SOVIET PHYSICS USPEKHI VOLUME 14, NUMBER 1 JULY-AUGUST 1971

53 + 523

Physics of Our Days

WHAT PROBLEMS OF PHYSICS AND ASTROPHYSICS ARE OF SPECIAL IMPORTANCE AND INTEREST AT PRESENT?

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Usp. Fiz. Nauk 103, 87–119 (January, 1971)

INTRODUCTION

PHYSICISTS and astrophysicists are engaged at the present time in the study of a tremendous number of different problems. In the overwhelming majority of cases, one deals with perfectly reasonable problems, attempts aimed, if not at solving the riddles of nature, at least at learning something new. It is difficult to say with respect to any such problem that it is not interesting or not important. And it is in general difficult to define in some consistent manner what is "unimportant" and (or) "not interesting" in science. Yet there is no doubt that a hierarchy of problems and tasks does exist. Its effects are felt in practice, it is reflected throughout scientific (and sometimes also in nonscientific) life, and it can even be discerned in the tables of contents of journals. The singling out of an "especially important physics problem" is often due to its potential technical or economic implications and is sometimes connected with some puzzling aspect of the problem, but at times it is a bow to the prevailing fashion or occurs under the influence of some inexplicable or chance factor.

Many lists of "most important problems" and commentaries concerning these lists have been compiled. In such cases a conference is usually convened or special commissions are organized, to meet frequently (to "suppress noise," sometimes at various resorts) and generate quite voluminous documents. Without undertaking to make generalizations, I can state that I have never known these proceedings concerning the most important problems to be read by anyone with consuming interest. Specialists seemingly have no great need for them, and the representatives of the broader "public" are not attracted by them.

Yet can the budding physicists and astronomers, and not only they, fail to be interested in the simple question: what is now "hot" in physics and astrophysics? In other words, what problems of physics and astrophysics are at the present time particularly important and interesting? Starting from the premise that such questions are indeed of interest, at least to physics students, I attempted to answer them in a lecture,* a revised text of which is called to your attention. Thus, this is not the labor of a commission or even the result of special research, as the literary specialists call it.

I list below problems which I consider to be presently particularly important and interesting, but without defining these concepts themselves and without motive in the character of the selection. Everyone has the right to have his own opinion concerning this question, and need not "reconcile" his opinion with any one else's, as long as no attempt is made to have this opinion accepted as the approved one or better than all other possible opinions. No such attempt, let alone proposals of organizational nature, are being made here, and I wish to emphasize the "personal" character of the exposition, without even attempting, as is done in the scientific literature, to avoid the use of the pronoun I, me, etc.

It would be of curious interest, and perhaps even useful, to compare lists of the "most important problems of physics and astrophysics" that had been compiled by different persons. Unfortunately, as far as I know, no appropriate sampling of opinion has been made in the scientific community. I can therefore only advance the suggestion that most such lists would have very much in common, provided only one would agree, and this is not easy, on one point: what are we to call a "problem" of physics, as distinct from, say, fields, trends, objects of physical research? Again, without going too deeply into the definition, let me remark that to me a problem is a question the character (content) of the answer to which remains unclear to an appreciable degree. We should not be speaking of technical developments, the need for performing a series of measurements, etc., but of the possibility per se of creating some substance with unusual properties (for example, a high-temperature superconductor), of elucidating on the limits of applicability of a theory (for example, general relativity theory), or of solving some basic mystery (say, explaining why combined parity is violated in K-meson decay). On the basis of just such considerations, no mention will be made below of quantum electronics (including most applications of lasers, let alone the development of laser technology itself), of many problems in semiconductor research (including the problem of miniaturization of circuits and instruments), nonlinear optics and holography, and also of many other interesting trends of modern optics, problems of computer technology (including the problem of creating new types of computers), and many others. The great importance of all the aforementioned trends and the abundant variety of not only technical but also physical questions associated with them (see, for example, [1]) are established beyond doubt. But I see at present no fundamental "physical problem" in these matters, or, if you will, no essential "uncertainty" involved in their physics. Prior to the development, say, of the first laser, such uncertainty still existed, although the principles that subsequently served as the basis of laser design were quite clear. On the other hand, to increase the power or to change the other parameters of the laser, or of any other device, is a necessary task, difficult and honorable, but clearly qualitatively different from the problem of developing an entirely new instrument.*

The subjective bias and the debatability of these remarks are obvious, but the reader has already been warned that this is in the character of the article (and furthermore, warnings and stipulations are usually of no help). Before we finally proceed to the content of the article, it remains to be noted that its subdivision into three parts (macrophysics, microphysics, astrophysics) is also quite arbitrary. Thus, the problem of superheavy nuclei is regarded as macrophysical, although it can be also considered microphysical. Analogously, general relativity is dealt with in astrophysics and not in macrophysics only because general relativity is used mainly in astronomy (let alone the fact that the distinction between astrophysics and, say macrophysics is entirely different in character from the subdivision of physics into microphysics and macrophysics). We emphasize, finally, that we do not touch upon biophysics at all, let alone less important scientific trends bordering on physics.

I. MACROPHYSICS

1. Controlled Thermonuclear Fusion

The problem of controlled thermonuclear fusion will be regarded as solved when nuclear fusion reactions can be harnessed for power-generating purposes. The main reactions dealt with here are (d and t—deuterium and tritium nuclei, p—proton, n—neutron):

$$\left.\begin{array}{c} d+d\to {}^3\mathrm{He}+n+3.25\ \mathrm{MeV},\quad d+d\to t+p+4.0\ \mathrm{MeV}, \\ d+t\to {}^4\mathrm{He}+n+17.6\ \mathrm{MeV}. \end{array}\right\} \tag{1}$$

Certain other reactions may also play a role, especially the reaction $^6\mathrm{Li} + \mathrm{n} \to \mathrm{t} + {}^4\mathrm{He}$, which makes it possible to use neutrons and regenerate tritium at the same time.

It is difficult to doubt the eventual utility of the nuclear fusion reaction—it suffices to mention the "trivial" possibility of employing underground explosions. On the other hand, controlled fusion has been under persistent study for 20 years,[2] but the outlines of the future thermonuclear reactor are still far from clear.[3]

The simplest concept of a reactor would be a plasma reactor with magnetic confinement of the plasma. Specifically, the toroidal magnetic traps (of the tokamak or stellarator type) are most progressive. However, no one has succeeded in suppressing all types of plasma instability in such traps, and the effective heat conduction at the walls is by no means small. Therefore, according to [3d], using the presently attained degree of thermal insulation of the plasma, the development of a self-maintaining reactor (i.e., the attainment of an en-

*Each year, the Department of Physics and Astrophysics Problems of the Moscow Physicotechnical Institute offers senior students a small cycle of 8–10 lectures devoted to various topical problems of physics and astrophysics. The lecture on which the present article is based was delivered on 17 September 1970 as an introduction to the aforementioned cycle.

*This does not apply, of course, to realization of x-ray or gamma-ray lasers, which might possibly be called rasers and gasers, respectively. The problem of developing a raser or a gaser should be included in our "list," but we confine ourselves to their mention in the present footnote. Another problem that will not be mentioned below, for more or less fortuitous reasons, is the elucidation of the nature of ball lightning and its synthesis in the laboratory.

ergy yield) even in an equal-component mixture of deuterium and tritium with a magnetic field of $H = 10^5$ Oe, would require that the small radius of the torus (in a trap of the Tokamak type) be $a = 14$ m. And if heat conduction (leakage) could be reduced by two orders of magnitude, then we would have $a = 1.4$ m, but it would still be necessary to produce a colossal field in volumes of the order of many cubic meters. Of course, this could be done, if at all, only by using superconducting magnets (otherwise, in addition to everything else, there are no grounds for expecting a favorable energy balance).

Such exceptional difficulties, which may turn out to be even more staggering when real systems are approached, fully justify the consideration of other approaches to the solution of the problem. There are many published suggestions in this direction: the use of "open" magnetic traps, the use of a short-duration discharge ("fast pinch"), the use of a high-frequency discharge in a plasma, heating of deuterium dust particles or plates with powerful electron beams or with the aid of laser radiation, acceleration of the particles and their use for the heating of deuterium, etc.

Since I myself was occupied with the problem of thermonuclear fusion only in 1950–1951 (see [2d]) I am not competent to evaluate the present status of this problem. Nonetheless, I permit myself to remark that just as at the dawn of the corresponding research, I intuitively regard quasistationary closed magnetic traps as most attractive from the point of view of obtaining energy (and not its utilization!). But the realization of controlled thermonuclear fusion should nonetheless be included among the physical problems, because ways to its solution are not yet clear. By the same token, competition between different trends and suggestions would be natural and necessary.

