What problems of physics and astrophysics are of special importance and interest at present? (Ten years later)

V. L. Ginzburg

P. N. Lebedev Physics Institute, USSR Academy of Sciences
Usp. Fiz. Nauk 134, 469–517 (July 1981)

Ten years ago, the author published an article with the same title in this journal [Usp. Fiz. Nauk, 103, 87 (1971) Sov. Phys. Usp. 14, 21 (1971)]. The aim of the present paper is to consider the changes that have occurred during the last decade with regard to the problems considered in the earlier paper. Naturally, some new problems have also arisen, and they too are considered in the present paper.

PACS numbers: 01.90. + g, 95.90. + v

CONTENTS

I. INTRODUCTION

In 1971, this journal published in the section "Physics of our days" a paper[1] with the same title as the present paper but, of course, without the addition "Ten Years Later." The paper[1] (called below 1) became a small book, was translated into several languages, and appeared in a third edition in 1980.[2] In view of this history, it appears to me that the questions and discussion in paper 1 do indeed interest many people, especially young physicists and astronomers. On the other hand, it is clear that the selection of a small number of problems as being of particular interest and importance is arbitrary in nature and must not lead to neglect of a great number of other problems. Also clear is the subjective nature of the choice made by the author, who never claimed, nor does now, that the paper 1 and the book of Ref. 2 should be regarded as anything more than works of a popular scientific nature. All this is said fully in 1 and Ref. 2, and need not be repeated here. But let me say this: If I were to rewrite paper 1, to avoid irritating some readers (under the assumption that all those who have criticized the paper did actually read it), I would choose for it a more neutral name (for example: Some Interesting and Important Problems in Physics and Astrophysics). Unfortunately, by its very nature, we cannot change the title of the present paper.

It will be clear from what I have said that during these ten years, in which the paper 1 has been reworked for new editions, I have followed the transformation in the directions and problems in the field of physics and astrophysics. Therefore, the idea arose naturally

 0038-5670/81/070585-29$01.10

of writing the present paper in order to trace the changes that have occurred in physics and astrophysics during the last decade. Of course, I am not referring here to physics and astrophysics as a whole, but basically only to the problems discussed in 1. The paper is therefore arranged similarly to 1, in which 17 problems were identified; in Ref. 2, there are already 21 problems, but some of them should be divided. In fact, we shall be concerned with about 25 problems and scientific directions.

Finally, I should like to emphasize the following. The present article is obviously not a "paper" in the customary sense, but neither is it a review of the literature. Therefore, it should not be judged by criteria appropriate in other cases. First, I have not considered it necessary (even in the list of references) to consider questions of priority. The inclusion of numerous names or references establishing priority would not help the reader. Moreover, one must frequently admit that the priorities "adopted by repetition" in the literature are frequently inaccurate or even incorrect. It would be entirely inappropriate to undertake here a special historical investigation into the numerous questions of priority. Second, I do not follow the impersonal style adopted in scientific papers, especially those in Russian. This style not only prohibits the use of the personal pronouns (I, me, etc.) but even requires the author to disappear into anonymity as far as possible. I remember how at his seminars Landau always interrupted speakers who began to say what they thought, assumed, and so forth by saying: "Don't forget that only your wife is interested in your biography." The impersonal style was developed through long experience in the development of science, and I regard it as entirely appropriate in scientific papers, reviews, monographs, and textbooks (and let me say that my own practice does not conflict with this). But journalistic articles, recollections, or a paper such as the present, which does not fit into any genre, are quite a different matter. By its very nature the present paper is personal and addresses my (i.e., by definition subjective) estimate of certain tendencies and directions in physics and astrophysics. I know colleagues who regard such an approach as inappropriate or immodest. Others will disagree with many of my estimates. This is all their business and their right. Least of all do I claim that my arguments are above dispute: indeed, I myself regard some of the comments as very contentious. I merely insist on a right to have my opinion and to be able to express it without fear. In such a situation, I cannot, nor do I wish to attempt "both to retain innocence and to acquire wealth." It is therefore necessary to use personal pronouns and not hide one's "biography" entirely. I hope very much that this form of exposition will not call forth a negative reaction in the readers.

II. MACROPHYSICS

Overall, macrophysics rests on a reliable foundation (classical and quantum mechanics, classical and quantum electrodynamics, including the special theory of relativity). It is therefore natural that, if one is speaking of something qualitatively new and fundamental, macrophysics develops more slowly and less dramatically than in the case of microphysics and astronomy (including cosmology). True, somewhat anticipating the event, I did include (see 1 and Ref. 2) in macrophysics the field of nuclear physics, which is barely separated from microphysics. On the other hand, the general theory of relativity (by which I mean Einstein's classical theory) belongs in essence to macrophysics, but "works" fully only in the cosmos, and it is therefore discussed in the astrophysical section of the paper. But even when allowance is made for the successes of nuclear physics and the general theory of relativity macrophysics has during the last decade notched up fewer deep and important new results than microphysics. In any case, the successes and results in science cannot be weighed on a balance, and many cannot be directly compared. Let us therefore eschew any rank ordering and get down to the specific problems.

1. Controlled thermonuclear fusion

This problem is already 30 years old. The original rosy optimism was fairly soon replaced by frequently even pessimistic estimates once it had become clear how capricious are hot plasmas and how difficult they are to confine in traps. But it gradually became clear that if one carefully controls the homogeneity of the magnetic field (or, more precisely, ensures there are no inhomogeneities of the field not forseen in the calculations), and also if heavier impurities are eliminated from a hydrogen plasma, then various magnetic traps (tokamaks, stellarators, and some others) operate basically in agreement with the expectations.[3a] As a result, there are no longer especial doubts with regard to the possibility of achieving success in systems with magnetic plasma confinement. But to verify the calculations and overcome various difficulties, it is necessary to construct larger and larger facilities. Naturally, this requires a lot of money, effort, and time. At the present period, tokamaks remain the favorites, but, so far as I can judge, their superiority over, for example, stellarators has not been proved. Investigation continues into "open" magnetic traps, which are known in the jargon as *probkotrons*. No one can really say for certain that open systems, which, in a certain respect, are the simplest and most convenient, will never be able to compete with toroidal systems.

During the last decade, there has been a considerable growth of interest in systems with inertial plasma confinement, in which a microscopic explosion is initiated in pellets containing a mixture of deuterium $D \equiv d$ and tritium $T \equiv t$ (of course, the possibility cannot be ruled out that pure D will be used in the future). In principle, the pellets can be initially ignited by light (lasers) or by electron or ion beams.[3b] It is particularly difficult to use electrons; hitherto, laser systems have been best studied; and interest is increasing in the use of ion beams. Unfortunately, as in the case of magnetic traps, to investigate the possibilities of the inertial method very large facilities are in general required. In fact, the study of the possibilities of controlled thermonuclear fusion has become in the seventies to an even greater extent than before not only a problem of physics but simultaneously a technological problem on an industrial scale. But physics is still the leading partner, since one needs the maturing of various principles and methods of plasma confinement, and an operating reactor with a net energy output has not yet been created by any of the routes under discussion.

2. High-temperature superconductivity

This problem was posed, at least in a modern form, in 1964. The aim is clear—to produce or find superconductors or inhomogeneous superconducting "elements" that remain superconducting at least at the liquid nitrogen temperature $T_{b,N_2} = 77.4$°K (the boiling point of nitrogen at atmospheric pressure). However, the present state of the theory of superconductivity, despite tremendous successes in different directions, is not yet capable of predicting the critical temperature T_c of the superconducting transition for more or less complicated compounds or insulator–metal–insulator sandwiches. Therefore, the recommendations that can be made in the search for high-temperature superconductors are qualitative and not sufficiently precise. Under the influence of these recommendations (and it is hard to say precisely what part they played) quite a large number of quasi-one-dimensional and layered (quasi-two-dimensional) compounds have been synthesized and not a few new superconductors discovered. But so far (with a reservation to be explained later concerning CuCl and CdS) the compound Nb_3Ge has the highest critical temperature, equal to $T_c \approx 23.2$ °K, as was found in 1973. However, it must be said that the searches for all the new superconductors have led to interesting results such as the discovery of metallic conduction (and superconductivity with $T_c \approx 0.3$°K) in the polymer sulfur nitride $(SN)_x$, which, obviously, does not contain atoms of metals. In 1980, superconductivity was discovered in an organic crystal: ditetramethyltletraselenafulvalene-hexafluorophosphate $[(TMTSF)_2PF_6]$. True, this crystal has metallic conduction at a low temperature, and also superconductivity with $T_c \sim 1$°K, only at a pressure of several kilobars.[4] Nevertheless, we here evidently have a new class of metals and superconductors, since the possibility of varying organic compounds comparatively easily in a number of cases is well known. In addition, for organic compounds there are some grounds for expecting fairly high critical temperatures.[5] Of course, no guarantee of success can be given in this direction, but the discovery of a new class of even low-temperature superconductors is of considerable interest. It is also worth mentioning here the experiments which led to the conclusion that sulfur (S) exhibits superconductivity under a high pressure and with a definite pressure treatment, the critical temperature T_c lying in the range 26–31°K.[6]

Returning directly to the problem of high-temperature superconductivity, it should be pointed out that the theoretical analysis[5] does not give grounds for denying the possible existence of equilibrium (or, perhaps, metastable) materials with $T_c < 300$ °K. At the same time, it is clear that if critical temperatures $T > T_{b,N_2} = 77.4$ °K are to be reached some rather stringent conditions must be satisfied, and there is no guarantee of success. Here it is necessary to seek and test for superconductivity more and more new substances, sandwiches, etc.

It is possible that successes in this direction have already been achieved. In 1978, the communication Ref. 7 reported the discovery of "superdiamagnetism"[1)] in appropriately prepared copper chloride (CuCl) under a pressure of several kilobars. The superdiamagnetism effect was observed at temperatures as high as 150–200°K. Unfortunately, it is not yet clear whether the observed effect is genuine or whether we have here an experimental error or something which mimics genuine superdiamagnetism. If superdiamagnetism really is observed in CuCl, it could be due to the occurrence of a high-temperature superconducting phase, which can in principle happen on the transition to the superconducting state of various semiconductors or semimetals (see Ref. 5, Ch. 5). Another possibility is the formation of sandwiches of Cu and CuCl or the occurrence of truly surface superconductivity.[8] However, an entirely different hypothesis has also been put forward, namely, there could exist materials of a hitherto unknown type with spontaneous currents than exhibit superdiamagnetism but are different from ordinary superconductors.[8] This last possibility is not yet sufficiently clear even theoretically, let alone experimentally. However, it must be pointed out that although the further study of CuCl has not brought clarity, the existence of high-temperature superdiamagnetism in this material under certain conditions which have not yet been completely determined[7] has been confirmed in one other laboratory.[9] As is correctly, in my view, emphasized in the review of Ref. 10, the difficulty in elucidating the behavior of CuCl is nothing exceptional. There are precedents for such difficulties (for example, in the case of a number of semiconductors) when one is dealing with materials whose properties cannot be readily controlled. Impurities, and also various lattice defects or residual stresses could also play a part. It is therefore entirely possible that high-temperature superconductivity will be observed in CuCl.

Moreover, in 1980 a strong diamagnetic effect analogous to that described for CuCl was observed at the liquid nitrogen temperature ($T_{b,N_2} = 77.4$°K) in CdS crystals treated by pressure quenching. In this method, a pressure of about 40 kbar was removed at a rate exceeding 10^6 bar/sec.[11] No details of the method used to prepare the samples are given in Ref. 11 (which could be explained by the affiliation of the authors: US Army Armament Research and Development

[1)]A sufficiently weak magnetic field does not penetrate into an ideal superconductor (this property is called the Meissner effect). Formally, one can say that in the case of the Meissner effect, the magnetic susceptibility, as for an ideal diamagnet, is $\chi_{id} = -1/4\pi$. In ordinary diamagnetic substances $\chi \sim 10^{-4}$–10^{-6}. I give the name (and believe it is appropriate) superdiamagnet to a substance for which χ is comparable with $\chi_{id} = -1/4\pi$, say if $\chi \sim (0.01$–$0.1)/4\pi$. Superconductors are superdiamagnets, but the opposite assertion may not be true, i.e., superdiamagnetism need not necessarily be accompanied by superconductivity (in the sense of the absence of resistance to the flow of an electric current).[8]

Command, Large Caliber Weapons Systems). Undoubtedly, the result for CdS enhances the interest in not only CuCl but also quite generally in the as yet entirely mysterious mechanism of high-temperature superdiamagnetism.

In contrast to investigations in the field of controlled thermonuclear fusion, searches for high-temperature superconductors do not require the construction of giant facilities. Therefore, success may be achieved in a small laboratory and be entirely unexpected for other physicists. Moreover, such success may already have been achieved in the case of CuCl and CdS. If this really is the case, then the prospects for obtaining and studying high-temperature superconductors can be regarded as very promising.

3. New substances (the problem of making metallic hydrogen and some other substances)

The synthesis of new materials is usually considered to be in the field of materials science or chemistry. But this is not the case when we come to substances like metallic hydrogen. This is undoubtedly a problem in physics, and we do not know how to solve it.

There is no doubt that a metallic phase of hydrogen exists at pressures exceeding 1.5–2 Mbar. Probably, metallic hydrogen will be a superconductor, indeed, a high-temperature one ($T_c \sim 100-200\,^\circ$K), which makes this metal even more interesting. There have already been some indications in the literature (see Ref. 2 for references) but metallic hydrogen has been obtained, but overall the problem is not yet clear. Specifically, one cannot be completely sure that the metallic phase of hydrogen really has been observed and, most importantly, its properties (in particular, with regard to superconductivity) still remain entirely unknown. The main difficulty is the need to create pressures greater than 2–3 Mbar. This can be readily achieved by means of shock waves, but then, in general, heating occurs, and one must also contend with the difficulties of measuring a number of parameters of the metal during a very short time. Under quasiequilibrium conditions, the necessary pressure can be created in small volumes (between miniature anvils) by simple presses, but suitable materials for this purpose are not available. At such pressures, even diamond begins to "flow."[12] Here, a new approach is evidently needed. We are clearly still a long way from the time when, by some means or other, a "piece" of metallic hydrogen will be obtained.

In paper 1, another "exotic" substance considered was anomalous (superdense or polymer) water, whose existence was widely debated in the literature at that time. It was noted in 1 that "Thus, the question should be regarded as open, although, in my opinion the communications [to which reference was made in 1, V.G.] leave little hope for the existence of pure polymer (superdense) water. Regardless of the final answer, however, the investigations already performed indicate how difficult it is to answer such a question as the possible appearance of a new form of one of the most abundant substances; .... This example is instructive in many respects, particularly as a reminder of the need to regard any discovery as finally established only after repeated and exhaustive verification."

This comment, which expresses doubt with regard to the existence of anomalous water, though the question was regarded on the whole as open, prompted a number of authors of papers on anomalous water to write a special letter to this journal [Usp. Fiz. Nauk, 105, 179 (1971); not translated in Sov. Phys. Uspekhi]. In this letter, I was advised "... not to draw premature negative conclusions which although of appealing simplicity are based on an incomplete, one-sided, and uncritical use of the literature of the question." However, only a short time passed before the problem of anomalous water was disposed of: it was found that the investigated liquid was ordinary water containing a number of impurities.

I dwell on this episode only because I should like to emphasize once more how important it is to examine the experimental data from all sides, especially when far reaching conclusions are drawn on the basis of them. The authors of such papers have a right to publish them, since they risk more than others. Moreover, and this is objectively more important, the publication permits more rapid verification in other laboratories. Therefore, in my view, one should not judge too strictly (as is sometimes done) authors who have published an incorrect paper, provided, of course, they made a genuine mistake and their experiment was basically at an adequate level. But no one has the right to demand the recognition of a "discovery" before it has been confirmed in several places. Within reasonable limits, authors have a right to err, but the rest of us have no less right to doubt.

4. Metallic exciton (electron–hole) liquids in semiconductors

In 1, the problem indicated in the title was considered separately from all the others in the field of semiconductor physics, and, as I believe, there were good grounds for this. But the problem is now basically solved (or, if you wish, confidence has been justified)—metallic exciton liquids in semiconductors have been produced and to a large extent investigated. This problem has even formed the subject of a special monograph[13a] the English original of which appeared in 1977. True, not everything has yet been done (but that is almost always the case), and essentially new problems have arisen (they are associated with quasi-one-dimensional and quasi-two-dimensional semiconductors; see Ref. 2 and the literature quoted there). Nevertheless, today it is hardly possible to justify mentioning the problem of a metallic exciton liquid as the only representative of semiconductor physics and almost the whole of solid-state physics. Subjects of great interest, which are investigated on a broad front, are metal–insulator transitions and disordered semiconductors.[13b] Here, we can also include the so-called spin glasses and quantum crystals, and also layered and filamentary compounds (materials).

5. Phase transitions of the second kind (critical phenomena). Some examples

Strictly, phase transitions do not constitute a single problem but something larger. Of course, in all phase transitions there are common features, and a general theory of phase transitions can be delineated. At the same time, the typical phase transitions of the first kind, in which (and near which) the thermodynamic potentials of the phases do not have singularities, are in no way remarkable (actually, this is not true if one considers superheating and supercooling, the formation of nucleating centers, and the kinetics of transitions). But transitions of the second kind, transitions of the first kind which are nearly of the second kind (for example, have a fairly small latent heat of transition, which is equal to zero for transitions of the second kind), and critical points do have, in contrast, singularities and have long attracted particular attention. Of course, the singularity in the thermodynamic potential is by no means always clearly manifested, and in a number of cases (for example, for a transition to the superconducting state and, usually, for magnetic and ferroelectric transitions) a fairly simple theory, in which fluctuations and the singularity are ignored, is suitable. Such an approach, which is due to van der Waals, Weiss, and others, was developed systematically by Landau and is frequently called Landau's theory of phase transitions.[14] The fact that such a theory disagrees with experiment near the critical point in a liquid was already noted at the end of the last century.[15a] A clear example of a transition whose description by Landau's theory is invalid is the λ transition in liquid helium (the transition HeI $\rightleftharpoons$ HeII in liquid ^{4}He).

The creation of a theory of phase transitions of the second kind and critical phenomena that takes fluctuations into account appropriately and, quite generally, makes it possible to describe, at least in principle, all real transitions, has become one of the most fundamental problems in the physics of condensed media. The problem turned out to be very difficult. However, as early as the sixties it proved possible to make significant advances, and these successes have been strengthened during the subsequent decade. The introduction of the so-called critical exponents, the use of the scaling hypothesis, and the development of rather powerful methods of approximate calculation of the critical exponents in conjunction with numerous more accurate measurements of the various quantities near the transition points—all this has greatly advanced the theory of phase transitions. Since the main achievements of theory in this field have already been reflected in a textbook of theoretical physics[14a] (see also Ref. 14b), we shall not dwell on them here.

