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On some advances in physics and astronomy over the past three years

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Abstract. In 1999 the author published a paper "What problems of physics and astrophysics seem now to be especially important and interesting (thirty years later, already on the verge of XXI century?" [1]. By its very nature and intention, the content of this paper should be modified on a continuous basis to keep up with advances in science. In the last three years, important results of a fundamental nature have been obtained which the author finds appropriate to summarize briefly in this article because Ref. [1] generated great interest among readers.

1. Paper [1], published in 1999, was a stage on the way of the educational or, if you like, pedagogical 'project' that I have been realizing over 30 years already. I mean the presentation and propaganda of a certain 'physical minimum'. Namely, I propose a 'list' of problems which are currently of particular importance in physics and astrophysics. In my opinion, every physicist must have at least a superficial idea of these problems and know what they are concerned with. It is quite obvious that any such 'list' is limited and subjective. It is also obvious that the very 'project', to say nothing of my attempts to realize it, does not meet with universal approval. But I have never meant something obligatory; those uninterested will not read the corresponding literature. But the number of physicists and astronomers interested in it appeared to be rather large. This fact is known because *Physics – Uspekhi* is readily accessible on the Internet (www.ufn.ru) both in Russia and abroad. It turned out that by January 1, 2002, paper [1] had been copied from Internet to personal computers by more than 3500 users. And the number of people who have read the paper in the journal itself in its Russian and English versions is unknown, although it is likely to be appreciable. Meanwhile, some important things in the paper have now become outdated and I feel awkward about it because the reader does not receive the latest information. This was the only reason for which the present paper was written. Its aim is to report on the advances in physics and astrophysics over the past three years, i.e., after the appearance of paper [1]. I shall dwell here very briefly on only the few most important issues, and all this will be a kind of supplement to paper [1]. As a matter of fact, I have done it three times already. So, in the English translation [2] of book [3] I added an updated version of paper [1], then it was done in book [4] and, finally, I prepared a somewhat revised edition of book [4] in English [5], where I again had to make some changes. The present paper is naturally based on that last presentation [5]. Unfortunately, such an approach is unavoidable when one tries to keep pace with a train going at full speed — the progress in science. It is quite obvious that a cursory enumeration of separate problems and results is of only limited significance and value. But I repeat what I have already said: the appearance of this short paper is an indispensable measure. Without it, paper [1] cannot play the role for which it was intended.

2. For convenience I shall give here the 'list' of problems which underlies the presentation in [5].

1. **Controlled nuclear fusion.**
2. **High-temperature and room-temperature superconductivity (HTSC and RTSC).**
3. **Metallic hydrogen. Other exotic substances.**
4. **Two-dimensional electron liquid (anomalous Hall effect and some other effects).**
5. **Some problems of solid state physics (heterostructures in semiconductors, quantum wells and dots, metal – insulator junctions, charge and spin density waves, mesoscopics).**
6. **Second-order phase transitions and related transitions. Some examples of such transitions. Cooling (laser cooling, in particular) to ultralow temperatures. Bose – Einstein condensation in gases.**
7. **Surface physics. Clusters.**
8. **Liquid crystals. Ferroelectrics. Ferrotoroics.**
9. **Fullerenes. Nanotubes.**
10. **The behavior of a substance in superstrong magnetic fields.**
11. **Nonlinear physics. Turbulence. Solitons. Chaos. Strange attractors.**
12. **Rasers, grasers, superpower 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. Interaction of high- and superhigh-energy particles. Colliders.**
18. **Nonconservation of CP-invariance.**
19. **Nonlinear phenomena in vacuum and in superstrong electromagnetic fields. Phase transitions in vacuum.**
20. **Strings. M-theory.**
21. **Experimental verification of the general relativity theory.**
22. **Gravitational waves and their detection.**
23. **Cosmological problem. Inflation. Λ-term and 'quintessence' (dark energy). Relation between cosmology and high-energy physics.**
24. **Neutron stars and pulsars. Supernova stars.**
25. **Black holes. Cosmic strings (?).**
26. **Quasars and galactic nuclei. The formation of galaxies.**
27. **The problem of dark matter (hidden mass) and its detection.**
28. **The origin of superhigh-energy cosmic rays.**
29. **Gamma-ray bursts. Hypernovae.**
30. **Neutrino physics and astronomy. Neutrino oscillations.**

