics and the general problem of microphysics, which is to investigate physical phenomena in ever smaller regions of space-time. This general problem is of cosmopolitan character -- it has absolute value independently of the result of the investigations: we must come to know the world of physical phenomena in these regions of the physical world, and this urge to know will always be with humanity. It is the same natural force that drives us to explore the ultramacroscopic depths of the Universe, to astronomy and astrophysics.

Turning back from these general comments to the specific topic of this paper, we can only repeat that with the aid of the accelerators of the present, and certainly the next, generation, at lengths of 10^{-17} cm, we shall uncover the secret nature of weak interactions. It does not matter what it turns out to be -- i.e. whether we find that the real interaction is analogous to an electrodynamic (three-particle) one or that a four-fermion interaction is in fact the true weak interaction -- both results are fundamental and will fundamentally change our level of understanding nature.

I have already mentioned how from the very birth of the theory of four-fermion formulation of weak interactions there appeared the alluring idea of reducing four-fermion interactions to three-particle interactions of the type of electrodynamic and other known interactions. The fact that weak interactions in this theory proved to be of special nature -- a rara avis in the family of all interactions -- made the idea of unifying all interactions natural and highly attractive. If, however, it is found in the experiments under discussion that the weak interactions maintain their four-fermion character, then we might turn to the directly opposite possibility concerning the structure of interactions -- that all interactions have a four-fermion structure. From the very moment such quantities as spinors appeared in physics it was known that from two spinors we can construct objects with various transformation properties -- vectors, tensors, scalars, etc.

Thus in its time arose the neutrino theory of light, to construct the electromagnetic field vector from two spinors describing the neutrino. From the very appearance of spinors in physics there has also appeared the idea of the fundamental nature of spinor fields, which perhaps structurally determine all other fields. Thus, should the reality of four-fermion interaction be confirmed, experiments in the energy region under discussion will undoubtedly revive this also in its way unusually attractive concept. *We see, therefore, that we are standing at most important crossroads, and that experiments in this very energy region we are considering will decide the further course of natural science.*

Major advances were made recently in astrophysics -- discoveries of fossil radiation, new astrophysical objects such as quasars, pulsars, neutron stars, and perhaps black holes -- all this is attracting broad scientific attention. We may even hear that astrophysics should now be given preference among other physical sciences [6]. No doubt the progress in astrophysics is great, no doubt it has not so far received sufficient material means and attention. At the same time, it has now become more of an experimental science than it had been. This is due, as Ginzburg rightly points out, to the fact that astrophysics extended its scope. Whereas once astrophysical research was confined to the optical wavelengths, today the appearance of radioastronomy on the one hand and of X-ray and gamma astronomy on the other has greatly extended its experimental possibilities of winning new specific data about the Universe. And now it is getting ready for the highly promising neutrino astronomy and gravitational-wave astrophysics.

There is no doubt that astrophysics has obtained important results and made important discoveries in recent years. But its most brilliant discovery is not one of those. It is the discovery that almost half a century ago (Hubble) led to the model of nonstationary expanding universe with Friedmann-type metrics.

In comparison with this, the discoveries of quasars, neutron stars, and black holes are naturally less impressive.

If we dare to commit the indiscretion already quoted in this paper, we could say that perhaps the most resplendent period in the history of astrophysics is already the past. But I would not like to play the part of a prophet.

Another point that is worth noting is that in a way astrophysics is coming closer to microphysics. Neutron stars are essentially immense atomic nuclei. In some phase of their own even hypernuclei. Neutron stars represent the macroscopic form of nuclear matter.