The question that should not be avoided here is the following: To what extent can the theory of phase transitions be regarded now as complete with regard to its foundations? The possibility of exact calculation of, say, the critical exponents is not of course needed for an affirmative answer to the question—in the physics of the condensed state, the exact calculation of constants or coefficients is the exception rather than the rule. But we must undoubtedly require of theory that it be capable of treating in a unified manner all the thermodynamic and kinetic processes and phenomena in the region near the transition point. The coefficients in the corresponding equations may, within definite limits, be chosen on the basis of experimental data. If we consider the theory of phase transitions with even these few limited requirements, it cannot be regarded as anywhere near completed. Leaving kinetics aside, we must point out that already in thermodynamics using critical exponents it is frequently impossible to specify the regions of applicability of particular limiting laws with increasing distance from the transition point.[2)] And the important thing is that a restriction is usually made to homogeneous media, whereas there are also numerous problems of considerable interest involving granules or defects, inhomogeneous external fields, and so forth. Finally, there are a number of kinetic and dynamic problems (flow in liquid crystals and in liquid helium, propagation of sound, relaxation of various quantities) which must also be solved near the phase transition point and, moreover, become particularly interesting near this point. In the light of such natural requirements, the incompleteness of the theory of phase transitions is manifest. Specifically, this can be seen in the example of the investigations into the superfluidity of helium II near the λ point,[16] although in this case the theory has advanced further than in other cases.

Thus, the problem of phase transitions remains important and occupies a particular position in the development of general theory. But we must also include in this problem some specific transitions and some particular phenomena near the transition points. An example here is the scattering of light, which has a number of interesting features near the transition points.[17] The same can be said of the scattering of x rays and neutrons.

With regard to individual phase transitions or even transitions in an entire class of substances, the last decade has brought many new results. We mention here "incommensurable" phases in ferroelectrics[15b] and magnets, phase transitions in liquid crystals, phase transitions in quantum crystals, quasi-one-dimensional and quasi-two-dimensional substances, phase transitions on surfaces, and phase transitions in liquid ^{3}He and atomic hydrogen. Each of these questions could and should be the subject of a separate paper, and this has already been reflected in part in this and other journals. Therefore, I shall not even attempt to consider all the listed cases and will restrict myself (see however Sec. 6 below) to some remarks about liquid ^{3}He and atomic hydrogen.

The possibility that in liquid ^{3}He (as in superconductors) "pairs" of two ^{3}He atoms with integral spin could

---

[2)] For example, the density of the superfluid part of helium II near the λ point, which corresponds to the temperature T_λ, is written in the form $\rho_s(T) = \text{const}(T - T_\lambda)^{2\beta}$, where the critical exponent β is close to $1/3$ (it follows from the experimental data that $2\beta = 0.67 \pm 0.01$). But what is the accuracy of such an expression for $\rho_s(T)$, especially with increasing distance from the λ point?

be formed was already discussed quite a long time ago. The formation of pairs with integral spin and their subsequent Bose–Einstein condensation must lead to superfluidity analogous to superconductivity (it is well known that superconductivity can be regarded as the superfluidity of a charged electron fluid in metals of a proton fluid in neutron stars). However, at that time, it did not prove possible to find a reliable theoretical estimate of the temperature of the superfluid transition, and the experimental results were to a considerable extent unexpected. Thus, in 1972 and 1973 it was found[18] that in liquid ^{3}He (true, at a pressure exceeding 34 atm) there occurs not one but two phase transitions at temperatures approximately equal to 2.6×10^{-3} and 2.0×10^{-3}°K, respectively. It was later established that we are dealing here with transitions to superfluid states differing in the total angular momentum of the "pairs." The attraction leading to the pair formation is evidently basically of the exchange type (forces of the same type lead to ferromagnetism). The investigations into the superfluidity and other effects in liquid ^{3}He (it is worth mentioning that this isotope is extremely rare—its abundance in nature is less than that of the isotope ^{4}He by several orders of magnitude) made during recent years are remarkable for their subtlety and range.[18] We are here dealing with the range of temperatures less than 3 mK from absolute zero and an object (superfluid ^{3}He) characterized by great complexity (compared with superfluid ^{4}He) due to the presence of orbital and spin angular momenta. I am inclined to believe that in the field of the physics of condensed media the successes in the study of liquid ^{3}He are the most striking during the last decade.

Let us also consider the possible superfluid transition in a gas of atomic hydrogen, since this example is rather intriguing though in all probability of much less general physical significance than the transitions in ^{3}He. Under ordinary conditions, a gas of atomic hydrogen, produced in some manner, is rapidly transformed into a gas of molecular hydrogen (H_2). However, at a low temperature $T \lesssim 1$°K in a vessel whose walls are covered with superfluid helium II, a gas of atomic hydrogen "survives" for many minutes.[19] If, in addition, the gas is placed in a sufficiently strong magnetic field, the stability of atomic hydrogen is raised,[20] and, apparently, under attainable conditions its recombination can be regarded as precluded (the reason for this is well known: In the H_2 molecule, the electron spins have opposite directions; in a strong magnetic field, the spins of all the electrons point in one direction, and for the formation of an H_2 molecule the spin in one of the H atoms must be reversed, which is not easy). The H atoms with parallel spins in the ground state repel one another (or rather, at large distances between the atoms there is a certain van der Waals attraction, but it is weak). Therefore, such a gas at atmospheric pressure does not become liquified right down to the absolute zero of temperature. At the same time, in the Bose gas of H atoms at a low temperature, which depends on the density of the gas, Bose–Einstein condensation must occur, and the resulting phase must be superfluid (it is here important that the gas is not ideal, and it is allowance for the corresponding interaction that leads to the superfluidity).

Altogether, the problem of phase transitions undoubtedly remains one of the main directions in physics.

6. Surface physics

The physics of surfaces and the various processes and phenomena on surfaces have attracted interest and have been developed for more than one decade. It is already clear from the most general considerations that atoms and electrons on and near a surface are under different conditions than those in the interior, so that there are grounds for believing that on surfaces one can have new phases, various transitions between these phases, new modes and branches of excitations, and so forth. By "new," I mean here phases and excitations different from the bulk situation. For example, on the surface (in which we include a thin surface layer) the crystal lattice may have a different structure and (or) parameters; in the surface layer there may be magnetic ordering absent at the given temperature in the interior, and so forth. It is also known that various surface waves (acoustic waves, polaritons, magnons)[21] can propagate. Very closely related are the properties of thin films and layers, in particular, monomolecular layers, and also the behavior on surfaces of individual atoms, molecules, defects, and inhomogeneities.

Nevertheless, the section Surface Physics did not appear in the paper 1, and although it did appear in Ref. 2 it received very modest treatment. It is of course a moot point whether such insufficient attention (in the paper 1 and the book of Ref. 2) to surface physics was justified. In fact, this also applies to some of the other problems considered here. But, whether the truth, it is now entirely clear that special attention must be paid to the role of surface physics. Basically, the reason is that in recent years something which appeared barely possible has become real through the progress of experimental techniques. In a number of cases at least, physicists have learned how to obtain and control purity and the state of a surface (the degree of roughness, etc.), and new or greatly improved methods have been developed for investigating surfaces and surface layers, atoms on a surface, and inhomogeneities of a surface (steps, etc.). We mention such methods as LEED (low energy electron diffraction), ARPS (angle resolved photoemission spectroscopy), inelastic scattering of ions with energy of order of 1 MeV, electron microscopy, and the study of surface sound waves and surface polaritons (surface electromagnetic waves).[22] There are also some other methods which use light, x rays, and neutrons.

Many results have already been obtained. Surface magnetic ordering has been discovered. The investigations of the inversion layers on the boundary of Si and SiO_2, the properties of electrons on the surface of liquid helium,[22] the study of surface polaritons,[21] and the reconstruction of a number of crystal surfaces[22,23] warrant particular mention.

By surface reconstruction one means the change in the lattice parameter for atoms situated on the surface. For example, on the Si surface (111 face) the lattice parameter under certain conditions is seven times greater than in the interior. It is possible that in considering the reconstruction phenomenon it is important in a number of cases to take into account the part played by surface electron levels.

Both as regards their scale and significance, the investigations into phase transitions in two-dimensional and quasi-two-dimensional systems are impressive. In fact, this is not a particularly new problem, but it gathers strength all the time.[22,24,25] The problems here are very varied and, naturally, intimately related to surface physics.

There is no doubt that surface physics is in a process of rapid growth and will bring many new results.

7. Behavior of matter in superstrong magnetic fields. The study of very large molecules. Liquid crystals

The problems listed in the title have not all that much in common, were all absent in 1, but are mentioned in Ref. 2. These three problems are combined into a single section solely by the desire not to dwell on them in too much detail. However, I did not wish to omit these problems, as was necessary with many others, as going beyond the scope of the present paper.

Superstrong magnetic fields are fields in which the structure of atoms, molecules, or condensed substances formed from them are determined to a large degree by a magnetic field rather than by Coulomb forces. Such a situation arises when the Zeeman (magnetic) splitting between the levels exceeds the distance between the levels in the absence of a magnetic field. For the hydrogen atom, the characteristic energy difference between the levels in the absence of a field (or in a weak field) is $E_a \sim e^4 m/2\hbar^2 \sim 10$ eV. The Zeeman splitting is $E_H \sim e\hbar H/mc \sim 10^{-8} H$ eV (here, the field H is measured in oersteds or gauss, since H can also be understood as the magnetic induction B). Obviously, $E_H \gg E_a$ for $H \gg e^3 m^2 c/\hbar^3 = (e^2/\hbar c)^2 (mc/e\hbar) mc^2 \sim 3 \cdot 10^9$ Oe. For heavy atoms, the factor Z^3 (Z is the atomic number) appears on the right-hand side of this inequality).

It is clear from this estimate that the question of the behavior of matter in superstrong magnetic fields remained fairly abstract prior to the discovery of pulsars (1967-1968). But we now know that on the surface of magnetized neutron stars—pulsars—the magnetic fields reach values of $H \sim 10^{12}-10^{13}$ Oe. Thus, the surface layer of neutron stars and especially pulsars (the field on the surface of some neutron stars may not be so strong) is in a strong field. If, as is very probable, iron is predominant in this surface layer, then such iron will be a material quite unusual for us, probably consisting of iron molecules Fe_2 elongated along the field and forming some polymer structure with large binding energy. This last circumstance is important for the entire electrodynamics of pulsars, since it determines the possibility of stripping of electrons and ions from the surface (see Ref. 2 for some references to the literature).

Fortunately, we are not restricted to considering the properties of the surfaces of very distant neutron stars. For atoms and molecules, we cannot create superstrong fields (in the above sense) in a laboratory. But there are situations in which the effect of the magnetic field is stronger than the influence of the Coulomb forces even in fields which can be attained on the Earth. For example, the binding energy of hydrogenlike excitons in semiconductors is $m_{eff}/m\varepsilon^2$ times smaller than for the hydrogen atom (here, m_{eff} is the effective electron or hole mass, m is the electron mass, and ε is the permittivity of the material). In this case, the level splitting in a magnetic field is m/m_{eff} times greater than for an atom. As a result, the field is superstrong provided $H \gg 3 \times 10^9 \times m_{eff}^2/m^2\varepsilon^2$ Oe. For values entirely attainable in a semiconductor, $m_{eff} \sim 0.1m$ and $\varepsilon \sim 10$, the effect of the field is dominant when $H \gg 3 \times 10^5$ Oe, i.e., in fields that can already be attained. Quite generally, "exciton matter" can already be investigated in strong and even superstrong fields in the laboratory.

It is to be hoped that even this brief comment will be sufficient to make clear the distinctive and remarkable nature of the problem of the behavior of matter in superstrong magnetic fields.

As in 1 and Ref. 2, we shall here entirely avoid biological questions, despite their exceptional importance for science and the development of the whole of human society. If a justification is needed, it is sufficient to recall the well-known advice of not attempting to comprehend the incomprehensible. The reference to giant molecules, which are of fundamental biological significance (proteins, nucleic acids) is nevertheless made here for two reasons. First, such molecules occupy an intermediate position between "ordinary" molecules and a condensed medium or drops and filaments of a condensed medium. With proper reservations, one can under such conditions use concepts such as phase transitions, ordering, conduction bands, and so forth. Second, so far as I can judge, the development of effective methods for analyzing the structure of giant molecules, in particular under conditions when there are very few of them and they are in a solution or a mixture with other molecules, still lags far behind other branches of physics. The potential importance of such investigations is so great that this should not be forgotten by physicists.

Liquid crystals have long been known. But I recall the time when physicists regarded them as something of a curiosity—something that is at once a crystal and a liquid. The existence of many objects more amenable to investigation and the absence of technical applications all had the consequence that the study of liquid crystals was neglected. The situation now is quite different. Liquid crystals are widely used in technology, play an important part in biology and, finally, liquid crystals of various types and the phase transitions in them have been found to be of interest in various investigations in the field of the physics of condensed media. The interest in liquid crystals does not abate.

8. Rasers, gasers, and new types of lasers

A section with this title was not included in 1, despite the fact that the interest in lasers in science and technology is tremendous and continues unabated for already more than 20 years. However, although the development of laser technology, and also the application of lasers (including here nonlinear optics) represents a substantial physical and technical problem, it does not figure prominently among the questions with which I am concerned in the present paper. It is a different matter when we consider lasers of fundamentally new types and lasers with a power exceeding by several orders of magnitude the existing lasers (it is highly probable that new methods or principles will be needed to achieve this last aim).

In fact, we see here how arbitrary is any list of "particularly important and interesting problems." In virtually every branch of physics and astrophysics a jump by several orders of magnitude, and even sometimes by an order of magnitude, always presents a large problem, although by no means always a real one. As an example (of course, one of many) we can take high-pressure physics. Pressures up to about 1 Mbar have been essentially mastered, but, as we have already noted in Sec. 3, it is not possible to advance much further in a static regime, and here fundamental difficulties are encountered. The transition to static pressures up to 10 Mbar is not too small volumes and under control would be a significant step forward. But such a problem does not occur in our list (or at least, not explicitly), since a real physical problem cannot reduce to merely desires and talk.

Returning to the subject of lasers, we note that in recent years much has been written on the subject of free electron lasers.[26] We have here the realization and, in some schemes, the significant development of the fairly old idea of the generation of electromagnetic waves by a beam of relativistic electrons passing through an ondulator or a wiggler (in the simplest variant, this is a system of magnets that produce a variables field along the beam and causes the electrons to oscillate in the beam). It is not easy to see the analogy with a laser in systems of this type, and the expression free electron laser appears rather unfortunate. However, the name is not the essence. It is entirely possible that free electron lasers will be of practical interest in the field of microwaves and optics. With regard to the transition to the x-ray region of the spectrum, the effectiveness of a device such as an ondulator using dense relativistic electron beams is still very problematic.

It should be noted that the problem of producing very powerful x-ray sources has been basically solved by the use of synchrotrons (one can use for the same purpose a linear electron accelerator in conjunction with an ondulator, or make an ondulator section in a synchrotron). But in this case we are usually dealing with the incoherent radiation of individual electrons. One would however like to use coherent radiation, as in a laser, and open up the possibility of attaining a high degree of monochromaticity. Such a device—the analog of a laser in the x-ray region—may be called a raser, and in the case of gamma rays we can use the word gaser.[3)]

In systems with an electron beam, coherence "operates" only in fairly dense beams and under a number of other conditions, which are hard to realize in the x-ray range. It is in this sense, i.e., in connection with a coherent free electron laser (and it is only when coherence is used that such a name is meaningful), that the absence of clarity on the transition to the x-ray part of the spectrum was noted above.

Besides dense electron beams, it has been suggested that rasers could be developed on the basis of atomic transitions, and gasers on the basis of transitions in nuclei. Some information on this subject can be found in Ref. 2, Sec. 7. So far as I know, significant advances have not been made in recent years in this field. All the suggestions made previously in the literature appear very complicated (we can include here, for example, the suggestions which involve the use of atomic explosions).

Not everything dreamt becomes reality and still less something of practical interest. It is therefore entirely possible that rasers and gasers will never be constructed or, at least, will not find wide application. But who knows ... ? Some unexpected idea may, as has happened more than once in the history of physics, radically change the situation.

9. Superheavy elements (far transuranic elements). Exotic nuclei

In 1 nuclear physics was not only included in the chapter Macrophysics but also was represented by only a single problem (superheavy elements). Both of these facts were contentious, and it is now clear that in any case there are more "particularly important" problems in the field of nuclear physics. Therefore, Exotic Nuclei already appeared in Ref. 2.

The problem of the search for superheavy elements has not changed significantly (with a reservation which will become clear). It is true that in 1976 a communication was published in one of the most authoritative physics journals (Physical Review Letters) reporting the discovery of very heavy elements with $Z=116$, 126 and others. However, the paper was wrong. But only those who do not work are ensured against mistakes. I have already said that such mistakes should not be dramatized (Sec. 3). The papers quoted in Ref. 2 dis-

---

3)It is well known that the word laser derives from the initial letters of the English expression light amplification by stimulated emission of radiation. It is of course therefore incorrect to speak of an x-ray laser or a gamma laser. The words raser and gaser arise if in the word laser the letter l is replaced by r (Roentgen) or g (gamma). But, of course, terminology is of secondary importance, especially in view of the fact that in the case of rasers and gasers there are as yet no real devices of this kind.

cuss ways in which attempts have been made or may be made to synthesize or discover superheavy elements. At the present time, I can merely add that at the end of 1980 observation of the track of a nucleus with $Z \geqslant 110$ was reported.[27] The track was found in an olivine crystal of meteoritic origin.

Nuclei in cosmic rays leave traces in crystals which can be revealed by special treatment (in particular, etching and annealing). The track length depends on the atomic number of the nucleus. In Ref. 27, the observation is reported of about 150 tracks of nuclei of the uranium group, their length being 180–240 μm. There was also observed the one track 365-μm long, which corresponds to a nucleus with $Z \geqslant 110$. Of course, this result requires confirmation—the finding of other such tracks and also further proof of the assertion that we are concerned here with a nucleus with $Z \geqslant 110$. If the observed track does indeed belong to a nucleus with $Z \geqslant 110$, then the abundance of such nuclei relative to uranium nuclei is of order 10^{-3}.

With regard to various other problems in nuclear physics, it should be noted that in a number of cases the study of nuclei casts light on the nature of the interaction between nucleons and between nucleons and leptons.[28] Much attention is devoted to nuclear matter, which exists in the first place in neutron stars. Here, there is an obvious connection with astrophysics (see, for example, Ref. 29). Very interesting is the possibility discussed in the literature of the existence of nuclear matter and nuclei with density exceeding the ordinary density by a factor of two or greater (some references are given in Ref. 2). In the known nuclei, such a dense phase is evidently not realized, but there has been discussion of the prospects of observing "precursors" of it in some nuclei (it is well known that a phase transition affects the properties of matter before the transition point is reached).[30a] Much attention has been devoted in recent years to collisions of relativistic heavy nuclei. Altogether, there is no doubt that the study of nuclei still involves a number of fundamental physics problems (see also Ref. 30b).

III. MICROPHYSICS

By microphysics I mean (in basic agreement with the general understanding of the word) the physics of "elementary particles"—the study of the properties, structure, and interaction of protons, neutrons, and other baryons, photons, mesons, and leptons. Frequently, this field is called high-energy physics, which is, of course, one sided, since by no means everything in the investigation of particles is directly related to high energies.

The paper 1 was written at a time when microphysics was undergoing a period of confusion and uncertainty, although the ideas which have now led to brilliant results and breathtaking propsects had already been born and developed. The situation ten years ago and, undoubtedly, the inadequate competence of the author in this field had the consequence that the section Microphysics in 1 was weak. I failed to reproduce the feeling of the time, which, using Einstein's words,[31] can be characterized as "long years searching in the dark, full of premonitions, tense expectation, and the alternation of hope and despair."