2. High-temperature Superconductivity

Superconductivity was discovered in 1911 and for many years remained not only an unexplained phenomenon (perhaps the most puzzling in the field of macrophysics), but also one with almost no practical applications. The latter was due primarily to the fact that superconductivity was observed, up to the most recent times, only at low temperatures. Thus, the chronologically first observed superconductor, mercury, has a critical temperature $T_c = 4.1°K$. The highest known value, $T_c \approx 20°K$, is exhibited by a certain alloy of Nb, Al, and Ge, which was investigated only recently (a better-known compound is Nb_3Sn with $T_c \approx 18.1°K$, the superconductivity of which was observed in 1954). At temperatures close to T_c (but of course below it, since by definition, a metal ceases to be superconducting at $T > T_c$), it is very difficult to use the superconductor. It suffices to state that in this region the critical magnetic field H_c and the critical current I_c, i.e., the field and current that destroy the superconductivity, are quite low (as $T \to T_c$ the values of H_c and I_c tend to zero). In view of the foregoing, superconductors can be used at present only if helium (boiling point $T_b = 4.2°K$) is used as a coolant, since liquid hydrogen (boiling point $T_b = 20.3°K$) has already solidified at $T_m = 14°K$ (the use of a solid coolant is in general both difficult and inconvenient).

As recently as 25–30 years ago, little helium was produced (there is still a shortage of this material), and the technique for its liquefaction was imperfect. As a result, there were in the entire world only a few low-capacity helium liquefiers. The use of superconductors to construct superconducting magnets (and this is still the most important instrument in which superconductors are used) was no less limited by the low values of H_c and I_c for the previously known materials (for Hg we have $H_c \approx 400$ Oe even as $T \to 0$).

At the very beginning of the last decade, however, the situation changed radically. It is now no problem to obtain liquid helium. In the presence of proper organization, the laboratories and institutes need no liquefiers at all, and order the desired amounts of liquid helium from a special firm or factory (it is transported in large dewars). The "magnetic and current barrier" was also overcome, and superconducting materials have been developed that are suitable for the construction of magnets with fields H_c reaching several hundred kOe (the superconductivity of the already mentioned alloy of niobium, aluminum, and germanium, with $T_c \approx 21°K$, vanishes only in a field $H_c \sim 400$ kOe). To be sure, the critical current and the field for the practically employed materials are not yet large enough to produce a magnet for 300–400 kOe. But this is evidently only a matter of technology and engineering. Apparently there are no fundamental obstacles to the development of magnets, say, for 300 kOe at helium temperatures.* Conversely, a fundamental and unclear aspect is the highly enticing possibility of developing high-temperature superconductors, i.e., metals that remain superconducting at the temperatures of liquid nitrogen ($T_b = 77.4°K$), and, better still, at room temperature.

I have treated the present status of the problem of high-temperature superconductivity in greater detail in reviews[4]. This is all the more reason for confining myself here to only a few remarks.

Superconductivity arises if the electrons in a metal are attracted to one another near the Fermi surface, as a result of which they form pairs that experience something similar to Bose-Einstein condensation. The critical temperature for the superconducting transition T_c is proportional to the binding energy of the electrons in the pair and, roughly speaking, is determined by two factors: the force of attraction (binding) which can be characterized by a certain parameter g, and by the width $k\theta$ of this energy region near the Fermi surface where attraction between electrons still exists. Here

$$T_c \sim \theta e^{-1/g}. \qquad (2)$$

Apparently we always have $g \lesssim 1$, and for some models even $g \leq 1/2$; for most known superconductors $g \lesssim 1/3 - 1/4$ [formula (2) is directly suitable precisely for $g \ll 1$]. The temperature θ in (2) depends on the mechanism that leads to the attraction between the electrons. In certain cases this mechanism is apparently determined by the interaction of the electrons with the lattice. In this case $\theta \sim \theta_D$, where θ_D is the Debye temperature,

*The development of superconductors with high values of H_c and I_c was basically the result of a large amount of experimental and technological work. Theory did not play a decisive role in this matter, especially where the high critical currents are concerned.

the physical meaning of which is that $k\theta_D$ is the energy of the shortest-wavelength phonons in the body. The wavelength of such phonons is $\lambda \sim a \sim 3 \times 10^{-8}$ cm (a is the lattice constant) and $k\theta_D \sim \hbar\omega_D$, $\omega_D \sim u/a \sim 10^{13}$–$10^{14}$, where $u \sim 10^5$–10^6 cm/sec is the speed of sound. Thus, $\theta_D \sim 10^2$–10^3°K.

For $\theta_D = 100°$ and $g = 1/2$, in accord with (2), we have $T_c \sim \theta_D e^{-2} = 13.5$°K, and in general for the phonon mechanism $T_c \lesssim 30$–40°K. By the same token, on the one hand, the possibilities for increasing T_c by traditional methods (the creation of new alloys and their treatment) are still far from completely exhausted (let alone substances of the type of metallic hydrogen; see below). On the other hand, it is understandable why it is difficult, or more probably impossible, to expect the phonon mechanism to serve as the basis for truly high-temperature superconductors with $T_c \gtrsim 80$–300°K (here, too, we leave aside metallic hydrogen).

Hopes for obtaining high-temperature superconductors are connected with the use of the exciton mechanism of attraction between electrons. In this case we have in (2) $\theta \sim 10^3$–10^5°K, and if sufficiently strong coupling were ensured ($g \gtrsim 1/5$), then T_c would turn out to be even larger. For the reason already mentioned (the availability of the reviews[4]) and for lack of space, we shall not deal in detail here with the prospects for and methods of applying the exciton mechanism. It suffices to state that there are several such ways, but there has been little concrete research at the required level. Whereas the problem of thermonuclear fusion has been under attack continuously for 20 years, research in the field of high-temperature superconductivity is only now beginning. On the other hand, it is possible that in this case there may be no need for some supercomplicated synthesis of new substances, and one cannot exclude the possibility of success by means of relatively modest (albeit modern) means. I would therefore not be surprised to read of the development of a high-temperature superconductor in the next number of a physics journal (moreover, this would in all probability create a sensation and we would learn the news from the newspapers or from broadcasts). But it is no less probable that high-temperature superconductors will be very difficult to develop, and perhaps impossible in principle. Briefly speaking, the question still remains open, and attempts to answer it are exceedingly fascinating.

3. New Substances (Metallic Hydrogen, Anomalous Water, etc.)

There exist on earth, under natural or artificial conditions, tremendous numbers of different substances (chemical compounds, alloys, solutions, polymers, etc.). The creation of new substances, generally speaking, is in the jurisdiction of chemistry or technology, and is not regarded as a physical problem. The situation changes when one deals with quite unusual (exotic, if you please) substances. These include the already mentioned high-temperature superconductors, and by way of two other examples we point to metallic hydrogen[5] and anomalous water.[6]

As is well known, under ordinary conditions (say, at atmospheric pressure), hydrogen is molecular, boils at $T_b = 20.3$°K, and solidifies at $T_m = 14$°K. The density of solid hydrogen is $\rho = 0.076$ g/cm^3, and it is a dielectric. But under sufficiently strong compression, when their outer atomic shells are crushed inward, all substances go over into the metallic state. A rough estimate of the density of metallic hydrogen can be obtained by assuming that the distance between the protons is on the order of the Bohr radius $a_0 = (\hbar^2/me^2) = 0.529 \times 10^{-8}$ cm. Hence $\rho \sim Ma_0^{-3} \sim 10$ g/cm^3 ($M = 1.67 \times 10^{-24}$ g is the proton mass). Quantitative calculations lead to a lower density: according to [5a], molecular hydrogen is in thermodynamic equilibrium with metallic hydrogen at a pressure $p = 2.60$ Mbar, when the density of the metallic hydrogen is $\rho = 1.15$ g/cm^3 (the density of molecular hydrogen is in this case $\rho = 0.76$ g/cm^3). It is possible that metallic hydrogen is superconducting, and with a high value of T_c, reaching 100–300°K (for metallic hydrogen the Debye temperature is $\theta_D \approx 3.5 \times 10^3$°K; therefore, according to formula (2) with $g \leq 1/2$, we get $T_c \lesssim 500°$).