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Compared to the analogous list given in [1], the changes are minor. So, in item 5 I have mentioned in addition quantum wells and dots. In item 8, ferrotoroics are now mentioned. And, finally, in the formulation of problem 23, instead of the word Λ-term, I wrote Λ-term and 'quintessence' (dark energy). More corrections could readily be introduced, but I tried to avoid it, for of importance is the content of the material rather than titles. The content has changed drastically in some cases, and this will constitute the subject of the present paper.

3. As concerns the problem of controlled nuclear fusion, I cannot add anything essentially important or interesting. The work is in full swing. The ITER project will not be realized in its original form because a cheaper design has already appeared; however, the site for the installation has not yet been chosen. The powerful 'machine' for the realization of laser thermonuclear fusion has not been put into operation. I believe it would be pertinent to point here to papers [6] which present the history of early research in the field of controlled thermonuclear fusion in the USSR.

No significant advances have been made in the study of high-temperature superconductors (HTSC), but many important and so-to-say promising results have been obtained. They are partly enlisted in paper [7]; of more recent works we may point out 'pseudo-gap' studies [8] and, what is important, the doping in cuprates not by the introduction of oxygen impurities, but by the application of an electric field which inserts electrons and holes into the sample [9]. The origin (mechanism) of HTSC in cuprates remains unclear (I shall refer the reader to review [10] and the new mechanism proposed by Yu V Kopaev [11]). For a number of years great attention has been drawn by the idea that the Fermi-liquid model, which perfectly describes the transport processes in most metals, may appear to be inapplicable to HTSC cuprates and some other substances [12]. In this connection, of particular interest were the results [13] published at the very end of 2001 and perhaps testifying to the fact that the 'elementary excitations' carrying current and heat in cuprates differ substantially from electrons and holes. The role of the strong magnetic field present in these experiments remained, however, unclear to me. The experiments [13] should (and undoubtedly will) be repeated and analyzed. I shall add that the study of thermoelectric effects is also interesting in this context (see [5], paper 6, and also Section 5 and footnote 11* in paper 7). The main problem in the investigation of superconductivity is of course the possibility of creating room-temperature superconductors (RTSC). But this question is quite open as yet. My intuitive opinion is that this is possible.

As to problem 5, alongside the trivial remark concerning the rapid development of this field, I can only mention the appearance of spintronics. The point is that until to recently only the motion of charges (electrons and holes) was considered in electronics (specifically, in semiconductors), whereas spin variables were ignored, or so-to-say, remained in the shadow. Today, great attention is also paid to spins of the carriers and their behavior is a subject of research [14, 15].