It should be said also that the paper 1, its further development in Ref. 2, and the present paper in no way pretend to an adequate solution of such an exalted task. With regard to the comments made in 1 and Ref. 2 concerning the changed position of microphysics and its role "yesterday, today, and tomorrow," let me just say, in order not to return to the subject, that my opinion on this count has not changed. Let me quote from paper 1: "The problems of microphysics are the most fundamental and therefore for many the most attractive problems of physics." If we understand by microphysics the frontier to which physics has advanced at each stage of its effort to study the structure of matter, my words were true yesterday, are undoubtedly true today, and will be true tomorrow. But the objects under investigation have changed. At the time when atoms and nuclei were at the center of the attention of microphysics, that science was predominant in the whole of natural science, and determined the path of the development of mankind. Quarks and gluons, new types and forms of particles—these are all exceptionally interesting and important for physics but they occupy a position in science as a whole and in the life of human society that is different from that of atoms and nuclei. The modern position of microphysics in science most closely resembles that of astrophysics (including cosmology). And it is a remarkable situation when many, reading about quarks and the instability of the proton and about neutron stars and black holes, forget about their daily bread and nourish themselves by their interest in science. It is in fact only this change in the situating respecting microphysics that is clear from the foregoing, that was emphasized in 1 (Sec. 12) and in Ref. 2 (Sec. 16). I completely respect different opinions and discuss them in 1 and in Ref. 2, but I still regard them as either incorrect or the fruit of misunderstanding (in disputes, the opponents very frequently simply do not understand each other and have in mind quite different questions). It is of course possible that in the future microphysics will throw up a new and exceptionally important field of practical applications (similar, say, to the use of nuclear energy). Of course, it is quite impossible to deny such a possibility in any definite way. I merely assume that the opposite possibility is also not ruled out (especially in the foreseeable future); this in no way casts a shadow on microphysics.

In paper 1, the section Microphysics discussed the following problems: the mass spectrum (third spectroscopy), a fundamental length (quantized space), the interaction of particles at high and superhigh energies, and the violation of CP invariance. All these problems can be mentioned today too, and their significance has not diminished. For example, the nonconservation of CP invariance, discovered in 1964, still cannot be regarded as sufficiently understood, while the Nobel Prize for Physics in 1980 was awarded for the discovery of this nonconservation. However, taking a broad view, even in the briefest consideration of the problems of microphysics, it is now necessary to identify other problems in the first place. We shall do this here almost summarily (besides the bibliography given in Ref. 2, see Refs. 32–38 and in particular the review of the present state and prospects for high-energy physics in Ref. 39).

10. Quarks and gluons. Quantum chromodynamics

The question of the simplest "elements" of which matter consists has always been at the center of interest of microphysics. As such "elements," molecules and atoms were replaced comparatively recently by electrons, protons, neutrons, photons, and then various hyperons and mesons, and also neutrinos. The number of such particles, which were frequently called elementary (this adjective is now encountered less and less) became larger and larger (this applies particularly to the strongly interacting particles, the hadrons, which include both baryons and mesons[4)]). It is natural that a tendency therefore arose or, rather, was strengthened, to seek a unification in terms of the "simplest" of the elementary particles. Various approaches were proposed, and one of these was the hypothesis of quarks, which appeared in 1963–1964.

Initially, three quarks were introduced, all the hadrons being assumed to be constructed out of these. Later, especially after the discovery in 1974 of new particles[40] with properties that could be successfully interpreted on the basis of the quark model using quarks of a fourth type, charmed quarks, the idea of quarks received wide recognition. Therefore, a certain summary of many years of searching for the nature and structure of baryons and mesons can now be seen primarily in the creation of the new quark model of the structure of these particles.

When the quark hypothesis was put forward, it encountered a very self-contradictory state of affairs. This can be explained, first, by some general considerations which are presented below and force one to ask whether one can justifiably pose the question: Of what does a proton consist? Second, quarks are usually ascribed fractional electric charges equal to 2/3 and −1/3 (the charge of the positron or the proton is taken as the unit of charge). But such fractional charges have never been observed and were unusual. Moreover, all searches for free isolated quarks, which were carried on vigorously after 1964, failed to come up with positive results. Of course, it is very difficult to assert categorically that something does not exist. But this is very similar to what is currently regarded as most probable, namely, that in the free state, i.e., as individual particles like baryons, mesons, or leptons,

TABLE I.

Flavor (Species of quark)	Charge	Baryon number	Strangeness (S)	Charm (c)
u (up)	2/3	1/3	0	0
d (down)	−1/3	1/3	0	0
s (strange)	−1/3	1/3	−1	0
c (charmed)	2/3	1/3	0	1

quarks cannot exist. It would seem that on this basis one really could with complete justice doubt the very existence of quarks as physically real. Nevertheless, the quark model has not only not been abandoned but rather has strengthened its position and has hitherto notched up one triumph after another.

Here, there is no need, nor would it be appropriate, to set forth in detail the quark model. Referring to the papers of Refs. 35 and 39–41 and those cited in Ref. 2, we give for convenience only Table I, which contains the quantum numbers of the quarks of the four species or, as one says, flavors. All the quarks have spin 1/2, and they are therefore fermions. The baryons consist of three quarks, and the proton and neutron have, respectively, the compositions uud and udd. The strange and charmed quarks s and c occur only in the "strange" and "charmed" particles. For the antiquarks, all the quantum numbers are reversed, so that, for example, the antiquark $\bar{u}$ has charge −2/3 and baryon number −1/3. Mesons consist of a quark and an antiquark. For example, the configuration (state) of the π^+ meson is $u\bar{d}$ (clearly, the charge of such a configuration is $2/3+1/3=1$, the baryon number is $1/3-1/3=0$, and the spin can be zero, as it must be). Unfortunately (?), it is not possible to make a restriction to these four particles and four antiparticles. It proved necessary to introduce one further quantum number, which, entirely arbitrarily, was called color, so that the quarks of each flavor can also be in three states of different color (called, for example, red, yellow, and blue). The three quarks that form a baryon must necessarily have three different colors, which makes the baryon "colorless." The mesons are also colorless, since the color of the antiquark corresponds to the "anticolor" of the quark.

Thus, the total number of quarks and antiquarks is, when color is taken into account, already 24. In fact, we are not yet at the end. Both theoretical and experimental data (beginning in 1976) now provide a basis for the introduction of a quark of a fifth flavor, and a quark with a sixth flavor has appeared in theory. For these two quarks (the fifth and the sixth), as for the other four (see Table I), the baryon number is 1/3 and the spin 1/2. The charge of the fifth quark b (it is called the bottom or beauty quark) is −1/3; this b quark has a mass of order 5 GeV (the mass of the c quark is of order 1.5 GeV and, apparently, appreciably exceeds the masses of the lighter u, d, and s quarks).[5)] As has already been said, the existence of

[4)]Earlier, the word mesons was used not only for hadrons with integral spin (such as $\pi^{\pm,0}$ mesons) but also for some other particles, for example, muons (which were called $\mu^{\pm}$ mesons). Here, we shall use the modern terminology, according to which particles with half-integral spin that do not interact strongly are called leptons ($e^{\pm}$, the positron and the electron, the $\mu^{\pm}$ muons, the $\tau^{\pm}$ leptons, neutrinos). The particles with integral spin that do not interact strongly are called, for example, scalar bosons, vector bosons, etc.

[5)]Since quarks do not exist (and have definitely not been detected; see however Ref. 111) in the free state, the concept of their mass is rather formal or, if you wish, of an extrapolated nature.

the fifth quark b is confirmed experimentally.[41] In contrast, the quark of the sixth flavor t with charge 2/3 (it is called the top or truth quark) has not yet been detected even indirectly (in fact, this is all that one can ever say of quarks bound in hadrons). Apparently, the mass of the t quark is $m_t > 15$ GeV, i.e., appreciably higher than the mass of even the b quark, by virtue of which hadrons containing the t or $\bar{t}$ quark cannot be produced[42] in the existing accelerators.

If there are six flavors and three colors, the total number of quarks and antiquarks is obviously 36. It has been conjectured in the literature that the numbers of flavors and colors could be greater. At the very least, it is impossible to assert that the quark model is limited to 24 or even 36 quarks and antiquarks. It is sufficient to say that the quarks interact with one another and that this interaction is associated with the exchange of the quanta of certain fields (just as the electromagnetic interaction is associated with the exchange of photons). But it is necessary to introduce several (usually eight) fields which hold the quarks together; they are called gluon fields (from the English word glue). With each such field there are associated the corresponding particles, or quanta (gluons). Recently, more or less definite experimental indications of the existence of gluons have been obtained.[36] Thus, the total number of particles in the quark model of matter reaches several tens. Is this not too many—such is the question which arises involuntarily if rhetorically when the advantages of the quark model are discussed. Of course, such doubt is by itself of no great importance; for despite the large number of quarks and gluons the reduction of the hundreds of hadrons to combinations of quarks, even of several species, introduces an order and possesses beauty.

Much deeper and more important is a different question: Is there a meaning in saying that there exist particles (quarks) which cannot be observed in the free state? What then does it mean to say that a baryon "consists" of three quarks? In fact, this last question can be given a fairly clear answer: the scattering of, say, electrons and neutrinos by the proton takes place as if the proton contains (consisted of) three point particles; they are called partons, and quarks could certainly play the part of these partons.

But this does not yet prove that quarks exist. For example, a magnetic needle, like any other magnet, behaves as if there were magnetic poles at its ends. In fact, no such magnetic poles exist (at least, under ordinary conditions) and everything reduces to currents (the motion of electric charges) and the dipole (spin) magnetic moments of a number of particles (electrons, protons, etc.). This analogy between magnetic poles and quarks would appear to be very deep: No matter how one divides a magnet, the poles still remain "paired" (i.e., every magnet has a north and a south pole); in exactly the same way, no known transformations of hadrons lead to the appearance of isolated quarks, and the quarks are produced only in the form of baryons and mesons, i.e., in triplets and pairs. It should also be noted that the quark model itself is not unique. Until recently, schemes were even proposed in which the charges of the quarks are integral. However, rather convincing experimental indications have now been found[43] supporting fractional quark charges.

The problem of the existence of quarks can be regarded as one of the aspects of the general problem of the possibility of distinguishing simple (elementary) and composite (complicated) particles. We can, for example, assert that the hydrogen atom consists of a proton and an electron, since this atom can be broken up (ionized) readily—it is necessary to expend only an energy greater than 13.6 eV, which is very small compared with the energy 1 MeV needed for the production of an electron–positron pair. For this last reason, the number of particles in atomic physics is effectively conserved and, specifically, the hydrogen atom can be split into a proton and an electron, which are stable particles that exist in the free state. But does the neutron consist of a proton and an electron, as was assumed when long before its discovery the neutron appeared as a hypothetical hydrogen "microatom"? It is well known that this question is answered in the negative, and the decay of the neutron is interpreted as the production of an electron and an antineutrino with transition of the neutron into a proton ($n \to p + e^- + \nu_e^- + 0.8$ MeV). In particular, it cannot be assumed that the neutron "consists" of a proton, electron, and antineutrino because the proton itself can decay into a neutron, positron, and neutrino (although this involves the absorption of energy, it takes place for protons in β^+ active nuclei). Such examples indicate the limited validity of the concept "consists of" when applied to particles with appreciable binding energy or high energy of the decay products. But, generally speaking, this is precisely the situation for the quark model of hadrons.

Thus, the comparatively large binding energies and, most importantly, the absence of quarks in the free state (this property is referred to as quark confinement) undoubtedly give grounds for suspecting that quarks are only auxiliary entities (such as magnetic poles in electrodynamics), convenient for describing various phenomena and properties of hadrons but not having any particularly fundamental nature. In particular, such a point of view was expressed at the end of his 50 year Odyssey in physics by one of the creators of the quantum theory: Heisenberg.[44] Physicists actively occupied with the problem[45] also express caution when speaking of the "existence" of quarks and the fundamental significance of the quark picture.

In science, doubts live long. Like caution, they are undoubtedly helpful. But life, development takes its own course, apparently "unconcerned" with care and doubts. The quark model and the theory of strong interactions constructed on its basis—quantum chromodynamics—have proved very fruitful and heuristic. Much may yet change, but it is hard to doubt that there is no way back: Quarks and quantum chromodynamics represent a great achievement of physics.

Ten years ago in the paper 1 only three lines were devoted to quarks when I listed the various directions taken in the attempts to solve the problem of the mass spectrum of the "elementary particles." Of course, this attitude to quarks betrays a lack of awareness on my part with regard to this question. But even now I believe that in 1971 the quark model was one of many, and its viability and fruitfulness were not yet clear (at least, not to a great many physicists). Now, however, the situation has changed completely, which makes it expedient even here to dwell on the quark model in somewhat more detail (in which use has partly been made of the text of Sec. 11 in Ref. 2).

What are the problems associated with quarks currently under discussion?

Although some experiments continue, there is now almost no doubt that quarks are "confined" in hadrons and, therefore, do not exist in the free state (in fact, it is conceivable that quarks could be "liberated" only under exceptional conditions, say, at a very high energy; then, perhaps, there would be no contradiction with facts—the impossibility of freeing quarks by the existing means and their very low concentration in natural materials). What is the mechanism of confinement (the English word confinement is also frequently used in Russian literature)? A definite answer is not yet available, though it may be contained in the scheme of quantum chromodynamics that is already used. The point is that the corresponding equations are nonlinear and in fact very complicated (say, compared, with the equations of quantum electrodynamics). Therefore, by no means everything has yet been elucidated even on the basis of the already existing theory. The development of quantum chromodynamics is a very substantial and topical problem.

At the same time, as we have already emphasized, even in the most positive attitude to the quark model the number of quarks cannot yet be regarded as finally established. In fact, at low energies this is not so important, since then it is the lighter quarks which mainly come into play, above all the u and d quarks. A deeper question is the following: Are quarks the final ultimate "bricks" out of which hadrons are constructed? The mere fact that there are many quarks had led to the hypothesis that there exist protoquarks or prequarks, which appear in the literature under various different names (preons, etc.).[32] In any case, further subdivision should, it would appear, "end" at some time. It is hard to believe in the existence of an "infinite" Russian doll with one doll within another *ad infinitum*. Actually, facts such as the mutual transformation of particles into each other (in the first place, the transformation of the proton into the neutron and vice versa), which was clarified in the previous stage in the development of microphysics, and the confinement of quarks, which now occupies the center of the stage, indicate the appearance of qualitatively new features in each successive "doll." But then the very idea of this "Russian doll" becomes nominal. But what will be the next stage? It is at least possible that the "splitting" of the hadrons ends at the quarks, but neither are there yet any real arguments against the introduction of protoquarks. What will one think about this matter a further ten years on? No one can attempt to answer such questions.

11. Unified theory of the weak and electromagnetic interactions. The $W^{\pm,0}$ bosons. Leptons

During the last three decades of his life, Albert Einstein devoted much effort to the creation of a unified field theory. When he began this work, only two interactions were known—the electromagnetic and the gravitational. Naturally, attempts were made to unify them. Subsequently, in fact, the weak and strong interactions became known too, but, so far as I know, Einstein did not make any attempts to broaden the spectrum of his efforts to a unified theory of all the interactions. Einstein's work on the creation of a unified field theory was not one of the fashionable directions at that time and, moreover, it was not successful from a pragmatic point of view. Therefore, "there has been, for some time, among some people, the impression that the idea of unification was some kind of obsession affecting Einstein in his old age."[46] But, again in Yang's words: "Yes, it was an obsession with an *insight* of what the fundamental structure of theoretical physics should be. And I would add that that insight is very much the theme of the physics of today."[46] [6)]

Indeed, the unified theory of the weak and the electromagnetic interaction (or, to use the increasingly popular term, electroweak interaction), grand unification—the unification of the weak, electromagnetic, and strong interactions—and, finally, superunification—the unification of all three interactions with the gravitational interaction—are now at the center of attention of theoretical physics.

As early as the thirties it was suggested that the weak interaction is transmitted by intermediate vector bosons $W^{\pm}$ in the same way as photons can be regarded as the "carriers" of the electromagnetic interaction.

[6)] In connection with Einstein's aim of constructing a unified field theory and his attitude toward quantum mechanics, much has been written and said about the "tragedy of Einstein" in the last period of his life. It is obvious even from the above quotation how unjustified is such a judgement on the basis of Einstein's work on unified field theory. As regards Einstein's attitude to quantum mechanics, it is quite wrong to assume that Einstein "did not understand" or did not value quantum mechanics. On the contrary, Einstein understood and recognized[47] the successes achieved by means of quantum mechanics, but regarded this theory as "incomplete" even in its region of applicability. More specifically, Einstein did not regard as final the probabilistic elements contained in quantum mechanics.[47] Like the majority of physicists, I do not share Einstein's position in this question, which is to a large degree a question of epistemology as well as physics. It is also worth saying that the discussion of the foundations of quantum theory, the questions relating to its completeness and statistics, the theory of quantum measurements, etc., do not leave the pages of scientific journals. Much has been written about this in the most recent time. One way or another, assertions about a "scientific tragedy" of Einstein appear entirely unjustified.

In this sense, a deep analogy between the weak and electromagnetic interactions is possible. But the idea came up against two very important circumstances. The mass of the photon is zero, and the photons themselves are well known to us. In contrast, the mass of the intermediate $W^{\pm}$ bosons must be very large, and they have not yet been detected (of course, this last fact is attributed to precisely the circumstance that the $W^{\pm}$ bosons are so massive that they cannot be produced in existing accelerators). Under such conditions, the hypothesis of intermediate bosons was on much the same footing as many other suggestions and predictions that have no solid foundation. However, in 1967 a theory was created in which photons and $W^{\pm}$ bosons are treated in a unified manner, and the difference between their masses is explained.[32,33,48]

The unified theory of the electroweak interaction, and also grand unification and superunification are based on deep ideas relating to symmetry, generalized gauge invariance, and spontaneous symmetry breaking. To avoid profanation and bearing in mind the aims of the present paper, I shall not attempt to explain these ideas even in outline. I shall merely refer to the papers of Refs. 32–35 and 48 and also Ref. 49, which is comprehensible not only to theoreticians and discusses the connection, very important for understanding the essence of the matter, between gauge theories and superconductivity.

But it is appropriate to emphasize two aspects here. First, the powerful features of the unified theory of the weak and electromagnetic interactions became clear only several years after its creation (I am referring here in the first place to the elimination of divergences or, as one says, the renormalizability of the theory). Second, one of the important elements of the theory is the introduction of not only the charged $W^{\pm}$ bosons but also the intermediate vector neutral boson $W^0 \equiv Z^0$. The exchange of such a neutral particle leads already in the first approximation to scattering processes which are absent in the same approximation if only the $W^{\pm}$ boson exists (this is the case, for example, for the scattering of the muon neutrino ν_μ by the electron e and for the scattering of both ν_μ and the "ordinary" electron neutrino ν_e by the proton or neutron). In theoretical jargon, processes involving the W^0 boson are said to be associated with neutral currents. And in 1973, and with even greater certainty in the following years, it was established experimentally that neutral currents do indeed exist.[32-35,48] In this, undoubtedly, one can see a triumph of theory. There are other confirmations of it as well. The Nobel Prize for Physics in 1979 was awarded for work in the unification of the weak and electromagnetic interactions.[32,33]

However, the existing theory of electroweak interactions can hardly be regarded as proved before the actual discovery of the $W^{\pm,0}$ bosons. According to some estimates, the mass of the $W^{\pm}$ boson lies between 77 and 84 GeV, while the mass of the $W^0 \equiv Z^0$ boson is in the range 88–95 GeV.[32] The masses of the $W^{\pm,0}$ bosons may be different, but there are no grounds for thinking that they are different in order of magnitude, and, thus, $W^{\pm,0}$ bosons should already be produced in the next generation of accelerators[50] (see also Sec. 13).