To obtain such a very simple (in certain respects) metal as metallic hydrogen, and to determine its critical temperature T_c, is obviously a matter of physical interest, and may also be of timely astrophysical significance (it suffices to state that large planets should contain considerable metallic hydrogen; see [5b]). But incomparably more important is the possibility that metallic hydrogen may remain stable (we mean, of course, metastable) even without pressure. The existence of such perfectly stable metastable modifications is universally known (as example is diamond, which at low temperature and pressure has a higher free energy than graphite). Insofar as we know with respect to metallic hydrogen, the question of its stability in the absence of pressure remains open. If the appropriate calculations give grounds for hope for an affirmative answer (i.e., offer evidence of stability of metallic hydrogen or its alloys with heavy elements even at zero pressure), then the creation of metallic hydrogen and its alloys will become one of the most important problems of macroscopic physics. Incidentally, as is clear from the foregoing, this problem is sufficiently interesting in any case.

Another already mentioned example of new substances is anomalous water (also called superdense or polywater). It was stated in [6a] that under certain conditions (specifically, for example, in quartz capillaries), pure water forms a certain new modification with density 1.4 g/cm^3 and with many other properties that differ greatly from the properties of ordinary water. It was proposed that polymer molecules of the type $(H_2O)_n$ are involved here. These results were, it would appear, fully confirmed;[6b] anomalous water was also obtained in capillaries containing no silicon.[6c]. On the other hand, recent communications[6d] state on the basis of a number of experimental data that "anomalous water" is a mixture of ordinary water and a number of impurities (hydrosols, HNO_3, Na, Cl, etc.).

Thus, the question should be regarded as open, although, in my opinion, the communications[6d] leave little hope for the existence of pure polymer (superdense) water. Regardless of the final answer, however, the investigations already performed indicate how difficult it is to answer such a question as the possible appearance of a new form of one of the most abundant substances;* theoretical calculations[6e] are likewise not reliable in this case. This example is instructive in many respects, particularly as a reminder of the need to regard any discovery as finally established only after repeated and exhaustive verification.

4. The Metallic Exciton (Electron-hole) Liquid in Semiconductors[8]

If a semiconductor contains electrons and holes (produced, say, by illumination), then at sufficiently low temperature they should combine into excitons—hydrogenlike "atoms" related to positronium. The energy and radius of such excitons in the ground state are, in first approximation, of the order of

$$E_{0,\mathrm{e}} \sim \frac{e^4 m_{\mathrm{eff}}}{2\varepsilon^2 \hbar^2} = \frac{E_0 m_{\mathrm{eff}}}{m\varepsilon^2}, \qquad a_{0,\mathrm{e}} \sim \frac{\hbar^2 \varepsilon}{m_{\mathrm{eff}} e^2} = \frac{a_0 \varepsilon m}{m_{\mathrm{eff}}}, \tag{3}$$

where $E_0 = e^4 m/2\hbar^2$ and $a_0 = \hbar^2/me^2$ are the known Bohr expressions for the energy and radius of the hydrogen atom, m_{eff} is the effective mass of the electron and of the hole (these masses are assumed to be equal here, and anisotropy is disregarded), and ϵ is the dielectric constant of the semiconductor.

Since in a number of cases $\epsilon \lesssim 10$ and $m_{\mathrm{eff}} \lesssim 0.1m$, it becomes clear that the exciton radius is $a_{0,e} \gtrsim 10^{-6}$ cm, and that the exciton energy is $E_{0,e} \gtrsim 10^{-2}$ eV $\sim 100°$K. Obviously, these changes (compared with the hydrogen atom) are connected with a weakening of the Coulomb attraction by a factor ϵ, and also with the smallness of the effective mass m_{eff} (compared with the mass of the free electron m).

As already mentioned in connection with the problem of metallic hydrogen, the criterion of high density and metallization, roughly speaking, reduces to a commensurability of the dimension of the electron shell with the internuclear distance. In the case of excitons in a semiconductor, this means that their aggregate remains dense at a concentration $n_e \sim a_{0,e}^{-3} \lesssim 10^{18}\ \mathrm{cm}^{-3}$. Thus, for excitons, the high density reached for hydrogen at pressures of millions of atmospheres corresponds to the perfectly usual density of electrons and holes in semiconductors, $n \sim 10^{18}\ \mathrm{cm}^{-3}$. Even the mere possibility of imitating a region of superhigh pressures in a semiconductor makes our problem sufficiently important. This conclusion becomes reinforced if we think of the possible behavior of a dense system of excitons in a semiconductor. Such a system should become liquid and form drops. More readily, these drops are an electron-hole metal, i.e., they are similar to a liquid metal, although the possibility of "molecular" structure is not excluded, in which case they are analogous to liquid hydrogen, which consists of molecules H_2 (the role of the molecules in the molecular, and consequently dielectric, exciton liquids is played by biexcitons—two excitons that are joined together). In an electron-hole (exciton) liquid one can observe, in principle, superconductivity or superfluidity. Briefly speaking, the exciton (electron-hole) liquid in a superconductor should have a number of very interesting properties and features, which depend, of course, on the characteristics of the "container" employed—the semiconductor. An experimental investigation of this problem has already been started.[8] It can be assumed that within the next few years it will be at the center of attention of semiconductor physics.

*It is curious that the "hypothesis" of the existence of a new form of ice (ice-9) is the basis of the "scientific" part of a well known science-fiction novel [7].

5. Second-order Phase Transitions (Critical Phenomena)

The superconducting transition, the transformation of helium I into superfluid helium II, the transition from the paramagnetic to the ferromagnetic state, many ferroelectric transitions, and many transformations in alloys, all are widely known examples of second-order phase transitions.[9] In such transitions there is no release (or absorption) of latent heat and there is no jump in the volume or in the lattice parameters, i.e., in a certain sense the transition can be regarded as continuous. At the same time, jumps of the specific heat, compressibility, and other quantities are observed at the transition point, near which many of them behave in an anomalous manner. Thus, the specific heat for the helium I $\rightleftharpoons$ helium II transition and certain other transitions is fairly well described by the law $c \sim \ln|T - T_c|$, where T_c is the transition temperature (the λ point). In the case of the ferromagnetic and ferroelectric transitions, the magnetic permeability and the dielectric constant, respectively, tend to infinity as $T \to T_c$, and are frequently described approximately by the Curie law $\chi \sim |T - T_c|^{-1}$.

Related to second-order phase transitions are certain first-order transitions close to the so-called critical Curie point (see [9a]). The gist of this phenomenon is that when several parameters change (for example, the pressure) the second-order transitions can become first-order transitions (the point at which the curves for transitions of different types change into one another on the p-T diagram is called the critical Curie point). Naturally, first-order transitions close to the critical Curie point are related to second-order transitions (the latent heat of the transition differs from zero but is small; at the same time, an anomaly is observed in the specific heat, etc.; such transitions include, for example, certain ferroelectric transformations and apparently the $\alpha \rightleftharpoons \beta$ transition in quartz). Finally, the liquid-vapor (gas) critical points and a few others are analogous to second-order phase transitions.

The problem of second-order phase transitions (and transitions closely similar to them; see also [9h]) obviously consists in the attainment of a sufficiently complete qualitative and quantitative understanding of the different phenomena near the transition points. In particular, we are dealing with the determination of the temperature dependence of all the quantities—their dependence on the difference $(T - T_c)$.

The continuous character of second-order transitions makes it natural to consider them on the basis of an expansion of the thermodynamic quantities (for example, the thermodynamic potential) in powers of a certain parameter η, which vanishes at equilibrium when $T \geq T_c$. Further, the coefficients A, B, C, etc., of the corresponding expansion

$$\Phi = \Phi_0 + A\eta^2 + B\eta^4 + C\eta^6 + \ldots$$

are in turn expanded in powers of $(T - T_c)$, so that near a typical second-order transition we have $A = A'(T - T_c)$ and $B = B_0 = \text{const}$. Such an approach, inspired by Gibbs and van der Waals, was systematically developed by Landau.[9a].

Within the framework of the Landau theory, the susceptibilities obey the Curie law $\chi \sim |T - T_c|^{-1}$; the magnetic or electric spontaneous polarizations M and P vary when $T < T_c$ like $M \sim \sqrt{T_c - T}$, $P \sim \sqrt{T_c - T}$, etc. At the same time, Landau's theory does not in general explain the anomalous temperature behavior of the specific heat and other quantities as $T \rightarrow T_c$. In addition, more detailed measurements have shown[9b] that the Curie law and similar relations are not exact, so that $\chi \sim |T_c - T|^{-\gamma}$ and $M \sim (T_c - T)^{\beta}$, with $\gamma \neq 1$ and $\beta \neq \frac{1}{2}$.