As far as phase transitions (problem 6) are concerned, this is a boundless sea. I shall only mention what was ignored in [1], namely, the so-called quantum phase transitions that occur at $T = 0$, say, owing to pressure variations (see, e.g., [16]; one interesting quantum phase transition is discussed in [66]). The 'boom' that has recently arisen in connection with the study of Bose – Einstein condensation (BEC) in rarefied gases has already been characterized at length in [1]. The 'boom' is generally under way, and it was heated by the 2001 Nobel Prize in physics being given to three physicists "For obtaining Bose – Einstein condensate in rarefied gases of alkali metals and for previous fundamental investigation of the properties of these condensates" (see, e.g., [17]). In paper [1], and also in paper 20 of book [4] (in [5] this is paper 21) I have already touched upon the question of Nobel Prizes in physics. Doing justice to these prizes, I would nevertheless like to emphasize a notable change in their spirit (though, not in total) observed over two or three past decades. As is known, the first Nobel Prize in physics was given in 1901 to W Roentgen for the discovery of X-rays. Further on, prizes in physics were also awarded for the individual achievements of a laureate or laureates (as is known, a Nobel Prize may be given to no more than three persons). Obviously, the choice of a winner or winners was in most cases a very difficult task, but it was still the competition of themes (discoveries, achievements) and a few authors, and frequently there was only one author. Nowadays, the choice of three cherished prizemen often literally resembles sports competitions on a running-track or in a swimming-pool, where the awardees are also three, although the medals are of different values (gold, silver, and bronze), whereas the Nobel medals are all gold. But in the above-mentioned kinds of sport the criterion is a stop-watch (incidentally, I do not quite believe in the accuracy of its readings up to a hundredth or a thousandth fraction of a second even if a photo-finish and other contrivances are used). And how is it possible to point to a winner in science where the research is carried out by a large team of scientists and engineers (the role of the latter in an experiment may be far from secondary)? It is a well-known fact that in some cases prizes and rewards are given merely 'for posts' to heads of departments, chiefs of research workers, and generally 'bosses'. They frequently appear as co-authors of various publications (papers), patents, etc., to the contents of which they have a rather doubtful relation. It would certainly be groundless and even offensive to refer these suspicions to the work of the Nobel Committee as a whole. They try to analyze thoroughly a large number of recommendations of a wide range of representatives of the scientific community. For example, as is reported in paper [18], when choosing awardees for the 2000 Nobel Prize in physics, the Nobel Committee sent more than 2000 invitations to physicists of many countries to propose nominees for the Prize. As a result, nearly 300 candidates were proposed, and the respondents usually know whom and why they nominate, and understand their role in the team if the result was obtained by joint effort. It should also be noted that the invitation of the Nobel Committee to propose nominees is followed by an insistent request to do it confidentially, without discussions at meetings, scientific councils, etc. Such an appeal frequently remains unheard, and I myself know a number of corresponding examples (the request for abstaining from public discussions is met willy-nilly, of course). Lest the reader should think of the above-said as ungrounded, I shall note as an example that even in the papers mentioned in the report [17] on the 2001 Nobel Prize in physics, references to two (obviously, fundamental) papers are given, where along with the three laureates, there were another nine co-authors. I shall also note that attempts to observe BEC in rarefied gases were also undertaken before the prizemen and their team by other authors (see [17, 19]) who reached the goal three years later (in 1998). I believe it was connected exclusively with the choice of gas. Those 'defeated' in the race[1] worked with hydrogen which for some objective reasons proved to be less convenient than alkali metals. An analogous situation occurred, for example, with the 1997 prize awarded for cooling gases to ultralow temperatures and their confinement in traps [20].

I think that some co-authors of awardees, who did not receive the prize, do not feel very comfortable, and the analogy with those who ran the distance a bit slower than the three winners is not very far from reality. What has been said above was not aimed at casting a shadow on the Nobel Prizes in science generally. I only wished to characterize the current situation, and to understand it is in any case useful. Well, this is life and it would of course be absurd to abolish awards in sports competitions, including the Olympic Games, because of such arguments. This concerns the Nobel Prizes in science to a still greater extent, because in their meaning and content they are on the whole very far from sports competitions. The only conclusion I wish to draw from all this is the advice, especially to representatives of the mass media, not to make a cult of Nobel Prizes and their winners.

Returning to science itself, I shall dwell on ferrotoroics which I placed in the 'list' for the first time (item 8).

Well known substances in physics are those possessing spontaneous magnetization $\mathbf{M}_s$ (ferromagnets) and spontaneous electric polarization $\mathbf{P}_s$ (pyroelectrics, including ferroelectrics). Less popular, although known for a number of years are ferrotoroics, that is, substances possessing a spontaneous toroidal moment (more precisely, toroidal moment density) $\mathbf{T}_s$. It seems out of place to clarify here what a toroidal moment is[2], the more so as it has recently been done in *Physics – Uspekhi* in a readily available paper [21]. No bright results have as yet been provided by the studies of ferrotoroics, but they are being investigated [22]. The main thing is that in connection with these investigations, hopes are cherished for the discovery of superdiamagnetics which were mentioned in [1] after the comments on problem 2 (HTSC and RTSC).