Besides the $W^{\pm,0}$ bosons, in gauge theories (especially in those in which one attempts to treat simultaneously the weak, electromagnetic, and strong interactions) a further series of particles is introduced, in particular, scalar particles. Unfortunately, the masses of some of them may be colossal (up to 10^{14} GeV and higher; see Refs. 32, 33, and 39), so that it may be necessary to wait many decades if not longer before we can establish whether such particles exist or not. This aspect will hardly prevent one making an overall judgment of gauge theories, since some uninvestigated problems and regions always remain. At the same time, at least one scalar boson is needed for the gauge theory of the electroweak interaction (the theory does not predict the mass of this particle, and it may lie in the range of energies already accessible[39]).

The uncertainty in this question leaves the theory somewhat incomplete even in its foundations. But there is now a further matter presenting the theory with a difficult problem. It follows from the unified theory, as it is interpreted, that the connection between the weak and electromagnetic forces must lead to some small but qualitatively new effects in atomic physics. Specifically, parity should not be conserved in interactions between electrons and nucleons. This must result in a rotation of the plane of polarization of light which passes through a vapor of, say, bismuth in the region of the frequencies of some atomic transitions (if parity is conserved, the corresponding rotation should be strictly zero). Corresponding experiments have been made at Oxford (in England), Seattle (in the United States), and Novosibirsk and Moscow (in the Soviet Union). At the present time, the English and American data appear somewhat uncertain; the data of the Novosibirsk group completely confirm the theory,[51] while the data of the Moscow group strongly contradict the prediction of the theory.[52] Thus, according to the data of Ref. 51, the quantity R, which characterizes the angle of rotation of the plane of polarization, is $(-20.2 \pm 2.7) \times 10^{-8}$, while the theoretical value of R is around -18×10^{-8}. According to the data of Ref. 52, there is virtually no rotation of the plane of polarization: $R = -(2.3 \pm 2.3) \times 10^{-8}$. What should our attitude be in such a situation? The answer is unambiguous: We need new experiments made by other groups. Evidently, it will not be necessary to wait long for the results. If the prediction of the theory is confirmed, this will be a great success, and no clouds will appear on the horizon of the existing theory[32,33] (as we have already noted, the $W^{\pm,0}$ bosons should not yet be produced in the existing accelerators, so that one cannot here speak of contradictions). But if the negative result of Ref. 52 is confirmed, it will probably indicate that a modification is needed in the theory of Refs. 32 and 33 but not yet its complete abandonment. We shall not attempt to speculate on the consequences.

Thus, the unified theory of the weak and the electromagnetic interactions has had great and impressive successes. But even without going into the questions relating to a larger unification (see the following section), at least the three listed basic problems remain in the theory of the electroweak interaction (the discovery of the $W^{\pm,0}$ bosons, the problem of the scalar particle, and the elucidation of the situation with regard to the rotation of the plane of polarization in bismuch).

Among the major successes of microphysics during recent years, we must also mention the discovery of another lepton (i.e., a particle like the electron and μ lepton with no strong interaction). This is the τ lepton with a mass of about 1780 MeV (see Ref. 53). A corresponding neutrino ν_τ can also be assumed to exist, although the proofs here are of an indirect nature.[53] The question of how many leptons can exist remains open, though some bounds do follow from cosmological arguments.[39,54]

Overall, the general problem of the mass spectrum of the particles, i.e., the prediction of the "parameters" (in the first place, the masses and spins) of all the existing particles, is far from solved, particularly if one has in mind particles which "do not fit" the grand unification and superunification schemes (see Sec. 12). Among such purely hypothetical particles are tachyons (which most probably cannot exist) and maximons,[55] and also other particles,[39] having only the gravitational interaction.

12. Grand unification. Decay of the proton. Superunification. The neutrino mass

The successes of the unified theory of the weak and electromagnetic interactions, on the one hand, and the achievements of the theory of the strong interactions (quantum chromodynamics) on the other stimulate the creation of a unified theory of all these three interactions (thus, leaving out only the gravitational interaction). This is the so-called grand unification, to which we have already referred. It is usually based on three quark species [the quark doublets (u,d), (c,s), and (t,b), each quark having three colors] and three lepton species [the doublets (ν_e,e), (ν_μ,μ), and (ν_τ,τ)]; every particle has an antiparticle (all the particles have spin 1/2, i.e., are fermions). Out of all these 24 particles (not counting the antiparticles) only the t quark has not yet been detected (true, the existence of the neutrino ν_τ associated with the τ lepton is established in a rather indirect manner[53]). All these particles together with a number of scalar (spin 0) and vector (spin 1) bosons are combined together subject to some requirements of symmetry and gauge invariance—and this gives the grand unification. It is still far from complete and by no means unique (see Refs. 32–35, 37–39, and 112). For me, much remains obscure, and therefore there are even fewer grounds for going into details here. The main qualitative results of grand unification, which I should like to emphasize here, appear natural on the basis of the most general considerations. Indeed, since quarks and leptons are combined in some manner, they must, in general, be transformed into each other and contribute to the masses of all particles.

From this there follows a striking possibility—the proton turns out to be unstable! Indeed, from energy considerations a decay of, for example, the type $p\to\pi^0+e^+$ is entirely possible. If the baryon number is conserved, such a decay is forbidden, but the possibility of transformation of quarks into leptons and vice versa entails precisely nonconservation of the baryon number. The available experimental data indicate that the mean proton lifetime satisfies $T_p>10^{30}$ years (we recall that the "age of the Universe," i.e., the time of its observed expansion, is only of order 10^{10} years). In volume containing 10^4 tons $=10^{10}$ g of water there are approximately $N=10^{34}$ nucleons and (assuming that a bound neutron decays with approximately the same probability as the proton) and that $T_p=10^{31}$ years $N/T_p=10^3$ decays must be observed in a year. However, the theory of the grand unification does not yet predict the exact value of T_p. There are variants of the theory in which $T_p\to\infty$ (the proton is stable), but in some of the investigated variants $T_p\sim10^{31}-10^{33}$ years. It is clear from the example that it is possible to measure a lifetime of $T_p\sim10^{31}$ years, but if $T_p>10^{33}$ years, the solution of the problem will probably be delayed for many years. The corresponding experiments are in the preparation stage (in the largest of the constructed detectors, there are 10^4 tons of water, which is why we chose this value for the above example). Besides the proton decay, in some variants of the theory transformation of a neutron into an antineutron and back is predicted (neutrino oscillations).[39]

If decay of the proton is discovered, this will be a triumph of grand unification, but, as is clear from what has been said, a negative result will not yet disprove it. If $T_p\lesssim10^{33}$ years, then the strong, weak, and electromagnetic interactions become comparable at the colossal energy $E_x\sim10^{15}-10^{16}$ GeV, to which there corresponds the mass $m_x=E_x/c^2\sim10^{-9}-10^{-8}$ g (the proton mass is $M\equiv m_p=1.67\times10^{-24}$ g). It is this large value of m_x that explains the low probability of proton decay. Note that the so-called gravitational or Planck mass (the maximon mass) $m_g=\sqrt{\hbar c/G}=2.2\times10^{-5}$ g ($E_g=m_gc^2\sim10^{19}$ GeV) exceeds by only 3–4 orders of magnitude the above value of m_x. The mass m_g corresponds to the length $l_g=\hbar/m_gc=\sqrt{G\hbar/c^3}=1.6\times10^{-33}$ cm, while $l_x=\hbar/m_xc\sim10^{-29}-10^{-30}$ cm. It follows from this that the grand unification involves the assumption that there is no fundamental length $l_f>10^{-29}$ cm (see Sec. 13).

The next step after grand unification (we emphasize once more that it is far from complete) must be the unification of all the interactions, including the gravitational. In the framework of the currently known ideas (in the absence of a fundamental length longer than l_g), this involves the transition to the region of lengths $l\sim l_g\sim10^{-33}$ cm, masses $m\sim m_g\sim10^{-5}$ g, and energies $E\sim E_g\sim10^{19}$ GeV $=10^{28}$ eV (above, we have sometimes measured the masses in eV, which, of course, is perfectly admissible; here, for clarity, we distinguish between the mass m and the energy $E = mc^2$).

Intensive work is currently being done on the unification of the various interactions in accordance with Einstein's dreams of a truly unified field theory. The theory which unifies the electromagnetic and gravitational interaction, and in which it is also necessary to introduce particles with spin 3/2 (gravitinos) is called supergravity. There exists an even larger scheme, superunification, which encompasses all the known interactions.[39,56] There is no possibility here to go into this problem in more detail, either in connection with cosmology or some other questions.[39,54]

The connection between the neutrino and other particles reflected in their "unification" has the consequence that, in general, the neutrino has a nonvanishing rest mass m_ν (of course, this mass can be different for the ν_e, ν_μ, and ν_τ neutrinos). At the present state of theory, it is impossible to calculate this mass—and even if it were possible, it would still be necessary to determine the neutrino mass experimentally. Such a situation is by no means new. Hitherto, the mass of the neutrino (by which the electron neutrino ν_e is meant) was generally assumed to be equal to zero for two reasons. First, it was known experimentally that the mass is small in the sense that $m_{\nu_e} \ll m_e = 5.1\times10^5$ eV (below, we shall measure the mass in energy units). Second, the theoretical scheme in which $m_\nu = 0$ is simpler and more elegant than when $m_\nu \neq 0$. But, of course, the inadequacy of such arguments was clear and experiments were made, which made it possible to establish the limit $m_{\nu_e} < 50$ eV $\sim 10^{-4} m_e$. The basic idea of such experiments is to study the spectrum of β decay, for which it is convenient to use the decay of tritium ($t \to {}^3\mathrm{He} + e^- + \bar\nu_e$), since in this case the limit of the spectrum is very low ($E_{e,\max} = 18.6$ keV). Recently, experiments of this type were made with, in the opinion of the authors,[57] even greater accuracy. As a result, it was asserted that the mass m_{ν_e} lies in the range 14–46 eV. There is no doubt that the experiments must be continued and, moreover, in several laboratories. Another indication of the existence of a nonzero mass of the neutrino has been obtained by a number of authors, in particular, from analysis of the reaction

$$\bar\nu_e + d \begin{cases} \longrightarrow n + n + e^+ \\ \longrightarrow n + p + \bar\nu_e \end{cases}$$

(see Ref. 58). Essentially, we are dealing here with the so-called neutrino oscillations—the transformation of the ν_e neutrino into the other neutrino species (ν_μ and ν_τ) and back.[59] If such oscillations occur, then the masses of the various neutrinos are different, and, therefore, at least one of the masses cannot be zero. Experimentally, the existence of oscillations must have the consequence that the intensity of even a nondiverging beam of, say, ν_e neutrinos in vacuum must vary with distance. Such a possibility is very important for interpreting the experiments on the detection of neutrinos from the Sun (see Sec. 21) and, of course, from the fundamental aspect. If the neutrino mass is $m_\nu \gtrsim 10$ eV, this has a tremendous cosmological significance (see Ref. 60 and Sec. 17). But if the masses of all the neutrino species are $m_\nu < 1$ eV, the significance of the neutrino for cosmology will be slight. For physics, naturally, it is necessary to know the masses of all the neutrino species whatever the corresponding values may be. The determination of the neutrino masses is undoubtedly one of the most important and topical problems of microphysics.

13. Fundamental length. Interaction of particles at high and superhigh energies

The problem of a fundamental length arose both from general considerations due to Riemann and Einstein (for references, see 1 and Ref. 2) as well as from "practical" requirements of theoretical physics. By the latter, I mean the need to "deal with"—eliminate or least render harmless—the divergences (infinite quantities) encountered in theory. Such divergences appear mainly when allowance is made for ever shorter wavelengths (ultraviolet catastrophe) in expressions containing a spectral expansion and determining the energy and various other quantities. For point particles, and in the existing relativistic quantum field theory particles are assumed to be pointlike, there does not exist any natural length which "cuts off" the spectrum, and divergences appear to be unavoidable. However, in classical physics a way was already found by which divergences could to a certain degree be rendered harmless by " renormalization" of the mass (for example, the replacement in the equation of motion of a charged particle of the sum of a mechanical or bare mass and the electromagnetic mass by the experimentally observed mass of the particle). The greatest achievement of quantum electrodynamics in the forties and the fifties was in the systematic " renormalization" of all divergent expressions when perturbation theory is used. This results in the construction of a theory in complete agreement with experiment.[61] But the experimental data correspond to lengths l not less than about 10^{-16} cm (which corresponds to an energy $E \sim \hbar c/l \sim 0.1$ erg ~ 100 GeV; see Ref. 62). In other words, it can at the present time be said with fair confidence that down to distances $l \sim 10^{-16}$ cm no new, fundamental length l_f exists and our usual notions about space are valid (for time, this corresponds to an interval $t \sim l/c \sim 3\times10^{-27}$ sec). However, early developments did not lead to the introduction of a fundamental length $l_f > 10^{-17}$ cm. Indeed, the value $l_f \sim 10^{-17}$ cm is used fairly widely as the limit of applicability for nonrenormalizable theories and, essentially, as a fundamental length at which the "cutoff" of all divergent expressions occurs more or less automatically. Such a cutoff was needed, in particular, in the theory of weak interactions prior to its unification with electrodynamics. But now that a unification has occurred, the theory has become renormalizable, the divergences have disappeared, and, in fact, this is one of the main achievements of the new theory.[32,33] Thus, the real justification for introducing a fundamental length at $l_f \sim 10^{-17}$ cm has disappeared. This circumstance encouraged the theoreticians so much that the fundamental length was virtually forgotten and theoreticians did not hesitate to work with lengths of order 10^{-29}–10^{-30} cm (see the previous section) right down to the gravitational (Planck) length $l_g = \sqrt{G\hbar/c^3} \sim 10^{-33}$ cm. It is this length that essentially plays the part of a fundamental length. Such an approach is sensible and justified, since there are no real grounds for introducing a length $l_f \gg l_g$. But it is just as clearly necessary to appreciate that an extrapolation (if one can call it that) is being made of the familiar space–time notions by 17 orders of magnitude (from $l \sim 10^{-16}$ to $l \sim l_g \sim 10^{-33}$ cm)! A bold extrapolation like this is typical of physics (another example is the assumption that the laws established in terrestrial laboratories are completely valid for the entire Universe except in the immediate proximity of the "initial" singularity; for a more precise statement, see Sec. 17 below). But this does not *mean that one can forget the possible existence of some* fundamental length $l_f \gg l_g$. If it exists, then it will probably radically change all physics at lengths $l \leq l_f$, not only in microphysics but also in mini black holes (see Ref. 63 and Sec. 18) and cosmology (Sec. 17).

This is the reason why there are no grounds for removing the problem of a fundamental length from the list of key problems of physics and astrophysics.

With regard to the problem of the interaction of particles at high and superhigh energies, it is one of the eternal problems. All that changes is the limiting energy achieved at a given time. In 1971, the maximal energy for protons achieved in an accelerator was 75 GeV (Serpukhov). Now (and already for several years) the maximal energy in the laboratory system for protons is 500 GeV (Batavia, USA). However, in the center-of-mass system, this energy corresponds to only $E_c \approx \sqrt{E \times Mc^2/2} \approx 15$ GeV (per proton), which corresponds to a length $l \sim (\hbar/m_\pi c)(Mc^2/E_c) \sim 5\times10^{-15}$ cm (M is the proton mass, $m_\pi \sim M/6$ is the pion mass; for strongly interacting particles, it is this length, and not $l \sim \hbar c/E_c \sim 10^{-15}$ cm which is relevant). Further progress is associated in the first place with colliding beams. At CERN, such an accelerator is already working, and in each of the beams the proton energy is 31 GeV. Soon (in a few years) an accelerator will be commissioned at Batavia with two colliding beams of protons (or protons and antiprotons) with energy $E_c \sim 1000$ GeV in each beam.[50] This corresponds to a proton energy in the laboratory system of $E \sim 2(E_c)^2/Mc^2 \approx 2\times10^6$ GeV $= 2\times10^{15}$ eV. In the Soviet Union, there is a project for an accelerator with $E_c = 3000$ GeV (hence $E \approx 2\times10^{16}$ eV).[50] Higher energies in accelerators will hardly be achieved before the end of this century. However, even at $E_c = 3000$ GeV we have the length $l \sim \hbar c/E_c \sim 5\times10^{-18}$ cm, and $l \sim (\hbar/m_\pi c)\times(Mc^2/E_c) \sim 3\times10^{-17}$ cm.

In cosmic rays, there are certainly particles with energy 10^{20} eV, though very few of them; however, for $E \geq 10^{18}$ eV the integrated intensity of the primary cosmic rays at the Earth is of order 10^{-2} particles $\cdot$ km^{-2} $\times$sr$^{-1}\cdot$ h$^{-1} \sim 10^2$ particles$\cdot$km$^{-2}\cdot$ sr$^{-1}\cdot$ year^{-1}. Therefore, in the region of energies up to $E \sim 10^{17}$–10^{18} eV it is entirely realistic to use cosmic rays for high-energy physics (see Ref. 64; for $E \geq 10^{16}$ eV the intensity of primary cosmic rays is of order 10^6 particles$\cdot$ km^{-2} $\times$sr$^{-1}\cdot$ year^{-1}). The neglect of cosmic rays for investigations in high-energy physics in the region not yet accessible through accelerators appears short sighted at the least. In fact, it may be more accurate here to speak of snobism, which is also widespread in the scientific world. One way or the other, the entire history of the development of high-energy physics (see Ref. 64 and the bibliography given there) indicates rather clearly that cosmic rays should be used in high-energy physics, and I hope that this conclusion will be further confirmed in the future.

14. Violation of *CP* invariance. Nonlinear phenomena in vacuum in superstrong magnetic fields. Some remarks on the development of microphysics

In 1, a special section was devoted to the problem of violation of CP invariance, while the set of problems touched on above in Secs. 10, 11, and 12 was hardly referred to. At the present time, the problem of CP violation remains important and essentially unresolved, but it has become one of many in a wide spectrum of problems discussed in the framework of gauge theories of the various interactions.[39,66]

Among the problems absent in 1 but present in Ref. 2 are those associated with nonlinear phenomena in vacuum in strong electromagnetic fields. This problem is by no means new and goes back to the start of the thirties. At that time it was realized that in strong electromagnetic fields, namely, an electric field of $\mathscr{E} = \mathscr{E}_c \sim m^2c^3/e\hbar \sim 10^{14}$ esu/cm $\sim 3\times10^{16}$ V/cm and magnetic field $H \sim H_c \sim m^2c^3/e\hbar \sim 10^{14}$ Oe, vacuum behaves like a nonlinear medium. Moreover, in a sufficiently strong electric field electron–positron pairs can be produced. However, for a long time it was only possible to dream about strong fields (in the above sense). The discovery of pulsars (rotating magnetized neutron stars), on whose surface the field is comparable with H_c (or, rather, is only one or two orders of magnitude less), changed the situation. In addition, particles with high energy E can produce e^+e^- pairs in a field that is E/mc^2 times lower than the field E_c (see Ref. 66). Superstrong electric fields also exist near atomic nuclei (for some literature references, see Ref. 2). It is natural that all this raises the interest in the problem of nonlinearity in vacuum in strong fields and distinguishes it as a special problem.