Landau's theory leads to the same results as the model theories (such as the well known Weiss theory of ferromagnetism), in which the method of the self-consistent (sometimes called molecular) field is used. It is clear both from this fact and from the gist of the matter that the limitations of the Landau theory result from neglect of the fluctuations. That is, the mean value, say, of the magnetization M was considered. Yet as $T \rightarrow T_c$ this mean value $M \rightarrow 0$, whereas the fluctuations of M not only fail to vanish, but conversely, increase strongly. It is therefore understandable why the region of applicability of the Landau theory, which differs for different transitions, is a region where the fluctuations are relatively small.[9c]. In the vicinity of the transition point, i.e., if the difference $T_c - T$ is sufficiently small, it is necessary to take the fluctuations into account, and this leads to anomalous behavior of the specific heat, to deviations from the Curie law $\chi \sim (T_c - T)^{-1}$, etc.

A consistent theory of second-order phase transitions for three-dimensional systems has not yet been constructed, although an exceptionally large amount of effort has been exerted towards the solution of this problem.* But all these efforts have not been in vain, and while the problem has not yet been solved, many important results have been obtained in recent years. Foremost among these are similarity laws[9b,d] that make it possible to connect the temperature dependences of different quantities near the transition point T_c. By virtue of these laws, when account is taken of certain experimental data, it becomes possible to predict, for example, that as $T \rightarrow T_c$ we should have for the magnetic susceptibility $\chi \sim (T - T_c)^{-\gamma}$ with $\gamma = \frac{4}{3}$ (in place of $\gamma = 1$ in accord with the theory of Weiss or Landau).

The development of a consistent theory of second-order phase transitions, with allowance for differences that are characteristic of different transformations and generalization of all the results to include kinetic processes near T_c, remains one of the central problems of solid-state physics.

By way of example, we present two more concrete problems, the choice of which (from among the others) may be accidental and is dictated only by my own interests. The first problem is the behavior of helium II near the λ point. In the Landau superfluidity theory, the density of the superfluid component of helium ρ_s is assumed to be a certain given function, say of the temperature T and of the pressure p. But from the viewpoint of the general theory of second-order phase transitions, the density ρ_s cannot be specified, and must itself be determined from the condition that the thermodynamic potential be a minimum. Such an approach[9e] leads to a number of interesting consequences—to dependence of $T_c \equiv T_\lambda$ and of the specific heat c on the thickness of the film of helium II, to inhomogeneity of ρ_s near a solid wall or near the axis of a vortex in helium II, etc. Apparently all these conclusions agree with reality, but on the whole the development of a theory of superfluidity of helium II near the λ point has not yet been completed. Another example is the scattering of light near second-order phase transition points, and specifically the point of the $\alpha \rightleftarrows \beta$ transformation in quartz.[9f]. Since the fluctuations increase as T_c is approached, it is immediately obvious that in this region one should expect an increase in the intensity of the scattering of x-rays, neutrons, and light. Such a phenomenon (critical opalescence) has long been known in the case of the critical liquid-vapor point. A sharp increase of the intensity of light scattering is also observed in quartz[9f] near its transition from the α into the β modification, which occurs at $T_c = 846°K$. It might seem that everything is clear in principle, but it was learned recently[9g] that the picture is more complicated and is apparently not described by the simple theory of [9f]. It is possible, as a matter of fact, that twinning occurs in the transition of β quartz into α quartz (that so-called electric or Dauphine twins are produced), and that the increased scattering occurs in large part from the boundaries between the twins. On the other hand, the twinning is not taken into account consistently in the theory, and on the whole the scattering picture remains unclear. Of course, the investigation of the scattered light in the $\alpha \rightleftarrows \beta$ transition in quartz is only one particular question; there are many such questions, and all these aspects of the problem of second-order phase transitions, when taken together, constitute one of the most important trends in macrophysics.

6. Superheavy (Far Transuranic) Elements[10]

The heaviest element observed in nature, uranium consists of Z = 92 protons and N = 146 neutrons (we have in mind ^{238}U). In 1940 we began to produce transuranic elements artificially by bombarding heavy nuclei (including uranium and transuranic nuclei) with neutrons and different nuclei. The first to be produced was neptunium (Np_{93}), followed by plutonium (Pu_{94}), americium (Am_{95}), curium (Cm_{96}), berkelium (Bk_{97}), californium (Cf_{98}), einsteinium (Es_{99}), fermium (Fm_{100}), mendelevium (Md_{101}), and the elements 102, 103, 104, and 105, which have not yet been officially named. The heaviest known transuranic elements have lifetimes amounting to seconds or even fractions of a second (the nuclei decay as a result of emission of α and β particles and as a result of spontaneous fission). A rough extrapolation leads to the conclusion that elements with $Z \geq 108$–110 should fission spontaneously at such high rates that the production and study of such elements is unlikely. However,

*L. D. Landau told me that no problem had consumed as much effort on his part as the attempt to solve the problem of second-order phase transitions.

although the transuranic elements contain 240–260 particles (nucleons) and in this respect resemble drops of liquid, their properties still do not vary monotonically with increasing Z or, for example, with increasing parameter Z^2/A. In other words, the single-particle and shell effects are noticeable and sometimes considerable even for the heaviest elements. In this connection, there is hope for the possible existence of relatively long-lived isotopes of elements with $Z > 105$. Specifically, it is proposed that an element with $Z = 114$ has a closed shell (i.e., that $Z = 114$ is a magic number), and that the isotope of this element $^{298}114$, which contains $N = 184$ neutrons, is even doubly magic. This does not yet mean that the nucleus $^{298}114$ is itself stable, since it is necessary to take into account all the possible decay modes (spontaneous fission and α and β decays). In particular, some calculations lead to the conclusion that the nucleus $^{294}110$ has the greatest "tenacity," and has a half life $T_{1/2} \sim 10^8$ years. According to an apparently widely held opinion, the accuracy of all such calculations is low, and they have no quantitative significance. But increased stability of nuclei in the region near $Z \sim 114$ and $N \sim 184$ is in itself probable, and one cannot even exclude the existence of high stability of individual isotopes or at least one isotope. In the latter case, such an isotope might be observed on earth, in meteorites, or in cosmic rays. Moreover, it goes without saying that more or less stable isotopes (say with $T_{1/2} > 1$ sec) might, hopefully, be synthesized and detected by methods used for the already known transuranic elements. Searches for far transuranic elements have already been initiated along all these lines. Such searches are of considerable interest for nuclear physics, and possibly also for astrophysics (let alone the fact that such searches are similar to hunting for yet unseen animals). Therefore there will hardly be any objection to the inclusion of the problem of superheavy elements in our "list." As to the listing of this problem under macrophysics, we encounter here, of course, the matter of a definition of microphysics, one of which will be given and used below.

II. MICROPHYSICS

7. Introduction

When we dealt with macrophysics, we needed no Introduction. But we must agree on what is to be meant by microphysics. The dimensions of the atom ($\sim 10^{-8}$ cm) and all the more those of the atomic nucleus (10^{-13}–10^{-12} cm) are regarded as microscopic, and from this point of view, atomic and nuclear phenomena should be classified as belonging to microphysics.

Actually, however, the situation is not so simple.

It is well known that in physics (and not only in physics), one can speak of "large" or "small" only in comparison with some quantity (a standard) which is regarded as neither large nor small. In the case of length (spatial distance), the characteristic dimension of the human body, say a meter, can naturally be taken as such a standard. However, compared with such a scale, not only are the atoms and nuclei very small, but, so for example, are the wavelengths of optical radiation, and also the dimensions of many artifacts. At the same time, one can hardly agree to consign films or wires with diameters on the order of a micron to the microworld. In addition, the dimensions of the earth, and all the more the distance from the earth to the sun, which equals 1.5×10^{13} cm, are already very large compared with the meter. Therefore, if we start only with a ratio of scales, the solar system should be differentiated from macroscopic objects with dimensions on the order of several meters with no less reason than atoms or atomic nuclei.

In view of such considerations, the microworld is frequently defined as the region where quantum laws are valid, whereas in the macroworld classical laws govern. Such an approach is quite profound, although its arbitrariness is also obvious. It suffices to state that in many cases the classical laws are also quite applicable when collisions between nucleons are considered, and on the other hand, quantum laws sometimes determine the behavior of altogether macroscopic systems (we recall, for example, the quantization of magnetic flux through hollow superconducting cylinders). Finally, it is important to emphasize that the very boundaries between the different regions and disciplines change with the development of science, and the scope of a given concept also varies.

All this gives grounds for considering the boundary between microphysics and macrophysics as historical. Concretely, I consider it reasonable and justified to assume that at the present time atomic and nuclear physics already belong to macrophysics and not to microphysics.

