PHYSICS OF OUR DAYS

What problems of physics and astrophysics seem now to be especially important and interesting (thirty years later, already on the verge of XXI century)?

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Contents

Abstract. About thirty years ago, the author published a paper [1] under the same title which, mainly educational in its nature, aimed primarily at widening the scientific horizons of the young generation of physicists. For this purpose, a list of the top agenda problems of the day was given and commented on, admittedly subjective and unavoidably inexhaustive and limited (one cannot do the impossible, after all). In the author's later articles, additions both to the list itself and the comments were made (see [2] for the latest version). In the present paper the author takes on an end-of-the-century perspective in addressing this theme once again.

1. Introduction

The rate of development of science nowadays is striking. Great changes in physics, astronomy, biology, and many other fields of science have come about within a period of not more than one-two generations. The readers may see it even on an example of their own families. My father, for instance, was born in 1863 and was a younger contemporary of Maxwell (1831 – 1879). I myself was already 16 when the neutron and positron were discovered in 1932. Before that only the electron, proton and photon were known. It is somehow not easy to realize that the electron, X-rays and radioactivity were discovered only about a hundred years ago, and quantum theory was born in 1900. At the same time, one hundred years is such a short period not only compared to the approximately 3 billion years since life appeared on the

Earth, but also to the age of modern man (homo sapiens), which amounts to nearly 50 thousand years! It is also useful to remember that first great physicists — Aristotle (384 – 322 B.C.) and Archimedes (about 287 – 212 B.C.) are separated from us by more than two thousand years. But the further progress of science was comparatively slow in which religious dogmatism played not the least part. Since the time of Galileo (1564 – 1642) and Kepler (1571 – 1630) the development of physics has been increasingly rapid. But, incidentally, even Kepler was of the opinion that there exists a sphere of motionless stars which 'consists of ice or a crystal'. The fight of Galileo for the acknowledgment of heliocentric concepts, for which he was convicted by the Inquisition in 1633, is generally known. What a path has been overcome since then in only 300 – 400 years! The result is contemporary science. It has already freed itself from religious chains, and the church today does not at least deny the role of science [3]. True, pseudo-scientific tendencies and the propagation of pseudo-science (especially astrology) do go on, in particular, in Russia. But it is only the triumph of totalitarianism (bolshevism-communism or fascism) that can radically obstruct the progress of science as a result of phenomena of which the most striking example was the appearance of Lysenko's 'theories' and their supporters. We shall hope that this will not happen. In any event, one can expect that in the twenty first century the science will develop no slower than it did in the passing twentieth century. The difficulty on this way, and may be even the largest one, is in my opinion associated with the mammoth increase of the accumulated material and the body of information. Physics is now so much extended and differentiated that 'the wood can't be seen for the trees' and it is difficult to catch in the mind's eye a picture of modern physics as a whole. Meanwhile, such a picture does exist, and in spite of all the branches, physics has its pivot. Such a pivot is represented by fundamental concepts and laws formulated in theoretical physics. The contents of the latter are clearly seen from the course by L D Landau, E M Lifshitz, and L P Pitaevskiĭ. The latter author continues the cause

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begun by his predecessors. The updated course has been reissued, although unfortunately rather slowly. The LLP course, as well as other manuals and monographs, constitutes the base underlying the work in all fields of physics and in related areas. However, all these books cannot reflect the most recent advances in science, and reading them one can hardly, if at all, feel the pulse of scientific life. As is known, seminars serve this purpose. I personally have been head of one such seminar in FIAN for over 40 years. It is conducted weekly (on Wednesdays) and lasts two hours. The typical agenda covers news from current literature, and then two, or sometimes one talk is given on various physical and near-physical topics. The 1500th session of the seminar took place on May 24, 1996 in a form close to a skit and was reflected in the journal *Priroda* (Nature) [4]. The 1600th session was held on January 13, 1999. The seminar is customarily attended by on average 100 people — research workers from FIAN and other institutes, as well as a few students from the Moscow Physico-Technical Institute. With a kind of surprise I should note that there are obviously rather few such many-sided seminars. Highly specialized seminars or, especially abroad, so-called colloquia prevail. The latter last an hour and are devoted to a single review report. But at the same time, such journals as *Nature*, *Physics Today*, *Physics World*, *Contemporary Physics* and some others containing a lot of news are wide-spread abroad. Unfortunately, all these journals are now not quite so easily accessible in Russia or appear with some delay. I believe that *Uspekhi Fizicheskikh Nauk* is accessible enough and of great benefit. However, I have long been of the opinion that all this is not enough, and I am advocating a 'project' (which is now a popular word) reflected in the title of the present paper. I mean a compilation of a certain 'list' of problems which currently seem to be the most important and interesting. These problems should primarily be discussed and commented on in special lectures and papers. The formula 'everything about a particular issue and something about everything' is rather attractive, but already unrealistic, for one cannot keep up with everything. At the same time, some subjects, questions and problems are somewhat distinguished for different reasons. The importance of an issue for humanity (to put it in a high-flown manner) may play its role. Such, for example, is the problem of controlled nuclear fusion with the purpose of obtaining energy. Of course, the questions concerning the fundamentals of physics, its forefront (this field has frequently been referred to as elementary particle physics) are also distinguished. Particular attention is undoubtedly attracted by some problems of astronomy which, as in the times of Galileo, Kepler and Newton, are now hard (and needless) to separate from physics. Such a list (of course duly updated) constitutes, I believe, a certain 'physics minimum'. It includes issues of which every physicist should have an idea. Less trivial is perhaps the opinion that it is not at all difficult to attain such a goal and not much time and strength is needed for that. But this requires some effort not only on the part of those who learn, but also on the part of 'senior fellows'. Namely, one should select problems to constitute the 'physics-minimum', compile the corresponding 'list' and comment on it, explaining and filling it with content. This is exactly what I tried to do at the chair of *Problems of physics and astrophysics* of the Moscow Physico-Technical Institute which was set up in 1968. For this purpose, special additional lectures were delivered (they were nearly 70 altogether and they were ended for 'technical reasons'; see Ref. [2], p. 229). For the

same purpose I wrote paper [1] in 1970, which had the same title as the present one. It was updated many times, the last version opening book [2] published in 1995. For the years that have passed since then not very many new results have been reported. Such a shortage can be compensated. Another thing is worse — over the 30 years my presentation has become morally antiquated. It is difficult to formulate this point clearly, but this is the fate of all papers and books of this kind. Incidentally, when I was young, a great role for me was played by O D Khvol'son's book *The Physics of Our Days* (New Concepts of Contemporary Physics in a Generally Accessible Presentation) which appeared in 1932 as the fourth 'revised and updated' edition [5]. As I think now, this book was then already somewhat outdated in regard to the latest news (at that time it was quantum mechanics). And O D Khvol'son (1852 – 1934) was at that time even a little younger that I am now. All in all, even if I now decided to write the necessary (in my opinion) book anew, I would not be able to do it. But as the well-known proverb says, 'let well alone', and in the hope, perhaps illusory, that my project, if not good, then still useful, I am writing the present paper. The 'list of 1999' including the problems which 'seem now to be especially important and interesting' is proposed below. I believe that every physicist should be acquainted with this 'physics-minimum' — to know, even if rather superficially, the outlines of each of the enumerated questions.

It need not be emphasized that singling out 'especially important and interesting' questions is not in the least equivalent to a declaration that a great many other physical problems are unimportant or uninteresting. This is obvious, but a habit of overcautiousness forces me to make a few more remarks. 'Especially important' problems are distinguished not because others are unimportant, but because within the period under discussion they are the focus of attention and go to some extent in line with the main directions. Tomorrow these problems may find themselves in the rear and other problems will come in their place. Singling out some problems as 'especially important' is of course subjective and different opinions are needed. But I would like to resolutely reject the reproach that such a distinction is dictated by some personal scientific preferences and personal activity in physics. So, in my scientific activity the questions associated with the radiation of uniformly moving sources [6] were and are most dear to me, but I did not and do not include them in the 'list'. Unfortunately I had to face disapproval of the 'list' for the reason that it had not included a subject which was interesting for the critic. I recall in this connection how my senior friend A L Mints (1895 – 1974) told me after the appearance of the paper [1]: "If you had written this paper before you were elected academician, you would have never been elected". He may have been right, but I still believe in the wider outlook of my colleagues.

2. The list of 'especially important and interesting problems' of 1999

There is a well-known saying that the proof of the pudding is in the eating. This is why I immediately proceed to the 'list'.

1. Controlled nuclear fusion.
2. High-temperature and room-temperature superconductivity.
3. Metallic hydrogen. Other exotic substances.
4. Two-dimensional electron liquid (anomalous Hall effect and some other effects).

5. Some questions of solid-state physics (heterostructures in semiconductors, metal-dielectric transitions, charge and spin density waves, mesoscopics).

6. Second-order and related phase transitions. Some examples of such transitions. Cooling (in particular, laser cooling) to superlow temperatures. Bose – Einstein condensation in gases.

7. Surface physics. Clusters.

8. Liquid crystals. Ferroelectrics.

9. Fullerenes. Nanotubes.

10. The behavior of matter in superstrong magnetic fields.

11. Nonlinear physics. Turbulence. Solitons. Chaos. Strange attractors.

12. Rasers, grasers, superhigh-power lasers.

13. Superheavy elements. Exotic nuclei.

14. Mass spectrum. Quarks and gluons. Quantum chromodynamics. Quark-gluon plasma.

15. Unified theory of weak and electromagnetic interactions. $W^{\pm}$- and Z^0-bosons. Leptons.

16. Standard model. Grand unification. Superunification. Proton decay. Neutrino mass. Magnetic monopoles.

17. Fundamental length. Particle interaction at high and superhigh energies. Colliders.

18. Nonconservation of CP-invariance.

19. Nonlinear phenomena in vacuum and in superstrong magnetic fields. Phase transitions in vacuum.

20. Strings. M-theory.

21. Experimental verification of the general theory of relativity.

22. Gravitational waves and their detection.

23. The cosmological problem. Inflation. Λ-term. Relationship between cosmology and high-energy physics.

24. Neutron stars and pulsars. Supernova stars.

25. Black holes. Cosmic strings (?).

26. Quasars and galactic nuclei. Formation of galaxies.

27. The problem of dark matter (hidden mass) and its detection.

28. The origin of superhigh-energy cosmic rays.

29. Gamma-bursts. Hypernovae.

30. Neutrino physics and astronomy. Neutrino oscillations.

The singling out of 30 particular problems (more precisely, items in the 'list') is of course absolutely conditional. Moreover, some of them might be divided. In Ref. [1] there were 17 problems, in Ref. [2] they were already 23. In note [7] 24 problems were listed. In the letters that came to *Physics Today* in respect of this note, the opinion [8] was expressed that the list should also have included star formation, atomic and molecular physics (true, I am unaware of what exactly was meant), and the question of exceedingly accurate measurements. I had to get acquainted with other suggestions that the list should be extended. Some of them have been taken into consideration, but others (for example, those concerning quantum computers, the 'optics' of atomic beams, semiconductor devices, etc.) I had to ignore.

Any 'list' is undoubtedly not a dogma, something can be discarded and something added depending on the preferences of lecturers and authors of corresponding papers. More interesting is the question of the evolution of the list with time as it reflects the process of the development of physics. In the 'list' of 1970 – 1971 [1] quarks were given only three lines in the enumeration of the attempts to explain the mass spectrum. This did not testify to my perspicacity, which was admitted in Ref. [2]. However, at that time (in 1970) quarks were only five or six years old (I mean the age of the corresponding hypothesis), and the fate of the concept of the quark was indeed vague. Now the situation is of course quite different. True, the heaviest t-quark was discovered only in 1994 (its mass, according to the data of 1999, is $m_t = 176 \pm 6$ GeV). The list [1] naturally contains no fullerenes which were discovered in 1985 [9], no gamma-bursts (the first report of their discovery was published in 1973; see Ref. [2] and below). High-temperature superconductors were synthesized in 1986 – 1987, but in the list [1] this problem was nonetheless considered rather thoroughly for it had been discussed since 1964. Generally, not little has been done in physics for the past 30 years, but, I believe, not very much essentially new has appeared. In any case, the 'lists' in Refs [1, 2], as well as that presented above, characterize to a certain extent the development and the state of physical and astronomical problems from 1970 – 1971 to the present day.

3. Some comments (macrophysics)

In Ref. [2], the paper with the same title occupies 155 pages. There, each problem of the 'list' is commented on. I cannot do the same here, and therefore I shall restrict myself to separate, sometimes fragmentary remarks and comments [1]. The basic goal is to elucidate the development of physics over the last four or five years, that is, after the book [2] was published.

The problem of controlled thermonuclear fusion (number 1 in the list) has not yet been solved, although it is already 50 years old. I remember how the work in this direction was started in 1950. A D Sakharov and I E Tamm told me about the idea of a magnetic thermonuclear reactor, and I was glad to set myself to the solution of this problem because at that time I had almost nothing to do in the elaboration of hydrogen bomb (I wrote about all this in the collected papers [10], paper 9, p. 205). This work was then considered to be supersecret (it was stamped as 'very secretly, special file'). Incidentally I thought at that time and much later that the interest in the thermonuclear problem in the USSR was due to the desire to create an inexhaustible energy source. However, as I have been told by I N Golovin, a thermonuclear reactor was then interesting for 'those who needed it' — largely for quite a different reason — as a source of neutrons (n) for the production of tritium (t) (apparently, with the help of the reaction $^6\mathrm{Li} + \mathrm{n} \rightarrow \mathrm{t} + {}^4\mathrm{He} + 4.6$ MeV). In any event, the project was treated as so secret and important that I was debarred from participation in it (it was either in late 1951 or early 1952) — in the secret department they simply stopped giving me the working notebooks and my own reports on this work. That was the apex of my 'specialized activity'. Fortunately, by the times of Khrushchev, I V Kurchatov and his colleagues had realized that the thermonuclear problem could not be quickly solved, and in 1956 it was declassified and opened to the public. As a reaction to what I had experienced, I published my thermonuclear reports [11] in 1962, although I do not at all claim that I have done anything significant in this field.