On the other hand, the global properties of the black holes are now widely discussed, and it is very likely that such a state of matter must be taken into account when constructing the next theory of elementary particles*. Astrophysics, or rather general relativity theory, in

* This last comment is connected with the fact that in modern elementary-particles theory states with unrestricted high-energy are allowed in intermediate states. The total mass of an intermediate state may exceed the mass of any cosmic body or even system. But at the same time, against all logic, modern theory ignores in these states the gravitational properties of these masses. It will be noted that if in the intermediate state we find a mass of the order of

$$M \sim \sqrt{\frac{\hbar c}{\varkappa}} \sim 10^{-5}\,\mathrm{gr}$$

then its gravitational radius $r_{grav} = \frac{2\varkappa m}{c^2} = 2\sqrt{\hbar c}\,\sqrt{\varkappa}/c^2$

coincides with the region of localization of this mass permitted by Heisenberg's uncertainty relationship:

$$\ell \sim \frac{\hbar}{Mc} \sim \sqrt{\hbar c}\,\sqrt{\varkappa}/c^2$$

When the energy E of the intermediate state is increased further, the gravitational radius should also increase. But, on the other hand, the regions of localization of the energy of the intermediate state should correspondingly decrease according to the Heisenberg equation, becoming smaller than the gravitational radius when $M > \sqrt{\hbar c}/\kappa$. If such a situation arose in the range of applicability of classical physics, we sould say we are dealing with a system whose mass is below Schwarzschild's gravitational sphere, i.e. a system of the type of a black hole.

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principle permits the existence of objects with almost closed internal metrics such as friedmons*. This possibility makes the very concept of "macro" and "micro" relative.

There are some grounds for thinking that the final stage of stellar collapse is a direct problem for microphysics. In point of fact, if long-range Coulomb forces can in principle arrest collapse, then the forces due to exchange of a heavy vector meson at distances of $\sim 10^{13}$ cm

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* The total mass of a closed Friedmann world is equal to zero: the gravitational defect of the masses completely extinguishes the naked mass (mass of the atoms of the substance). The total mass of a closed world is given by

$$M_{tot.} \sim \text{const.} \sin^3\chi; \quad 0 < \chi < \pi$$

When $\chi = \pi$, the total mass is zero. If the "world" is almost closed $\chi = \pi - \delta$, where δ is very small, then $M_{tot} \sim \text{const.}\sin^3\delta$ also as small as we like. To an external observer our entire Universe with its galaxies and perhaps civilizations will appear as a particle of arbitrarily small mass (suppose, to stress this point, of mass of the order of some elementary particle) and arbitrarily small size. The sphere "enclosing" the material system appears to an external observer also in the form of a microscopically small object: $S^2 \sim \text{const.}\sin^2\delta$. It is interesting that if such a closed world is "unpicked" with an electric charge (ε), its metrics turn out to be semi-closed. The degree of openness is connected with the magnitude of the charge. When ε is equal to the charge of an electron the whole mass of the system is equal to one of the maximons, i.e. $M = e/\sqrt{\kappa}$. A friedmon is a maximon realized in such almost closed Friedmann metrics [15].

already play the part of long-range repulsion forces. And the density of a collapsing star in such a small volume is 10^{72} g/cm^3, i.e. still 20 orders smaller than the so-called quantum density (10^{94} g/cm^3), at which -- as some believe -- collapse could be brought to a halt by some still unknown feature of quantum phenomena.

Finally, consider this: at the initial moment of evolution of the Universe, when the Universe was localized, suppose, in a region of 10^{-13} cm* -- would such an object come under macro- or microphysics ?

This instant of birth of the Universe is still shrouded in profound secrecy. What unexpected changes may appear in our ideas about physical laws when we understand the physics of this event ? Perhaps this will be the most brilliant period in the history of astrophysics (and maybe also microphysics) ?

These last comments represent an essential correction to Anderson's thesis on the autonomous nature of sciences.

Of course, high-energy physics, or microphysics in general, is not the hierarchic basis of all sciences. Indeed, an isolated result in high-energy physics may have no relationship to biology, chemistry, sociology, or philosophy. Nevertheless, the direct and indirect influence of the whole evolving field of microphysics on all science is on the whole greater than that of any other field. It is also very important to note the indirect effect of modern fundamental research in high-energy physics on science and technology in general. The point is that this field of research is accompanied by the appearance of fundamentally new, highly refined, physical apparatus, often completely new techniques, which finds extensive application not only in many fields

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* The quantum density of matter $S \sim 10^{94}$ g/cm^3 is reached at approximately this size of the Universe.

of science and technology but also in national economy and exerts a strong bearing on technological progress in general. The scale of this influence still requires investigation. Here we may recall the major part played by accelerators in various sciences, medicine, and national economy.