The reasons for this are as follows. First, atoms and nuclei are aggregates of particles, and furthermore, they are systems consisting of only a few of the most widespread particles (protons, neutrons, and electrons). Second, the nonrelativistic approximation usually holds quite well in atoms and nuclei, i.e., the splendidly mastered nonrelativistic quantum mechanics is widely applicable. Both these circumstances make nuclear physics and atomic physics kindred to macrophysics.

That it is natural to shift the arbitrary boundary between microphysics and macrophysics is also clear from the following example. Prior to the invention of the microscope, one could with full justification regard as microphenomena everything that could not be seen by the human eye. Then things that could not be seen in the microscope itself were called microscopic, for example individual atoms. Now, when the atomic and, to a considerable degree, also the nuclear scales have already been mastered and are sufficiently easily accessible to our mind's eye,* there are grounds for regarding only what is poorly visible or invisible as microscopic. By the same token, microphysics should include almost without reservation those fields which have been called and are still being called the physics of elementary particles, high-energy physics, meson physics, neutrino physics, etc.

The objects of investigation in microphysics are consequently mainly only the "simplest" ("elementary") particles, their interaction, and the laws governing them.

Like most definitions, this definition and understanding of microphysics are conditional, and to a certain de-

*Incidentally, individual atoms have already been observed, perhaps even directly, with the aid of the field-ion microscope [E. W. Muller, Science **149**, 591 (1965)], and apparently also by using a special electron microscope (see Physics Today **23** (8), 41 (1970)).

gree even arbitrary. But the definition seems to me at least no less definite and no less admissible than other definitions. At any rate, we shall henceforth use the term "microphysics" in precisely this sense. Thus, almost automatically, microphysics, as in the past, is the field of research where the foundations have not yet been excavated and where the concepts are not yet clear. When speaking of the types of laws, relativistic quantum theory predominates in microphysics (as here defined) at the present time. Finally, if we take a certain distance as a basis, then the characteristic length in microphysics is presently of the order of or smaller than 10^{-13} cm (the Compton length for the electron is $\hbar/mc = 3.85 \times 10^{-11}$ cm, and for baryons $\hbar/Mc \sim 10^{-14}$ cm).*

The difficulties that lie in the path of solving the fundamental problems of microphysics are similar to those which arose in the construction of the theory of relativity and quantum mechanics. Such investigations, even if they gain relatively modest results, call for exceptional effort, imagination, and enthusiasm. They give rise to a special atmosphere, they raise a high level of passion, many passions . . . but this is already a different topic† and I confine myself here to stating the fact that I am not capable of adequately describing the scope and variety of problems in microphysics. Nor do I undertake such a task. More arbitrarily than in the other cases, I single out below four microphysics problems and present only the sketchiest descriptions of them. It is possible that it is precisely the feeling of dissatisfaction with the exposition of the microphysical part of the present paper that has induced me to write this introduction, as well as Sec. 12 that follows here, without which the article, would, possibly, only profit. Fortunately, problems of microphysics are being discussed quite frequently and competently, so that there are sources to which to refer the reader (see the article [12] and also [13-19]).

8. The Mass Spectrum (The Third Spectroscopy)

Up to 1932 there were only three known "elementary" particles—the electron, proton, and photon. Then came the discovery of the neutron, positron, the $\mu^{\pm}$ mesons, the $\pi^{\pm}$ and π^0 mesons, the heavier mesons, hyperons, resonances, the electronic and muonic neutrinos, and the antineutrinos. Some of these particles are no less (but no more) elementary than the proton or the electron. Others (for example, the hyperons and the resonances) are more aptly characterized as excited states of lighter particles. Most of the particles are unstable and are transformed into one another. By the same token, the concept of elementarity or complexity of particles itself becomes highly nonelementary and complicated. The present article is so full of stipulations and definitions anyway that I will not attempt to discuss in greater detail the question of elementarity or complexity of the particles with which one deals in microphysics. These particles are characterized by mass, spin, charge, lifetime, and a number of other quantities and quantum numbers.[12a, f]. All these characteristics are determined experimentally or, in the best case, predicted on the basis of certain semiempirical laws and formulas.

By the same token, the fundamental and far from solved problem of microphysics can be stated as the development of a theory from which, at least in principle, one could determine the masses and also all other parameters of the existing particles. For simplicity, this problem is sometimes called the problem of determining the mass spectrum, although everyone understands that one deals not only with the particle masses, but also with other characteristics.*

The present status of the problem of the mass spectrum as a whole is analogous to the status of atomic spectroscopy prior to the appearance of Bohr's theory of the atom. Then, too, there were certain known spectral laws (primarily Balmer's formula), but they were not derived theoretically. Now the situation in the field of the third spectroscopy† is the same or somewhat better, but certainly far from being comparable with the situation that has developed in atomic physics after the advent of quantum mechanics.

In a certain sense, the problem of the mass spectrum is quite old, since the question of what causes, the difference between the proton and electron masses had already been raised half a century ago. The problem of the mass spectrum then came to be discussed from the point of view of the development of a relativistic theory of particles with excited states.[13] Now that data on such states and experimental data in the field of the third spectroscopy in general have been accumulated, the problem of the mass spectrum apparently has at least an empirical foundation. But in the field of theory, it seems to me that one cannot speak of any true progress, since attainments such as systematics and classification of the particles,[12] no matter how important, are still not fundamental in character.

Attempts were made to solve the mass-spectrum problem by developing relativistic models of tops, oscil-

*The most profound classification is apparently the one based on the type and character of the laws. In this connection, the most consistent one at present is a separation into three regions in which the classical laws, nonrelativistic quantum mechanics, and finally relativistic quantum theory govern. These three regions could also be called macrophysics, microphysics, and, say, ultramicrophysics. But most consistent is not always most convenient or most comfortable. We therefore consider it best to proceed as is done in the text, i.e., to speak, as in the past, only of macrophysics and microphysics, but to shift the boundary between them.

†This topic is more suitable for a writer, and unfortunately I cannot present an example of a completely successful solution. To be sure, by way of a clear illustration that conveys the nature of work on fundamental problems, there come to mind the words with which Einstein concluded a lecture devoted to the history of development of the general theory of relativity [11]: "In light of the results already obtained, that which we have been fortunate enough to find out is almost self-explanatory, and any intelligent student can master the theory without great difficulty. What is left behind is many years of groping in the dark, full of forebodings, tense expectancy, alternation of hope and despair, and, finally, a breakthrough to clarity. But this will be understood only by one who has experienced it himself."

*Moreover, the differences that exist, for example, between baryons and leptons are qualitative and deeper than say, between baryons having different masses.

†We use here the seemingly fortunate and lucid terminology of [12a], according to which atomic and molecular levels belong to the first spectroscopy, nuclear levels to the second spectroscopy, and the levels of "elementary" particles to the third spectroscopy (incidentally, the term "third spectroscopy" is used directly in [12a] only for the baryon spectrum).

lators, etc.[13] Another trend was the unified field theory of elementary particles,[14a] which can also be called the theory of primordial matter, since an attempt is made to place at its basis a certain primary spinor field with spin $\frac{1}{2}$. In a third trend, attempts are made to treat baryons and mesons as particles consisting of different combinations of three fundamental particles (quarks). Quarks, however, have not been observed in the free state, and the theory of bound quarks is still in its infancy.[12a] A fourth trend is connected with account for effects of general relativity theory ("friedmons"[14b] and the existence of a fundamental length connected with gravitation; see Secs. 9 and 15).

Somewhat apart is the question of the mass spectrum of leptons, and, concretely, the difference between the masses of the electron and the muon. We mention also the search for "exotic" particles (magnetic poles—monopoles, quarks, etc.).

No attempt to solve the problem of the mass spectrum, as already mentioned, has led to genuine and definite success. Such has been the situation for decades and no one can predict when, finally, the "ice will break." But someday this will occur, and in spite of all the disenchantment, this historic event is still awaited with unflagging high anticipation.