Abroad, thermonuclear studies also began (approximately at the same time) mostly as secret, and their

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[1] A large number of references to the literature could be given in connection with practically each item. But this seems to be out of place here. Moreover, the problem of priority would arise, and I would not like to touch upon it here. I have tried to make as few references as possible. Sometimes they are of an incidental character, and preference was naturally given to papers published in *Usp. Fiz. Nauk* and *Physics Today*.

declassification in the USSR (which was quite non-trivial for our country at that time) played a great positive role — the solution of the problem became the subject of international conferences and collaboration. But 45 years have passed and no operating (energy-producing) thermonuclear reactor has been accomplished, and we shall probably have to wait for another ten years or longer (see Ref. [2], Sec. 1; the latest review on this subject which is known to me and easy to access is paper [12]; for reference to the Soviet papers see Ref. [13]). Work on thermonuclear fusion is being carried out all over the world on a fairly wide front. An especially advanced system — a favorite — is the tokamak. The ITER (International Thermonuclear Experimental Reactor) project has been elaborated for several years. This is a gigantic tokamak which will cost nearly 10 billion dollars. It was supposed to be accomplished by 2005 as a real prototype of the thermonuclear reactor of the future generation. But now that the project is mostly complete, financial difficulties have arisen. Moreover, some physicists find it reasonable first to think over alternative smaller-scale constructions (see Ref. [12] and e.g., [14]). This question is being discussed on the pages of *Physics Today* and other journals, but it does not seem pertinent to dwell on it in the present paper. Generally, the possibility of creating a real thermonuclear reactor is now beyond doubt, and the center of gravity of the problem, as I see it, has shifted towards the engineering and economical spheres. However, such a titanic and unique installation as ITER or competing ones remain, of course, interesting for physics.

As for alternative ways of fusing light nuclei for obtaining energy, the hopes for the possibility of 'cold thermonuclear fusion' (e.g., in electrolytic elements) were abandoned [133]; muon catalysis is very elegant (and should, I think, be elucidated in a course of general physics), but seems to be an unrealistic energy source, at least when not combined with uranium fission, etc. There also exist projects with a sophisticated use of accelerators, but I am unaware of any success in this field. Finally inertial nuclear fusion is possible, and specifically 'laser thermonuclear fission'. Gigantic corresponding installations are being constructed, but they are not widely known because of secrecy — they are obviously intended for imitation of thermonuclear explosions. However, I may simply be ignorant of the situation. In any case, the problem of inertial fusion is important and interesting.

The problem of controlled nuclear fusion is now technical rather than physical. In any case, there is no enigma here typical of a number of unsolved physical problems. That is why there exists an opinion that the problem of nuclear fusion may be excluded from our 'list'. This is however an exceedingly important and still unsolved problem, and therefore I would discard it from the list only after the first effective thermonuclear reactors starts operating.

We now proceed to high-temperature and room-temperature superconductivity (abbreviated as HTSC and RTSC, problem 2). To those who are not closely engaged in solid state physics it may seem that it is time to discard the HTSC problem from the list. In 1970 [1] high-temperature superconductors had not yet been created and to obtain them was a dream which was then mocked at here and there. But in 1986–1987 such materials were created, and even though they are included in Ref. [2] by inertia, maybe it is time to place them among the numerous other substances investigated by physicists and chemists? But this is not the case. Suffice it to say that the mechanism of superconductivity in

cuprates (the highest temperature $T_c = 135$ K was reached for $HgBa_2Ca_2Cu_3O_{8+x}$ without pressure, while under a rather high pressure we already have $T_c \approx 164$ K for this cuprate) remains unclear [15–17]. It seems undoubted, to me personally in any case, that a very significant role is played by the electron–phonon interaction with strong coupling, but this is not enough. 'Something else' is needed, perhaps an exciton or spin interaction. In any case the question is open in spite of the great efforts made to investigate HTSC (about 50 000 publications on the subject have appeared in the ten years). But the main question, which is of course intimately related to the preceding one, is the possibility of creating RTSCs. Such a possibility does not face any contradiction [15], but success is not guaranteed. The situation is here quite similar to that observed before 1986–1987 in HTSC.

In the list of Ref. [2], Sec. 2 we also find the problem of superdiamagnetism, i.e., the possibility of creating an equilibrium non-superconducting diamagnetic with magnetic susceptibility χ close to $\chi = -1/4\pi$ (it is a well-known fact that for superconductors one can formally assume $\chi = -1/4\pi$). From experiment we know that there exist diamagnetics with $\chi = -(10^{-4} - 10^{-6})$. Materials with $\chi = -(0.1/4\pi - 0.01/4\pi)$ can be called superdiamagnetics. I do not know why they might not exist, but I cannot say anything sensible in this respect.

Metallic hydrogen (problem 3) has not yet been obtained even under a pressure of 3 million atmospheres (at low temperatures). However, the study of molecular hydrogen at high pressure has revealed a whole number of unexpected and interesting features of this substance [18]. Moreover, under compression by shock waves at a temperature of 3000 K, the transition to a metallic (i.e., well conducting) liquid phase was clearly observed.

Water (more precisely, H_2O) and a number of other substances also exhibited some peculiarities at a high pressure [18]. In addition to metallic hydrogen, fullerenes may also be attributed to 'exotic' substances. Quite recently, along with the common fullerene C_{60} the study of fullerene C_{36} began; this substance may have a very high superconducting transition temperature under doping [19]. Examples of exotic substances are numerous.

In 1998, the Nobel prize in physics was given for the discovery and explanation of the fractional quantum Hall effect. Incidentally, the discovery of the integer quantum Hall effect also won the Nobel prize (1985). I mention here and below the Nobel prizes not because of some extraordinary respect for them (sometimes one can observe an excessive respect for these prizes). As any deed of the human, awards should not be raised to the rank of the absolute. Even the best of the awards are in most cases somewhat conditional, and sometimes errors occur (see, for example, Refs [20, 21]). But on the whole the Nobel prizes in physics have gained immense authority and are the landmarks fixing the progress in physics.

The fractional quantum Hall effect was discovered in 1982 (the discovery of the integer quantum Hall effect goes back to 1980). The quantum Hall effect is observed when a current runs in a two-dimensional electron 'gas' (in fact, certainly in a liquid because the interaction between the electrons is substantial, particularly for the fractional effect). The 'system' (a two-dimensional conducting layer on a silicon surface) is, of course, in the magnetic field perpendicular to this current, as under the usual Hall effect. I shall restrict myself here to references [22, 23] and the remark that the

unexpected and particularly interesting feature of the fractional quantum Hall effect is the existence of quasi-particles with a fractional charge $e^* = (1/3)e$ (e is the electron charge) and other fractional charges. It should be noted that a two-dimensional electron gas (or, generally, a liquid) is interesting not only in respect of the Hall effect, but also in other cases and conditions [24, 25].

Problem 5 (some problems of solid state physics) is currently absolutely boundless. In the 'list', I only sketched (in brackets) some possible topics, and if I had to deliver a lecture, I would dwell on heterostructures (including 'quantum dots') and mesoscopics just because I am acquainted with these questions better than with some other ones from this area. I shall only mention the whole *Usp. Fiz. Nauk* issue [24] devoted to this subject and refer to the most recently noticed paper on the metal-dielectric transition [26]. It is not at all easy to choose what is most interesting, so the reader and the student should be helped in this respect.

As to problem 6 (phase transitions, etc.), I would like to add to [2], Sec. 5 the following. The discovery of low-temperature superfluid ^{3}He phases won the 1996 Nobel prize in physics [27]. Particular prominence for the past three years has been given to Bose – Einstein condensation (BEC) of gases. These works are undoubtedly of great interest, but I am sure that the 'boom' around them was largely due to the lack of historical knowledge. It was as far back as 1925 that Einstein paid attention to BEC [28], and now this question is naturally included in text-books (see, for example, Ref. [29], Sec. 62). Then, true, BEC had long been ignored and sometimes even called in question. But those are bygone times, especially after 1938 when F. London associated BEC with superfluidity of ^{4}He [30]. Helium II is of course a liquid, and BEC does not manifest itself, so to say, in a pure form. The desire to observe BEC in a rarefied gas is quite understandable and justified, but one should not think of it as a discovery of something unexpected and essentially new in physics (see a similar remark in Ref. [31]). The observation of BEC in gases, such as Rb, Na, Li, and finally H, which was made in 1995 and later on, was on the contrary a great achievement of experimental physics. It only became possible owing to the development of methods of cooling gases to superlow temperatures and keeping them in traps (which, by the way, won the 1997 Nobel prize in physics [32]). The realization of BEC in gases initiated a stream of theoretical papers (see reviews [33, 34]; new articles permanently appear, in particular, in *Physical Review Letters* [2]). In Bose – Einstein condensate, atoms are in a coherent state and interference phenomena can be found, which has led to the appearance of the concept of an 'atomic laser' (see, for example, Refs [35, 36]). BEC in a two-dimensional gas [127] is also very interesting.

Problems 7 and 8 touch upon numerous questions which I have not followed and cannot therefore distinguish anything new and important. I only wish to point out the acute and justified interest in clusters of various atoms and molecules (i.e., formations containing a small number of particles [134]). The studies on liquid crystals and simultaneous ferroelectrics should also be mentioned. I shall only

refer to the latest work [37] of those known to me on this subject. The study of thin ferroelectric films [38] is also attractive.

Fullerenes (problem 9) have already been casually mentioned above (see also Refs [9, 19]), and along with carbon nanotubes [39] this branch of studies is flourishing.

I have not heard anything new either of matter in superstrong magnetic fields (specifically, in the crust of a neutron star) or of the simulation of the corresponding effects in semiconductors (problem 10). Such a remark should not discourage or cause the question of why these problems were introduced into the list. First, in Ref. [2], Sec. 8 I tried to elucidate the physical meaning of this problem and to explain why it has, in my opinion, such a charm for a physicist; there are neither particular grounds nor especially spare room to repeat myself here. Second, the understanding of the importance of a problem is not necessarily related to a sufficient acquaintance with its current state. My whole 'program' is aimed at stimulating interest and prompting specialists to elucidate the state of a problem to non-specialists in accessible papers and lectures.

As far as nonlinear physics (problem 11 in the list) is concerned, the situation is not as in the previous case. There is a lot of material, *Physical Review Letters* publish papers on this subject in every issue, they even have a special section partly devoted to nonlinear dynamics. Moreover, nonlinear physics and, in particular, the problems listed in item 11 are also presented in other sections of the journal; in total, up to $10 - 20\%$ of the whole journal is devoted to nonlinear physics (see, e.g., Ref. [40]). Generally, it should perhaps be emphasized once again, in addition to Ref. [2], Sec. 10, that attention to nonlinear physics is increasingly high. This is largely connected with the fact that the use of modern computer facilities allows the analysis of problems whose investigation was earlier no more than a dream.

It is not for nothing that the twentieth century was sometimes called not only the atomic age, but the laser age as well. The perfection of lasers and the extension of their application are in full swing. But problem 12 concerns not lasers in general, but first of all superpower lasers. So, an intensity (power density) $I \sim (10^{20} - 10^{21})$ W cm^{-2} has already been attained. With such an intensity the electric field strength is of the order of 10^{12} V cm^{-1}, i.e., this field is two orders of magnitude stronger than the proton field at the ground level of the hydrogen atom. The magnetic field reaches $10^9 - 10^{10}$ Oe [41] and very short pulses of duration up to 10^{-15} s (i.e., a femtosecond) are used. Employment of such pulses opens a lot of possibilities, in particular, for obtaining harmonics lying already in the X-ray band and, accordingly, X-ray pulses with a duration of attoseconds ($1 \text{ a} = 10^{-18}$ s) [41, 42]. A related problem is the creation and use of rasers and grasers which are analogs of lasers in respectively X-ray and gamma-ray bands. Unfortunately, I do not know of any achievements in this field (other than those mentioned in Ref. [2], Sec. 9).

Problem 13 is that of nuclear physics. This is, of course, a vast area which is not very familiar to me. For this reason I distinguished only two points. First, I point out far transuranium elements in connection with the hopes that some isotopes have long lives owing to shell effects (as an example of such an isotope, the nucleus with $Z = 114$ and the number of neutrons $N = 184$, i.e., mass number $A = Z + N = 298$ was pointed out in the literature). The known transuranium elements with $Z < 114$ live only seconds or fractions of a

second. The indications of the existence in cosmic rays of long-lived (millions of years) transuranium nuclei, which appeared in the literature (see Ref. [2], Sec. 11), have not yet been confirmed. At the beginning of 1999 a preliminary (not yet verified) report [124] appeared on the fact that the 114th element with mass number 289 and a lifetime of nearly 30 s had been synthesized in Dubna. Therefore, there are hopes that the element $\binom{114}{298}$ will actually prove to be very long-lived. Second, I mentioned 'exotic' nuclei. These are nuclei of nucleons and antinucleons, some hypothetical nuclei with a heightened density, to say nothing of nuclei having a non-spherical shape and some other specific features. Included here are the problems of quark matter and quark-gluon plasma (see. e.g., Refs [43, 135 – 137] and the references therein).

4. Some comments (microphysics)

Problems 14 to 20 pertain to the field which I refer to as microphysics although it would apparently be more correct to call it elementary particle physics. This name was once seldom used considered outdated. The reason was, in particular, that nucleons and mesons at a certain stage were considered to be elementary particles. Now they are known to consist (true, in a conditional sense) of quarks and antiquarks. Quarks, too, were sometimes supposed to consist of other tiny particles (preons, etc.). However, such hypotheses are totally ungrounded today, and the 'matryoshka' [Russian doll] — the division of matter into successively smaller parts, must one day be exhausted. In any event, we think today that quarks are indivisible and in this sense — elementary. Without anti-quarks they include six flavors: u (up), d (down), c (charm), s (strange), t (top), and b (bottom or beauty); antiquarks are denoted by the same letters but with a bar ($\bar{u}$, etc.). Next, leptons are also elementary: the electron and the positron (e^- and e^+), $\mu^\pm$, $\tau^\pm$ and the corresponding neutrinos ν_e, ν_μ, ν_τ. Finally, the four vector bosons (the photon γ, the gluon g, Z^0, and $W^\pm$) are elementary. I shall not give here a more detailed account of the state of elementary particle physics as a whole because I may refer, besides [2], to the review by L B Okun' "The Present State of Elementary Particle Physics" published in *Usp. Fiz. Nauk* in 1998 [44]. All written there I attribute to the 'physics minimum'. I shall however make some comments and add some points.