Interest in fullerenes and nanotubes (problem 9), especially in the latter, is increasing. We try to elucidate the corresponding news in this journal, but it seems irrelevant to give here the latest references to the literature. In paper [1], the advances of laser physics and generally optics were somehow 'kept down'. It is out of place to fill in the gap as a whole now, and I shall therefore restrict myself to mentioning the transition to the study of attosecond pulses (1 as $= 10^{-18}$ s) [23] and the creation of an atomic 'nanoscope' [24] which allows the examination of individual atoms with the help of light. I shall also restrict myself to references to new achievements in the investigation of a comparatively long-lived nucleus with $Z = 114$ [25], quark-gluon plasma [26], and generally 'exotics in the world of elementary particles' (this is part of the title of paper [27]).

4. No new fundamental achievements in elementary particle physics, apart from in connection with the neutrino (see below), have been reported in recent years. This is obviously explained by the fact that the 'cream' from the results obtained on the existing accelerators has already been skimmed, and the Large Hadron Collider (LHC) is hoped to be put into operation only in 2006 or 2007 (this accelerator will yield a total energy of 14 TeV in the center-of-mass system for nucleons). However, new results may be obtained earlier on 'old' accelerators that are now under reconstruction (for example, at Fermilab).

In item 17 of the 'list', the fundamental length is mentioned among other things. In paper [1] I nearly made excuses for leaving this problem in the 'list', for it was especially topical many years before, when the problem of renormalizations in quantum electrodynamics had not yet been solved. In the past decades it has most often been assumed that the only existent fundamental length is

$$l_g = \sqrt{\frac{G\hbar}{c^3}} = 1.6 \times 10^{-33}\ \text{cm} \tag{1}$$

dealt with in cosmology and high-energy physics (here $G = 6.67 \times 10^{-8}$ cm g^{-1}s^{-2} is the gravitation constant, $\hbar = 1.055 \times 10^{-27}$ erg s is the quantum constant, and $c = 3 \times 10^{10}$ cm s^{-1} is the velocity of light; the energy is

$$E_g = \frac{\hbar c}{l_g} \sim 10^{19}\ \text{GeV}\,, \quad m_g = \frac{E_g}{c^2} \sim 10^{-5}\ \text{g}).$$

The existence of another fundamental length $l_f > l_g$ was thought of as improbable and so-to-say unnecessary (the length $l_f < l_{f_0} \sim 10^{-17}$ cm, where l_{f_0} is the characteristic length along which the space is 'probed' on accelerators because $E_{l_{f_0}} = (\hbar c/l_{f_0}) \sim 1$ TeV). But in recent years, after the appearance of paper [1], the problem of the fundamental length $l_f > l_g$ again came out of the shadow and became topical. The point is that the possibility has long been discussed that aside from the usual three spatial dimensions x, y, z and time t, other dimensions also exist and somehow work in the real world (see below). However, it has been customarily believed up to recently that the fifth and all other spatial dimensions are so-to-say compactified with a characteristic size of the order of l_g (roughly speaking, this means that they roll up into 'tubes' with a radius of the order of l_g). Nowadays, the possibility has widely been discussed that one (and, perhaps, more than one) of the 'additional' dimensions is compactified not with radius l_g, but with a different, and possibly much larger radius l_c. As is clear from what has been said above, this radius will in a certain sense play the role of fundamental length l_f (i.e., $l_c = l_f$). I have seen papers in which the additional dimensions had by definition a radius $l_c \gg l_g$, which affects the behavior of the gravitational field. So, the presence of the length l_c may cause a change in the dependence of the gravitational force on the distance between the interacting bodies (particles, etc.). This means, specifically, that the Newton law for the gravitational potential energy $\varphi \propto 1/r$ will be steeper for small r (at present we only know that the law $\varphi \propto 1/r$ is valid for $r \gtrsim 0.01$ cm). I am sure that the corresponding direction of research will be the focus of attention in the near future [28, 29, 69, 70].

5. Now I shall proceed to the problems which in [1] were conditionally referred to astrophysics (items 21 – 30 in the 'list'). Over the three years, the most impressive results have been achieved in this field. True, somewhat earlier (in 1997) a great discovery was made — high-power gamma-ray bursts were found to be of cosmological origin. This fact was reflected in [1], and I shall restrict myself here to mentioning the gamma-ray bursts GRB000131 with a red shift parameter

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[1] By the way, it is precisely the word 'race' that was used, for instance, in [17] to characterize the competition for obtaining BEC in gases.