9. The Fundamental Length (Quantized Space, etc.)

Special and general relativity theory, nonrelativistic quantum mechanics, and the existing theory of quantum fields use the notion of continuous, in essence classical, space and time (a point of space-time is characterized by four coordinates $x_i = x, y, z, ct$, which are capable of assuming a continuous sequence of values). But is such an approach always valid? How do we know that space and time do not, "in the small," become entirely different, "grainy," discrete, quantized? This question is far from new; it was apparently first raised by Riemann back in 1854 (see [15a]), and has since been discussed many times. Thus, in his well-known lecture on "Geometry and Experiment" Einstein said in 1921:[15b]

"The physical interpretation of geometry proposed here cannot be applied directly to regions of space of submolecular dimensions. Nonetheless, even in questions of the construction of elementary particles it retains a certain meaning. In fact, when we describe the elementary electric particles that compose matter, we can attempt to retain the physical meaning for those aspects of the field which were used in physics for the description of the geometric behavior of bodies that are large compared with molecules. Only success can serve as a justification for such an attempt to ascribe physical reality to the main concepts of Riemannian geometry outside the region of their physical definition. It may turn out, however, that such an extrapolation is no more justified than the extension of the concept of temperature to parts of a body with molecular dimensions."

This clearly formulated question of the limits of applicability of Riemannian geometry (i.e., in essence, of macroscopic or classic geometric representations) remains unanswered to this day. As progress is made towards ever-increasing energies, and consequently "closer" collisions of different particles (see Sec. 10), the scale of the regions of space accessible to investigation decreases. Here we can apparently state that down to distances on the order of 10^{-15} cm the usual spatial relations are valid, or, more accurately, their use does not lead to contradictions. From certain considerations[16] this limit might possibly be moved to approximately 10^{-20} cm. In principle, one cannot exclude the possibility that there is no limit at all, but still much more probable is the existence of some fundamental (elementary) length, $l_0 \lesssim 10^{-15}$–10^{-20} cm, which limits the possibilities of the classical spatial description. Moreover, it is reasonable at the present time to assume that the fundamental length l_0 in any case is no smaller than the gravitational length $l_g = \sqrt{G\hbar/c^3} \sim 10^{-33}$ cm (see Sec. 15).

The problem of the fundamental length has been discussed for many years in different forms and variants (this length enters in the theory of primordial matter,[14a] in different variants of the theory of quantization of space[17], etc.). Closely connected with the problem of the fundamental length is the question of possible violations of causality in the microworld (or, as is said, violation of microcausality; see [17c]), and also a number of other trends in microphysics and the problem of singularities in the general theory of relativity and cosmology (see Sec. 15 below). If some fundamental length does exist, then it is natural to assume that it also plays a role, and even a decisive role, for the solution of the problem of the mass spectrum. The fundamental length would probably serve as a "cutoff" factor, which is required to some degree or another by the existing quantum field theory; in a theory containing the fundamental length, divergent expressions would automatically disappear by conception. From the experimental point of view, searches for a fundamental length involve investigation of collisions between particles at ever-increasing energies, as well as ultraaccurate measurements of the widths of nuclear levels.[16] In general, any disparity between existing theory (such as quantum electrodynamics) with experiment is an indication of a possible violation of the space-time representations and the need for introducing the elementary length.

10. Interaction of Particles at High and Ultrahigh Energies

Study of the interaction of particles at high and ultrahigh energies serves many purposes: "probing" the structure of the particles and of space itself at small distances, observation of more and more new particle-resonances (excited baryons and mesons), the determination of the energy dependence of the cross sections for elastic and inelastic scattering.

When a nucleon collides with a nucleon, the distance attained is $l \sim (\hbar/m_\pi c)\, Mc^2/E_{c.m}$, where $\hbar/m_\pi c \sim 10^{-13}$ cm is the Compton length of the pion, M is the mass of the nucleon ($Mc^2 \approx 1$ GeV), and $E_{c.m}$ is the energy of the nucleon in the c.m.s. (for more details see [18]). If one of the nucleons is at rest and the other has an energy E, then $E_{c.m} = \sqrt{\frac{1}{2}(E + Mc^2)Mc^2}$. By now an energy $E \approx 75$ GeV has been attained in accelerators (Serpukhov), and in a year or two this will be raised to 500 GeV (USA). Even at $E \sim 500$ GeV, obviously, $E_{c.m} \sim 15$ GeV and $l \sim 5 \times 10^{-15}$ cm. In cosmic rays one encounters particles with energies up to $E \sim 10^{20}$ eV, but it is hardly possible to use cosmic protons with E $> 10^{15}$ eV for the analysis of collisions (see [18a,b]); in this case $E_{c.m.} \lesssim 10^3$ GeV and $l \gtrsim 10^{-16}$ cm. In collisions of particles that have no strong interaction (muons, electrons, photons), the smallest length involved in the collisions is of the order of the wavelength in the c.m.s., i.e., $l \sim \hbar c/E_{c.m}$ (it is assumed that $E_{c.m} \gg m_i c^2$, where m_i are the masses of the colliding particles), and the possibility of reaching small distances is somewhat better than in the case of nucleons. Moreover, in view of the attained high accuracy of the measurements and the thorough comparison with theory it is possible, generally speaking, to probe distances that are somewhat smaller than those obtained simply on the basis of the presented rough estimates. But it is perfectly clear how difficult it is to advance beyond the limit $l \sim 10^{-15}$–10^{-16} cm. Comparison of scattering theory with experiment at ever-increasing energies, together with investigation of more and more new resonant states for baryons and mesons and with determination of the different effective cross sections, constitutes the main problem of high-energy physics. At high energies one observes in this case more than the scattering and production of individual particles; what occurs primarily is multiple production of particles. Multiple production has its own distinguishing features, which one attempts to take into account with the aid of statistical and hydrodynamic methods.[18c] The foregoing pertains mainly to nucleon collisions, and special mention must therefore be made of interactions between matter and high-energy muons[18d] or high-energy neutrinos, which are produced in the earth's atmosphere by cosmic rays (we are speaking mainly of the neutrinos from the decay of muons and pions produced by cosmic rays).[18e]

Unlike the mass-spectrum problem and the question of the fundamental length, investigations of particle interactions at high and ultrahigh energies are auxiliary and less definite from the point of view of formulating some clear and attractive physical purpose. This may be so, but it is more likely that this impression is due to imperfection of our exposition. By way of some justification it can be noted that all the microphysics problems already touched upon are closely intertwined and are not independent of one another to any considerable degree. In singling out the problem of particle interaction at high energy, I wish to emphasize primarily that by no means all of the subject matter of high-energy physics reduces to the problems of the mass spectrum and of the fundamental length. Thus, the question of the energy dependence of different interaction cross sections of different particles (especially at ultrahigh energies, or, formally, as $E \to \infty$) has a quite deep, and to a certain degree independent, significance for the theory.

11. Violation of CP Invariance[19]

Nonconservation of spatial parity (P) in weak interactions was discovered in 1956. However, the decays observed up to 1964 satisfied the principle of combined parity, according to which all the interactions are invariant against CP conjugation, i.e., simultaneous spatial inversion and charge conjugation C (replacement of the particle by the antiparticle).

The year 1964 saw a discovery the significance of which is apparently exceedingly great, though not yet fully understood. We have in mind observation of the decay $K_2^0 \to \pi^+\pi^-$ (K_2^0 is the long-lived neutral K meson), which can occur only if CP invariance is violated. This result may possibly lead to the fundamental conclusions that right and left are not equivalent, that the forward and backward directions of time are not equivalent, and that particles and antiparticles are not equivalent. On the other hand, one apparently cannot exclude the possibility of relating CP nonconservation to the action of some new (hitherto unknown) superweak interaction.

What is the cause and what is the deeper physical content of CP noninvariance? What is the role of this nonconservation in microphysics, macrophysics, and astrophysics (cosmology)?

In spite of very great efforts, no answers to these questions have been obtained in the last six years. There is no doubt that the problem of CP nonconservation is one of the most intriguing and in all probability one of the most important problems of modern physics (the availability of detailed reviews[19] allows us to confine ourselves to these cursory remarks).

12. The Microphysics of Yesterday, Today, and Tomorrow

All is flux, everything changes—changes take place not only in the content of the field called microphysics, but also in the position it occupies in science in general and in physics in particular. It suffices to review the physics, abstract, and popular-science journals to verify the following: the share of problems of microphysics in all these journals has decreased greatly during the last 20 years as compared with the preceding several decades. Unfortunately, I do not have exact figures,* but I think that the ratio of the number of scientific papers on micro- and macrophysics is now smaller by at least one order of magnitude than 20 years ago. If we use other indicators of scientific activity (the numbers of specializing graduating students, the numbers of conferences, etc.), then the picture will probably be about the same. What does this mean? The main cause, in my opinion, is that even in the recent past (say, for concreteness, 20 years ago), microphysics occupied a certain exclusive position in science.