One of the most topical problems (in Ref. [44] it is even called problem number 1) of elementary particle physics is the search for and, as everybody hopes, the discovery of higgs — the scalar Higgs-boson with spin zero. According to the estimates, the higgs mass is below 1000 GeV or rather even below 200 GeV. The higgs is now being sought and will be sought on the available accelerators and those being reconstructed (in CERN and Fermilab). The main hope of high-energy physics (may be also in the search for higgs) is the LHC accelerator (Large Hadron Collider) which is now being built in CERN. An energy of 14 TeV (in the center-of-mass of colliding nucleons) will be reached, but obviously not before 2005. Another very important problem (number 2, according to Ref. [44]) is the search for supersymmetric particles (see below). I cannot but point out the problem of CP non-conservation and, by virtue of CPT-invariance (spatial inversion P, charge conjugation C and time reversal *T*), non-conservation of T-invariance (noninvariance under the time reversal $t \to -t$). This is of course a fundamental question, in particular, from the point of view of the explanation of

irreversibility of physical processes (see Section 6). CP-nonconservation was discovered in 1964 on an example of meson decay $K_2^0 \to \pi^+ + \pi^-$. Incidentally, this discovery won the 1980 Nobel prize in physics. At the same time, the known processes with CP-nonconservation have a small probability (compared to processes that conserve CP-invariance). The processes with CP-nonconservation are under study; their nature is not yet clear. One more process with CP-nonconservation has recently been investigated [45]. Finally, CP-nonconservation is being sought in B-meson decay [46]. Proton decay has not yet been found. According to recent data [125], the mean proton lifetime when determined from the reaction $p \to e^+ + \pi^0$ is longer than 1.6×10^{33} years. The neutrino mass which is mentioned among the other items of problem 16 will be touched upon below in the discussion of problem 30 (neutrino physics and astronomy).

I shall dwell here on problem 17 or, more concretely, on the fundamental length. 'Elementarists', as those specialized in elementary particle physics are sometimes called, will perhaps scornfully shrug their shoulders wondering what problem is this. If I began compiling the 'list' today, I would probably not mention such a problem because it was many years ago that it 'rang at the top of its voice' and was pointed out in Ref. [1] and then also in Ref. [2]. It was only at the end of the 1940s that the technique (the renormalization method, etc.; see, e.g., Ref. [47]) was developed allowing an unlimited use of quantum electrodynamics. Before this, calculations had sometimes yielded divergent expressions, and to obtain final results one had to make a cutoff at a certain maximum energy E_{f_0} or at a corresponding length $l_{f_0} = \hbar c / E_{f_0}$ (here $\hbar = 1.055 \times 10^{-27}$ erg s is the quantum constant). The most frequently encountered values were $l_{f_0} \sim 10^{-17}$ cm and $E_{f_0} = \hbar c / l_{f_0} \sim 3$ erg $\sim 10^{12}$ eV = 1 TeV. Approximately the same values correspond to the highest energies (in the center-of-mass frame) and the lowest 'impact parameters' reached on modern accelerators. Given this, 'everything is all right', i.e., the conventional physics, for example, quantum electrodynamics, works well. This implies that up to distances $l_{f_0} \sim 10^{-17}$ cm (true, the length 10^{-16} cm is more often mentioned) and times $t_{f_0} \sim l_{f_0}/c \sim 10^{-27}$ s the existing space–time concepts are valid. And what is going on at smaller scales? Such a question, along with the difficulties encountered in the theory, led to the hypothesis of the existence of a fundamental length l_f and time $t_f \sim l_f/c$ for which a 'new physics' makes its appearance, in particular, some unusual space-time concepts ('granular space–time' and other things). There are no grounds now to introduce the length $l_f \sim 10^{-17}$ cm. On the other hand, another fundamental length, namely, the Planck or gravitational length $l_g = \sqrt{G\hbar/c^3} = 1.6 \times 10^{-33}$ cm (here $G = 6.67 \times 10^{-8}$ cm (g s^2)$^{-1}$ is the gravitational constant) is known and plays an important role in physics; this length corresponds to the time $t_g = l_g/c \sim 10^{-43}$ s and energy $E_g = \hbar c / l_g \sim 10^{19}$ GeV. The Planck mass $m_g = E_g/c^2 \sim \sqrt{\hbar c/G} \sim 10^{-5}$ g is also frequently used. The physical meaning of the length l_g is that on smaller scales one cannot already apply the classical relativistic theory of gravity and, in particular, the general theory of relativity (GR) whose construction was accomplished by Einstein in 1915 [3]. The point is that for $l \sim l_g$ and

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[3] In GR, a gravitational field is completely described by the metric tensor g_{ik}. Furthermore, g_{ik} obey quite definite equations (see, e.g., Ref. [48]). There exist a lot of other classical relativistic theories of gravity in which besides g_{ik} other variables also appear (e.g., a certain scalar field φ), higher-order derivatives, etc.

especially on scales $l < l_g$ quantum fluctuations of the metric g_{ik} are already large. Hence, the quantum theory of gravity should be used here which has not yet been created in a somewhat completed form. So, the length l_g is of course a certain fundamental length which limits the classical concepts of space-time. But can one be sure that these classical concepts do not stop 'working' before that, at a fundamental length $l_f > l_g$? As has already been said, we definitely have $l_f < l_{f_0} \sim 10^{-17}$ cm, but this value of l_{f_0} is 16 orders of magnitude larger than l_g. Physicists have got used to giant-scale extrapolations, for instance, to the assumption that laws obtained on the Earth from various data are identical throughout the whole Universe or at any rate in colossal space-time regions. An example of such a far-reaching extrapolation is the hypothesis that over the entire interval between $l \sim l_{f_0} \sim 10^{-17}$ cm and $l \sim l_g \sim 10^{-33}$ cm no other fundamental length l_f exists. Such a hypothesis now seems natural, but it has not been proved. The latter should be borne in mind, and for this reason I included this problem in the list. As a matter of fact, however, the length is attacked on two sides. On the side of comparatively low energies — this is the construction of new accelerators (colliders), primarily the already mentioned LHC (see Refs [44, 49] and Chapters 11 and 12 in Ref. [50]). This collider, as mentioned above, is going to reach an energy $E_c = 14$ TeV (in the center-of-mass frame) which corresponds to the length $l = \hbar c/E_c = 1.4 \times 10^{-18}$ cm. In cosmic rays, particles with a maximum energy $E = 3 \times 10^{20}$ eV have been registered in the laboratory frame (a proton with such an energy, when colliding with a nucleon at rest in the center-of mass frame, has an energy $E_c \sim 800$ TeV and $l_c \sim 10^{-20}$ cm). Such particles are however very few, and it is impossible to use them directly in high-energy physics [51, 52]. Lengths comparable with l_g arise only in cosmology (and, in principle, within the horizon of black hole events). Energies frequently encountered in elementary particle physics are $E_0 \sim 10^{16}$ GeV. They figure in the yet incompleted theory of 'grand unification' — the unification of electroweak and strong interactions. The corresponding length is equal to $l_0 = \hbar c/E_0 \sim 10^{-30}$ cm and is still three orders of magnitude larger than l_g. It is obviously very difficult to say what is going on at scales between l_0 and l_g. It may be here that a certain fundamental length l_f such that $l_g < l_f < l_0$ is hidden. Today such an assumption is pure speculation.

As to the terminology, the theory of strong interaction is called quantum chromodynamics. As has already been said, the scheme uniting the electromagnetic, weak and strong interactions is referred to as 'grand unification'. At the same time, the currently used theory of elementary particles which consists of the theory of electroweak interaction and quantum chromodynamics is called the standard model. Finally, the theories with grand unification (which is not yet ultimately shaped) generalized so as to include gravity are called superunification. No satisfactory superunification has yet been constructed. The superstring theory discussed below claims the role of superunification, but the goal has not yet been achieved.

As regards the set of problems 19, one may assert that they are fairly topical, but I do not know what is to be added to the material of Ref. [2], Sec. 17. I may have missed some news worthy of note (I shall only point to paper [53] devoted to phase transitions in the early Universe). Incidentally, in Ref. [2], Sec. 7 I quoted the remark made by Einstein as far back as 1920 [54]: "...the general theory of relativity endows

space with physical properties, and so ether does exist in this sense...". Quantum theory also 'endowed space' with virtual pairs of various fermions and zero oscillations of electromagnetic and other Bose-fields. This seems to be known to everyone. Nevertheless, *Physics Today* — the organ of the American Physics Society and of another nine analogous societies was opened in 1999 by the article "The persistence of ether" devoted to speculations concerning the physical vacuum named ether [55].

Before proceeding to the problems of astrophysical nature and those close to them (items 21 – 30 in the list), I shall dwell on problem 20: strings and the M-theory. This is so to say the leading direction in theoretical physics today. Incidentally, the term 'superstrings' is frequently employed instead of the term 'strings', first, not to confuse them with cosmic strings (see below about problem 25) and, second, to emphasize the use of the concept of supersymmetry. In the supersymmetric theory, each particle corresponds (in the equation) to its partner with other statistics, for example, a photon (a boson with spin unity) corresponds to a photino (a fermion with spin 1/2), etc. It should be noted at once that supersymmetric partners (particles) have not yet been discovered. Their mass is evidently not less than 100 – 1000 GeV. The search for these particles is one of the principal problems of experimental high energy physics both on the existing accelerators and those under reconstruction and on LHC.

Theoretical physics cannot yet answer a whole number of questions, for example, how the quantum theory of gravity should be constructed and united with the theory of other interactions, why there exist apparently only six types (flavors) of quarks and six leptons, why the electron neutrino mass is very small, why μ- and τ-leptons differ in their mass from the electron precisely by the factor known from experiment; how the fine structure constant $\alpha = e^2/\hbar c \approx 1/137$ and a number of other constants can be determined from the theory, and so on. In other words, grandiose and impressive as the achievements of physics are, there remain more than enough unsolved fundamental problems. The string theory has not yet answered such questions, but it promises success in the desired direction. Since I cannot refer to a sufficiently accessible paper on strings in the Russian language, I planned to clarify some essential points. It turned out however that I cannot do that briefly and at a proper level. I would merely retell the popular reviews [56 – 59] and [50], Chapter 13. So, I shall only make some remarks.

In quantum mechanics and quantum field theory, elementary particles are considered to be point particles. In string theory, elementary particles are oscillations of one-dimensional objects (strings) with characteristic dimensions $l_s \sim l_g \sim 10^{-33}$ cm (or, say, $l_s \sim 100 l_g$). Strings may have a finite length (a 'segment') or may be ring-like. Strings are considered not in the normal four-dimensional space, but in multi-dimensional spaces with, say, ten or eleven dimensions. The theory is supersymmetric. The change of point particles for non-point ones is not at all a new idea and its main difficulty is the relativistic formulation. As an example I dare refer to the paper by I E Tamm and me [60] (see also Ref. [61]). No progress had been made in this direction before the string theory. The idea of multi-dimensional spaces, that is, the introduction of the fifth and higher dimensions is still older (the Kaluza – Klein theory [62, 63]; see Ref. [64], p. 296), but before the string theory it had not led to any physical results either. In the string theory, however, one can also speak mainly of 'physics-hopes', as L D Landau would say, rather

than results. But what do we mean by results? The mathematical constructions and the discovery of various symmetry properties are also results. As concerns physics, the string theory has not yet given answers to any of the questions listed above. This did not prevent the physicists engaged in the study of strings from speaking already not only about the 'first superstring revolution' (1984–1985), but also about the 'second superstring revolution' (1994-?) [57][4]. A not very modest terminology has been applied to the string theory — it was called 'The Theory of Everything'. It should be noted that the string theory is not too young; according to [50], Chapter 13 it is already 30 years old and 15 years have passed since the 'first superstring revolution', but no physically clear results have been obtained. In this connection it is worth noting that the true revolution in physics — the creation of quantum mechanics, for the most part by de Broglie, Schrödinger, Heisenberg, Dirac, and Bohr, did not last longer than 5–6 years (1924–1930). It took Einstein eight years (1907–1915) to create the general theory of relativity. But I do not set a great deal on these comments. The problems and questions of theoretical physics discussed here are deep and exceedingly involved, and nobody knows how much time it will take to answer them. The theory of superstrings seems to be something deep and developing. Its authors themselves only claim the comprehension of some limiting cases and only speak of some hints of a certain more general theory which is called the M-theory. The letter M is chosen because this future theory is called magic or mysterious [56]. The superstring theory would noticeably fortify its position if supersymmetric particles were discovered, although there exist other ways of verification [59].

5. Some comments (astrophysics)

Problems 21–30 in our 'list' belong to astrophysics, but in some cases it is rather conditional. This particularly and even largely concerns the question of experimental verification of GR — the general theory of relativity (problem 21). It would be more logical to discuss the possibility of the analysis of relativistic effects in gravity (see, e.g., Ref. [66]). However, in view of the actually existing situation and the history of the corresponding studies, it would be more correct to bear in mind just the verification of GR — the simplest relativistic theory of gravity [5]. The effects of GR in the solar system are rather weak (the strongest effects are of the order of $|\varphi|/c^2$, where φ is the Newton gravitational potential; even on the Sun's surface $|\varphi|/c^2 = GM_\odot/(r_\odot c^2) = 2.12 \times 10^{-6}$). It is for this reason that the verification which was successfully started in 1919 and has lasted till the present day has not led to accuracies which have become customary in nuclear physics.

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[4] In the book *The Structure of Scientific Revolutions* by Kuhn [65], which is widely known and popular in the West, the author writes: "For me, a revolution is the form of a change including a certain type of reconstruction of the axioms by which the group is guided. But it need not necessarily be a large change or seem revolutionary to those who are outside a separate (closed) community consisting of not more than 25 persons" ([65] p. 227). If we adopt such a definition of revolution (I have already had an opportunity to express my opinion of it; see Ref. [2] p. 159), then in the majority of fields of physics revolutions break out every few years.

[5] The theory in which the gravitational field is described by a certain scalar rather than the metric tensor g_{ik} as in GR is logically the simplest relativistic theory of gravity. But the scalar theory certainly contradicts experiment (for example, light beams are not at all deflected by the Sun in this theory).

According to the recent data reported at the 19th Texas "Relativistic Astrophysics and Cosmology" Symposium (December 1998), for the deflection of radio waves by the Sun the ratio of the observed quantity to the calculated one is 0.99997 ± 0.00016 according to GR. The same ratio for the rotation of Mercury perihelion is equal to 1.000 ± 0.001. So, GR has been checked in a weak gravitational field for $|\varphi|/c^2 \ll 1$ with an error up to a hundredth of a percent and no deviations from GR were found. A further verification even in a weak field (for example, involving the terms φ^2/c^4) seems to be quite meaningful, although not stimulating because it is hardly probable to observe any deviations from GR and the experiments are very involved. Nevertheless, a whole number of projects exist and will evidently be realized. The verification of the equivalence principle is a special question; the validity of this principle was confirmed up to 10^{-12}, but this is not a new result [66].