[2] I shall nevertheless say that a toroidal solenoid with current, which does not possess dipole electric and magnetic moments, has toroidal dipole moment. In such a solenoid, the entire magnetic field directed along the azimuth is concentrated inside the solenoid.

$z = 4.5$, which corresponds to a distance of approximately 11×10^9 light years [30]. Furthermore, it should be emphasized that the radiation of gamma-ray bursts is more likely to be nonisotropic. Moreover, this has obviously been proved of late [64]. The anisotropy of the radiation is so high according to [64] that the estimate of the energy release has been lowered sharply compared to that discussed earlier in the assumption of radiation isotropy (in the latter case, an energy release reaching $W \sim 3 \times 10^{54}$ erg $\sim M_{\odot}c^2$ would be observed). The value $W \sim 3 \times 10^{51}$ erg for a typical energy release is given in [64]. This slightly exceeds the typical energy release in supernova flares. However, the sources of cosmological gamma-ray bursts, sometimes referred to as hypernovae, are hardly associated with supernovae. More probable, for instance, is the collapse of a pair of neutron stars, although the question is not yet clear.

Before 1999, more precisely in 1998, another most eminent discovery of recent years was made, namely, neutrino oscillations were discovered testifying to the fact that neutrinos of at least one flavor (type) have a nonzero mass. This fact was mentioned in [1], but it still needed confirmation. Now, the existence of neutrino oscillations is beyond doubt, and these particular oscillations were found to be responsible for the so-to-say enigma in the field of solar neutrino radiation. I should note that it was precisely the necessity of elucidating the question of solar neutrino radiation as well as the establishment of the accelerating expansion of the Universe and the role of 'dark energy' in cosmology (see below) that stimulated me to write this paper. Without mentioning these two most distinguished discoveries, which naturally could not be done in [1], the picture of the current situation with physics and astronomy would be incomplete.

As was reported in [1], the measured neutrino flux from the Sun turned out to be two or three times smaller than that calculated according to the so-called Standard solar model (in this model certain assumptions are made concerning the structure and temperature in the central part of the Sun). Attempts to somehow eliminate the contradiction by modifying the Sun model (i.e., changing various parameters of the model), which seemed most attractive to theoreticians (to me, in any case), looked less and less realistic as the calculations were specified. Therefore, the hypothesis of the role of neutrino oscillations and, concretely, on the conversion of some of ν_e-neutrinos into ν_μ- and (or) ν_τ-neutrinos on the way from the Sun center to the Earth has come out as the leading one.

In 2001 this hypothesis was brilliantly confirmed [31 – 34].

On the underground setup SNO (Sudbury Neutrino Observatory) containing 1000 tons of ultrapure heavy water (D_2O), the flux of electron neutrinos with energy exceeding 6.75 MeV was measured to be equal to $(1.75 \pm 0.14) \times 10^6$ cm^{-2} s^{-1} [31]. Such a flux makes up only 35% of the flux predicted by the standard model (see above). However, the combination of these data with the results obtained on the Super-Kamiokande setup, which registers all the three types of neutrinos, ν_e, ν_μ, and ν_τ, made it possible to determine the total flux of all the three types (or, alternatively, flavors) of neutrinos emitted by the Sun or, more precisely, coming from the Sun, but observed on the Earth. This flux appeared to be equal to $(5.44 \pm 1) \times 10^6$ cm^{-2} s^{-1} [31]. This value coincides perfectly well with the calculated flux of 5×10^6 cm^{-2} s^{-1} and thus confirms both the validity of the Standard solar model and, what is most important, the existence of neutrino oscillations [31 – 34]. The available data obviously restrict the mass of all neutrinos to the value of (2 – 3) eV; this is the upper limit of perhaps a much smaller mass. The possible contribution of neutrinos to the dark matter mass is obviously not large (several percent at maximum), but no exact values have yet been obtained. Much is to be done, but the main question, or more correctly the enigma concerning the neutrino radiation from the Sun, has already been answered.