First, the scope of microphysics includes the most fundamental, principal, and therefore for many the most attractive problems in physics. Second, the same problems were, until the middle of the present century, of decisive significance for the development of science in its entirety. In fact, the main content of microphysics was at that time the study of atoms, and then also the atomic nuclei. To unravel the structure of the atom, to understand the laws governing in it (for which purpose

*In this connection I wish to express again my regret that we pay so little attention to statistical or to any other analysis of trends in the development of science, the role of different forms of information, etc. I note also that there are no grounds whatever for relating the discussed change in the relative position of microphysics simply to the fact that we have assigned the principal part of atomic and nuclear physics to the field macrophysics. It suffices to state that such divisions of microphysics as high-energy physics, meson physics, neutrino physics, etc. did not exist at all earlier. On the other hand, the unique place of microphysics in the vanguard of physics in its customary definition has still remained in force (see also Sec. 7).

it was necessary to discover quantum mechanics!) meant to give a most powerful impetus to many branches of physics, astronomy, chemistry, and biology. The same can be said with respect to the atomic nucleus—its study has made it possible to make use of nuclear (atomic) energy and even given certain grounds for calling the 20th century the atomic century.

The overwhelming majority of physicists engaged in the corresponding problems of microphysics did not think of any practical results of their work; their enthusiasm and persistence were fed by their interest in the problems as such, and were due to the irrepressible desire to know "what makes this tick," to overcome difficulties, to attain the truth. But the concentration of the effort as a whole, the extent of the work, the support and interest on the part of society (in particular, scientific public opinion), all this was also dictated to no small degree by recognition of the role of microphysics in the development of the natural sciences as a whole, and, if you wish, understanding of its significance to humanity in general.

The situation has now changed radically. The particles investigated in microphysics either live for negligible fractions of a second, or, as in the case of the neutrino, penetrate practically freely through the earth's sphere and are captured only with colossal difficulty. It is quite obvious that the scientific significance of the problem is not determined either by the lifetimes of the particles or by their penetrating ability. The problems that microphysics faces now are no less vitally mysterious and no less difficult in nature than the problems of yesterday. In other words, microphysics has, of course, remained (and will always remain, in accordance with the definition used for it), an outpost of physics, its most advanced and profound division. But the situation has changed with respect to the character and the role of the objects investigated by microphysics. These objects (atoms, atomic nuclei) were the bread and butter, and the new objects were the exotic and rare fruits. Yet, as laready stated, microphysics occupied a literally dominating position in science to a major degree also because of the universal importance of the problems that it investigated.

Thus, according to the opinion defended here, the position of microphysics has changed radically, both in physics itself and in science in general, and (this statement will be particularly debatable) I believe that this change is a permanent one, or will at any rate be with us for a very long time.

If I were to express the foregoing in nonscientific terms, I would say that microphysics was the first lady of the natural sciences in the first half of our century. Today and tomorrow it remains and will remain "only" a most beautiful lady. But, and here is the rub, different persons can regard different ladies as the most beautiful, whereas the first lady (unlike first stand-ins) is by definition the only one (for example, this is what they call the President's wife). I might add that for me personally, microphysics was and remains the most beautiful lady of physics. But, unlike some of my colleagues, I only believe that our respect should not be accompanied by neglect of changes in age and character, or by neglect of other objects worthy of admiration.

The foregoing remarks are to a considerable degree trivial, but. . . only to those who agree with them. They are in fact included here only because they are debatable. I became convinced of that when, a few years ago, practically the same thing was written on another subject.[20] To be sure, as is customarily the case, certain objections and critical remarks were only the result of misunderstanding or egocentrism. Thus, the statement that the role of microphysics has changed and has diminished to a certain degree has been taken, if not as an appeal for stopping the construction of powerful accelerators and in general withdrawing all-out support of microphysics research, at least as a justification for such an action. There is no need to say that I am far from enteraining such thoughts, and yet I am afraid that besides noble concern for the development of a branch of physics close to certain hearts, the sharpness of the criticism has at times been dictated by less lofty feelings not germaine to the present discussion.

Serious attention should, however, be paid to an objection that reduces essentially to the following. During the first stage of the research on the atomic nucleus, the prospects of nuclear energy were still far from clear or were even estimated in an entirely incorrect manner. There are many such examples. In general, the development of science is difficult to predict in the form of a concrete plan, and sometimes entirely unpredictable. It is therefore possible, and on the basis of a number of analogies even quite probable, that microphysics will again assume its position as the progenitor of gigantic new problems similar to the mastery of nuclear energy. Then, naturally, the relative position of microphysics could again be greatly strengthened.

It goes without saying that no one will undertake to exclude such a possibility completely. Even this one circumstance—the existence of even a hazy prospect for new discoveries of practical importance—should be sufficient to continue all-out development of microphysics in interest other than those of "pure science."

On the other hand, even acknowledgement of the possibility of a new revolution with respect to the practical role of microphysics in the future does not in any way contradict the statements made above concerning its present position. In addition, it is difficult to understand why it was considered heretical or in poor taste to make the suggestion (which I am not afraid to make here) that in a sense, the most brilliant period in the life of microphysics is already behind us. After all, not all of us are obliged to believe in the existence of an infinite "doll inside a doll," with successively smaller ones fitted into one another without end.

Unfortunately, I shall have no chance to verify the correctness of my opinion concerning the future of microphysics, but on the other hand it will hardly be necessary to confess that I am wrong, for even the optimists do not tend to expect any radical change in the role of microphysics in science during the lifetime of our generation.

III. ASTROPHYSICS

13. Experimental Verification of General Relativity Theory[18b, 21]

General relativity theory (GRT) was formulated in final form by Einstein in 1915. By that time he had already pointed to three famous ("critical") effects capable of verifying the theory: the gravitational shift of spectral lines, the deflection of light rays in the field of the sun, and the displacement of the perihelion of Mercury. Since that time, more than half a century has elapsed, but the problem of experimental verification of GRT remains of vital importance and continues to be at the center of interest.

What is the reason for this?

All the effects indicated by Einstein exist and have been observed, but the accuracy attained is still low. Thus, in the case of the gravitational frequency shift, it amounts to approximately 1%, and furthermore the effect is insensitive to the form of gravitational theory (see [21a]). The deflection of light rays in the field of the sun (which reaches, according to GRT, 1.75″ when the light ray grazes the solar disk) has been measured only with accuracy on the order of 10%, although within these limits it agrees with GRT. The accuracy of measurement of the deflection, near the sun, of radio waves coming to us from quasars[21e] is approximately of the same order, as is the accuracy of radar determinations of the relativistic time delay in signal propagation near the sun.[21b,d,18b] The displacement of the periheliion of Mercury is known with accuracy of about 1%, and the agreement between theory and the observations in this question was regarded until recently as the best confirmation of GRT (if we disregard exact measurements of the equality of the heavy and inertial masses[21a,c]). The hypothesis has been advanced, however, that this agreement is only illusory, since no account was taken of the influence of the quadrupole moment of the sun. Such an objection, which seems at first glance to be quite artificial, has found a certain confirmation in connection with the observed oblateness of the sun.[21c]

Thus, we can presently state that even for weak fields (i.e., in the case of smallness of the parameter $|\varphi|/c^2$, which equals, even at the sun's surface, $|\varphi|/c^2 = GM_\odot/r_\odot c^2 = 2.12 \times 10^{-6}$), GRT has been verified accurate only to several percent. This being the situation, there are at least possibilities, if not grounds, for discussing gravitational theories that are alternatives with respect to GRT. Of these, the greatest attention is presently being paid to the tensor-scalar theory, in which the gravitational field is described not only by the metric tensor g_{ik} but also by a certain scalar χ. The relativistic deflection of the rays should then equal $\alpha = (1 - s)\alpha_E$, and the displacements of the perihelia of planets should be equal to $\Psi = (1 - {}^4\!/_3 s)\Psi_E$, where α_E and Ψ_E are the corresponding values in accordance with GRT, (i.e., according to Einstein's theory, which connects gravitation only with the field g_{ik}) and s is the fraction of the weight of the body due to the presence of the hypothetical scalar field χ. As is clear from the foregoing, according to the observations $s \lesssim 0.1$, and the next problem in the experimental verification of GRT is to increase the accuracy of the upper limit for the parameter s. If it is demonstrated that $s < 0.01$, then the tensor-scalar theory (at least in its presently discussed form [21c]) will be fully refuted.

We are unable to dwell in greater detail on the prospects for research toward the experimental verification of GRT. It suffices to say that these prospects, if we have in mind accuracies on the order of 1% or even fractions of a percent, are quite good.[21a, b, f] It is possible that a verification with accuracy on the order of 1% has already been realized! The point is that the space rockets "Mariner-VI" and "Mariner-VII," launched in the direction of Mars in 1969, "set" behind the sun in April-May 1970, and their signals were used to measure the relativistic delay in the propagation time of signals passing near the sun (the relativistic effect reaches only 2×10^{-4} sec). The corresponding observations are presently being processed, and the first results agree with GRT with accuracy on the order of 5%, but perhaps more accurate data will be obtained.