Within the discussion of light deflection in the field of the Sun, some comments of a historical nature would not be uninteresting. Generally speaking, I do not think that priority questions should take a distinguished place in the lectures and articles whose program is presented here. The point is that such questions are often rather intricate and are decided in the literature in quite an accidental manner. Some statements are adapted by repetition only. And to undertake a historical examination in each such case is a troublesome affair and draws attention from the physical essence of the matter. At the same time, some historical excursus provide insight into a problem and, of course, pay tribute to the pioneers. The deflection of light beams in a gravitational field is a good example of this. A hint of such an effect was given already by Newton. In the framework of the corpuscular theory of light and in the assumption of equality or even proportionality of a heavy and inert mass, the existence of the deflection is obvious. The deflection of a light ray in the field of the Sun was calculated by Soldner as far back as 1801. The deflection angle turned out to be equal to

$$\alpha' = \frac{2GM_\odot}{c^2 R} = \frac{r_{g\odot}}{R} , \tag{1}$$

where R is the impact parameter (the shortest distance between the beam and the center of the Sun) and $r_g = 2GM/c^2$ is the gravitational radius ($r_{g\odot} = 3 \times 10^5$ cm because the Sun mass is $M_\odot = 2 \times 10^{33}$ g).

Obviously not knowing about this result Einstein, in his first publication on the way to creating GR (1907), pointed out the deflection of rays and in 1911 he obtained expression (1) on the basis of the then incompleted GR which allowed only for the variation of the component $g_{00} = 1 + 2\varphi/c^2$. After the creation of GR in 1915, the final result was obtained in the same year:

$$\alpha = \frac{4GM_\odot}{c^2 R} = \frac{2r_{g\odot}}{R} = 1''.725 \, \frac{r_\odot}{R} , \tag{2}$$

where $r_\odot = 7 \times 10^{10}$ cm is the Sun's photosphere radius. The distinction between (2) and (1) is due to the account of the fact that the components of the metric tensor $g_{11} = g_{22} = -(1 - 2\varphi/c^2)$ are important, too. Expressions (1) and (2) differ exactly by a factor of two, but the classical calculation is inconsistent (we mean the application of classical mechanics to a corpuscle moving at the velocity of light), and therefore ratio 2 is accidental. The deflection of a light ray in the field of the Sun was first observed in 1919 and it confirmed the GR

expression (2) though not with a high accuracy. The further specifications have been discussed above (references are not given here, they can be found in Refs [66, 67]).

In astrophysics, the deflection of rays in a gravitational field is used more and more frequently in the observation of 'lensing', that is, focusing of electromagnetic waves under the action of a gravitational field in application to galaxies (they lens light and radio waves emitted by quasars and other galaxies) and stars (microlensing of more remote stars) [67]. This, of course, is not a verification of GR (the accuracy of measurements is rather low), but a use of it. I note that the lensing effect with its characteristic features was to the best of my knowledge first considered by Khvol'son in 1924 [68] and Einstein in 1936 [69]. The characteristic cone arising upon lensing is called the Einstein cone or the Einstein–Khvol'son cone. Only the latter term is correct, of course. Some time ago the observation of gravitational lenses was believed to be practically impossible (see, for example, Ref. [69]). However, the lensing of a quasar was discovered in 1979. At the present time, the observation of lensing and microlensing is a rather widely employed astronomical method. In particular, the data on lensing allow the determination of the Hubble constant H_0. The result is in agreement with the other data which are presented below.

The verification of GR in strong fields, i.e., for neutron stars (on their surface $|\varphi|/c^2 \sim 0.1 - 0.3$) and in the vicinity of black holes and generally for black holes is topical. A method [70] was recently proposed to verify GR in a strong field by oscillations of radiation in a binary star, one of whose components is a neutron star. Although black holes might be imagined in pre-relativistic physics, they are essentially a remarkable relativistic object. Black holes will be discussed later on, but we can note now that their discovery confirms GR. However, as I understand the situation, one cannot state that what is known about black holes confirms GR but not some relativistic theories of gravity that differ from GR.

A significant verification of GR [up to terms of the order of $(v/c)^5$] is the study of the binary pulsar PSR $1916 + 16$. It has shown that the energy loss by two moving neutron stars joined in a binary system is in perfect agreement with GR provided allowance is made for the gravitational radiation (whose intensity was calculated by Einstein in 1918). This work won the Nobel prize for physics in 1993 [71].

The latter work leaves no doubt as to the existence of gravitational waves, though none of the qualified physicists has ever doubted it before (but the quantitative agreement with GR could not be guaranteed in advance). But there exists another problem (number 22 in the list) — the reception of gravitational waves coming from space. Technically, the problem is fairly complicated and giant installations are now being built to solve it. For example, the LIGO system (Laser interferometer gravitational-wave observatory, USA) consists of two widely spaced 'antennae' 4 km long each. In this installation, it will be possible to detect a mirror displacement (occurring under the action of an incoming gravitational wave) of 10^{-16} cm, and further on even smaller displacements. The LIGO and analogous installations now being constructed in Europe and Japan will be put into operation in the near future. This will be the starting point of gravitational wave astronomy (for more details see Ref. [72]). For orientation I shall note that radio astronomy was born in 1931 and its intense development began after 1945. Galactic X-ray astronomy appeared in 1962. Gamma astronomy and neutrino astronomy are still younger. The development of

gravitational wave astronomy will open up the last known 'channel' through which we can receive astrophysical information. As in other cases, of great importance will be joint (simultaneous) measurements in different channels. This may be, for instance, studies of the formation of supermassive black holes simultaneously in neutrino, gravitational-wave and gamma channels [73]. I shall not write here in more detail about the reception of gravitational waves but refer the reader to Ref. [2], Sec. 20 and, mainly, to Ref. [72] and the references therein.

The set of problems under item 23 in the list represents perhaps the most crucial points in astrophysics. It also includes cosmology (not everybody will agree with such a classification, but this does not change the essence of the matter). The cosmological problem is undoubtedly a grand problem. It has always attracted attention to itself, for Ptolemy's and Copernicus's systems are none other than cosmological theories. In the physics of the twentieth century, the theory of cosmology was created in the works of Einstein (1917), Friedmann (1922 and 1924), Lemaitre (1927) and many other scientists. But before the late 1940s, all the observations significant from the point of view of cosmology had been made in the optical range. Therefore, only the red shift law had been discovered and thus the expansion of Metagalaxy had been established (the works by Hubble which are typically dated 1929, although the red shift had also been observed before and not only by Hubble). The cosmological red shift was justly associated with the relativistic model of the expanding Friedmann Universe, but the rapid development of cosmology began only after relic thermal radio emission with a temperature $T_r = 2.7$ K was discovered in 1965. At the present time it is measurements in the radio wavelength band that play the most prominent role among the observations of cosmological importance. It is impossible to dwell here on the achievements and the current situation in the field of cosmology, the more so as the picture is changing rapidly and can only be discussed by a specialist. I shall restrict myself to the remark that in 1981 the Friedmann model was developed to the effect that at the earliest stages of evolution (near the singularity existing in the classical models, in particular, those based on GR) the Universe was expanding (inflating) much more rapidly than in the Friedmann models. The inflation proceeds only over the time interval $\Delta t \sim 10^{-35}$ s near the singularity (recall that the Planck time is $t_g \sim 10^{-43}$ s, and so the inflation stage can still be considered classically because the quantum effects are obviously strong only for $t \sim t_g$). After the inflation, the Universe develops in accord with Friedmann's scenario (at any rate, this is the most widespread opinion). A very important parameter of this isotropic and homogeneous model is the matter density ρ or, which is more convenient, the ratio of this density $\Omega = \rho/\rho_c$, where ρ_c is the density corresponding to the limiting model (the Einstein–de Sitter model) in which the space metric is Euclidean and the expansion proceeds unlimitedly. For this model $\Omega = \Omega_c = 1$. Here

$$\rho_c = \frac{3H^2}{8\pi G}, \qquad (3)$$

where the Hubble constant H appears in the Hubble law

$$v = Hr, \qquad (4)$$

which relates the velocity of cosmological expansion v (going away from us) with the distance r to a corresponding object, say, a Cepheid in some galaxy. The quantity H varies with

time; in our epoch $H = H_0$. This quantity H_0 has been measured all the time since the Hubble law was established in 1929 (Hubble assumed that $H_0 \approx 500 \text{ km s}^{-1} \text{ Mpc}^{-1}$). Now the value $H_0 \approx 55-70 \text{ km s}^{-1} \text{ Mpc}^{-1}$ has been reached using various techniques (so, the value $H_0 = 64 \pm 13 \text{ km s}^{-1} \text{ Mpc}^{-1}$ has been reported [74] quite recently). For $H_0 = 64$, the critical density is

$$\rho_{c0} = \frac{3H_0^2}{8\pi G} \simeq 8 \times 10^{-30} \text{ g cm}^{-3} . \tag{5}$$

Note that from considerations of dimensionality, the Planck density is

$$\rho_g \sim \frac{c^3}{\hbar G^2} \sim \frac{\hbar}{c l_g^4} \approx 5 \times 10^{93} \text{ g cm}^{-3} . \tag{6}$$

Probably ρ_g is the maximum density near the singularity in which, according to the classical theory $\rho \to \infty$. Thus, the evolution of the Universe or, more precisely, of its region accessible for us, has changed up to the present day (if we now have $\rho \sim \rho_{c0}$) by 123 orders of magnitude (one should not, of course, attach any importance to the latter figure).

One of the main, perhaps, the principal goal in cosmology is the determination of the quantity $\Omega = \rho/\rho_c$. If $\Omega > 1$, the expansion of the Universe will stop and contraction will begin (a closed model; we mean the Friedmann models). If $\Omega < 1$, the model is open, that is, the expansion is unlimited. The simplest model with $\Omega = 1$ is, as mentioned above, an open one with a Euclidean space metric. To find Ω, it suffices to know ρ_{c0}, but the determination of this quantity or the establishment of Ω by other methods is a rather sophisticated task. I refer the reader to the books on cosmology (unfortunately, there is no up-to-date book on cosmology in the Russian language; I can now only recommend the books [75, 76]). An important result which has long been known is that it is not only the normal baryon matter (and, of course, electrons) that contribute to Ω (or to ρ, which is the same), but something else which does not contribute to the observed glow of stars and gas. This something is called hidden or dark matter. It is discussed below. But, apparently, the contribution to Ω is also made by some 'vacuum matter' associated with the Λ-term.

This term, which has been considered since 1917, should be discussed. It was in 1917 that Einstein, turning to the cosmological problem in the framework of GR, considered the static model [77]. He came to the conclusion that a solution existed only if one used GR equations with a Λ-term of having the form

$$R_{ik} - \frac{1}{2} g_{ik} R - \Lambda g_{ik} = \frac{8\pi G}{c^4} T_{ik} . \tag{7}$$

The notation is conventional here, and I shall not specify it (see, e.g., Ref. [48], Sec. 95). In his preceding works Einstein did not introduce the Λ-term (i.e., formally speaking, assumed $\Lambda = 0$). The physical meaning of the Λ-term (for $\Lambda > 0$) is a repulsion which is absent from Newton's theory of gravity. Since without the Λ-term GR in a weak field passes over into the Newton theory, a static solution is clearly impossible without the Λ-term. For this reason Einstein introduced the Λ-term, which is incidentally the only possible generalization of GR which satisfies the requirements underlying the derivation of equations (7). However, after the work of Friedmann (1922) and the discovery of the Universe

expansion (provisionally in 1929) it became clear that the static model was far from reality, and the Λ-term was no longer needed. Moreover, Einstein considered the introduction of the Λ-term to be 'unsatisfactory from the theoretical point of view' [78] and discarded it. Pauli, in the appendix to his well-known book published in English in 1958, totally shared Einstein's opinion (see Ref. [64], p. 287). L D Landau hated the idea of the Λ-term, but I could not make him give his reasoning. Naturally, I could not put this question to Einstein or Pauli [6]. As has already been mentioned above, the introduction of the Λ-term is quite admissible from the logical and mathematical points of view. Why then did the great physicists revolted against it? They must have obviously understood that the introduction of the Λ-term was equivalent to an assumption about the existence of some 'vacuum matter' with an energy-momentum tensor $T_{ik}^{(v)} = (c^4 \Lambda/8\pi G) g_{ik}$ [see (7) with the momentum tensor T_{ik} of the normal matter]. If we put $g_{00} = 1$, $g_{\alpha\alpha} = -1$, the equation of state of this vacuum matter is as follows

$$\varepsilon_v = -p_v = \frac{c^4 \Lambda}{8\pi G} , \tag{8}$$

that is, for a positive energy density $\varepsilon_v > 0$ the pressure is $p_v < 0$, which corresponds to repulsion. Now this is clear, but obviously it was not then understood widely among physicists and cosmologists. In any case, I did not understand it and supported the introduction of the Λ-term only from the above-mentioned formal considerations. As far as I know, Gliner was the first to write about the 'vacuum energy' (8) in 1965 [79]. Since *Zh. Eksp. Teor. Fiz.* was then edited by E M Lifshitz, it is clear that he did not consider work [79] to be obvious either.

The Λ-term played a crucial role at the inflation stage because then it was very large. Now this term is rather small or may, in principle, even be equal to zero. The question of the Λ-term and its evolution in time has been widely discussed [80] and is being discussed at the present time [132]. What has been said accounts for the desire of some physicists to have $\Lambda = 0$. But if the Λ-term is introduced at early stages and decreases with the expanding Universe (the decrease proceeds in the simplest scheme in jumps under phase transitions of the vacuum), it seems that there are no grounds to assume it to be equal to zero in our epoch. In any case, the parameter Ω is currently written in the form

$$\Omega = \Omega_b + \Omega_d + \Omega_\Lambda , \tag{9}$$

where Ω_b corresponds to the contribution of baryons (and, of course, electrons), Ω_d allows for the dark matter and Ω_Λ for the contribution of the 'vacuum energy'. In view of (3) and (8) we have

$$\Omega_\Lambda = \frac{\rho_v}{\rho_c} = \frac{c^2 \Lambda}{3H^2} , \qquad \Lambda = \frac{3\Omega_\Lambda H^2}{c^2} . \tag{10}$$

For $\Omega_\Lambda \sim 1$ and $H \sim H_0 \sim 2 \times 10^{-18} \text{ s}^{-1}$ we have $\Lambda_0 \sim 10^{-56} \text{ cm}^{-2}$. The estimates according to observations are as follows: $\Omega_b \sim 0.03 \pm 0.015$, i.e., there are few baryons.