A little more than 70 years have passed since in 1930, in a letter addressed to a physical congress, W Pauli, with uncharacteristic shyness, expressed the thought about the existence of neutrinos (see, e.g., [35]). Nowadays, a whole field of physics and astronomy is devoted to neutrinos.

6. I shall now turn to the second distinguished discovery of the past few years which was not reflected in [1]. As has already been mentioned, it concerns cosmology, the structure and evolution of the Universe. As far back as 1981 an important step was made towards generalization of what may now be called the classical relativistic Friedmann cosmology. Namely, the inflation model was proposed in which the Universe 'inflates' very rapidly near the 'singularity' present in a number of cosmological (including Friedmann) models based on the general relativity theory. After such an inflation, the Universe is customarily assumed to evolve according to the Friedmann model. I should state with regret that I do not conceive properly the quantitative aspect of the inflation problem and the inflation model, the more so as it has been subject to criticism [36, 37]. That is why I naturally cannot write about it here. I hope that this important problem will be elucidated in *Physics – Uspekhi* (in this connection I shall only refer the reader now to papers [37 – 39]). Obviously, the very existence of the inflation stage may hardly be doubted, but it is particularly important for us here (in the present paper) that the latest advances in cosmology are largely connected with the post-inflation stage. This was to a certain extent already reflected in [1], where the possible role of the Λ-term and 'vacuum matter' was emphasized. This 'vacuum matter' is now more frequently referred to as 'dark energy' or quintessence.

As is explained in sufficient detail in [1], if the Λ-term is present, everywhere in the Universe there exists 'dark energy' — 'vacuum matter' whose energy density ε_v is equal to (formula (8) in [1])

$$\varepsilon_v = -p_v = \frac{c^4 \Lambda}{8\pi G} , \qquad (2)$$

where p_v is the pressure and Λ is the constant encountered in the main Einstein equation in general relativity (formula (7) in [1]).

When Λ and, therefore, the density ε_v are positive, the pressure p_v is negative, which exactly corresponds to gravitational repulsion (antigravity). The latter point was not clarified in [1], and it should be done now.

The matter is that in general relativity the 'acting gravitational mass' of unit volume is equal to $(\varepsilon + 3p)/c^2$ (see, e.g., [40]). Then clearly the pressure may be said to have weight. So, for the equation of state (2) for $\varepsilon_v > 0$ (which means that $\Lambda > 0$) the 'gravitational mass' density is $-2\varepsilon_v/c^2$, i.e., is negative ('antigravity'). In other words, at a negative pressure p it 'works' against the normal gravitational attraction (formally, no 'gravitational masses' and 'forces' exist in general relativity, and since I am using the classical language I put quotation marks in corresponding places).

The smoothed-out and homogeneous cosmological Friedmann model is characterized first of all by the density $\rho = \varepsilon/c^2$ of all matter. For the spatially flat Einstein-de Sitter model (with the Euclidean space metric), which is now considered to be realistic (this assertion is also a great achievement of recent time; see [38, 39, 46, 47]) we have

$$\rho = \rho_{\mathrm{c}} = \frac{3H^2}{8\pi G}\,, \tag{3}$$

where H is the Hubble constant relating the velocity v of cosmological expansion to the distance r to a corresponding object; in the Hubble law $v = Hr$. In our epoch, the value $H_0 = 64 \pm 13\,\mathrm{km\ s^{-1}\ Mpc}$ for $H = H_0$was given in [1]. In [41] the value $H_0 = 71 \pm 8\,\mathrm{km\ s^{-1}\,Mpc^{-1}}$ is presented. For $H_0 = 64\,\mathrm{km\ s^{-1}\,Mpc^{-1}} = 2.07 \times 10^{-18}\,\mathrm{s^{-1}}$, the critical density ρ_{c} is now equal to

$$\rho_{\mathrm{c}_0} = \frac{3H_0^2}{8\pi G} \simeq 8 \times 10^{-30}\ \mathrm{g\ cm^{-3}}. \tag{4}$$