For example, multichannel analyzers were developed in the experimental equipment for microphysics, and just consider their wide application in various fields of science. How should we assess the benefit brought to the world economy by experimental microphysics and the use of computers ? Synchrotron radiation in various wavelength intervals is beginning to be used widely in chemistry, solid-state physics, and biology. We cannot say to what new biological discoveries we may be led by the new rich possibilities of studying the changes occurring in cells with time by means of synchrotron radiation in the angstrom range. Still earlier, biology had received the gift of the electron microscope. Solid-state physics in the synchrotron range of 10-2000 Å still requires development.

We can, for example, say that the next generation of accelerators will be built with the use of superconductivity technology. This last too will be developed further in the construction of accelerators of this generation, which in turn will exert a strong influence on the applicability of superconductivity in many sectors of national economy. Above I spoke of the indirect effect of experimental high-energy physics on other sciences and technological progress. But it is no less important to remember that the theoretical apparatus and the formalism of the theory developed within the framework of requirements of elementary-particles physics has found and is still finding brilliant and effective application in other branches of physics, in particular in solid-state physics. I should not like to repeat Anderson's advice to slow down theoretical research in high-energy physics. The part played by the story-book figure who, constricted by his narrow mental outlook, used to say solemnly "there would be nothing but acorns..."* is a highly unenviable one.

* Translator's note: This is probably a familiar and instantly recognizable quotation to a Russian reader. However, the translator has been unable to trace the source.

The statement that broad practical application of the effects of microphysics itself is already exhausted is not a logical conclusion based on any firm premises.

Consideration of the practical application of microphysics in the past usually brings to mind the utilization of energy released in nuclear reactions. But this is only about 1% of the whole energy locked in matter.

Ever since the formulation of $E = mc^2$ there has been the tantalizing possibility of total conversion of matter into energy. I can already hear outcries and phrases such as the one quoted at the beginning of this paper from Rutherford's speech. However, on the basis of historical evidence we know how wrong this prediction of a distinguished scientist turned out to be.

It is perfectly true that we do not yet know how to utilize this energy; but does this mean we shall never be able to do it ? In any event, there is an unopened storehouse of energy in nature.

It is probable that in the future means will be found of long-term storage of appreciable quantities of antimatter, this most efficient of fuels.

According to modern views, the energy released by the Sun appears as a result of nuclear reactions together with a powerful flux of neutrinos. This neutrino flux has not as yet been detected.

If a tenfold refinement of the experiments still fails to detect the solar neutrinos we shall be forced to look for some nontrivial explanation. One nontrivial possibility has, as a matter of interest, been prepared by theoretical physicists to cover this case. It arose from an attempt to explain the still enigmatic effect of violation of so-called complex parity in the decay of a K^0_γ meson into two π-particles. In one variant of theory developed at Serpukhov there appears a violation of the conservation of energy. It turns out that this violation of the law of conservation of energy (corresponding constant of this theory) is sufficient for the observed energy liberation of the Sun without the release of the expected number of neutrinos.

Of course, from the standpoint of healthy scientific conservation we should feel morally obliged to resist this extraordinary possibility (scientific ecology...), but what if ... ?

Most regrettably, we cannot -- by definition -- say much about the future possibilities of science. We cannot talk about something we do not yet know.

History teaches us that what is most important and significant in a new field of research is usually unexpected and unpredicted. *A powerful argument for microphysics is that it is precisely here that the unexpected is most likely to appear.* We cannot foresee what practical applications will arise from future microphysics research, and it would be still more *unjustified* to make any *negative* assessments.

The phenomena that do not find explanation within the established framework are usually associated with far-reaching consequences. In truth, we do not know where understanding of the situation with violation of complex parity in the decay of K^{0}_{γ} -meson will take us.

We do not know the secret hidden in the failure to observe the expected flux of solar neutrinos. We are still thinking in conventional categories. Perhaps we are still dealing with the microcosm with macroscopic crudeness.