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[6] I automatically wrote the word 'naturally' meaning the impossibility of speaking to Einstein and Pauli. This impossibility is, in fact, not at all natural, it is unnatural. Einstein died in 1955 and Pauli in 1958 when I was already nearly 40. Neither I nor my Soviet colleagues could communicate with them because of the existence of the iron curtain. I was first able to go abroad (to Poland) to a scientific conference in 1962.

For the dark matter $\Omega_d \sim 0.3 \pm 0.1$, and therefore if $\Omega = 1$ then $\Omega_\Lambda \sim 0.7 \pm 0.1$. But as I understand, these estimates are far from being reliable [132]. Nevertheless, the 'vacuum matter' is apparently noticeable, this is literally a 'new ether' which is of course in perfect agreement with the theory of relativity. New advances in cosmology may be expected with confidence in the near future.

The early Universe appeared to be intimately related to elementary particle physics. We mean the region of very high energies which cannot be reached in any other way. I recall that even on the LHC accelerator an energy of 1.4×10^4 GeV will be obtained (I hope in 2005) in the center-of-mass frame, in cosmic rays the energy of up to 3×10^{11} GeV is fixed, and the Planck energy is $m_p c^2 \sim 10^{16}$ erg $\sim 10^{19}$ GeV. In the Grand Unification theory energies figure of up to 10^{16} GeV (particles of mass $m_{GUT} \sim 10^{-8}$ g). This region is the arena of intense theoretical studies.

Turning to problem 24 (neutron stars and pulsars, supernova stars) I note first of all that the hypothesis of the existence of neutron stars was formulated, as far as I know, in 1934. It could hardly appear much earlier because the neutron was discovered experimentally only in 1932. Neutron stars (with a characteristic radius of 10 km and $M \sim M_\odot$) seemed at first to be practically unobservable. But when X-ray astronomy was created in 1962, there appeared hope that hot neutron stars would be observed in the X-ray range. Now even single neutron stars, to say nothing of binary stars, are actually studied in the X-ray band. But even before this, in 1967–1968, the radio emission of neutron stars — pulsars were discovered. This discovery was rather dramatic and has been described elsewhere, so I shall not write about it here (see, for example, Ref. [81]).

Nearly 1000 pulsars are now known with radio pulse periods P (it is also the period of star rotation) from 1.56×10^{-3} s [7] to 4.3 s. The magnetic field of millisecond pulsars (on the surface) is of the order of $10^8 - 10^9$ Oe. The majority of pulsars ($P \sim 0.1 - 1$ s) have a field $H \sim 10^{12}$ Oe. Incidentally, the existence in nature of such strong magnetic fields is also an important discovery. Neutron stars with still stronger fields (magnetars) reaching, according to the estimates, $10^{15} - 10^{16}$ Oe (!) have recently been discovered. These magnetars do not emit radio waves but are observed in soft gamma-rays.

A gamma-flare clearly from such a magnetar was fixed on August 27, 1998 (the period of radiation bursts after the flare was 5.16 s; the energy interval of radiation was $25 - 150$ keV [82]). Going back to pulsars I should note that the creation of the theory of their radiation turned out to be quite a sophisticated task, but on the whole the theory is constructed [83]. An up-to-date review of pulsars [84] will be accessible to the reader of *Usp. Fiz. Nauk.*

Neutron stars, both emitting radio waves (pulsars) and all the other ones (single, stars in binary systems, magnetars) - are interesting and unusual physical objects. Their density lies within the limits from 10^{11} g cm^{-3} on the surface up to 10^{15} g cm^{-3} in the center. Meanwhile, in atomic nuclei $\rho = \rho_n \simeq 3 \times 10^{14}$ g cm^{-3} and there is no such variety of densities. The external crust of a neutron star consists, of course, of atomic nuclei and not of neutrons. The neutronization process with penetration into the depth of the star, the corresponding equation of state, the possibility of pionization

(the formation of a pion condensate) and the appearance of quark matter in central regions of the star, superfluidity of the neutron liquid (making up the main component of the star), superconductivity of the proton–electron liquid which is present in the star to the level of several percent (of the number of neutrons) — such are some problems of neutron star physics (see also Ref. [128]). The possibility of the existence of stars of neutron-star type but consisting of strange quarks, etc. is considered in the literature. The questions concerning the crust should be specially singled out: the 'cracks' which appear owing to the star's rotation deceleration caused by the loss due to electromagnetic and corpuscular radiation are appreciable; such cracks are associated with 'starquakes' recorded by the variation of the pulsar radiation frequency. For the physics of pulsars the structure of the stellar magnetosphere is of course also important. The question of stellar cooling and, mainly, of its formation should be specially emphasized. Obviously, neutron stars are principally formed through supernova flares. We mean the loss of stability by a normal star and its explosion. A possible, but not inevitable product of such an explosion is a neutron star. In a supernova flare, heavier (compared to helium and some other nuclei) elements are 'boiled', cosmic rays are accelerated in shock waves generated in the interstellar gas and in the envelopes (remnants) of supernovae, electromagnetic radiation of all bands occur. During a flare itself, neutrinos are also emitted. We were lucky in 1987 for the supernova NS 1987 A flared up comparatively close to us (in the Large Magellanic Cloud which is at a distance of 60 kpc from the Earth). I said 'lucky' because the previous supernova observed by the naked eye flared up in the Galaxy in 1604 (the Kepler supernova). The well-known Crab Nebula was formed from a supernova in 1054 and inside it there is a pulsar PSR 0531 radiating even in the gamma-ray band. Neutrino radiation was first registered from supernova SN 1987A. For orientation it is worth noting that the kinetic energy of the remnant of this supernova is $E_K \sim 10^{51}$ erg and the energy output in neutrinos is $E_\nu \sim 3 \times 10^{53}$ erg (recall that $M_\odot c^2 \sim 3 \times 10^{54}$ erg). I hope that what has been said is a clear evidence of how interesting and topical problem 24 is. I believe that a single two-hour lecture or a not very long review will suffice to elucidate this range of questions in the volume necessary for the 'physics-minimum'.

Black holes and particularly cosmic strings are much more exotic objects than neutron stars. Cosmic strings (they should not, of course, be confused with superstrings) are some (not the only possible) topological 'defects' which may occur under phase transitions in the early Universe [85, 129]. They are threads which can be closed rings of cosmic scales and may have a characteristic thickness $l_{CS} \sim l_g(m_{GUT}/m_g) \sim 10^{-29} - 10^{-30}$ cm (here m_{GUJ} is the characteristic mass corresponding to Grand Unification, i.e., $m_{GUT} \sim 10^{-8}$ g $\sim 10^{16}$ GeV, whereas $m_g \sim 10^{-5}$ g $\sim 10^{19}$ GeV). Cosmic strings have not yet been observed, and I do not even know any candidates to this role. For this reason I was on the point of including cosmic strings in the 'list' along with black holes, but put instead the interrogative sign. I can repeat once again that it is impossible 'to bound the unbounded' and having thought twice I came to the conclusion that cosmic strings should not be included in the list (see, however, Refs [96, 138]).

As to black holes, the situation is quite different. They are very important astronomical and physical objects. In spite of the fact that it is very difficult to 'seize a black hole's hand',

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[7] It is amazing that there exists a star with a mass close to the mass of the Sun and a radius of nearly 10 km which makes 640 revolutions per second!

their existence and their great role in the cosmos are now beyond doubt. It is curious that black holes were in a sense predicted as far back as the late eighteenth century by Mitchell and Laplace. They asked themselves a question of whether an object (a star) might exist with such a strong gravitational field that the light from it could not go to infinity. In the framework of Newton's mechanics and the notion of light as corpuscles with a certain mass m, the energy conservation law for the radial corpuscle motion with a velocity v has the form $GMm/r_0 = mv_0^2/2$ (the inert and the heavy masses are assumed to be equal, r_0 is the radius of a star with mass M or, more precisely, the distance from its center from which radiation is emitted and goes to infinity at a velocity v_0). Assuming $v_0 = c$ (the velocity of light), we can see that if $r_0 < r_g$, the light cannot escape from the star and

$$r_g = \frac{2GM}{c^2} = 3\frac{M}{M_\odot} \text{ km} . \qquad (11)$$

In such a calculation, the gravitational radius r_g appeared to be exactly coincident with that calculated in GR. The coincidence of even the numerical factor is of course accidental (I personally do not see any reason for such a coincidence). To the best of my knowledge, the formation of a resting (non-rotating) 'black hole' was first considered within the framework of GR only in 1939 [86] and it was only in the 1960s that black holes entered into astrophysics. Nowadays, black holes and their study is a whole chapter of GR and astrophysics (for a detailed review occupying 770 pages see Ref. [87]). Here I can only make a few remarks (see also [141] about astrophysical observations).

Black holes of two types (with stellar masses $M \lesssim 100\, M_\odot$ and giant holes in galaxies and quasars with $M \sim (10^6 - 10^9)\, M_\odot$) are observed or, to put it more carefully, are most probably observed. Holes with stellar masses are mainly revealed in the observation of binary systems. If one of the stars in such a binary star is invisible (does not radiate) and at the same time its mass is $M \gtrsim 3M_\odot$, this is most probably a black hole. The point is that another possibility of identifying the invisible component in a binary star is to assume that this is a neutron star. But the mass of neutron stars cannot be greater than approximately $3M_\odot$ because a star of a larger mass will collapse to become a black hole. Incidentally, one should not think that a black hole, which does not radiate by itself (i.e., does not emit radiation from the region $r < r_g$), cannot be visible — it may emit radiation from the region $r > r_g$ where the matter (the accretion disk) incident on it or rotating around it is located. In the Galaxy, rather many black holes have already been identified in different ways, mainly in binary systems, according to the indicated criterion (the mass of the invisible component is $M > 3M_\odot$). Giant black holes are located in the nuclei of galaxies and quasars. In the center of a galaxy there exists a potential well, and matter gradually losing its angular momentum flows in to it. Such matter may form star clusters. The fate of the clusters is rather difficult, but it is quite natural that in many cases, if not always, a collapse with the formation of a black hole must ultimately occur. On the other hand it is a well-known fact that in the centers of many galaxies bright, sometimes even very bright nuclei are observed. Such galaxies with very bright nuclei include quasars which were first discovered (or, more precisely, identified as far extragalactic objects) in 1963 with the identification of quasar 3C273. I would not like to go into

the history of the problem. Suffice it to say that nuclei bright in optics do not exist in all the galaxies or all the time. Among them, quasars are those which are also bright in the radio band (QSR or QSS — quasi-stellar radio sources). Quasi-stellar objects which are not powerful radio sources are referred to as QSO (quasi-stellar objects). There is apparently some ambiguity in the terminology, but it is of no importance for us. Bright galactic nuclei may be compact star clusters or black holes. They can be distinguished by the star motion near the nucleus. If we are dealing with a black hole, then the attracting mass is obviously concentrated within a radius smaller than r_g, and even for $M_{bh} \sim 10^9 M_\odot$ this radius is $r_g \sim 3 \times 10^{14}$ cm, that is, negligible on galactic scales (recall that the astronomical unit, i.e., the distance between the Earth and the Sun, makes up 1.5×10^{13} cm). Hence, if it were possible to trace the star motion near the nucleus up to distances comparable with r_g, everything would immediately become clear. But this is impossible even in the case of our Galaxy whose center is at a distance of nearly 8 kpc $= 2.4 \times 10^{22}$ cm from the Sun. Nevertheless, in this case it has been determined, using a radio interferometer, that the radiation source was of the order of astronomical unity. Optical observations of the velocity field of stars near the galactic center have shown that the motion proceeds around a mass with dimensions smaller than a light week, i.e., smaller than 2×10^{16} cm. As a result, one can say with confidence that it is precisely a black hole of mass $M_{bh} \simeq 2.6 \times 10^6 M_\odot$ (for $r_g \simeq 8 \times 10^{11}$ cm) that is located in the center of the Galaxy [88]. For other galaxies, even close ones, the resolution is, of course, worse. Nonetheless when visible, their nuclei, too, are most likely to be black holes rather than some dense star or gas clusters. Investigations in this field are being intensively carried out.

Besides the above-mentioned black hole, relic miniholes may exist which were formed at early stages of evolution of the Universe. The conclusion, drawn in 1974, that owing to quantum effects black holes must emit all sorts of particles (including photons) [89] is generally significant for miniholes (in this connection see Ref. [87] and the most recent paper, as far as I know, on this subject [90]). The radiation of black holes is thermal (i.e., the same as in the case of a black body) with a temperature

$$T_{bh,r} = \frac{c^3\hbar}{8\pi GMk_B} = 10^{-7}\frac{M_\odot}{M} K = 10^{-7}\frac{2 \times 10^{33}}{M\,(\text{gram})}K, \qquad (12)$$

where $k_B = 1.38 \times 10^{-16}$ erg/K is the Boltzmann constant. Obviously, even for a black hole of mass $10^{-2}M_\odot$ (there exist no smaller self-luminous objects) quantum radiation is negligible. But for miniholes the situation changes, and a minihole of mass smaller than approximately $M_{bh} \sim 10^{15}$ g would not have lived up to our epoch (see Ref. [2], Sec. 22). The radiation of such miniholes can, in principle, be revealed, but no indications of the existence of such objects have been reported. One should bear in mind that miniholes can be formed, but the efficiency of this process is unknown. It is therefore clear that there are either very few or no miniholes in the Universe.

We have in fact also touched upon problem 26, more precisely, the question of quasars and galactic nuclei. The question of the formation of galaxies, which was somewhat artificially combined with the preceding question, constitutes a special chapter in cosmology. The theoretical part of its contents includes the analysis of the dynamics of density and

velocity inhomogeneities of matter in the expanding Universe. At a certain stage, these inhomogeneities increase greatly to form the so-called large-scale inhomogeneities of matter in the Universe. This process ends with the appearance of galaxies and galactic clusters. I repeat again that this is a whole field of cosmology (see, in particular, Ref. [126]). The synthesis of chemical elements in the course of Universe expansion is in a sense a similar problem. This is also an interesting and important problem which might well have figured in the 'list', but it is already largely inflated and something should be sacrificed. The choice is, of course, not at all unambiguous.