Instead of ρ the quantity $\Omega = \rho/\rho_{\mathrm{c}}$ is more often used. In the flat model, obviously, $\Omega = \Omega_{\mathrm{c}} = 1$. The Ω value receives the contributions of baryons (b), dark matter (d) and, finally, dark energy (we shall denote it by the index Λ, although the use of the index v would be more correct, see below). Thus,

$$\Omega = \Omega_{\mathrm{b}} + \Omega_{\mathrm{d}} + \Omega_{\Lambda}\,. \tag{5}$$

The contributions of electrons and photons are neglected here, which for our purposes is quite normal (see [39]). Within the general relativity theory, without any of its extensions, the Λ-term (more precisely the quantity Λ itself), as far as I understand, is a constant. This quantity (in the assumption of its constancy and with the use of data for our epoch) is fairly small, say, $\Lambda_0 \sim 10^{-56}\ \mathrm{cm^{-2}}$ (this value corresponds to the estimate $\Omega_\Lambda \sim 1$ for the quantity Ω_Λ). The question of the Λ-term and related issues has been widely discussed [42–49]. Some theoretical estimates of the quantity Λ led to very large values, whereas this term is incomparably smaller in our epoch. For this reason, multiple attempts have been made to prove or somehow substantiate that the actual Λ value is zero [42]. The observations, however, suggested the conclusion that $\Lambda \neq 0$ and, moreover, the value of Λ (or, more precisely, Ω_Λ) turned out to be so large compared to the other contributions to Ω, that in our epoch the Universe expansion rate does not decrease but on the contrary increases [39, 43, 44, 47, 48, 50]. And this was the latest remarkable discovery in cosmology.

In [1] I have already presented the values of Ω_{b}, Ω_{d}, and Ω_Λ for $\Omega \simeq 1$, namely, $\Omega_{\mathrm{b}} = 0.03 \pm 0.015$, $\Omega_{\mathrm{d}} = 0.3 \pm 0.1$, and $\Omega_\Lambda = 0.7 \pm 0.1$ (provided that $\Omega = 1$). Now I am aware of the more precise estimates: $\Omega = 1$ and $\Omega_{\mathrm{b}} = 0.044 \pm 0.01$ (see [31]; this paper also points to the good agreement between the theory and observational data on the chemical composition of matter in the Universe). However, it was not said in [1] that the given high value of Ω_Λ testifies to the acceleration of the Universe expansion in our epoch.

It should be emphasized here that the important role of 'dark energy' does not obviously give cause to any doubt. However, the use of the equation of state (2) in application to the dark energy is problematic. The equation of state

$$\varepsilon_{\mathrm{v}} = \frac{1}{w}\,p_{\mathrm{v}}\,, \quad w < 0\,, \tag{6}$$

is possible and generally leads to close results. Obviously, the value $w = -1$ corresponds to the Λ-term. Such 'dark energy' is also called quintessence [43, 44]. In paper [44], quintessence is associated with the existence of a certain scalar field Φ. If both the Λ-term and quintessence are simultaneously present, then their contributions to Ω_Λ [see (5)] are obviously simply summed up. In this connection, as has already been mentioned, it would be more correct to introduce the notation Ω_v implying the contribution to Ω of the entire 'vacuum matter' (dark energy) irrespective of its concrete definition. The w value in (6) can in principle be determined from observations [51]. Generally, this whole problem is of course in the early stage of research, but we are undoubtedly dealing here with one of the 'hottest' issues of modern cosmology.

Astronomy, which is presently inseparable from so-called cosmic studies, is now developing very rapidly. Naturally, its recent advances and specifically those reported after the publication of [1] are not exhausted by what has been said above. This refers, in particular, to gamma astronomy [30, 52, 53] and to high-energy cosmic and gamma ray studies. For example, interesting is the discovery of hard gamma rays emitted by the remnants of supernova Cas A [53]. Generally, the detection of X-ray and gamma-ray photons from remnants of various supernovae is continuing. Sources of such photons are probably relativistic electrons accelerated as a result of flares.