"In each stage of history some scientific discipline out of those belonging to the broad range of natural science emerges as an outpost and becomes the symbol of scientific progress" (Artsimovich [6]). It follows from what went before that there are definite grounds for thinking that at lengths close to the energetic unitary limit (300 GeV in the C.M. system) physics -- *high-energy physics* -- *will again become this outpost.* I should like to emphasize once more that this paper is not intended to demonstrate the necessity of building high-energy accelerators in general. It deals with accelerators of a definite energy (~ 100 GeV in the C.M. system) with fully defined aims*, and here the *game is definitely worth the candle*.

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* In justifying the desirability of building such an accelerator I limited myself to problems which have alternative solutions any one of which would justify the project. I did not touch at all on the extensive program of physical investigations (asymptotic problems etc.) which passes to this accelerator as a "heritage", continuation of work done at CERN, Serpukhov, and Batavia. I did this deliberately, in order to be able to say: "of course, apart from this *there is a very extensive program for investigations on this accelerator*, which we shall not deal with here".

Should we then build accelerators of still higher energy ? This question remains open; at present we have no specific arguments in favor of such a proposal. It is not impossible that accelerators of this generation will prove to be the last (as far as the limiting energy is concerned) in the history of accelerator technology. In this paper we are not discussing which type of accelerators should be regarded as preferable for a given energy: traditional or crossed-beam (proton-proton or proton-antiproton, electron-proton, or electron-positron). An electron-proton crossed-beam accelerator is in fact very attractive in many respects, but the discussion of a specific variant of an accelerator of this generation constitutes a separate problem, which requires prolonged and painstaking work.

The increasing cost of science

We hear very often that high-energy physics has become expensive. This is quite true. Unfortunately, it would be still more correct to say that *all science is becoming expensive*. The point is that we are now in a time of so to speak industrialization of science. It is true that it was precisely nuclear physics that was the initiator and the first object of serious industrialization of its experimental basis, which constituted a precedent for the construction of large installations in just about all fields of science. In atomic physics scientists for the first time broke through the purely psychological barrier of former "modesty" of scientific research equipment. It became practice to build experimental installations to a scale greater than ever before. And what is most important, the real possibility and desirability of such industrialization has been demonstrated. Of course, in actual fact this is not a psychological effect but real and material process in the development of science. Science in the broad sense of the word became more than ever before an essential element of technological progress, and high technological progress industrializes science. It should be noted that space research long ago far outstripped microphysics as regards cost. In many other fields of science the necessity for high expenditure is rapidly growing. It has become evident that the construction in a given country, of an accelerator with limiting parameters is today more than the establishment of yet another institute -- it is the appearance of a new national (and in its tendencies international) center of high-energy physics*.

Such a center coordinates the scientific activity of many scientific institutes which take part in the center's work.

The organization of such a center in almost any field of science requires material expenditure which in many branches of science is becoming of the same order. Thus, in its August 1972 number, Physics Today published a program of proposed financing of astrophysical research for the forthcoming decade. This program had been developed by a special committee under J. Greenstein. The total cost of the program will be around 800 million dollars. The cost of only one radio telescope, proposed for completion by 1980, is expressed by a sum close to 80 million dollars.

For example, a scientifically leading national center for solid-state physics should comprise in its complex a sufficiently large research reactor, various types of accelerators, including one making use

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* This tendency of national centers to grow into international ones may greatly ease the problem of financing such establishments.

of synchrotron radiation, a modern assortment of computers -- all this plus the main cost of building such a center requires the investment of approximately the same hundred million roubles.

The establishment of a national center for thermonuclear research in various directions -- traditional, electronic, laser -- only in the first stages of its development requires not less than the same hundred million roubles.

A well-equipped national biological center (centrifuges of most up-to-date construction, electron microscopes, appropriate accelerators -- including ones producing synchrotron radiation in the angstrom range -- assortment of computers, etc.) in a few years requires sums of the order of the same 100 million roubles.

The construction of a modern national center, for example for cancer research, equipped with modern leading technology up to π-meson therapy inclusively, and with computer diagnosis facilities, will need expenditure of the same order. An accelerator of the next generation, a unitary limit accelerator, requires before 1990 no more money than it is planned to spend on astrophysics research in the United States.