It should be pointed out too that in microphysics the individual subjects and problems are in fact more intimately related to one another than in macrophysics and astrophysics. Essentially, this is understandable, since many directions in microphysics are relatively young and have not had time to develop far. Further, it is clear from the titles of Secs. 10–14 that the list of problems is rather arbitrary and could be readily changed and particularized. Finally, no mention has been made at all of, for example, pion condensation in dense matter and shock waves produced by the collisions of heavy nuclei (which actually should appear in Sec. 9), the behavior of matter at superhigh densities and temperatures,[49,67] the problem of magnetic monopoles, and the problem of the physical content of the concept of "vacuum," especially under nonstationary conditions. Moreover, the listed problems, like those relating to the baryon asymmetry of the Universe and the variation in time of various physical quantities and "constants," are not only an aspect of physics but also (and even to agreater extent) cosmology.

Of course, this is not surprising. Physics and cosmology were always related, but this connection has now become particularly intimate and two sided. In the language of the distances or energies that we have already used in Sec. 12, we can say that for physics distances $l_x \sim 10^{-29}-10^{-30}$ cm and energies $E_x \sim \hbar c/l_x \sim 10^{15} - 10^{16}$ GeV are very important. But for laboratory physics such distances and energies are at present quite out of reach. The only "place" where one can study matter under such conditions are the early stages of cosmic evolution, the length $l_x \sim 3\times 10^{-30}$ cm corresponding to the density $\rho_x \sim \hbar/cl_x^4 \sim 10^{80}$ g/cm^3. (We recall that the Planck density is $\rho_g \sim \hbar/cl_g \sim 10^{94}$ g/cm^3, since $l_g \sim 10^{-33}$ cm.[7)]) Of course, significantly lower densities right down to the nuclear density $\rho_{nuc} \sim 3\times 10^{14}$ g/cm^3 are of considerable interest. Thus, the only source of information about matter with $\rho \gg \rho_{nuc}$ is cosmology.[54,67,68] Unfortunately, neither here nor in the following sections devoted to astrophysics can we develop this theme.

Progress in definite directions in science occurs within definite limits irregularly. There are years and decades of stormy development, but also quieter periods and even confusion. This applies particularly to a field like microphysics, which, when defined and understood as here, is always at the frontier. In our century, the interval bounded, on the one had, by 1924–1925 and, on the other, by 1930–1932, will probably be recognized as the most brilliant. These years saw the construction and, to a large extent, the development and understanding of nonrelativistic quantum mechanics and the laying of the foundations of relativistic quantum theory (the Klein–Gordon and Dirac equations for particles with spins 0 and 1/2, respectively, and the quantum theory of radiation). Moreover, in 1932 the positron and neutron were discovered, and in 1931 the existence of the neutrino was mooted.

But then the difficulties began. The most serious was associated with the appearance of the divergent expressions which hindered the development of even electrodynamics, to say nothing of the incipient theories of the weak and strong interactions. Difficulties were also encountered in the relativistic theory of particles with spin greater than 1/2, i.e., with spin 1, 3/2, 2, etc.

There are no prescriptions or rules that tell one how to advance into an unknown region. One operates by trial and error. Those with the best intuition and ability to solve complicated problems triumph. It also seems to me that, excepting giants like Einstein, luck and circumstance play a no less important part.

Let me recall if I can, the main directions at the time when I began theoretical physics in 1938: the lambda-limiting process, nonlocal field theory, allowance for the inertia of the self-field in a theory with higher spins and relativistic equations for particles with many spins, the method of renormalization in quantum electrodynamics, the method of dispersion relations, the axiomatic approach, the S-matrix method (which denied a role to Lagrangian and Hamiltonian equations), the bootstrap approach, reggeism .... Of all these, great success was achieved (at the end of the forties) only in electrodynamics by the use of renormalization. The result was brilliant,[61] but viewed theoretically it appeared rather local, "technical," and limited. At the least, one was looking for a theory free of all renormalization and, in addition, not limited to electrodynamics. Of the other listed directions, I myself, when I was occupied with particle theory, worked only in the field of the theory of spins. Although I am not ashamed of this work (and cite the last publication Ref. 69, where other references are given), neither can I speak of any clear success. The remaining approaches (apart from the method of renormalization) are also unable to make great claims. Some of them always appeared barren, and "no sparrows fly from an empty nest."[8)]

In contrast, what is happening now may not be entirely new but is based on a rich conceptual foundation (symmetry, and, in particular, generalized gauge invariance, spontaneous symmetry breaking, and nonlinear equations with rich possibilities). Our ideas about the structure of matter have been raised to a

[7)]For simplicity, we shall restrict ourselves here to dimensional arguments. In specific cosmological models, one can naturally go further. For example, in hot Freidman models the temperature of the Universe is T (GeV) $\sim 10^{-3} t^{-1/2}$, where t is the time in seconds measured from the classical singularity (for more details, see, for example, Ref. 54). The mass $m_x \sim 10^{-9}$ g mentioned in Sec. 12 corresponds to the energy $E_x = m_x c^2 \sim 10^{15}$ GeV and the same characteristic temperature T in GeV. Hence, $t \sim 10^{-36}$ sec.

[8)]These comments may be understood incorrectly. Indeed, as has already been emphasized, it is success alone which guarantees the correctness of a path chosen into an unknown region. Therefore, nobody is in a position to say seriously in advance whether a particular approach is conceptually rich or poor. Nevertheless, when new hypotheses arise and suggestions are made, every interested observer makes his own intuitive judgement and estimate of the prospects. Subsequently, of course, the observer is pleased if he is proved right and disappointed by an error. It is only in this sense that I permit myself to make judgements of the type in the text. For example, I am annoyed that I underestimated the quark hypothesis when it appeared but pleased that I correctly felt (or perhaps simply guessed ?) the unproductive nature of some attempts to construct a new theory.

With regard to renormalization (which has long been used even in classical electrodynamics in the case of the mass of particles), some physicists (perhaps even the majority) regard it as entirely satisfactory. The more reserved attitude to renormalization reflected in the text can also be found in the literature. It would be difficult to go into this question in more detail here, but it is not important for what follows.

new level (quarks, gluons, etc.). There are numerous real achievements in the field of the theory of weak and strong interactions. The contrast is very striking. Therefore, although a bystander, I extol the recent successes in microphysics. It is entirely possible that the period through which we are now living will soon be recognized as being as fruitful and significant in the history of physics as the period, already mentioned, when quantum mechanics was created.

Despite such an attitude, it must be said that it is still quite impossible to speak of the completion of a unified theory of the interactions. As has been emphasized, this is true even in the case of the theory of the electroweak interaction. This theory could be regarded as essentially established were the $W^{\pm,0}$ bosons discovered, the question of the scalar meson resolved, and clarity achieved with regard to the agreement between theory and the rotation of the plane of polarization in bismuth. With regard to quantum chromodynamics, grand unification, and superunification, the incompleteness of theory in these fields is even more obvious and surprises are entirely possible. It will be all the more interesting to follow the development of events in both theory and experiment.

IV. ASTROPHYSICS

The decade before the last (the sixties) was particularly rich in astronomical discoveries of fundamental importance. It is sufficient to mention quasars, the microwave background ($T \approx 3$°K), x-ray "stars," cosmic masers involving various molecules, and, finally, radio pulsars. Although there was an element of chance in this clustering of discoveries, there is no doubt that we can here speak of the harvest collected by the transformation of astronomy from a purely optical discipline into one encompassing all wavelengths.

The seventies were characterized by further impressive development of astronomy. There is no real justification for saying that progress has been slower, despite the fact that significantly fewer great discoveries were made. Only the discovery of x-ray pulsars in binary systems, and also the discovery of gamma and x ray bursts can be put on a par with the discoveries mentioned above. But at the same time we cannot avoid mentioning a great achievement of theory—the discovery of black hole evaporation. Much too has been done in theoretical cosmology, mainly on the basis of and in connection with the successes of microphysics (see above).

Below, I shall list the achievements relating to all the problems mentioned ten years ago in 1. It is characteristic that here, in contrast to the section devoted to microphysics and even, though to a lesser degree, macrophysics, it has been necessary to add (which was already done in Ref. 2) only two subtitles: "Black Hole Physics" (in Sec. 18) and the Formation of Galaxies (in Sec. 19). In fact, the formation of galaxies was mentioned in 1 (in Sec. 16), and even now we can say very little about it. As regards black holes, I now find it even difficult to understand why they were not even mentioned explicitly in 1! This was undoubtedly an oversight, but it probably reflected the attitude to black holes of the "scientific community" at that time. The reasons for the rather late recognition of the importance of black holes for not only astronomy but also physics are not particularly clear (see, however, Sec. 18). Indeed, black holes were treated on the basis of the general theory of relativity in 1939,[70] and in prerelativisitc physics the black hole problem already arose in the 18th century (for rather more detail, see Ref. 2, in which the introduction of the concept of black holes in 1798 was associated with the name of Laplace; in fact, the idea was put forward even earlier, in 1784, by Michell).[71] There now follow brief commentaries on the progress of astrophysics during the last ten years, these being restricted to the problems mentioned in 1 and above in the present paper.

15. Experimental verification of the general theory of relativity

The general theory of relativity has been subjected to verification at least since 1919, when for the first time it was possible to measure the theoretically predicted deflection of light rays in the gravitational field of the Sun. However, the verification of general relativity continues, and the achieved accuracy is still not so impressively great, and for a well-known reason. The point is that within the solar system the gravitational field is weak, the corresponding parameter having the value $|\varphi|/c^2 < 2\times 10^{-6}$ (here, φ is the Newtonian gravitational potential). In 1 it was noted that a "storm cloud" had appeared on the horizon following indications of an oblateness of the Sun. In the meanwhile, this difficulty has been "resolved." There have also been a number of new experiments, and all results are in complete agreement with general relativity. But the accuracy is still not yet high—usually, we have an accuracy (or, rather, an error) of about 1%. An exception is the measurement[72] of the gravitational frequency shift with an error of order 0.01% and the more important measurement[73] of the delay of signals made possible by the artificial satellites of Mars (the Vikings) with an error of order 0.1%. Both results agree with general relativity to the indicated accuracy. Thus, in the weak field (to terms of order $|\varphi|/c^2$) general relativity can basically be regarded as verified with an error of about 0.1%. This is no mean progress compared with the past (see Ref. 74 and the references given there).

Among the interesting general relativistic effects capable of being observed even in a weak field there is the focusing effect of masses (stars, galaxies) on electromagnetic waves (radio waves, light, etc.) passing near them. Einstein published a calculation of this gravitational lens effect in 1936, and in 1979 it was suggested that the double quasar 0957 + 561 A, B is in fact two images of a single quasar (an elliptical galaxy approximately halfway between us and the quasar plays the part of the gravitational lens).[75] There is now no doubt with regard to the correctness of this interpretation. This and similar observations of gravitational lenses can and must be used, not to verify general relativity (which in the weak field is already verified with much greater accuracy than that needed to calculate the gravitational lens effect of a galaxy), but to obtain valuable astronomical information by comparing calculations with the observed picture.

In 1 I said: "If it is demonstrated (which I ardently hope) that "all is in order" with the experimental verification of general relativity in the sun's field, then the question of such verification will assume an entirely different character. Namely, there remains the question of the validity in strong fields or in the vicinity, and even in the interior of supermassive cosmic bodies." This is now the problem—the verification of general relativity in strong fields. For this purpose, neutron stars (on whose surface $|\varphi|/c^2 \sim 0.1-0.3$) are of some interest, but it is black holes which are at the center of the stage. Their mere discovery would at the least, be a qualitative confirmation of the validity of general relativity in strong fields. Quantitative measurements near the Schwarzschild radius or, speaking somewhat less precisely (but more generally if we have in mind rotating black holes), near black holes can also serve for detailed verification of general relativity. I cannot here expand on these comments, since this has been done recently on the pages of this journal in Ref. 74.

The verification of general relativity in strong fields is an important and topical problem. It is another matter that physicists and astronomers, without waiting for such verification, do not hesitate to make wide use of general relativity in strong fields too (but still in the region in which quantum effects are small; see Ref. 17 and Sec. 17 below). Such an approach, which is typical of theoretical physics, is entirely reasonable and in no way contradicts one's concern about the strength of its "support" —the recognition that there must be further verification of general relativity, particularly in a strong field.

16. Gravitational waves

The question of gravitational waves on the firm foundation of general relativity was already posed by Einstein in 1916–1918. But it has not yet been possible to observe them, which is a good illustration of the fact that certain scientific problems, even when precisely posed, cannot be solved for many decades. True, in 1 I quoted a paper claiming the detection of cosmic gravitational radiation. However, these observations were not confirmed. Nevertheless, they were of value, in that they stimulated the development of more sensitive detectors of gravitational radiation (see Ref. 76 and the bibliography there). One gets the impression that within a few years we shall see the commissioning of gravitational antennas capable of detecting bursts of gravitational waves generated, in the first place, by supernova explosions in not only our Galaxy but also in the comparatively nearby galaxies. This last circumstance is very important, since in the Galaxy supernovae explode on the average once every 15–30 years. When explosions in other galaxies are taken into account, we can hope to detect several events in a year. Another matter in which there is serious uncertainty concerns the estimate of the energy radiated in the form of gravitational waves in an explosion.[76] But, altogether, the outlook is rather optimistic and, as I have said, in the current decade we can hope for the creation of observational gravitational-wave astronomy.

The main aim of the detection of gravitational waves is undoubtedly to use this "channel" as well as to obtain astronomical information. But this is only possible under the assumption that we have a theory which describes the generation, propagation, and detection of gravitational waves. Such a theory, which, in principle, makes it possible to answer all the questions that arise, is general relativity. It appears very probable that in this respect general relativity will provide an entirely reliable basis. But it must still be borne in mind that this theory is insufficiently tested, and confirmations of general relativity in the form of observed effects in a weak field are few. It is sufficient to say that there exist non-Einsteinian, i.e., different from general relativity, theories of the gravitational field in which gravitational waves do not behave as in general relativity but the relativistic effects in the solar system correspond to the observations. In this connection, it is interesting that the change in the orbit of the binary pulsar PSR 1913 + 16 apparently agrees with the assumption that this system radiates gravitational waves in accordance with general relativity.[77] True, this result requires confirmation and must be made more accurate, but it is basically important and of much significance. At the same time, the non-Einsteinian theories of the gravitational field have not, in general, been well developed, and in a number of cases they encounter difficulties. Leaving firm ground and relying on physical intuition, I should like to express confidence in the complete validity of general relativity as a basis of gravitational wave astronomy. One way or another, such an assumption is entirely reasonable and in effect is always made as a working hypothesis.

The main task now is to achieve the detection of cosmic gravitational waves. If this can be done, then the analysis of the corresponding data and the further study of the binary pulsar (and, it is to be hoped, some other binary pulsars) will make possible verification of the validity of general relativity (for such problems) and yield astronomical information. As I have said, we may not have to wait too long for the first results.

17. The cosmological problem

The cosmological problem can be formulated as the problem of studying the structure of space on a large scale and finding the law of evolution of the Universe in time. Of stars there are a tremendous number, and even the number of galaxies is about a billion. We make the proviso that here and in what follows we shall be speaking of the so-called Metagalaxy, i.e., the system of galaxies (including quasars) which can be observed from the Earth. This proviso reflects a genuine caution and is not an overreaction to the danger of unqualified criticism (examples of which are given in

Ref. 78). The point is that the true Universe may be topologically very complicated,[79] while consideration is usually restricted to the simplest models, in particular, the Friedmann models. In them, the Universe is assumed on the average (on a sufficiently large scale) to be isotropic and homogeneous. In Friedmann models (with vanishing Λ term) and in some more general though still topologically simple models there was in the past a singularity—the time $t=0$ (the choice for t of the value 0 is, of course, purely nominal) when the matter density was infinite, $\rho\to\infty$. We recall also that in isotropic and homogeneous (Friedmann) models with $\Lambda=0$ the model is closed (representing an expanding and then contracting three-dimensional sphere) if the mean matter density satisfies $\rho>\rho_c=3H^2/8\pi G$; for $\rho<\rho_c$, the model is open. Here, G is the gravitational constant and H the Hubble constant, the contemporary value of which is about 75 km $\cdot$ sec$^{-1}\cdot$ Mpc$^{-1}=2.4\times10^{-18}$ sec^{-1}, which corresponds to an "age" of the Universe of $T\sim 1/H\sim 10^{10}$ years. For this value of H, the critical density is $\rho_c\sim 10^{-29}$ g/cm^3; in the past, ρ_c was higher, since H decreases with time. The determination of the density ρ or, more concretely, the density $\rho=\rho_0$ at our epoch, has proved to be a very difficult task. The mean density associated with visible objects (galaxies, quasars) is about one and a half orders of magnitude lower than ρ_c. But the value of ρ_0 could be determined by invisible "ingredients": hot intergalactic gas (basically, ionized hydrogen, so that its presence is very difficult to detect), black holes, neutrinos, or even gravitational waves.[9)] Thus, if the neutrino mass is $m_\nu\gtrsim 10$ eV, the intergalactic neutrinos formed in the past, when the Universe was sufficiently hot, could at the present time ensure a density equal to or even exceeding ρ_c (for a more accurate calculation, it is necessary to know the number of species of stable neutrinos and their mass, and, if neutrino oscillations exist, to take into account this factor as well). However, for $m_\nu<1$ eV (for all quasistable neutrinos), the contribution of the neutrinos to the density is rather small. It is of course very interesting and significant, that, as we noted in Sec. 12, the problem of the neutrino mass has acquired such great cosmological significance.[39,54,60,80]

I shall not fear another repetition (see Sec. 14) when I emphasize that the early Universe has become a unique laboratory of high-energy physics.[39,54,67,68,81]

But the fundamental problem of cosmology itself is still that of the singularity. In the framework of general relativity, i.e., Einstein's classical theory of gravitation, the appearance of a singularity is regarded as unavoidable. There are no doubts (at least, such as the opinion of the majority of physicists, including myself) that the appearance of a singularity is an indication of a limitation of the theory which shows the need for its generalization under conditions in the regions close to the singularity. Here at least three possibilities are conceivable. The first is a generalization of Einstein's theory already at the classical level which leads to elimination of the singularity (see, for example, Ref. 82 and the references in Ref. 74). The second possibility is the existence of some fundamental length l_f which limits the radius of curvature of space to scales l_f, and densities $\rho_f\sim\hbar/cl_f^4$ (see Sec. 13 and Ref. 63). Finally, the third possibility is that the applicability of general relativity is restricted by quantum effects, the restrictions occurring at the scales already mentioned: $l_g=\sqrt{G\hbar/c^3}=1.6\times10^{-33}$ cm, $t_g\sim l_g/c\sim 10^{-43}$ sec, $\rho_g=c^5/\hbar G^2=\hbar/cl_g^4=5\times10^{93}$ g/cm^3. Thus, even if the applicability of general relativity as a classical theory is not restricted in any other way, it is still invalid for $l\lesssim l_g$, $t\lesssim t_g$, and $\rho\gtrsim\rho_g$ because of the need to take into account quantum effects. This possibility appears to be the simplest in the sense that it "operates" automatically, of necessity. Whatever the truth, the main exertions are currently directed toward the quantization of general relativity and the creation of quantum cosmology. Here, there are already some results[83] which offer hope of elimination of the singularity and the creation of a sensible cosmological model without singularities.

Cosmology and the problem of black holes, which are intimately related to the singularity problem and the limits of applicability of general relativity, occupy in astronomy a position analogous to that of microphysics within physics as a whole. At the same time, it is true that I proceed from the assumption, which appears the most probable and natural, that in all other cases and for all other objects "new physics" is not needed in astronomy (for more details see Ref. 2, Secs. 20 and 24). In fact, even if this last assumption is not correct, the problem of the singularity, the quantization of general relativity, and creation of quantum cosmology certainly exists and remains at the center of attention.