On the Large Hadron Collider (LHC), which is to be put into operation in CERN most probably in 2006 or 2007, the proton energy in each of two colliding beams will make up 7 TeV. This is the highest energy to be reached on accelerators in the near future (clearly, the total energy in the center-of-mass system will amount to 14 TeV). A proton of energy $3 \times 10^{20}\,\mathrm{eV} = 3 \times 10^8\,\mathrm{TeV}$ (the highest energy observed in cosmic rays) colliding with a resting nucleon has in the center-of-mass system an energy of nearly 400 TeV. Thus, in spite of a very low intensity at superhigh energies, cosmic rays will obviously long remain an interesting object for high-energy physics (for super-high energy cosmic rays see reviews [54, 55]).

Concluding the enumeration of the achievements in astrophysics, including cosmology, I think it would be wrong to ignore the existence of some unorthodox views. In science, as is well known, there has always been a struggle of ideas and opinions. And it is not infrequent that conventional views and theories are disputed, including the relativity theory and quantum mechanics which are being contested even today. In respect of these two great theories all critical remarks known to me do not seem to be serious. The above-mentioned unorthodox views in astrophysics belong to F Hoyle and G Burbidge who can by no means be considered ignorant dilettantes (suffice it to say that G Burbidge is editor at a very reputable journal "Annual Review of Astronomy and Astrophysics"). And these authors deny the cosmology of "Big Bang" [67] and assert that there exists a substantial non-cosmological red shift in galactic spectra [67, 68]. As to the "Big Bang" I somehow fail to find weighty arguments for criticism, but in what concerns the red shift, all the references in [68] are made to observational data. I cannot judge of these data and of whether or not their denial is sufficiently grounded or, more precisely, whether they may be thought of as unconvincing. It is desirable that this question be fully clarified.

7. In [1] I also touched upon three 'great' problems: i) the entropy increase and the 'time arrow', ii) the interpretation and possible perfection of nonrelativistic quantum mechanics, and iii) reductionism, i.e., the possibility of so-to-say reducing biology to physics. I cannot say anything new concerning these fields of research. At the same time, judging by the literature, interest in the interpretation of quantum mechanics is not exhausted. In the USSR and Russia, a free discussion of this problem was in fact for many years forbidden, but after the censorship was repealed, no discussions occurred, perhaps, merely from force of inertia. Meanwhile, the value of discussions of methodological questions of natural science is doubtless (at least, in my opinion). In order to somehow stimulate the activity in this direction, a large paper by M B Menskiĭ "Quantum mechanics: new experiments, new applications, and new formulations of old problems" was published in *Physics – Uspekhi* [56]. Then six responses to this paper appeared [57] where the authors might of course express any views. The proceedings [58] of the conference devoted to the centenary of quantum theory will serve the same purpose of free exchange of opinions on the interpretation of quantum theory. Unfortunately, I derived little from these discussions.

I hope the fact that the goal of the present paper is rather limited was sufficiently stressed from the very beginning. I only wished to somehow supplement paper [1] and, if you like, to update it. The emphasis is laid on the information about new actual advances and results. Meanwhile, in physics itself and in the literature devoted to it great attention is paid to the study and discussion of quite numerous methodical and technical questions and, what is important, to new approaches to the solution of many vague problems. Such vague and very deep problems are not few, especially in elementary particle physics and cosmology, although they also exist in condensed-matter physics and in some other fields. But one thing are hopes or, as Landau would say, 'phys-hopes', and another thing are results and factual advances. 'Phys-hopes' and their discussions are often not at all less interesting, and in a sense even more attractive than already obtained results. But obviously I said rather little of 'phys-hopes' in this paper, and in this connection I shall only refer the reader to papers [59 – 63]. I think, nevertheless, that this paper will be helpful and will to a certain extent fulfill the task I set.

Obviously, the reader should at the same time gain much additional information on all the questions touched upon in the paper. We try in *Physics – Uspekhi* to go in this direction. But one should not forget that the possibilities of *Physics – Uspekhi* are limited, and it cannot, along with its main functions, be a journal of the type of Physics Today and Physics World. That is why, I am convinced that in Russia it is high time to begin publishing a journal "Physics and Astronomy Today", analogous to the above-mentioned monthlies (for more details in this respect see my paper [65]).

In conclusion I take the chance to thank V A Rubakov for valuable comments he made on reading the manuscript.

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[3] This paper is also placed in [4, 5] as article 6.

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