We sometimes hear that the next generation of accelerators should be built not with the aid of great sums of money but with the aid of "gray matter", discovering new, unconventional, possibilities in accelerator technology. There is no doubt that such new possibilities must be looked for, and the search is in progress, but experience with construction in high-energy physics indicates that everything provided for an accelerator including the technology necessary for experimentation, will need more money than the accelerator itself. Thus, if in the future we were lucky enough to find a possibility that would reduce the cost of the accelerator itself to zero, the cost for the whole center would still not even be halved. Thus, in practice the total cost of building a center can be reduced only by not very many percent, which is hardly of fundamental significance for the problem under discussion.

Dayson's advice [4] to go over from research on accelerators to research in cosmic-ray physics is not useful, and Dayson's arguments are largely unsound.

Investigations on cosmic rays have yielded much of value and importance to high-energy physics, and Dayson rightly emphasizes this point. He is also right in recommending intensification of research in cosmic-ray physics. The point is that workers in high-energy accelerator physics did not -- and still do not -- appreciate the results and possibilities of cosmic-ray physics. This lack of appreciation and sometimes neglect of cosmic-ray data is often due to the fact that they are not familiar with them. In part, also, it is due to the qualitative approach of cosmic-ray physicists, which is foreign to accelerator technology; cosmic-ray physicists are very often forced into conclusions based on far from sufficient or sufficiently accurate data, though in many cases these conclusions did turn out to correspond to reality.

On the other hand, the strictly quantitative character of the results obtained on accelerators "demoralized" the cosmic-ray physicists working earlier in the same energy range. A remarkable "inferiority complex" appeared in cosmic-ray physics, which slowed down industrialization in this field.

This low cost of cosmic-ray physics which was emphasized by Dayson is thus not an advantage but rather a handicap.

If really serious attention is paid to cosmic-ray physics, then it too will stop being cheap. The extra-atmospheric physics of cosmic rays requires the establishment of well placed orbiting space stations with changing personnel. A modest part of such a program is planned in Greenstein's paper, together with extra-atmospheric X-ray and gamma astronomy, to a total sum of about 380 million dollars. But even more improved and more expensive extra-atmospheric cosmic-ray physics cannot fulfil the research program that is within the capabilities of the next generation of accelerators. Individual sectors of Earth-based cosmic-ray physics are of course of major interest in this energy range and in these problems that do not intersect in the current decade with high-energy accelerator physics. They could play the part of qualitative and semi-quantitative pointers for the physics of the next generation of accelerators*. But here too the cost of experimentation increases considerably owing to the necessity of introducing major industrialization and correspondingly raising the scale of the installations.

Thus gradually the myth of the low cost of modern science other than high-energy physics is gradually fading away. The high cost of modern experimental work is due to the same reason as the high cost of a modern airliner in comparison with that of the most luxurious stagecoach of the 18th century.

* The Batavia accelerator, even when it reaches its design limiting energy of 500 GeV in the C.M. system, will be equivalent to a 2 x 16 GeV crossed-beam accelerator. From the standpoint of the "hierarchy of lengths" (Table 1), there seem to be few reasons to expect at these lengths fundamentally new physics, the appearance of which Dayson regarded as the justification for building the Batavia accelerator. Even so, here too generosity of Nature cannot be excluded. It is not impossible that much of what we can confidently expect to discover on the "unitary limit" accelerators can in fact be discovered on the Batavia accelerator, which would introduce certain modifications into the plans for the next-generation accelerator.

However, such expenditure in various sectors of national economic activity is becoming possible by the continuously rising national income, which in turn is due to technological progress and ultimately to science. At the same time, it should be remembered that the total world expenditure on science is only a small fraction of the global budget.

The allocation of means on the development of science still largely preserves the characteristics of patronage of the fine arts -- it is not always determined rationally by the intrinsic needs of science but by how much "can" be spent on science among other expenses, And in this "can" too there is much uncertainty and chance.

The point is not to share out at will previously specified and not very large portions of the pie (in Anderson's terminology) but to bring allocations for the development of science in natural, expedient, and rational fashion -- and therefore one ultimately beneficial to the national economy -- into sensible correspondence with the intrinsic and natural needs of various sectors of scientific endeavor.

If this is not done then it will be the fault of the scientist that he failed to convince the modern world and society that the approach to financing science must be rational and not one of a patron of arts.

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