18. Neutron stars and pulsars. Black hole physics

We recall that discussion of the possible existence and discovery of neutron stars began as early as 1934, and that they were discovered in 1967–1968. More precisely, pulsars were discovered—magnetized rotating neutron stars emitting fairly powerful radio radiation. Such pulsars, except for rare exceptions,[77] are single stars, i.e., they do not belong to any close binary system. Soon, at the start of the seventies, x-ray pulsars were discovered in close binary systems (particularly well known are the first x-ray pulsars to be discovered: Centaurus X-3 with a period of 4.8 sec and Hercules X-1 with a period of 1.2 sec).[84] In a close binary system consisting of a neutron star and an "ordinary" star with an extended plasma atmosphere, plasma can be transferred in large amounts to the neutron star. As the plasma reaches the immediate proximity of the neutron star it acquires a high velocity as a result of attraction. When it is forced to stop, this plasma is strongly heated (to a temperature $T\sim 10^7-10^8$ °K and higher) and emits basically in the x-ray range. The modulation of the radiation—its periodic nature in the form of bursts that follow one another in a fairly strict sequence—is ensured by the rotation of the neutron star (the period

[9)]In connection with such a possibility, it is better not to speak of the density of matter but rather of the density of energy divided by c^2, since it would be stretching a point to call gravitational waves matter.

of all pulsars in all the wavelength ranges is the period of their rotation; of course, there may sometimes be radiation with a period equal to half the rotation period, but this does not affect the essence of the matter).

Many hundreds of pulsars are now known, and the number of papers devoted to them is even greater.[85-87] Here we must restrict ourselves to a few comments, and basically list the main direction of the investigations (see also Ref. 2, Sec. 21).

For physics, the most important thing is to study the neutron stars themselves and the matter from which they are made. The formulation of the problem is revealed rather clearly already in the title of the paper of Ref. 29, recently published as a translation in this journal: "Pulsars and compact x-ray sources: cosmic laboratories for the study of neutron stars and hadron matter." This is a very large and interesting subject, from which one can pick out for special attention the investigation of the outer crust of neutron stars. Here, the main features are not due to the high density, superfluidity, or nuclear effects but rather to the action of a superstrong magnetic field (see Sec. 7 above).

Although simpler from the fundamental point of view, the problem of pulsar magnetospheres and the mechanism of their radiation is in practice more difficult in some ways. The superstrong field, the presence of rotation but the absence of axial symmetry (in pulsars, the axis of rotation and the magnetic moment are not parallel), and the need to take into account relativistic plasma effects all render the problem very complicated.[85,87] Certainly, understanding in this field lags behind the study of the neutron stars themselves.

I well remember the discovery of pulsars and the first "heroic" period of their investigation. It then appeared (at least to me) that the analysis of the radiation mechanism—all that we observe after all—would be much easier than even the identification of the nature of the pulsars themselves, i.e., the choice between the white-dwarf and neutron-star models. But it turned out quite differently. The discovery of the fast-period pulsars in the Crab and Vela immediately eliminated the white-dwarf model. The observation of irregularities in the pulsation period of the radiation (and, therefore, in the rotation period) of the pulsars in conjunction with theoretical progress made it possible to "penetrate" into the interior of neutron stars.[29,85,86] With regard to the magnetosphere models and the radiation mechanisms, particularly those of the radio emission, which for the overwhelming majority of pulsars is all that is observed (I am referring here to single stars; binary systems which are x-ray sources form a separate class), difficulties and obscurities were encountered.[85,87] However, there has been progress in the recent past, and soon we can hope for the creation of a fairly clear picture.

X-ray pulsars in binary systems serve for the analysis of problems more typical of astronomy than physics. Indeed, through such pulsars one can study matter transfer (accretion) and the entire evolution of stars in binary systems, including supernova explosions.[84,86]

No matter how important and interesting the problem of pulsars of various types and neutron stars generally (not every neutron star need necessarily be detectable as a pulsar), it has to a large extent been transformed during the last decade into a familiar and even routine problem of astronomy, though there may be some exaggeration in this statement. But it can at least be said that in astronomy the most exotic objects are now the black holes.

As I have already said, black holes appeared in 1939 in a modern guise (on the basis of general relativity).[70] But then for a rather long time and for not entirely clear reasons (one can of course say that "the time was not ripe" or "we never got around to it," but this is inadequate) black holes did not attract particular attention.[10)] It was only in the sixties that interest quickened, and since then, especially during the last decade, black holes have come to the fore in both physics and astronomy. Since one can already read about gravitational collapse and black holes in a course of theoretical physics,[88] I shall assume that the subject of the discussion is known (in fact, this is how we proceed in almost all the other cases above, since it is almost impossible to do otherwise in a paper of the present kind). I shall therefore merely list the reasons why black holes attract attention and are particularly interesting.

First, and this was already mentioned in Sec. 15, the gravitational field near black holes is strong, and the very possibility of their existence is a consequence of general relativity. As a result, the discovery and investigation of black holes is an extremely important element in the verification of general relativity and the elimination of some alternative relativistic theories of gravitation (for more details, see Ref. 74). The use in the previous sentence of the word "elimination" is, of course, tendentious and betrays my adherence to general relativity and doubts as to the possibility of its replacement for strong fields by any other theory. But nevertheless such theories exist (the literature is given in Ref. 74; see also Ref. 82), although their consistency has not always been proved. In such a situation, I am sure it would be wrong to assume without proof that general relativity is correct and black holes certainly must exist. Nevertheless, and I repeat this once more, it is entirely natural and justified to proceed without waiting for the final verification of general relativity in a strong field and freely use it to analyze relativistic effects in astronomy and cosmology. This is the customary procedure in theoretical physics: It is necessary to verify physical theories but they must be used boldly without waiting for proof. If a theory is false or limited, this will be most rapidly revealed in such an approach.

10)One of the reasons could be failure to recognize the fact that when allowance is made for accretion of matter a black hole not only influences the surrounding medium but could, perhaps, even be detected from its radiation.

Second, it was recognized that black holes are not absolutely black in the ordinary understanding of this word. Namely, in everyday practice a body is said to be black if it is nonluminous and nonradiating. The expression black hole evidently arose in such a sense. The point is that once a collapsing mass has passed below its gravitational radius (or, generally speaking, below the event horizon), nothing can be radiated in the framework of general relativity, and electromagnetic waves and all particles or bodies which enter the black hole are "swallowed up" and nothing leaves the hole. Such properties recall those of the well-known model of a black body—a small opening in a large closed cavity. If the walls of the cavity absorb radiation and (or) are rough, a light ray which enters the opening has virtually no chances of escaping again. The opening will therefore appear as an absolutely black body in the scientific sense of this expression (as a body which absorbs all radiation incident on it). But, as is well known, if the temperature of the black body is nonzero, it will itself emit thermal radiation. The total intensity (power) of the radiation emitted by unit surface of the black body is

$$I=\sigma T^4, \quad \sigma=\frac{\pi^2 k^4}{60\hbar^3 c^2}=5.67\cdot 10^{-5}\frac{\text{erg}}{\text{cm}^{-2}\cdot\text{sec}^{-1}\cdot\text{deg}^{-4}}.$$

Note that the quantum constant $\hbar=1.05\times 10^{-27}$ erg·sec occurs here in the denominator. In the "nonquantum" world with $\hbar\to 0$, the intensity $I\to\infty$, which corresponds to an ultraviolet catastrophe. As is clear from what we have said above, in the framework of general relativity (and we recall that by general relativity we mean Einstein's classical theory of gravitation) a black hole not only absorbs everything but also radiates nothing, i.e., it is a black hole at the temperature $T=0$. But it was found (this discovery was made[89] in 1974) that when allowance is made for quantum effects black holes radiate as black bodies with temperature

$$T=\frac{c^3\hbar}{8\pi GkM}=\frac{GM\hbar}{2\pi kcr_g^2}\approx 10^{-7}\left(\frac{M_\odot}{M}\right)=10^7\left(\frac{2\cdot 10^{33}}{M\,(\text{g})}\right)\text{K},$$

where G is the gravitational constant, k is Boltzmann's constant, M is the mass of the body (in the last expression, M is measured in grams), $M_\odot=2\times 10^{33}$ g is the mass of the Sun, and $r_g=2GM/c^2\approx 3\times 10^5(M/M_\odot)$ cm is the gravitational radius of the body.

The quantum nature of the radiation of black holes is already clear from the fact that the corresponding temperature is proportional to $\hbar$. We shall not dwell here on the mechanism of the radiation produced by the strong gravitational field at distances r and r_g,[89-91] although this is a very interesting question. The temperature of black holes with stellar masses is negligible (as can be seen from the above formula, $T\sim 10^{-7}$ °K) for the Sun, and such a black hole can be regarded as classical and nonradiating. In principle, however, mini black holes can exist, and for them the radiation is significant or even very great. For example, for a black hole with mass $M\sim 2\times 10^{15}$ g (which is not so small!) $T\sim 10^{11}$°K, $r_g\sim 3\times 10^{-13}$ cm, $\tau\sim 10^{10}$ years, where τ is the total time of evaporation of the black hole (with the chosen mass $M\sim 2\times 10^{15}$ g). Mechanisms for the formation of mini black holes at the present epoch are not evident, but they could in principle have arisen in the early stages of the cosmological evolution (near the classical singularity for the Friedmann models, etc.). It is clear from our example that primordial mini black holes with mass $M<10^{15}$ g would have already evaporated by our epoch, but that a hole with $M\sim 10^{15}$ g could at present be observed in the stage of more or less violent "evaporation." Searches have already been made and, probably, will continue, but as yet without success. The absence of a particular phenomenon is sometimes rather difficult to interpret unambiguously. Specifically, if the "evaporation" (radiation) of mini black holes is not detected, it need not be due to invalidity of general relativity but simply to the circumstance that they were not formed at the assumed epoch. The evaporation of mini black holes would also be significantly modified if there exists a fundamental length $l_f\gg l_g$ (see Ref. 63 and Ref. 2, Sec. 21). The problem of mini black holes has an obvious physical and astronomical (in particular, cosmological) interest—and I only refrain from applying the epithet "gripping" and its like to the word "interest" in order to avoid repeating them endlessly in connection with many questions.

Third, black holes of stellar or even greater mass can have astronomical significance of the first order. If general relativity holds, a cold star with a mass greater than two or three solar masses cannot exist in equilibrium (in the form of a white dwarf or a neutron star) but must collapse and be transformed into a black hole. It would therefore seem that black holes should be rather common. They could be revealed, in principle, by two effects. A black hole in a binary system does not have a photosphere, but its gravitational field at large distances (for $r\gg r_g$) is the same as for ordinary stars and, therefore, influences the motion of the second star (of course, this also applies to systems of three or more stars). In addition, before it enters and is absorbed by the black hole, the gas accreted by a black hole forms a rotating disk around it or, at the least, does not fall into the black hole immediately. The heated and, probably, magnetized plasma surrounding the black hole could be revealed by its radiation.

Thus, black holes could be detected, but hitherto, despite many years (or rather, about a decade) of attempts this has not yet happened with certainty. True, there is a fairly good candidate for a black hole—the x-ray source Cygnus X-1. The observations,[92] which have already lasted for almost a decade, do not contradict the black hole hypothesis, but there are still no proofs yet, and there are alternative explanations for the observations.[74,92] One gets the impression that among stars black holes are at the least a rarity. If this is indeed the case, and general relativity is correct, the explanation must be sought in the mechanism of formation of black holes.

A star could end its life in one of four ways: explode without leaving a remnant, be transformed into a white dwarf, or into a neutron star or, finally, become a black hole. It is possible, and some calculations in the literature support the suggestion, that a final state in the form of a black hole is achieved only in the case of a rare combination of conditions and parameters.

Besides black holes with stellar masses ($M \lesssim 10^2 M_\odot$), there has been much discussion of the question of massive and supermassive black holes. They have been put everywhere: in globular clusters, the nuclei of normal galaxies, the nuclei of active galaxies, and quasars. In our country we have even nicknamed such enthusiasts "black holers" (chernodyrochniki). Quite why I do no know, but I myself am not a "black holer"; perhaps it is a negative reaction to the enthusiasm of the others and my participation in attempts to get by without black holes, at least in some cases. But such a position is by no means identical to the not totally infrequent "nonrecognition" of black holes, the tendency to regard them as an undesirable consequence of general relativity, and so forth. Quite the opposite, gravitational collapse and black holes appear to be among the most interesting and beautiful (such terminology is, of course, by no means counterindicated for physics) consequences of general relativity. I merely plead for a certain caution in this question, and so far such an approach has been justified. There are no massive black holes at the centers of globular clusters and, probably, there are none in many galaxies. Quasars and active galactic nuclei will be discussed in the following section.

If, as is done by many, the cosmological problem is regarded as the No. 1 astronomical problem, then the problem of black holes is problem No. 2.

19. Quasars and nuclei of galaxies. The formation of galaxies

Quasars were discovered—if by this one means the measurement of the red shift in their spectrum (concretely, the spectrum of the quasar 3C 273)—in 1963, i.e., four or five years earlier than pulsars. But whereas the nature of pulsars was rapidly clarified (see Sec. 18), this cannot be said of quasars. True, the hypotheses advanced in the early days (actually, for several years) of noncosmological distances to the quasars[11)] and the attribution to them of an entirely unusual nature are today no longer found (or hardly at all) on the pages of scientific journals. There are already known about 400 quasars (quasistellar radio sources—QSR), and they are regarded as a subclass of the much more numerous family of quasistellar objects (QSO) and active nuclei observed in a number of galaxies (Seyfert galaxies and some others). One gets the impression that, if the quantitative differences are ignored, we are dealing with a single phenomenon—the formation at the center of a galaxy (i.e., in a collection of a large number of stars and gas) of a nucleus that is comparatively small in size but gigantic in mass. The corresponding radius of the nucleus is $R \lesssim 10^{16}-10^{17}$ cm (we recall that the distance from the Sun to the center of the Galaxy is about 3×10^{22} cm). Its mass reaches $10^8-10^9 M_\odot \sim 10^{41}-10^{42}$ g (the mass of the Galaxy is $M \sim 10^{11} M_\odot$). The formation of such a nucleus in the center of the Galaxy, if it rotates sufficiently slowly, appears natural, the gas and stars "flowing" into the deep potential well. The gravitational contraction of a large mass is naturally accompanied by the release of a large amount of gravitational energy—an energy of order GM^2/R. Thus, for $R\sim10^{16}$ cm and $M\sim10^9 M_\odot \sim 10^{42}$ g the energy is $GM^2/R \sim 10^{61}$ erg $\sim 10^{-2} Mc^2$. The luminosity of the known quasars reaches 10^{48} erg/sec (this is the highest luminosity observed in nature; the luminosity of our entire Galaxy is of order 10^{44} erg/sec). Obviously, an energy release of $W\sim10^{61}$ erg suffices to sustain even this giant luminosity for 3×10^5 years. Besides the radio and, mainly, infrared and visible radiation, at least some quasars are sources of powerful x-ray radiation. Thus, of the 111 quasars investigated by the x-ray space observatory Einstein (the satellite HEAO B ≡ HEAO 2 launched on November 13, 1978), 35 have also been found to be emitters in the x-ray range (photon energy in the interval $0.5 < E_x < 4.5$ keV) with luminosity $L_x \sim 10^{43}-10^{47}$ erg/sec.[93] In the quasar 3C 273, the luminosity is $L_x \sim 10^{46}$ erg/sec. For this quasar (and, so far, for it alone) the gamma luminosity is also known: $L_\gamma(50 < E_\gamma < 500\ \text{MeV}) \sim 2\times10^{46}$ erg/sec. There is no doubt that these tremendous luminosities in the hard part of the spectrum are very significant (see Sec. 20 below).

But what is this radiating nucleus with radius $R\sim10^{16}-10^{17}$ cm? The actual radiating region itself is evidently not under any extraordinary conditions. It contains many relativisic particles (in particular, electrons), a high radiation density, and there is a magnetic field of $H\sim1-10^2$ Oe which is appreciable for low-density regions in the cosmos. Synchrotron radiation and inverse Compton scattering (the scattering of soft photons by relativistic electrons), and to some extent thermal radiation (i.e., bremsstrahlung) of a hot plasma can explain the observed radiation. Moreover, this picture depends little on what happens within the radiating nucleus, i.e., in its core, where the "machine" that drives the quasar or nucleus is situated.[94] Therefore, the radiating nucleus is sometimes referred to as a "black box." But what is in the black box; what is the nature of the cores of the quasars and the active galactic nuclei?

To this question there is as yet no answer, nor is it known when there will be one. There are two most probable models of the core: a massive black hole and a magnetoid or spinar—a rotating magnetoplasma mass (a superstar) without a black hole at its center. There is also the model of a dense cluster of stars, but for a number of reasons it is less probable than the other two.[94]

If it is assumed that black holes can exist, i.e., we rely on general relativity (and this indeed is the most sensible approach, as has been emphasized several times), the model of a massive black hole as the core of quasars and active galactic nuclei appears natural

[11)]The cosmological distance to an extragalactic object (galaxy, quasar) is the distance calculated from the data on the red shift of the spectral lines in the spectrum of the object under the assumption that the displacement is due to participation in the expansion of the Metagalaxy.

and attractive. Indeed, large masses are incapable of remaining in equilibrium, and a black hole is the state at which they could arrive.[71,94] But, on the other hand, if one argues in this way, one could expect the presence of massive black holes at the center of our Galaxy and many other galaxies. But this contradicts a number of observations and theoretical arguments.[95] Complete collapse, i.e., down to the formation of a massive black hole, is hindered by the need to shed angular momentum. More precisely, this slows down the collapse. Further opposing factors could be fragmentation of the large mass into smaller masses, the formation of close binary stars, and nuclear processes. It is therefore conceivable that a dense gas mass or cluster could break up or, at least, not collapse with the formation of a massive black hole for a very long time. It is sufficient if this delay in the formation of massive black holes is several billion years for their appearance in galaxies and quasars to be a rarity or practically not to occur at all.

This is by no means a decisive objection to the possibility of attributing the activity in quasars and galatic nuclei to massive black holes. All I am saying is that without further proof one cannot adopt such a hypothesis as an almost necessary or even the most probable one (and such a tendency is clear in the literature). The problem is to elucidate the nature of the cores of quasars and active galactic nuclei by observations. Some definite, albeit not brilliant possibilities are available here, in particular, through study of the variations in the radiation intensity. Mention should also be made of the prospects opened up for this purpose by the development of high-energy neutrino astronomy (see Ref. 96 and Sec. 21 below).

In the study of galaxies and quasars there is one further great problem (besides the question considered above): How are galaxies (including quasars) and clusters of galaxies formed? We must include here some cosmological questions such as the problem of the missing mass. I shall content myself here with a reference to the proceedings of a symposium devoted to this subject[97] and the mention of Refs. 60 and 80, in which the possible changes in the situation if neutrinos have a nonzero mass are discussed.

20. The origin of cosmic rays and cosmic gamma and x-ray radiation

A more accurate and up-to-date title of the present section is High-Energy Astrophysics, though this field also includes the astrophysics of high-energy neutrinos, which will be discussed in the following section. With regard to the remaining (and main) part of high-energy astrophysics, it can be divided into the astrophysics of cosmic rays, x-ray astronomy, and gamma-ray astronomy. Because of the historical tradition and adherence in the English literature to the expression "origin of cosmic rays," the astrophysics of cosmic rays is frequently called the problem of the origin of cosmic rays. This is reflected in the title of the present section.