I shall now dwell on problem 27 — the question of dark matter. It has already been briefly discussed. This is essentially quite a prominent and unexpected discovery whose history, as far as I know, goes back to 1940 [91]. The amount of luminous matter is determined from observations, for the most part in the visible light. The total amount of gravitating matter has an effect on the dynamics — the motion of stars in galaxies and galaxies in clusters. The dynamics are manifested in the simplest and most obvious way in the determination of the 'curves of star rotation' in spiral galaxies, in particular, in our Galaxy. This method is, in principle, elementary; it was clarified in Ref. [2], Sec. 23. It is however convenient to turn to it again since, I am sure, if something can be elucidated already at the school level, it will be useful also for specialists in fields of physics far from astronomy. So, we shall consider the motion of a star with mass M along a circular orbit around a spherically symmetric mass cluster. The equality

$$\frac{Mv^2}{r} = \frac{GMM_0(r)}{r^2} \tag{13}$$

must obviously hold, where v is the star velocity, r is the radius of its orbit relative to the galactic center and $M_0(r)$ is the mass of the galaxy concentrated inside the region with radius r; from (13) immediately follows the Kepler's third law $\tau^2 = (4\pi^2 r^3)/GM_0$, where τ is the star revolution period. Next, suppose the mass M_0 is concentrated in the region with $r \leqslant r_0$ and when $r > r_0$ there are already no masses. Then, obviously, for $r > r_0$ we have

$$v^2(r) = \frac{GM_0(r_0)}{r} , \tag{14}$$

Observations testify to the fact that the dependence $v(r)$, which represents the rotation curves, is substantially different from the law $v(r) = \mathrm{const}/\sqrt{r}$ in the range of values $r > r_0$, where there is already little luminous matter. Briefly speaking, it has been established with confidence that non-luminous matter exists in the Universe which manifests itself owing to its gravitational interaction. Dark matter is distributed not at all uniformly, but it is present everywhere — both in the galaxies and in the intergalactic space. Thus, there arose one of the most important, and I would even say the most urgent questions of modern astronomy — what is the nature of dark matter, frequently referred to earlier as hidden mass? It is most simple to assume that this is neutral hydrogen, a strongly ionized (and therefore weakly luminous) gas, planets, weakly luminous stars — brown dwarfs, neutron stars or, finally, black holes. All these assumptions are however disproved by various types of observations. For example, neutral hydrogen is fixed by the radio-astronomy method, hot gas is registered by X-ray emission, neutron stars and black holes are also observed, though with difficulty. It is

not easy to observe brown dwarfs which are dwarf stars with such small masses $M \ll M_\odot$ that they glow very weakly. However, such stars have also been discovered [92] and in all probability they do not contribute appreciably to the dark matter. The analysis of all these questions is not simple; there exist different opinions concerning the contribution of particular types of baryonic matter to the total matter density. Above, we have pointed out the estimate $\Omega_b \lesssim 0.05$. In general, the conventional point of view is now as follows: dark matter is largely of non-barionic. The most natural candidate is the neutrino. But this version is unlikely to hold: the electron neutrino mass ν_e is obviously insufficiently large (the value known to me is $m_{\nu_e} < (3-4)$ eV), while a mass $m_\nu > 10$ eV is needed. The masses ν_μ and ν_τ will be discussed below, but they are apparently insufficient as well (the possible role of ν_τ is discussed in Ref. [93]). The hypothesis is very popular according to which the role of dark matter is played by the hypothetical WIMPs (Weakly Interacting Massive Particles) with masses of gigaelectronvolts and higher (the proton mass is $M_p = 0.938$ GeV). WIMPs include hypothetical (I repeat) heavy unstable neutrinos and supersymmetric particles — photinos, neutralinos, etc. There also exist some other candidates for the role of dark matter (for example, pseudoscalar particles — axions) [130]. Cosmic strings and other 'topological defects' should also be mentioned. There are hopes to detect WIMPs by their radiation of gamma-photons and other particles upon annihilation with corresponding antiparticles. Another way is observation of the events, although very rare, of collisions with particles of normal matter [94, 131]. The idea of the possibility of WIMP concentration into some friable quasi-stars which can, in principle, be detected by microlensing [95] is very elegant.

The origin of cosmic rays (CR) discovered in 1912 has been enigmatic for many years. But now it is definite that their main sources are supernova stars. In respect of CR with $E_{CR} < 10^{15} - 10^{16}$ eV there generally remain some vague points, but on the whole the picture is clear enough [51]. It is only the problem of the origin of CR with superhigh energies that may be 'particularly important and interesting', according to the terminology adopted in this paper. So, the origin of the 'break' ('knee') in the energy spectrum of CR for $E_{CR} \sim 10^{15} - 10^{16}$ eV is not quite clear and especially the situation with the energy range $E_{CR} > 10^{19}$ eV — such CR are sometimes called ultrahigh-energy CR (UHECR; see Ref. [97]). The highest energy observed in CR is $E_{CR} \sim 3 \times 10^{20}$ eV as has already been mentioned in another context. It is not easy, but obviously possible to accelerate particles (say, the proton) to such an energy, especially in active galactic nuclei. But then the following difficulty arises: when colliding with microwave (relic) radiation (with a temperature $T_r = 2.7$ K), particles with ultrahigh energies generate pions and, thus, lose energy, and as a result cannot reach us from very great distances (the effect of Greizen, Zatsepin and Kuz'min, 1966). For this reason, a cutoff (steepening) must occur in the CR spectrum; under the simplest assumption it proceeds at a characteristic energy $E_{BB} = 3 \times 10^{19}$ eV [97]. In fact, however, this cutoff is absent [52, 97]. The question is how the appearance of CR with $E_{CR} > 3 \times 10^{19}$ and up to 3×10^{20} can be explained. Several possibilities are under discussion. Active galactic nuclei at distances $20-50$ Mpc are apparently insufficient. Moreover, it is not clear whether the known galactic nuclei can provide acceleration up to an energy of 3×10^{20} eV. Particles might be accelerated by cosmic strings and some other 'topological

defects' located outside the Galaxy at distances up to 20 Mpc [97]. There exist no indications of the existence of such 'defects' especially at comparatively close distances. Another hypothesis is as follows. Primary UHECR particles are not 'ordinary' particles (protons, photons, nuclei, etc.), but some not yet known particles which, say, have not undergone strong losses. Then they may come from a large distance, and closer to us, or even in the Earth's atmosphere transform into ordinary particles and yield an extensive air shower (EAS). Finally, it seems most simple to assume that in the galactic dark matter which forms the corresponding galactic halo there exist supermassive particles of mass $M_x > 10^{21}$ eV that have lived longer than the Universe ($t_0 \sim 10^{10}$ years), but are still unstable. The products of their decay are observed in the atmosphere and give rise to EAS (for the reader not closely related to this subject it may be not out of place to explain that the UHECR particles, the same as particles of lower energies, say, $E_{CR} \gtrsim 10^{15}$ eV are registered in cosmic rays only by EAS). On the whole, the problem of CRs with the very high energy is actually enigmatic and already for this reason interesting.

We now proceed to problem 29 , that is, to gamma-bursts. A series of Vela satellites were launched in USA in the 1960s, which were equipped with apparatus for registering soft gamma-rays and were intended for the control of the treaty banning atomic explosions in the atmosphere. No explosions were made, but gamma-bursts of an unknown origin were registered. Their typical energy was $(0.1 - 1)$ MeV and the duration amounted to seconds. The received energy flux in the bursts integrated over time was rather large — it reached the values $\Phi \sim 10^{-4}$ erg cm^{-2}. If a source located at a distance R radiates isotropically, its total energy output in gamma-photons is obviously $W_\gamma = 4\pi R^2 \Phi_\gamma$. This discovery was reported only in 1973 [98]. Gamma-bursts have been intensively investigated since then, but their nature has long remained unclear. The point is that the angular resolution of gamma-telescopes is not high, and observations in other bands (radio wavelength, optical, and X-ray) in the direction of a gamma-burst were not carried out immediately. Thus, the source remained absolutely unknown. One of the probable candidates were neutron stars in the Galaxy. In this case, for comparatively close neutron stars at a distance $R \sim 100 \, \mathrm{pc} \simeq 3 \times 10^{20}$ cm the energy output was $W_\gamma \lesssim 10^{38}$ erg. This is already very much if we recall that the total luminosity of the Sun is $L_\odot = 3.83 \times 10^{33}$ erg s^{-1}. However, the distribution of even weak gamma-bursts over the sky proved to be isotropic, which means that their sources cannot be located in the galactic disc. If they are located in the giant galactic halo so that $R \sim 100$ kpc (this does not already contradict the data on the angular distribution of sources), then $W_\gamma \lesssim 10^{44}$ erg. Finally, if the bursts are of cosmological origin and, for example, $R \sim 1000$ Mpc then we already have $W_\gamma \lesssim 10^{52}$ erg. This value is so large that many scientists (including me) gave preference to the halo model, but in 1997 it was finally managed to 'look' in the direction of a gamma-burst immediately, and sources with a large red shift were discovered [99, 100]. So, for the burst GRB 971214 (the designation implies that this burst was registered on December 14, 1997) the red shift parameter [8] was $z = 3.46$ [101]. For the burst GRB 970508 this parameter was $z \geqslant 0.8$. The sources (it is already known that they are several) were

observed both in the X-ray and optical bands, and some of them also in the radio wavelength band. The work is in full swing, and literally a day after the above was written, on January 23, 1999, a powerful burst GRB 990123 was observed over the entire investigated gamma-ray band from 30 keV to 300 MeV, which lasted 100 s. Simultaneously with the gamma-burst, a burst of light was registered whose maximum luminosity reached $L_0 \sim 2 \times 10^{16} L_\odot \sim 10^{50}$ erg s^{-1}. The total energy output in all the electromagnetic bands was $W \sim 3 \times 10^{54}$ erg if radiation was isotropic (the red shift of the event was $z = 1.61$). More details concerning gamma-bursts will be given in the review [102]. But it may already be asserted that gamma-bursts represent the most powerful explosive phenomenon observed in the Universe, of course except for the Big Bang itself, referring to the energy output of up to approximately $10^{53} - 10^{54}$ erg in the gamma-ray band only. This is appreciably larger than the optical radiation of supernova explosions. For this reason, some sources of gamma-bursts are now referred to as hypernovae. The coalescence of two neutron stars, a collision or a coalescence of a massive star with a neutron one, etc. are now candidates for the role of hypernovae. However, such sources are unlikely to radiate 10^{54} erg $\sim M_\odot c^2$ either. In any case, one can hardly doubt that the discovery of the cosmological origin of gamma-bursts (or, quite rigorously speaking, the discovery of X-ray, optical and radio emission caused by gamma-bursts) is the most distinguished achievement of astrophysics not only of 1997, but of many years (perhaps since the discovery of pulsars in 1967 – 1968).

It remains to discuss the last problem, number 30, from the list. This is neutrino physics and astronomy. I recall that the hypothesis of the existence of neutrinos was suggested by Pauli in 1930. Neutrinos have long been thought of as practically undetectable because the reaction cross section

$$p + \bar{v}_e \to n + e^+ \tag{15}$$

(here $\bar{v}_e$ is an electron antineutrino) is negligibly small: $\sigma \sim 10^{-43}$ cm^2. However, in 1956 this reaction (15) was fixed on an atomic reactor, for which the 1995 Nobel prize in physics was awarded (more precisely, half of the prize [103], the other half was given for the discovery of the τ-lepton [104]). The question of the neutrino mass probably arose at the very beginning but the mass m_{v_e} is clearly very small compared to the electron mass. The assumption of zero neutrino mass (only the electron neutrino was discussed at first) did not face any contradictions. After the discovery of the muon and tau neutrinos v_μ and v_τ (more precisely, only the τ-lepton was discovered, but nobody doubted the existence of v_τ, too) the same could be said about these neutrinos. However, an idea arose (back in the 1960s) that neutrino oscillations, that is, mutual transformations of neutrinos of different types (flavors) were possible. This is only possible if the mass of a neutrino of at least one flavor is nonzero. In any event, the question of the neutrino mass arose long ago and remains very topical. There have been attempts to determine the neutrino mass m_{v_e} by examining the region near the end of the β-spectrum of tritium (the reaction $^3H \to {}^3He + e^- + \bar{v}_e$; by virtue of the CPT theorem it is now undoubted that $m_v = m_{\bar{v}}$). The maximum decay energy is small in this case — close to 18.6 keV. Measurements are being carried out; as far as I know, it is now believed that $m_{v_e} < 3$ eV. The difficulty of the measurements is connected with the necessity of controlling the energy given to the

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[8] Recall that $z = (\lambda_{obs} - \lambda_{source})/\lambda_{source}$, where λ_{obs} is the observed wavelength of the spectral line and λ_{source} is the wavelength in the source.

molecules of the surrounding medium. Incidentally, some of the theoretical estimates (see, e.g., Ref. [105]) are as follows:

$$m_{\nu_e} \sim 10^{-5}\,\text{eV}, \quad m_{\nu_\mu} \sim 10^{-3}\,\text{eV}, \quad m_{\nu_\tau} \sim 10\,\text{eV}. \quad (16)$$

I do not know any direct methods of measuring m_{ν_e} and m_{ν_τ} which are in principle possible. But the study of oscillations opens such possibilities. It is probably pertinent to clarify the very idea of oscillations. This is the assumption that neutrinos of one or other flavor emitted upon decays or born under weak interactions are not eigenstates of the mass operator. That is why, when propagating in space-time, a neutrino of a certain flavor may gradually become a neutrino of another flavor (for more details see Refs [105, 106]). Neutrino oscillations have already been sought for 30 years, and in 1998 a definite success was clearly achieved — the transformation of ν_μ into ν_τ was discovered [107, 108]. This is the most prominent discovery in elementary particle physics for many years. It was made on the Japanese installation Super Kamiokande whose basic element is a tank (1 km underground) filled with 50 000 tons of perfectly purified water. The tank is surrounded by 13 000 photo multipliers which register the Cherenkov radiation from the muons, electrons and positrons produced in the water by neutrinos that get into the tank. Here we are speaking of the electron and muon neutrinos formed by cosmic rays in the atmosphere on the opposite side of the Earth. If there are no oscillations then, according to reliable calculations, in the tank there should be twice as many electron neutrinos as muon neutrinos. But in reality the numbers of ν_e and ν_μ are the same (their energy is of the order of 1 GeV). The most probable explanation of the observations is that oscillations between ν_μ and ν_τ are observed. Here, the quantity $\Delta m^2 = (m_2^2 - m_1^2)$, where $m_{1,2}$ are neutrino masses, is measured. According to [108], $5 \times 10^{-4} < \Delta m^2 < 6 \times 10^{-3}$ (eV)^2. If one assumes that one mass is much smaller than the other, the heavier mass will be $m_\nu \sim 0.05$ eV. Such a neutrino (this is either ν_μ or ν_τ) is of no interest for cosmology. As is stated (see Ref. [107]), if m_2 and m_1 are very close, then masses are admissible that could be responsible for the dark matter. I cannot judge the significance of the difference of the neutrino mass from zero for elementary particle physics.