But enough of terminology. Whatever name is chosen, the subject of discussion is fairly clear. Modern astronomy simply cannot be conceived without high-energy astrophysics. I have been concerned with this field for exactly 30 years and have written, especially on the origin of cosmic rays, so much (for the latest papers in this journal see Ref. 98; see also the papers in the reviews of Ref. 99) that I do not find within me the strength to attack this subject once more. Fortunately, this is not necessary in the present paper, and some comments will suffice.

During the last decade, the progress in x-ray astronomy has been particularly impressive. The first galatic x-ray source was discovered in 1962 by means of apparatus carried on a rocket. The preparation of special x-ray satellites required several years, and they were flown only during the last decade. A culmination was the launch in 1978 of the Einstein space observatory, which we have already mentioned in Sec. 19. The angular resolution of the x-ray telescope on it is seconds of arc, i.e., it approaches the best angular resolution of terrestrial optical telescopes. The Einstein observatory, and also some other satellites to some extent, has already yielded so many results and of such quality[93,100] that x-ray astronomy can be regarded as having come of age and of reaching a par with optical and radio astronomy.

The observations in the different ranges by no means duplicate each other. The radio, optical, and x-ray skies are in many ways quite different, even though the Sun can be "seen" at all these wavelengths. In this connection, the achievements of x-ray astronomy cannot be reduced to a few discoveries. Nevertheless, we shall select two of them. The first is the discovery of powerful "x-ray stars"—close binary stars, including x-ray pulsars (see Sec. 18). The second discovery was made in 1975 and later, and consisted of the discovery of x-ray bursts in what have become known as bursters. Here, we are evidently dealing in the first place with x-ray radiation formed near the surface or on the surface of neutron stars during the nonstationary accretion of plasma and as a result of the thermonuclear "burning" of accreted matter.[100]

Observational gamma-ray astronomy was effectively born during the last decade. As yet, its successes have been much more modest than in the case of x-ray astronomy. There are however grounds for believing that in the present decade gamma astronomy will catch up radio, optical, and x-ray astronomy in its significance. Indeed, a number of results have already been obtained and, moreover, in different parts of the huge gamma spectrum from a photon energy of hundreds of keV to energies $E_\gamma \gtrsim 10^{11}-10^{12}$ eV. In this last case, the observations are made by means of terrestrial apparatus which detects bursts of Cherenkov radiation in the atmosphere produced by gamma photons and the extensive air showers which they produce. The gamma radiation is investigated in both the continuum[99,101] and in lines.[102]

The European space satellite COS-B (launched in 1975), like some other satellites, observed general radiation concentrated in the galactic plane and produced by cosmic rays (the electron component of the cosmic rays gives gamma bremsstrahlung, and the proton–nucleus component generates through collisions in the interstellar gas various unstable particles, in particular π^0 mesons; these last, and also some other particles, decay with the emission of gamma rays). According to the latest data,[103] the satellite COS-B has also discovered 25 discrete gamma sources (for photon energy $E_\gamma > 100$ MeV). Two of them have been identified with the known pulsars PSR 0531 + 21 (in the Crab) and PSR 0833 − 45 (Vela); one source has been indentified with the quasar 3C 273 (see Sec. 19) and one, apparently, with the molecular cloud ρ Oph. Of the remaining 21 sources, 20 lie at low galactic latitudes and are therefore in all probability galactic. Their gamma luminosity is $L_\gamma(E_\gamma > 100\ \text{MeV}) \sim (0.4-5)\times 10^{36}$ erg/sec. It can be estimated from the data of the survey of the part of the sky in Ref. 103 that there are several hundred such sources in the Galaxy. What are these sources? As we have seen, their gamma luminosity exceeds by two or three orders of magnitude the total luminosity $L_\odot = 3.8\times 10^{33}$ erg/sec of the Sun. They are probably neutron stars, but the question is essentially entirely open. The huge gamma luminosities of the already detected sources guarantee that in gamma astronomy the radiation does not belong to the "tails" of radiation in softer ranges, and the phenomena are in some measure typical of precisely the gamma part of the spectrum.

We have already mentioned the x-ray bursts. But even earlier (the first publications appeared ten years ago; see Ref. 2) gamma bursts were detected. Until recently, their nature remained unknown, and it is only comparatively recently that it has become fairly clear that the gamma bursts are formed in the Galaxy and are in some way associated with stars, in the first place, or even exclusively with neutron stars.[104]

Particular mention must be made of the powerful and unusual gamma burst observed on March 5, 1979 (see Refs. 105 and 106 and the literature quoted there). It is possible that the source of this burst is the remnant (evidently, a neutron star) of a supernova that exploded in the Large Magellanic Cloud. Such an assumption involves energy difficulties, but apparently is still admissible.[106]

Strictly speaking, the problem of the origin of cosmic rays arose simultaneously with their discovery in 1912. But for a number of reasons[98,99] it is not really possible to speak of the creation of cosmic-ray astrophysics before 1951–1953. During 30 years much has been done, but ten years ago in 1 it was still necessary to emphasize the lack of clarity in the fundamental question of the choice of the model of the origin of the major part of the cosmic rays observed at the Earth. Thus, it was not possible to prove with confidence the galactic model with halo that I supported (viable alternatives were the metagalactic model and the disk galactic model). Today, I am convinced that a choice in favor of the galactic model with halo can be made with complete confidence. This has come about as a result of the discovery of radio halos in the galaxies NGC 4631 and NGC 891, which are seen edge on, and also other data which leave no doubt about the existence of a "halo of cosmic rays" around our Galaxy. Another important achievement is the discovery (though it still needs to be made more accurate) by gamma astronomy of a decrease in the intensity of cosmic rays at the periphery of the Galaxy. But, as has already been said, we refer the reader for details to Refs. 98 and 99.

Ten years ago, in 1, the problems listed in the title of the present section were narrower, more local, and more concrete. Now, bearing in mind the huge field of astrophysics—high-energy astrophysics—it is no longer possible to speak of one or even three problems. But we are writing about the progress during the decade, and have therefore retained the present section with its old name. But it is not only a matter of continuity. High-energy astrophysics is basically a young science in the process of becoming established (this is true at least for a number of directions). Therefore, in a modern list of "particularly important and interesting problems" it is necessary to include problems in high-energy astrophysics. What these problems are has been already said in part, and some details can be found in Refs. 98 and 99. I believe that this is sufficient here, since the present paper is not an authoritative document; indeed, it is not a document at all but only a paper for the section Physics of Our Days of this journal, reflecting moreover merely the opinion of its author.

21. Neutrino astronomy

If one speaks of experimental results, little has occurred in the field of neutrino astronomy during the decade. The attempts over many years to detect solar neutrinos by using the nuclear reaction $^{37}Cl + \nu_e \to {}^{37}Ar + e^-$ did not lead to positive results for a long time, but for the flux of the corresponding neutrinos a value has recently been given: 2.2 ± 0.3 SNU (see Ref. 107) or 1.8 ± 0.4 SNU (see Ref. 108). The solar neutrino unit (SNU) used here is such that at a flux of 1 SNU 10^{36} nuclei of ^{37}Cl capture on the average one neutrino per second. The theoretical calculations for the so-called standard models of the Sun give fluxes equal to 4.7 SNU (the somewhat older data) and 6 ± 2 SNU according to Ref. 107.

I must admit (or even say I am sorry) that such discrepancy has not and does not impress me, bearing in mind how difficult it is to calculate exactly the neutrino flux from the Sun (it is important here that the reaction in ^{37}Cl proceeds through neutrinos with a fairly high energy above 0.81 MeV, emitted basically by the decay of the nucleus ^8B; the flux of such neutrinos is very sensitive to the temperature at the center of the Sun and, generally, to the choice of the solar models). It is true that the neutrino oscillations,[58,59,107] which have been so much discussed recently, could under certain conditions (in the first place, the important thing is the mass difference of the various neutrino species, i.e., ν_e, ν_μ, and ν_τ) explain the experimentally observed lowering of the neutrino flux by three times compared with the value calculated without allowance for oscillations.[107] But to conclude from this that the discrepancy between theory and experiment is due to neutrino oscillations would be entirely premature.

The solar neutrino problem could perhaps be to a large extent resolved as a result of further measurements with a chlorine detector, but it is also necessary to make measurements using other detectors, in the first place with ^{7}Li and especially with ^{71}Ga. The isotope ^{71}Ga absorbs neutrinos with an energy exceeding only 0.23 MeV, and is then transformed into ^{71}Ge. Therefore, a gallium detector can measure the main fraction of the neutrinos emitted by the Sun and formed in the reaction $p+p \to d+e^{+}+\nu_e$; these have an energy which reaches 0.42 MeV. In a good approximation, the flux of such neutrinos is determined by the solar luminosity and, therefore, does not depend on the model of the Sun (under the assumption that the flux is stationary). It is perfectly possible to separate germanium from gallium, and thus a gallium detector (which must weigh 20–40 tons) is very promising.[108,109]

The birth of neutrino astronomy is a great event, since the detection of neutrinos is the only way of obtaining information from the central regions of stars (true, gravitational waves would also arrive from these regions, but apart from the difficulties of their detection such waves will not, in general, be generated by stars). In the forseeable future, one cannot hope for the detection of neutrinos from ordinary stars. But supernova explosions and the formation of neutron stars (it is not certain that the formation of neutron stars is always accompanied by an observable explosion) could generate powerful neutrino fluxes.[108,109] The corresponding fluxes are observable, and several subterranean neutrino telescopes suitable for this purpose are already operating. It would be exceptionally important to detect neutrinos of cosmological origin, i.e., formed during the early evolution of the Universe,[109] but as yet no real prospects for achieving this aim are in sight.

Finally, in recent years more and more attention is being paid to high-energy neutrino astronomy (see Refs. 96, 109, and 110 and the literature there). Neutrinos with a high energy exceeding a hundred MeV and, *a fortiori*, many GeV are produced almost exclusively by the proton–nucleus component of the cosmic rays. In this respect, they are analogous to the gamma rays from the decay of π^0 mesons (see Sec. 21). However, if we are speaking of detection, we are concerned with cosmic rays of much higher energy, generating neutrinos with energy $E_\nu \gtrsim 10^3$ GeV. There are projects (in the first place, the project DUMAND, in which a neutrino shower must be detected deep under water by an optical method) whose realization will probably make it possible to detect neutrinos from quasars and active galactic nuclei. In this way it may be possible to establish whether the core of a quasar is a massive black hole or a magnetoid.[96]

The decade has been too short for neutrino astronomy to become established. But the experimental problems here are so complicated that this should not occasion surprise. In another ten years the situation will probably be different. However, it appears to me that the flowering of neutrino astronomy cannot occur before the end of the century.

V. CONCLUDING REMARKS

In my mind's eye and, I hope, the reader's there will have passed, flashed by, the last decade, filled with the concentrated effort of physicists and astronomers. Ten years is a long time for a man. For a young man, because ten years ago he had not, perhaps, yet grown up. For a grown man, a decade in science is also a long time, but for quite a different reason—his chances of still participating for a long time in the development of science or at least following its development become less and less. But if we discount the subjective perception of time and its flow, a decade in science is not such a long period. Let us recall that the special theory of relativity is about 75 years old, the general theory of relativity 65 years old, and quantum mechanics 55 years old. These theories are the foundation of modern physics. Superconductivity was discovered in 1911 and cosmic rays in 1912. But even today both problems—superconductivity and cosmic rays—are at the center of attention or occupy many more people than during the first two or three decades after their discovery. Thus, the time scale in modern physics is longer than the duration of active human life, to say nothing of a decade. We may add that the complexity of some modern experimental facilities (accelerators; space observatories; terrestial, optical, and radio telescopes, etc.) are such that from their first conception to their fruition not less than 10 or 15 years usually pass.[12)]

In the light of this comment, it is quite natural that although the decade which separates the paper 1 from the present paper has brought forth not a little that is new the majority of the problems remain in our list. True, in microphysics there have been significant changes, but this evidently makes the past years exceptional (and, in fact, many new ideas appeared earlier; for example, the quark hypothesis in 1963–1964).

Thus, a decade in the development of physics and astrophysics is not an exceptionally long period but does permit the recording of much that is new.

Therefore, it appears to me that if it was worth writing the present paper as a continuation of paper 1 at all, it was appropriate to do it now—ten years on. But should these papers have been written ? Others must judge. I shall content myself with the remark that the writing of both papers was difficult, but I found it interesting. Physics and astronomy have grown to such an extent that it is not at all easy to follow the couple of dozen directions and problems

---

[12)]So as not to overburden the paper, I shall refrain here from making some other comments concerning the development of physics and astrophysics. My opinion in this matter is clear from Refs. 2 and 78 (I may mention that I have now changed the opinion which I expressed in Sec. 24 of Ref. 2 about the nature of the "second astronomical revolution" and now adhere to the position given in Ref. 78).

identified here. On the other hand, in each given period one could perhaps go into the details of just one or two problems, treating them professionally. In this connection work on a paper such as this provides a stimulus for becoming acquainted, albeit cursorily, with a larger and broader spectrum of material. One learns not a little that is interesting, the trees do not obscure the wood, a vista of the future opens up, and the breadth and, at the same time, deep unity of physics and the richness of its content become clearer. If even some of the readers share these feelings to an extent, the aim of the paper will have been achieved. To those colleagues who read the paper but remain wholly or partly dissatisfied with it, or even annoyed, I make the request for constructive criticism. In my view, this should be reflected in the writing of other papers, large or small, in which some of the problems and questions are treated in a manner different from here and placed in a different light. I believe that many readers of this journal desire this in particular.

Finally, I should like to take the opportunity of thanking all those who made comments after reading the paper in draft. As in 1, I shall not name them, so as not, even indirectly, to place the responsibility for the shortcomings and inadequacies of the paper on others.

[1]V. L. Ginzburg, Usp. Fiz. Nauk **103**, 87 (1971) [Sov. Phys. Usp. **14**, 21 (1971)].

[2]V. L. Ginzburg, O fizike i astrofizike (kakie problemy predstavlyayutsya seǐchas osobenno vazhnymi i interesnymi?) [On Physics and Astrophysics (What Problems of Physics and Astrophysics are of Special Importance and Interest?)], Nauka, Moscow (1980).

[3]a) Novosti termoyadernykh issledovaniǐ v SSSR: Operativnaya informatsiya (News About Thermonuclear Investigations in the Soviet Union: Operational Information), Institute of Atomic Energy, Moscow (1980,1981); Nucl. Fusion **20**, 1063 (1980); b) G. Yonas, Fusion power with particle beams, Scientific American **239** (5), 50–61 (November 1978) [Russian Transl. published in Usp. Fiz. Nauk **133**, 159 (1981)].

[4]D. Jerome, A. Mazaud, M. Ribault, and K. Bechgard, J. Phys. (Paris), Lett. **41**, 95 (1980); K. Andres, F. Wudl, D. B. McWhan, G. A. Thomas, D. Nalewajek, and A. L. Stevens, Phys. Rev. Lett. **45**, 1449 (1980); R. L. Greene and E. M. Engler, Phys. Rev. Lett. **45**, 1587 (1980).

[5]Problema vysokotemperaturnoǐ sverkhprovodimosti (The Problem of High-Temperature Superconductivity) (eds. V. L. Ginzburg and D. A. Kirzhnits), Nauka, Moscow (1977); see also, Usp. Fiz. Nauk **118**, 315 (1976) [Sov. Phys. Usp. **19**, 174 (1976)].

[6]G. N. Stepanov and E. N. Yakovlev, Pis'ma Zh. Eksp. Teor. Fiz. **32**, 657 (1980) [JETP Lett. **32**, 643 (1980)].

[7]N. G. Brandt *et al.*, Pis'ma Zh. Eksp. Teor. Fiz. **27**, 37 (1978) [JETP Lett. **27**, 33 (1978)]; C. W. Chu *et al.*, Phys. Rev. B **18**, 2116 (1978).

[8]B. A. Volkov, V. L. Ginzburg, and Yu. V. Kopaev, Pis'ma Zh. Eksp. Teor. Fiz. **27**, 221 (1978) [JETP Lett. **27**, 206 (1978)]; B. A. Volkov, Yu. V. Kopaev, *et al.*, Pis'ma Zh. Eksp. Teor. **27**, 615 (1978); **30**, 317 (1979) [JETP Lett. **27**, 582 (1978); **30**, 293 (1979)]; V. L. Ginzburg, Solid State Commun. (1981) (in press).

[13)]A fairly large number of references to the literature on virtually all the problems considered in the present paper can be found in Ref. 2. Moreover, in many cases additional references can be found by examining the index of papers published in this journal every year in the December issue.

[9]I. Lefkowitz, J. S. Manning, and P. E. Bloomfild, Phys. Rev. B **20**, 4506 (1979); see also, Phys. Rev. B **23**, 3022 (1981).

[10]T. E. Gebale and C. W. Chu, Commun. Solid State Phys. **9**, 115 (1979).

[11]E. Brown, C. G. Homan, and R. K. MacCrone, Phys. Rev. Lett. **45**, 478 (1980).

[12]S. M. Stishov, Usp. Fiz. Nauk **127**, 719 (1979) [Sov. Phys. Usp. **22**, 283 (1979)]; see also, Pis'ma Zh. Eksp. Teor. Fiz. **33**, 136 (1981) [JETP Lett. **33**, 128 (1981)].

[13]a) T. M. Rice, J. C. Hansal, T. G. Phillips, and G. A. Thomas, Elektronno-dyrochnaya zhidkosh' v polyprovodnikakh [Electron-hole liquid in Semiconductors; Russ. Transl. Mir, Moscow (1980)]; b) M. V. Sadovskiǐ, Usp. Fiz. Nauk **133**, 223 (1981) [Sov. Phys. Usp. **24**, 96 (1981)].

[14]a) L. D. Landau and E. M. Lifshits, Statisticheskaya fizika, Part 1, Nauka, Moscow (1976); English translation: Statistical Physics, Vol. 1, 3rd ed., Pergamon Press, Oxford (1980); b) G. S. Ma, Modern Theory of Critical Phenomena, Reading, Mass. (1976) [Russ. Transl. Mir, Moscow (1980)].

[15]a) J. M. H. Levelt Sengers, Physica (Utrecht) **A82**, 319 (1976); b) A. P. Levanyuk and D. G. Sannikov, Usp. Fiz. Nauk **132**, 694 (1980) [Sov. Phys. Usp. **23**, 868 (1980)].

[16]V. L. Ginzburg and A. A. Sobyanin, Usp. Fiz. Nauk **120**, 153 (1976) [Sov. Phys. Usp. **19**, 773 (1976)].

[17]I. L. Fabelinskiǐ, Molekulyarnoe rasseyanie sveta (Molecular Scattering of Light), Nauka, Moscow (1965); V. L. Ginzburg, A. P. Levanyuk, and A. A. Sobyanin, Usp. Fiz. Nauk **130**, 615 (1980) [Phys. Rep. **57**, 152 (1980)].

[18]Sverkhtekuchest' geliya-3. Sb. stateǐ (Superfluidity of Helium 3; Russian Collection of Translations), Mir, Moscow (1977); Kvantovye zhidkosti i kristally: Sb. stateǐ (Quantum Liquids and Crystals; Russian Collection of Translations), Mir, Moscow (1979).

[19]W. N. Hardy *et al.*, Phys. Rev. Lett. **45**, 453 (1980); R. W. Cline *et al.*, Phys. Rev. Lett. **45**, 2117 (1980).

[20]J. T. M. Walraven, I. F. Silvera, and A. P. M. Mattey, Phys. Rev. Lett. **45**, 449, 915 (1980).

[21]V. M. Agranovich and V. L. Ginzburg, Kristallooptika s uchetom prostranstvennoǐ dispersii i teoriya eksitonov (Crystal Optics with Allowance for Spatial Dispersion and the Theory of Excitons), Nauka, Moscow (1979).

[22]Proc. Intern. School on Condensed Matter Physics, Varna, Bulgaria (1980).

[23]D. Tabor, Surf. Sci. **89**, 1 (1979).

[24]V. L. Pokrovsky, Adv. Phys. **28**, 595 (1979).

[25]D. R. Nelson, in: Proc. Summer School in Statistical Mechanics, Enschede, Netherlands (1980).

[26]V. L. Brantman, N. S. Ginzburg, and M. I. Petelin, Opt. Commun. **30**, 409 (1980); Nature **285**, 15 (1980); Science **204**, 394 (1979).

[27]V. P. Perelygin and S. G. Stetsenko, Pis'ma Zh. Eksp. Teor. Fiz. **32**, 622 (1980) [JETP Lett. **32**, 608 (1980)].

[28]R. J. Blin-Stoyle, Contemp. Phys. **20**, 377 (1979); N. N. Nikolaev Usp. Fiz. Nauk **134**, 369 (1981) [Sov. Phys. Usp. **27**, 531 (1981)].

[29]D. Pines, Science **207**, 597 (1980); **J. Phys. (Paris) 41**, C2-111 (1980) [Russ. Transl. Usp. Fiz. Nauk **131**, 479 (1980)].

[30]a) W. M. Alberico *et al.*, Phys. Lett. **B92**, 153 (1980); D. Vasak *et al.*, Phys. Lett. B93, 243 (1980); L. R. R. Mohan and R. W. Minich, Phys. Lett. **B93**, 467 (1980); b) L. A. Sliv, Usp. Fiz. Nauk **133**, 337 (1981) [Sov. Phys. Usp. **26**, 142 (1981)].

[31]A. Einstein, "Einiges uber die Eutstehung der allgemeinen Relativitatstheorie," George A. Gibson Foundation Lecture, Glasgorv (1933) [Russ. Transl. published in the Collected Works of Einstein, Vol. 2, Nauka, Moscow (1966), p. 403].

[32]A. Salam, Rev. Mod. Phys. **52**, 525 (1980).

[33]S. Weinberg, Rev. Mod. Phys. **52**, 515 (1980); S. Glashow, Rev. Mod Phys. **52**, 539 (1980).

[34]D. Cleine and C. Rubbia, Phys. Today **33**, No. 8, 44 (1980).
[35]J. Iliopoulos, Contemp. Phys. **21**, 159 (1980); G. t'Hooft, Sci. Am. **242**, No. 6, 90 (1980).
[36]Ch. Berger *et al.*, Phys. Lett. **B86**, 418 (1979); Phys. Rev. Lett. **43**, 830 (1979); Ya. I. Asimov, Yu. L. Dokshitser, and V. A. Khoze, Usp. Fiz. Nauk **132**, 443 (1980) [Sov. Phys. Usp. **23**, 732 (1980)]. A. I. Vaĭnshteĭn, V. I. Zakharov, and M. A. Shifman, Usp. Fiz. Nauk **131**, 537 (1980) [Sov. Phys. Usp. **24**, 429 (1980)].
[37]A. I. Vaĭnshteĭn, V. I. Zakharov, and M. A. Shifman, Usp. Fiz. Nauk **131**, 537 (1980) [Sov. Phys. Usp. **24**, 429 (1980)].
[38]H. Georgiand and S. L. Glashow, Phys. Today **33**, No. 9, 30 (1980).
[39]L. B. Okun', Usp. Fiz. Nauk **134**, 3 (1981) [Sov. Phys. Usp. **27**, 1 (1981)]; see also L. B. Okun', Leptony i kvarki (Leptons and Quarks), Nauka, Moscow (1981).
[40]B. Richter, S. C. C. Ting, Les Prix Nobel en 1976, Stockholm (1977).
[41]L. M. Lederman, Sci. Am. **239**, No. 4, 72 (1978).
[42]W. Bartel *et al.*, Phys. Lett. **89**, 136 (1979); D. P. Barber *et al.*, Phys. Rev. Lett. **44**, 1722 (1980).
[43]M. S. Chanowitz, Phys. Rev. Lett. **44**, 59 (1980).
[44]"On the passing of Werner K. Heisenberg," Usp. Fiz. Nauk **121**, 657 (1977) [Sov. Phys. Usp. **20**, 335 (1977)].
[45]Y. Nambu, Sci. Am. **235**, No. 5, 48 (1976); S. D. Drell, Phys. Today **31**, No. 6, 23 (1978).
[46]C-N. Yang, Physics Today, **33**, No. 6, 42 (1980) [Russ. Transl. Usp. Fiz. Nauk **132**, 169 (1980)].
[47]A. Pais, Rev, Mod. Phys. **51**, 861 (1979).
[48]S. Weinberg, "Unified theories of elementary particle interaction," D. B. Cline, A. K. Mann, and C. Rubbia, Sci. Am. **231**, No. 6, 108 (1974); **234**, No. 1, 44 (1976); J. Iliopoules, "An introduction to gauge theories," A. A. Slavnov, Usp. Fiz. Nauk **124**, 487 (1978) [Sov. Phys. Usp. **21**, 240 (1978)].
[49]D. A. Kirzhnits, Usp. Fiz. Nauk **125**, 169 (1978) [Sov. Phys. Usp. **21**, 460 (1979)].
[50]B. Richter, Lecture presented at G. I. Budker Memorial Session of the American Physical Soc. on April **28**, 1978 in Washington, D. C., SLAC-PUB-2274 March 1979; R. R. Wilson, Sci. Am. **242**, No. 1, 26 (1980).
[51]L. M. Barkov, M. S. Zolotarev, and I. B. Khriplovich, Usp. Fiz. Nauk **132**, 409 (1980) [Sov. Phys. Usp. **23**, 713 (1980)].
[52]Yu. V. Bogdanov, I. I. Sobel'man, Yu. N. Sorokin, and I. I. Struk, Pis'ma Zh. Eksp. Teor. Fiz. **31**, 234, 556 (1980) [JETP Lett. **31**, 214, 522 (1980)].
[53]Ya. I. Azimov and V. A. Khoze, Usp. Fiz. Nauk **132**, 379 (1980) Usp. Phys. Nauk **132**, 379 (1980) [Sov. Phys. Usp. **23**, 699 (1980)].
[54]A. D. Dolgov and Ya. B. Zel'dovich, Usp. Fiz. Nauk **130**, 559 (1980) [Rev. Mod. Phys. **53** (1), 1–41 (January 1981)].
[55]M. A. Markov, Usp. Fiz. Nauk **111**, 719 (1973) [Sov. Phys. Usp. **16**, 913 (1974)].
[56]D. Z. Freedman and P. van Nieuwenhuizen, Sci. Am. **238**, No. 2, 126 (1978).
[57]V. A. Lyubimov *et al.*, Preprint ITEP, Moscow (1980); Phys. Lett. **B94**, 266 (1980); V. S. Kozik *et al.*, Yad. Fiz. **32**, 301 (1980) [Sov. J. Nucl. Phys. **32**, 154 (1980)].
[58]Reines *et al.*, Phys. Rev. Lett. **45**, 1307 (1980); V. Barger *et al.*, Phys. Lett. **B93**, 194 (1980).
[59]C. M. Bilen'kiĭ and B. M. Pontecorvo, Usp. Fiz. Nauk **123**, 181 (1977) [Sov. Phys. Usp. **20**, 776 (1977)].
[60]J. E. Gunn *et al.*, Astrophys. J. **223**, 1015 (1978); Ya. B. Zel'dovich *et al.*, Pis'ma Astron. Zh., **6**, 451, 457 (1980) [Sov. Astron. Lett. **6**, 249, 252 (1980)]; G. S. Bisnovatyĭ-Kogan and I. D. Novikov, Astron. Zh. **57**, 899 (1980) [Sov. Astron. **24**, 516 (1980)].
[61]S. D. Drell, Physica **96A**, 3 (1979).
[62]W. Bartel *et al.*, Phys. Lett. **B92**, 206 (1980).
[63]V. L. Ginzburg, Pis'ma Zh. Eksp. Teor. Fiz. **22**, 514 (1975) [JETP Lett. **22**, 251 (1975)]; V. L. Ginzburg and V. P. Frolov, Pis'ma Astron. Zh. **2**, 474 (1976) [Sov. Astron. Lett. **2**, 184 (1976)].
[64]S. I. Nikol'skiĭ, E. L. Feĭnberg, V. V. Avakyan, *et al.*, Usp. Fiz. Nauk **132**, 392, 394, 395 (1980) [Sov. Phys. Usp. **23**, 708, 709, 711 (1980)]; E. L. Feĭnberg, Vestn. Akad. Nauk SSSR, No. 1, 25 (1981).
[65]Problema narusheniya *CP*-invariantnosti (The Problem of *CP* Violation), Usp. Fiz. Nauk **95**, 401 (1968) [Sov. Phys. Usp. **11**, 461 (1968)]; D. V. Nanopoulos *et al.*, Ann. Phys. (N.Y.) **127**, 126 (1980).
[66]A. A. Nikishev and V. I. Ritus, Kvantovaya elektrodinamika yavleniĭ v intensivnom pole (Quantum Electrodynamics of Phenomena in Strong Fields), Tr. Fiz. Inst. Akad. Nauk SSSR **111** (1979).
[67]A. D. Linde, Rep. Prog. Phys. **42**, 389 (1979).
[68]M. S. Turner and D. N. Schramm, Phys. Today **32**, No. 9, 42 (1980).
[69]V. L. Ginzburg and V. I. Man'ko, Fiz. Elem. Chastits At. Yadra **7**, 3 (1976) [Sov. J. Part. Nucl. **7**, 1 (1976)].
[70]J. R. Oppenheimer and H. Snyder, Phys. Rev. **56**, 455 (1939); [Russ. Transl. published in a collection entitled Albert Einstein and the Theory of Gravitation, Mir, Moscow (1979)].
[71]M. J. Rees, Contemp. Phys. **21**, 99 (1980).
[72]R. F. C. Vessot and M. W. Levine, Gen. Relat. Gravit. **10**, 181 (1979); see also, Phys. Rev. Lett. **45**, 2081 (1980).
[73]R. D. Reasenberg *et al.*, Astrophys. J. Lett. **234**, L219 (1979); C. M. Will, Proc. R. Soc. London Ser. A **268**, 5 (1979).
[74]V. L. Ginzburg, Usp. Fiz. Nauk **128**, 435 (1979) [Sov. Phys. Usp. **22**, 514 (1979)]; this paper is also published in the collection: V. L. Ginzburg, O teorii otnositel'nosti (On the Theory of Relativity), Nauka, Moscow (1979).
[75]V. F. Mukhanov, Usp. Fiz. Nauk **133**, 729 (1981) [Sov. Phys. Usp. **24**, 331 (1981)]; F. H. Chaffee, Sci. Am. **243**, No. 5, 60 (1980).
[76]V. B. Braginskiĭ, Usp. Fiz. Nauk **132**, 387 (1980) [Sov. Phys. Usp. **25**, 704 (1980)]; Zemlya i Vselennaya No. 3, 28 (1980); V. N. Rudenko, Usp. Fiz. Nauk **126**, 361 (1978) [Sov. Phys. Usp. **21**, 893 (1978)].
[77]J. H. Taylor *et al.*, Nature **277**, 437 (1979).
[78]V. L. Ginzburg, Vopr. Filos. No. 12, 24 (1980).
[79]I. N. Bernshteĭn and V. F. Shvartsman, Zh. Eksp. Teor. Fiz. **79**, 1617 (1980) [Sov. Phys. JETP **52**, 814 (1980)].
[80]J. R. Bond *et al.*, Phys. Rev. **45**, 1980 (1980).
[81]A. D. Linde, Phys. Lett. B **92**, 119, 327, 394 (1980); D. A. Kirzhnits and A. D. Linde, Priroda (Moscow) No. 11, 20 (1979); M. Kighuchi, Prog. Theor. Phys. **63**, 146 (1980); D. V. Nanopoulos and S. Weinberg, Phys. Rev. D **20**, 2484 (1979).
[82]J. D. Bekenstein and A. Meisels, Astrophys. J., **237**, 342 (1980).
[83]V. Ts. Gurovich and A. A. Starobinskiĭ, Zh. Eksp. Teor. Fiz. **77**, 1683 (1979) [Sov. Phys. JETP **50**, 844 (1979)]; see also, Pis'ma Zh. Eksp. Teor. Fiz. **30**, 719 (1979) [JETP Lett. **30**, 682 (1979)]; V. P. Frolov and G. A. Vilkovisky, Intern. Center for Theoretical Physics Preprint IC/79/69, Miramare, Trieste (1979); Ya. B. Zel'dovich, Usp. Fiz. Nauk **133**, 479 (1981) [Sov. Phys. Usp. **24**, 216 (1981)]; V. F. Mukhanov and G. V. Chibisov, Pisma Zh. Eksp. Teor. 33, 549 (1981) [JETP Lett. **33**, 532 (1981)].
[84]H. Gursky and E. P. J. van den Heuvel, "X-ray emitting double stars" 85 R. N. Manchester and J. H. Taylor, Pulsars, W. H. Freeman (1977) [Russ. Transl. Mir, Moscow, 1980]; F. G. Smith, Pulsars, C. U. P. (1977) [Russ. Transl. Mir, Moscow, 1979]; D. ter Haar, Contemp. Phys. **16**, 243 (1975).
[85]R. N. Manchester and J. H. Taylor, Pulsars, W. H. Freeman (1977) [Russ. Transl. Mir, Moscow, 1980]; F. G. Smith, Pulsars, C.U.P. (1977) [Russ. Transl. Mir, Moscow, 1979]; D. ter Haar, Contemp. Phys. **16**, 243 (1975).
[86]V. K. Lamb, Pulsars, IAU Symp. No. 95, Bonn, West Germany, August 1980 [the proceedings of this symposium are the last detailed list of data on pulsars (usually, the proceedings of the IAU Symposia are published by Reidel in Holland)].

[87]V. L. Ginzburg and V. V. Zheleznyakov, Ann. Rev. Astron. Astrophys. **13**, 511 (1975); Yu. P. Ochelkov and V. V. Usov, Pis'ma Astron. Zh. **5**, 180 (1979) [Sov. Astron. Lett. **5**, 96 (1979)]; G. Z. Machabeli and V. V. Usov, Pis'ma Astron. Zh. **5**, 455 (1979) [Sov. Astron. Lett. **5**, 238 (1979)].
[88]L. Landau and E. M. Lifshits, Teoriya polya, Nauka, Moscow (1973); English translation: The Classical Theory of Fields, 4th ed., Pergamon Press, Oxford (1975).
[89]S. W. Hawking, Nature **248**, 30 (1974); Phys. Rev. D **13**, 191 (1976).
[90]V. P. Frolov, Usp. Fiz. Nauk **118**, 473 (1976) [Sov. Phys. Usp. **19**, 244 (1976)]; in: Einshteĭnovskiĭ sbornik (Einstein Collection) 1975–1976, Nauka, Moscow (1978), p. 82; Chernye dyry: Sb. stateĭ (Black Holes; Collection of Russian translations), Mir, Moscow (1978).
[91]D. A. Kirzhnits and V. P. Frolov, Priroda (Moscow) (1981).
[92]A. P. Laĭtman, R. A. Syunyaev, *et al.*, Usp. Fiz. Nauk **126**, 515 (1978) [Comments on Astrophysics **7** (5), 151–160 (1978)].
[93]W. H. M. Ku, D. J. Helfand, and L. B. Lucy, Nature **288**, 323 (1980).
[94]V. L. Ginzburg and L. M. Ozernoĭ, Astrophys. Space Sci. **48**, 401 (1977).
[95]V. I. Dokuchaev and L. M. Ozernoĭ, Pis'ma Astron. Zh. **3**, 391 (1977) [Sov. Astron. Lett. **3**, 209 (1977)]; Astron. Zh. **55**, 27 (1977) [Sov. Astron. **22**, 15 (1978)]; V. G. Gurzadyan and L. M. Ozernoĭ, Pisma Astron. Zh. **5**, 630 (1979) [Sov. Astron. Lett. **5**, 337 (1979)]; M. J. Duncan and J. C. Wheeler, Astrophys. J. Lett. **237**, L27 (1980).
[96]V. S. Berezinsky and V. L. Ginzburg, Mon. Not. R. Astron. Soc. **194**, 3 (1981).
[97]The Large Scale Structure of the Universe (eds. M. S. Longair and J. Einasto), IAU Symposium No. 79 (a Russian translation is being prepared).
[98]V. L. Ginzburg and V. S. Ptuskin, Usp. Fiz. Nauk **117**, 585 (1975) [Sov. Phys. Usp. **18**, 931 (1975)]; V. L. Ginzburg, Usp. Fiz. Nauk **124**, 307 (1978) [Sov. Phys. Usp. **21**, 155 (1978)].
[99]Origin of Cosmic Rays. IUPAP/IAU Symposium No. 94, Bologna, Italy, D. Reidel, Amsterdam (1980); VII Evropeĭskiĭ simpozium po kosmicheskim lucham (Seventh European Symposium on Cosmic Rays), Leningrad (1980).
[100]D. J. Helfand *et al.*, Nature **283**, 337 (1980); X-ray Astronomy (COSPAR) (eds. W. A. Baity and L. E. Peterson), Pergamon Press, Oxford (1979).
[101]A. M. Gal'per, B. I. Luchkov, and O. F. Prilutskiĭ, Usp. Fiz. Nauk **128**, 313 (1979) Sov. Phys. Usp. **22**, 456 (1979); K. Pinkau, Nature **277**, 17 (1979); Usp. Fiz. Nauk **132**, 700, 702, 704 (1980) [Sov. Phys. Usp. **23**, 873, 874, 875 (1980)].
[102]I. L. Rozental', V. V. Usov, and I. V. Estulin, Usp. Fiz. Nauk **127**, 135 (1979) [Sov. Phys. Usp. **22**, 46 (1979)]; M. Levental and C. J. MacCallum, Sci. Am. **243**, No. 1, 50 (1980).
[103]B. N. Swanenburg *et al.*, Second COS-B Catalog of High-Energy Gamma-Sources Preprint (1980).
[104]E. P. Mazets *et al.*, Pis'ma Astron. Zh. **6**, 609, 706 (1980) [Sov. Astron. Lett. **6**, 318 372 (1980)].
[105]E. P. Mazets *et al.*, Nature **282**, 587 (1979); **290**, 378 (1981); J. Terrell *et al.*, Nature **285**, 383 (1980).
[106]R. Ramaty *et al.*, Nature **287**, 122 (1980).
[107]Nature **284**, 507 (1980).
[108]J. M. Bahcall, Space Sci. Rev. **24**, 227 (1979).
[109]Neutrino-77. Proc. of the Intern. Conference on Neutrino Physics and Neutrino Astrophysics, Vol. 1, Nauka, Moscow (1978).
[110]V. S. Berezinskiĭ, Priroda (Moscow) No. 3, 13 (1981); Usp. Fiz. Nauk **133**, 545 [Sov. Phys. Usp. **26**, 242 (1981)].
[111]G. S. LaRue, J. D. Phillips, and W. M. Frank, Phys. Rev. Lett. **46**, 967 (1981).
[112]H. Georgi , Sci. Am. **244**, No. 4, 40 (1981).

Translated by Julian B. Barbour

 613