The Sun and the stars are known to radiate at the expense of nuclear reactions proceeding in their depths and must therefore emit neutrinos. Such neutrinos, whose energy is $E_\nu \lesssim 10$ MeV, can currently be registered only from the Sun. Such observations have already been carried out for 30 years primarily using the reaction $^{37}\text{Cl} + \nu_e \rightarrow {}^{37}\text{Ar} + e^-$. Argon atoms in a tank filled with chlorine (more precisely, with a chlorine-containing liquid) are given off through a chemical method. The observed flux makes up several SNU (solar neutrino units): for a flux of 1 SNU, 10^{36} nuclei of ^{37}Cl or other nuclei capture one neutrino a second on average. According to calculations for different solar models, the flux should be $(8 - 4)$ SNU, and I am unacquainted with the most recent data. I did not try to find them out now because it is of importance that the following fact is established: the observed flux is appreciably smaller than the calculated one, roughly speaking, by a factor of two or three. In view of the complexity of computations for models of the Sun, etc., such a result is of course not impressive. Therefore, there have been attempts to observe solar neutrinos by other methods. So, the scattering of neutrinos on electrons $\nu_e + e^- \rightarrow \nu_e' + (e^-)'$ was recorded by the Kamiokande

installation (the predecessor of Super Kamiokande), where only neutrinos with energy $E_\nu > 7.5$ MeV which were emitted by a ^{8}B nucleus were fixed. The observed flux was again approximately half the calculated one. Finally, two installations were created: the Soviet-American SAGE and the European GALEX in which the working substance is gallium ^{71}Ga transforming into germanium ^{71}Ge upon capture of neutrinos. Such a detector has a low energy threshold and, as distinct from the chloride one, reacts to the bulk mass of neutrinos emitted by the Sun (these are neutrinos from the reaction $p + p \rightarrow d + e^+ + \nu_e$). And again, the observed flux is smaller than calculated (the most recent data of solar neutrinos [109]). All the available information suggests the conclusion that the flux of neutrinos from the Sun is indeed much smaller than calculated, but the calculations disregarded possible neutrino oscillations. This gave rise to the assumption about the existence of such oscillations for ν_e and about their effect upon the observed flux of solar neutrinos (see Ref. [106]; for the latest, to the best of my knowledge, discussion of this question see Ref. [110]). Several improved installations for detection of solar neutrinos with different energies are being built or have already been put into operation. I therefore found it irrelevant to go into detail of the already known data for they may appear to become outdated before the paper comes to light. I do not doubt that the problem of solar neutrinos will basically be solved within the next few years, if not quite soon. Probably, the question of neutrino oscillations and the neutrino mass will also be clarified.

Neutrino astronomy is not only solar astronomy. I have already mentioned the reception of neutrinos upon the flare of supernova SN 1987A. Monitoring is now being carried out, and if we are lucky and another supernova flares near the Sun (in the Galaxy or Magellanic Clouds), we shall obtain a lot of material (supernovae flare in the Galaxy on average approximately once every 30 years, but this figure is inaccurate and, which is important, a flare may occur any moment). The problem of detecting relic low-energy neutrinos which may contribute to the dark matter is especially noteworthy. Finally, high-energy ($E_\nu \gtrsim 10^{12}$ eV) neutrino astronomy is just opening up. A number of installations for the detection of such neutrinos are under construction [111, 139]. The most probable sources are galactic nuclei, coalescence of neutron stars, and cosmic 'topological' defects. Finally, simultaneous observations in all electromagnetic bands and using gravitational-wave antennas will be carried out. So, the prospects are most impressive.

My comments on the list are on the whole over, and there is now every reason to return to the remark made at the beginning of the paper. Only 69 years have passed since Pauli, with uncharacteristic shyness, expressed the idea of the existence of neutrino in a letter addressed to some physical congress (see, e.g., Ref. [103]). And now whole fields of physics and astronomy are devoted to neutrinos. The rate of development is so high that it is difficult to foresee even roughly what physics will look like in a hundred of years. But this will be considered in Section 7.

6. Three more 'great' problems

My whole project — the compilation of the 'list' and the comments on it planned as a pedagogical or educational program and to some extent a guide to action — is not approved by everybody. Some will not like the manner and

the style of the presentation. This is natural. I can only advocate the right to express my own points of view, which is no obstacle for respecting other opinions. I hope the present paper will be beneficial. At the same time, to make the picture complete, I would like to mention three more problems (or ranges of questions) which were not touched upon above. Meanwhile, the teaching of physics and the discussion of its state and the ways of development of it cannot and should not disregard these three branches, three 'great' problems. First, I mean the increase of entropy, time irreversibility and the 'time arrow'. Second is the problem of interpretation and comprehension of quantum mechanics. And third is the question of the relationship between physics and biology and, specifically, the problem of reductionism.

L D Landau was notable for a clear comprehension of physics, at any rate of something that had already 'settled'. In certain accord with this, he did not like any 'substantiations' (Neubegrundung, as he would say using this German word), i.e., obtaining known results in another way or using another method [9]. Of particular value in this connection are the critical remarks made by Landau in respect of the law of entropy increase and the arguments in favor of it. In the Course (see Ref. [29], Sec. 8) he definitely said about the ambiguities that remained in this field: 'The question of the physical grounds of the law of monotonic increase of entropy thus remains open' (Ref. [29], p. 52). The discovery (1964) of CP-parity nonconservation (and, therefore, T-parity nonconservation, i.e., time irreversibility) is clearly related to this subject, but all this is not yet sufficiently investigated and realized. I am ignorant of the present state of the problem and unfortunately cannot even suggest an appropriate reference. There is no doubt that the question is still unclear, and this fact should not be veiled.

The situation with quantum mechanics (I mean nonrelativistic theory) is different. The majority of physicists obviously believe that the so-called orthodox or Copenhagen interpretation of quantum mechanics is consistent and satisfactory. This point of view is reflected in the Course [112]. Landau often added something like this: "Everything is in general clear, but tricky questions are possible which only Bohr is able to answer". In 1939 L I Mandel'shtam delivered lectures on the basic principles of quantum mechanics in Moscow State University. These lectures were published posthumously [113]. They were prepared for publication by E L Feǐnberg and looked through by I E Tamm and V A Fock. As I understand, L I Mandel'shtam completely shared the orthodox interpretation and analyzed it thoroughly. Unfortunately, these lectures are not very well known to the scientific community; they were published with great difficulty and in very hard times. Moreover, during that period (in the 1950s) the discussion of the interpretation or, more correctly, the basic principles and understanding of quantum mechanics somewhat faded. Now this range of problems is given prominence in serious literature. I shall refer to the monographs [114, 115] and the papers [116 – 118], where a lot of references are given. The current interest in the fundamentals of quantum mechanics is partially due to new experiments, mainly in the field of optics (see Ref. [116]). All these experiments testify to the perfect validity and, one can say, the triumph of quantum mechanics. At the same time,

they exposed features of the theory which have long and well been known but do not seem obvious. This is not an appropriate place for discussing all these questions. I would only like to note that the discussion of the fundamental principles of nonrelativistic quantum mechanics remains topical and should not be ignored [10]. The majority, if not the overwhelming majority of critics of quantum mechanics are dissatisfied with the probabilistic nature of part of its predictions. They would apparently like to return to classical determinism in the analysis of microphenomena and, figuratively speaking, to come ultimately to know exactly where each electron goes in the diffraction experiments. There is no reason to hope for this now.

If we turn to the history, we know that the creation of the relativity theory and quantum mechanics has led to an understanding of the range of applicability of classical (Newtonian) mechanics. Nevertheless, Newtonian mechanics remained unshakeable. The applicability limits of nonrelativistic quantum mechanics associated with relativism are already known. Generalization of the existing relativistic quantum theory (perhaps in the way outlined in the string theory) is unlikely to introduce anything new to nonrelativistic quantum mechanics and to answer the notorious question of 'where the electron will go'. However, when we speak of the possibilities of the future theory and of its influence on the existing one, we cannot give an *a priori* answer. As has been said above, the orthodox (Copenhagen) interpretation seems to be consistent, and many scientists are satisfied with it. I can only express my intuitive judgment — nonrelativistic quantum mechanics will not undergo substantial changes (we shall not come to know 'where the electron will go'), but some deeper understanding (outside the limits of the orthodox interpretation) is still not excluded.

I have just used the term 'intuitive judgment'. The notion seems to be clear from the words. But this is, in fact, a deep issue which was analyzed by E L Feǐnberg [120] [11]. The methodology and philosophy of science are not now respected in Russia. This is a natural reaction to the perversions of the Soviet period when there was no freedom of opinion and dogmatic dialectic materialism was implanted. But the methodology and philosophy of science remain, of course, the most important ingredients of scientific Weltanschauung (world outlook). Under conditions of freedom of ideology, the attention to these problems should be revived.

The last 'great' problem to be discussed here concerns the relationship between physics and biology. From the late nineteenth century until approximately the 1960s or 1970s,

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[9] I dare say that I do not at all agree with Landau in this respect, and I have already written about it many times (see, e.g., various papers in Refs [2, 10]).

[10] The aforesaid is particularly clear if we, for example, take into consideration that at the end of 1998 a fairly serious journal published a paper [119] in which the works of D Bell are called 'the most serious discovery in science' (probably for some period of time). Bell was, in fact, (and remained up to his death in 1990) unsatisfied by the orthodox interpretation of quantum mechanics and tried to replace it by a theory with 'hidden parameters'. However, Bell's analysis and the subsequent experiments confirmed quantum mechanics largely against his aspiration. But Bell hoped that his future theory would provide insight into the existing nonrelativistic quantum mechanics. But that was no more than a hope. I failed to find a 'serious discovery' in the works of Bell.

[11] The tern 'intuitive judgement' seems to suit well judgements that can be neither proved nor disproved. In such cases one customarily applies to the word 'belief' (for example, "I believe that ... will be obtained"). But the term 'belief' appeared to be closely related to belief in God and religion. However belief in God is an intuitive judgement which differs essentially from intuitive judgement in science (see Refs [120, 121]).

physics was so to say the prime science, the first and dominating. All kinds of ranks are of course conditional in science, and we only mean the fact that the achievements in physics in the indicated period were particularly bright and, which is important, largely determined the ways and possibilities of the development of the whole of natural science. The structure of the atom and atomic nucleus, and the structure of matter was established. It is absolutely obvious how important it is, for example, also for biology. The development of physics led in the middle of our century to a culmination — the mastering of nuclear energy and, unfortunately, atomic and hydrogen bombs. Semiconductors, superconductors and lasers — all these are also physics which determine the face of modern technology and thus, to a great extent, modern civilization. But the further development of fundamental physics, the basic principles of physics and, concretely, the creation of the quark model of the structure of matter are already purely physical problems which are not essentially significant for biology and other natural sciences. At the same time, using for the most part increasingly perfect physical methods, biology progressed quickly, and after the genetic code was deciphered in 1953 its development was particularly rapid. It is biology, especially molecular biology that has now taken the place of the leading science. One may disagree with this terminology and with the essentially unimportant distribution of 'places' in science. I would only like to emphasize some facts which not all physicists understand, especially in Russia. Physics for us remains the cause of our life, young and beautiful, but for human society and its evolution the place of physics has now been taken by biology. A good illustration of these words is the following detail. The journal *Nature*, whose role and place in science need not be explained, elucidates all the sciences, including physics, astronomy and biology in its weekly issue. At the same time, *Nature* today has sprouted six satellites — the monthly journals *Nature-Genetics*, *Nature-Structural Biology*, *Nature-Medicine*, *Nature-Biotechnology*, *Nature-Neuroscience*, and *Nature-Cell Biology*. They all are devoted to biology and medicine. For physics and astronomy, the basic *Nature* issue and certainly the numerous purely physical journals are enough (of course, in biology such journals also exist). The achievements of biology are so widely elucidated even in popular literature that there is no need to mention them here. I am writing about biology for two reasons. First, modern biological and medical studies are impossible without the many-sided use of the physical methods and apparatus. Therefore, biological and near-biological subjects must and will occupy more and more space at physical institutes, physical faculties and in physical journals. One should understand this well and promote it actively. Second, the question of reductionism is simultaneously a great physical and biological problem, and I am convinced that it will be one of the central problems in the science of the twenty first century.

We believe that we know what all life consists of, meaning electrons, atoms and molecules. We are aware of the structure of atoms and molecules and of the laws governing them and radiation. The hypothesis of reduction, i.e., the possibility of explaining all life on the basis of physics, the already known physics, therefore seems natural. The main problems are those of the origin of life and the appearance of thinking. The formation of complex organic molecules under conditions that reigned on the Earth several billion years ago has already been traced, understood and simulated. The transi-tion from such molecules and their complexes to protozoa and their reproduction seems to be imaginable. But a certain jump, a phase transition exists here. The problem is not solved, and I think will unreservedly be solved only after 'life in a test-tube' is created. As to the physical explanation of the mechanism of the appearance of thinking, I am not aware of the situation and can only refer to the discussions of the possibility of creating an 'artificial intellect'. Those who believe in God certainly 'solve' such problems very simply: it was God who breathed life and thinking into inorganic matter. But such an 'explanation' is nothing but a reduction of one unknown to another and lies beyond the scope of the scientific Weltanschauung and approach. At the same time, can the possibility of reduction of biology to modern physics be taken as undoubted? The key word here is 'modern'. And with this word in mind I think it would be incorrect to answer this question in the affirmative. Until the result is obtained, the possibility cannot be excluded that even at the fundamental level we do not know something necessary for the reduction. I make this reservation just to be on the safe side, although my intuitive judgment is as follows: at the fundamental level no 'new physics' is needed for the reduction — the understanding of all biological processes. No dispute concerning this issue will be fruitful — the future will show.

One cannot but think about this future with jealousy — how many interesting and important things we shall learn even in the next ten years! I shall venture a few remarks on that score.

7. An attempt to predict the future

In connection with forecasts for the future, the phrase may often be heard: forecasting is a thankless occupation. It is meant perhaps that life and reality are much richer than our imagination, and forecasts often prove to be erroneous. More important is the circumstance that unpredicted and unexpected discoveries are the most interesting. They cannot, of course, be prognosed, and thus the validity of prognoses seems to be particularly questionable. Nevertheless, attempts to foresee the future seem to be reasonable if one does not attach too much importance to them. This is what I shall do concluding the present paper by a forecast concerning only the problems mentioned above (I apologize for some repetition).

The decision to begin the construction of the giant tokamak ITER which will cost ten if not twenty billion dollars has been delayed for three years. I am afraid that this project will not be realized at all, but the research work in the field of thermonuclear fusion continues and alternative systems and projects are being elaborated. The very possibility of constructing an operating (commercial) reactor does not arouse any doubts. The future of this direction is mostly determined by economical and ecological considerations. I think that some experimental reactor (but, of course, with a positive energy output) will in any case be constructed in a couple of decades. Laser thermonuclear fusion will also be realized because such an installation is possible and needed for military purposes. Of course, physical experiments will also be carried out on it.

As mentioned in Section 3, the problem of high-temperature superconductivity has been investigated since 1964 and I had thought of it as quite realistic all the time before the discovery of HTSC in 1986 – 1987. But at that time there was no real prediction of the possibility of HTSC. It was only

found that no known fundamental difficulties existed on the way to the creation of HTSC. The present-day situation with room-temperature superconductivity (RTSC) is the same. In 1964, the maximum known critical temperature for super-conductors was 23 K, and now for HTSC we have $T_{c,max} = 164$ K, i.e., the temperature T_c has increased sevenfold. In order to reach room temperature, it now suffices to increase T_c by 'only' a factor of two. Therefore if we proceed from 'kitchen' considerations, the possibility of obtaining RTSC seems probable. At the same time there inevitably remain some doubts. If the HTSC mechanism in cuprates, which is still unclear, is basically a phonon or a spin (or a phonon-spin) mechanism, then even a twofold increase of T_c is very difficult. If the exciton (electron) mechanism is decisive then the creation of RTSC is on the contrary quite plausible. I can only express here an intuitive judgment. Namely, I believe that RTSC will be obtained in the not very remote future (maybe tomorrow or maybe in several decades).

I remember the times when the creation of metallic hydrogen seemed to be 'a matter of technique'. One can of course say the same thing today, but the static pressures of nearly three million atmospheres now attained to obtain the metallic phase turned out to be insufficient. It is unknown (at least to me) how the pressure can be heightened appreciably if new materials stronger than diamond are not discovered. Dynamical compression leads to heating, and it is unclear how to avoid it. I am of the intuitive opinion that these difficulties may rather soon be overcome. At the same time, the hopes (which existed) to obtain a 'piece' of metallic hydrogen and to use it do not seem to be realistic.

In respect of all the other problems (4 – 13) of Section 3 it is clear that they will be intensively investigated and many interesting things will be clarified. But being perhaps insufficiently informed, I cannot point to any vivid expecta-tions. A surprise may however be expected from fullerene C_{36} or K_3C_{36} type compounds if they show HTSC properties. The study and application of nanotubes is promising. Long-lived transuranium nuclei may obviously be obtained.

Macrophysics should also include the fireball (ball light-ning) problem which I did not include in the list. The existence of the fireball is beyond doubt. The problem of its origin has long been discussed. Many models and hypotheses have been proposed, but a notorious consensus has not been attained. The origin of the fireball, I believe, will clearly and unambiguously be established only after these objects are created in the laboratory where all the conditions and parameters can be controlled. Incidentally, such attempts were repeatedly made and claims expressed that fireballs were born. But no such statements have been confirmed.

In the field of microphysics (elementary particle physics) an obvious recession (in the number of discoveries, etc.) has been observed within the last two decades compared to the previous period. This is perhaps largely due to the want of accelerators of a new generation. But the LHC will go on line in 2005, and some other existing accelerators that are now under reconstruction will become operational even before that date. Therefore, one can expect the discovery of the scalar Higgs-boson or even of several 'higgs'. If such a particle is not discovered (which is difficult to believe), the theory will face a great difficulty. On the contrary, if new particles or, more specifically, supersymmetric partners of already known particles are not found even on the LHC, this may only signify that the masses of these particles exceed 14 TeV =

1.4×10^{13} eV. As I understand, this will not mean anything special. Among the anticipated results we can point out a further investigation of neutrino oscillations and the determi-nation of the masses of the neutrinos ν_e, ν_μ, and ν_τ. New results concerning the nonconservation of CP-invariance, in particular, at higher energies will also be obtained. It may appear to be important in the analysis of the 'time arrow' problem. Magnetic monopoles have been sought for many years and the hope for their discovery is now practically gone. But who knows? On new installations (especially on Super Kamiokande), attempts are continuing to discover proton decay. In collisions of relativistic heavy nuclei, progress can be expected in the question of quark – gluon plasma and, generally, quark matter.

In spite of the fact that the forefront of physics — elementary particle physics is no longer the 'queen of sciences', studies in this field have scaled up and diversified. The future will undoubtedly bring us many new results in this field, too, but it is senseless to scrupulously enumerate here the projects, tasks and separate questions. What is however necessary to distinguish is the 'question of questions' — quantum gravity and its unification (superunification) with other (strong and electroweak) interactions. Something of the kind is claimed by the string (superstring) theory. To think that the string theory is already nearly thirty years old would be an overestimation, but the notorious 'first superstring revolution' took place fifteen years ago (see Section 4). Nevertheless, an accomplished theory, the 'theory of every-thing' is out of the question. And the theory of superstrings may not be the way at all in which the future theory will evolve. But can such remarks be treated as a reproach to or an underestimation of the string theory? I ask the reader not to think so. This is an exceedingly deep and difficult problem. What are fifteen or even thirty years on this way? We have got so used to the rapid development of physics and its successes that we seemingly lose perspective. The same as in economics and population, an exponential growth, in this case the gain of our physical knowledge, cannot last very long. I do not dare make forecasts in the field of quantum cosmology and generally a new and really fundamental theory.

I shall now proceed to what was attributed in the 'list', sometimes conditionally, to astrophysics.

An experimental verification of GR in weak and strong fields is under way and will continue. The most interesting thing would certainly be at least the slightest deviation from GR in the non-quantum region. I am of the intuitive opinion that GR does not need any modification in the non-quantum region (some changes in superstrong gravitational fields may however be necessary, but these changes are most likely to be of a quantum nature, i.e., they will disappear as $\hbar \to 0$). Such an assumption is not at all the absolutization of GR. I only mean to say that the applicability range of GR is exclusively quantum. Logically, some other restrictions are possible. To make this clear, I shall give an example from Newtonian (classical) mechanics. We know that this system of mechanics is restricted, so to speak, from two sides — relativistic and quantum. Some other restrictions, for instance, in the case of very small accelerations (see Refs [122] and [2], Sec. 23) are also logically imaginable. The change of GR associated with quantum theory is already a different problem which was discussed above.

From the very beginning of the twenty first century, gravitational waves will be detected by a number of installa-tions now being constructed, first of all LIGO in USA. The

first pulses to be received will apparently be those generated by the coalescence of two neutron stars. Correlations with gamma-bursts and with high-energy neutrino radiation are possible and even quite probable. So, gravitational-wave astronomy will be born (its possibilities are described in Ref. [72]).

The whole extra-galactic astronomy, which is now rapidly developing, is connected with cosmology to this or that extent. New wide-aperture telescopes are already operating. For example, in two 'Keck'-telescopes (on the Hawaiian islands) the mirror diameter is 10 m (they were put into service in 1992 and 1996, respectively), while the famous Palomar telescope which has been in operation since 1950 has a mirror 5 m in diameter; the Russian telescope in Zelenchuk (operating since 1976) has a mirror 6 m in diameter. The Hubble space telescope launched in 1990 (mirror diameter 2.4 m) is very efficient. New telescopes for various bands (from X-ray to radio wavelength) are permanently being built. Worthy of special note are satellites — gamma-observatories and installations for reception of cosmic neutrinos (they can of course be called neutrino telescopes). As a result of titanic work on all these telescopes, the value of the Hubble constant will finally be specified and the parameters Ω_b, Ω_d, and Ω_Λ (see Section 5 above) evaluated. Thus, the cosmological model, at least at the stage after the formation of relic radio emission (i.e., for the red shift parameter $z \lesssim 10^3$) will eventually be selected. The role of the Λ-term and the contribution of dark matter not only on the average (the parameter Ω_d), but for various objects (the Galaxy, galactic clusters, superclusters) will be determined. I have somehow got to the enumeration of various astronomical problems and objects, what are beyond the scope of the paper. New material will be obtained for practically all the problems and questions, but disputable, unclear and to an extent problematic issues are particularly noteworthy. Such issues include the discovery of black miniholes and cosmic strings (they may be of different types) and some other 'topological defects'.

Since the nature of dark matter is absolutely unclear, the solution of this problem may now be thought of as the most important in astronomy if we do not touch upon the principal question of cosmology (the region near the classical singularity, i.e., the quantum region; our Universe as part of a branched and apparently infinite system). The possible means of dark matter studies were already discussed in Section 5. This is a truly enigmatic problem, and success can only be hoped for. But I shall not be surprised if it is solved soon.

In respect of problem 28 — the origin of superhigh-energy cosmic rays, there is an essential vagueness, as was mentioned in Section 5. The situation resembles that associated with the origin of dark matter, and it is not excluded that these questions are interrelated. The directions of further studies are obvious, and they are under way. The same can be said about gamma-bursts and neutrino astronomy. Incidentally, the most significant achievements in physics and astrophysics for the past five years has been the proof of the cosmological origin of gamma-bursts (more precisely, of their considerable part) and the discovery of neutrino oscillations, and thus the proof of the fact that at least one sort of neutrinos has a nonzero mass (it should be noted that the establishment of neutrino oscillations requires additional verification). The gamma-burst studies will probably yield many interesting results, but a greater sensation than the discovery of hypernovae may hardly be expected. Installations for the investigation of neutrinos are now operating and new ones will soon appear. Hence, the solution of the solar neutrino problem (i.e., comparison of experiments with the theoretical calculations of neutrino fluxes with different energies) may be expected in the near future. The role of neutrino oscillations will also be clarified. Neutrino 'telescopes' for detecting high-energy neutrinos are to be put into operation. As has already been mentioned, their simultaneous operation with gravitational antennas and gamma-telescopes will undoubtedly be beneficial. As to the reception of relic neutrinos and relic gravitational waves, I am not aware of the situation (in respect of gravitational waves see, however, Ref. [140]).

As has already been emphasized, the distinction of some problems among others is rather conditional and is connected with some awkwardness — quite a lot of significant and interesting ones appear to have fallen overboard! I felt this especially keenly when I singled out gamma-bursts and did not mention the development of other branches of gamma-astronomy (see, e.g., Ref. [123]).

Summarizing I can state that almost all the directions discussed above are fairly promising. I think that within the coming twenty – thirty years we shall receive answers to all the above-mentioned questions except perhaps for the fundamental problems of elementary particle physics (superstrings, etc.) and quantum cosmology near classical singularities. I simply dare not foretell anything in these two directions.

Concluding, I would like to return to the three 'great' problems mentioned in Section 6. As far as the 'time arrow' is concerned, I do not see any new experiments which might provide an insight into this problem. My intuition suggests that CP- and thus T-invariance nonconservation is of importance. But what can be contributed by new experiments? As to the basic principles of nonrelativistic quantum mechanics, the question of interpretation is largely of gnosiological nature. The new refined experiments which are now being carried out to verify the uncertainty relations, the notorious teleportation, etc. do not in the least go beyond the limits of the known theory. My intuitive judgment is that we shall never be able to predict 'where the electron goes' in diffraction experiments. The future theory (conditionally, the superstring theory and its development) may provide some new results, but I cannot imagine what particular results they could be (the concept of time is under suspicion in quantum mechanics). As concerns the third of the 'great problems' — reductionism — I acknowledge my incompetence. Perhaps it is for this reason that I would not be surprised if 'life in a test-tube' were created in the twenty first century. But if at all, this may only be achieved by biochemical methods, while physics may play an auxiliary role. One way or another, I cannot make predictions in this field.

Having finished the article, I clearly see its shortcomings. The large scope of the paper accounts for a sketchy manner of the presentation and perhaps for some superficiality. Everything has its price. But the reader will judge of whether the price is too high. However, the very idea of the paper cannot be discredited by some shortfalls. I call on those who agree with it for constructive criticism — maybe someone will succeed better where I failed.

Finally I shall make a last remark.

From the above presentation it is clear that very many new, important and interesting things may be anticipated in the coming years and the more so in the first half of the twenty first century. The rather pessimistic foresight encountered in

the literature and concerning the development of physics and astrophysics in the foreseeable future seem to be a result of a lack of information, incompetence or simply misunderstanding. Another thing is that the exponential law of the development of science in respect of some 'indices' (the number of research workers, the number of publications, etc.) is limited in time and a certain saturation sets in (for more details see Ref. [2], Sec. 27 and Ref. [120]). However this circumstance does not on the whole contradict what has been said above, for we have discussed the near future. I think that in about ten years it will be quite pertinent to write a new article having the same title as the present one. It will be interesting to see what will be realized and how my 'list' will have to be updated by discarding the outdated and adding new items. I hope that there will be a physicist who will do this work, and that *Uspekhi Fizicheskikh Nauk* will offer some space for the corresponding paper.

In conclusion I take the opportunity to thank all those whom I consulted on this or that question and who made critical remarks on the manuscript (I do not mention the names because I do not want anybody to be responsible, even indirectly, for the shortfalls of the paper).

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[12] The literature on the problems touched upon in the paper is inconceivably extensive. I have tried to make only a minimum number of references allowing the reader to somehow catch hold of the corresponding material. Preference was given to references to the most easily accessible journals (*Usp. Fiz. Nauk*, *Physics Today*, *Physics World*, etc.) and the most recent publications known to me and those containing many references.

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