If it is demonstrated (which I ardently hope) that "all is in order" with the experimental verification of GRT in the sun's field, then the question of such verification will assume an entirely different character. Namely, there remains the question of the validity of GRT in strong fields or in the vicinity, and even in the interior, of supermassive cosmic bodies. This will be discussed later.

On the other hand, if the most minute deviations from the predictions of GRT are reliably established within the confines of the solar system, this would be a discovery of exceptional importance. The probability of such a result appears negligibly small to the majority of physicists (including myself). But what is probability in such cases? Furthermore, if such a probability of discovery is nevertheless introduced, then it would also be necessary to use the concept of the "mathematical expectation" of the discovery, equal to the product of the probability by the significance of the discovery. In this case the mathematical expectation of deviations from GRT would turn out to be appreciable even though the probability of observing these deviations is negligible. But reasoning of this type is, as they say, semantics. It is quite obvious that progress in the problem of verification of GRT is possible only via new observations and measurements. This will in all probability be done in the nearest future.

14. Gravitational Waves[21b,22]

From the viewpoint of any relativistic theory of the gravitational field, gravitational waves should exist in vacuum, in analogy to electromagnetic waves. This analogy is even more far-reaching in GRT, since in this theory the waves are purely transverse. The notion of gravitational waves in vacuum was born together with GRT, and the well-known and widely used formula for the powerful gravitational radiation emitted by moving masses [see formula (105.12) in [22a]] was derived by Einstein back in 1918.[22b]

Gravitational waves should be emitted by all masses with nonzero and time-varying quadrupole moments. The simplest cosmic objects of this type are binary stars or planetary systems.

The gravitational interaction is, however, the weakest of all the known interactions. As to all the known macroscopic (and, if you will, everyday) manifestations of gravitation, they are so appreciable only as a result of the existence of tremendous clusters of masses and, say, the large mass of the earth (in the case of two protons, on the other hand, their gravitational attraction is smaller by a factor $e^2/GM^2 \sim 10^{36}$ than the electrostatic repulsion; here $G = 6.67 \times 10^{-8}\ g^{-1}\ cm^3\ sec^{-2}$ is the gravitational constant, $e = 4.8 \times 10^{-10}$ cgsesu is the proton charge, and $M = 1.67 \times 10^{-24}$g is the proton mass). It is therefore not at all surprising that the power of gravitational radiation is usually (say, in the case of binary stars) relatively small, and the detection of gravitational waves is far from simple. At any rate, gravitational waves have not yet been observed with full certainty, and the prospects of receiving gravitational waves from binary stars and pulsars seem to be quite remote. It suffices to state that were the pulsar NP0532 in the Crab nebula to radiate gravitational waves even with a power $L_g \sim 10^{38}$ erg/sec,* then the flux of the gravitational radiation on earth would amount to only $F_g \sim 3 \times 10^{-7}$ erg/cm^2 sec. At the same time, the sensitivity of the existing gravitational-wave receivers is on the order of or smaller than $F_g \sim 10^4$ erg/cm^2 sec, i.e., smaller by at least 11 orders of magnitude than is required (see [21b,22c]). To receive radiation with $F_g \sim 3 \times 10^{-7}$ erg/cm^2 sec by presently known methods, it would be necessary to cool a receiver weighing several tons to a temperature of 10^{-2}–10^{-3}°K. This is possible, but, of course, extremely difficult. Nonetheless, one of the most sensational announcements of the present era would be precisely the statement that cosmic gravitational radiation had been received.[22c] Concretely, it is assumed in [19c] that massive aluminum "ingots" (cylinders) weighing 1.5 tons begin to vibrate at their natural frequency $\nu \sim 10^3$ Hz under the influence of gravitational radiation arriving from the direction of the center of the Galaxy. The power of such radiation, if it is really emitted near the galactic center (at a distance of approximately 10^4 parsec $\approx 3 \times 10^{22}$ cm) should range from 10^{50} erg/sec (the estimate in [22c]) to 10^{52} and more (according to [21b,22d]). The energy corresponding to the rest mass of the sun is $M_\odot c^2 \sim 10^{54}$ erg; consequently, if radiation with power 10^{50}–10^{52} erg/sec actually emanates from the center of the Galaxy, then the mass of this central region should decrease annually by $(3 \times 10^3$–$3 \times 10^5) M_\odot$ solely as a result of gravitational radiation. It is difficult to believe in the existence of such a powerful gravitational radiation, although it still does not contradict simple energy considerations (the mass of our entire Galaxy is $M_G \sim (1–3 \times 10^{11} M_\odot)$. The question of the possible mechanism of such radiation remains open, and even more importantly, the measurements were carried out only by one group,[22c] and their interpretation raises certain objections.[21b, 22d]. By the same token, there is no doubt of either the need for continuing the investigations or that it is premature to draw such far-reaching conclusions. It must be emphasized at the same time that owing to the investigations reported in [22c], the problem of reception of gravitational waves has finally proceeded from the stage of discussions to the experimental phase.† In any case, this is no small accomplishment, and if powerful gravitational radiation has indeed been observed, we are already faced with a remarkable and most important discovery.

*This is the luminosity of the Crab nebula in all the parts of the electromagnetic spectrum taken together. In my opinion, there are no grounds for expecting the power of the gravitational radiation of the pulsars to reach such a value; probably this power is smaller by several orders of magnitude.

†Attempts to duplicate the measurements of [22c] are now being undertaken in several countries, and other events will be recorded simultaneously with the gravitational-wave pulses (radio pulses, cosmic-ray showers).

15. Cosmological Problems. Singularities in the General Theory of Relativity and Cosmology[22a,23,24]

The problem of cosmology is to study space-time "in the large," on large scales, over a long period of time. By the same token, cosmology is inseparably linked with the entirety of extragalactic astronomy and encompasses a very wide range of research. But the "question of questions" in cosmology is the clarification of the very character of evolution of the universe in time, and the choice of a cosmological model that corresponds to reality (we assume here that the main concepts and the mileposts on the path of the development of modern cosmology are known to the reader—this is justified, in particular, by the possibility of referring the reader to the elementary article[23] and also to other sources[22a,24]).

In the homogeneous and isotropic cosmological models (these were first considered by Friedmann in 1922 and 1924 and subsequently investigated by Lemaitre and many others*), the universe, in accordance with the observational data, is an expanding system. It is curious that not until 1934 did Milne and McCrea understand the nature of such nonstationary behavior, which has a classical character, i.e., which follows under a given approach from the very Newtonian theory of gravitation (the point is simply that if gravitational forces correspond to attraction only, a system of bodies cannot remain at rest or, in general, in any stationary state).

Regardless of the nature of the expansion, it is perfectly clear that it could not have been going on for all past eternity. Indeed, in all the homogeneous and isotropic models, the expansion began at some time after a compression phase, or else started at some instant $t = 0$ when the density ρ of matter was infinite (a singularity). If the cosmological constant is $\Lambda = 0$, then all solutions belong to the latter class—they have the singularity (and the solutions with $\Lambda \neq 0$, which have no singularities, do not agree with the observational data[24c]).

The appearance of the singularity ($\rho \to \infty$) is logically admissible, but, in the opinion of many persons (including myself), an indication of some defect, inapplicability, or limitation, etc. of the theory. At one time it was hoped that the singularity appeared in the Friedmann models as a result of their high symmetry, and that such a singularity would disappear in inhomogeneous and anisotropic cosmological models, just as the focus of a highly symmetrical lens spreads out when the lens becomes distorted. Recently, however, it became clear that this is not so,[24d] and quite general GRT solutions, which correspond to cosmological models and are anisotropic and inhomogeneous, also have a singular point (the approach to this point, generally speaking, has a very curious oscillating character).

Thus, it is apparently impossible within the frame-

*More accurately, the first relativistic cosmological model, which was furthermore, an isotropic and homogeneous model, was proposed by Einstein in 1917 (see [22b], p. 601). The model, however, was static. It corresponds to one solution of a two-parameter family of solutions (which are nonstationary in all other cases) obtained by Friedmann. We note that Friedmann did not regard the cosmological constant Λ introduced by Einstein as equal to zero. At $\Lambda = 0$, all the homogeneous and isotropic models are nonstationary.

work of GRT to get rid of the singularities in the problems of cosmological expansion (or of the collapse of supermassive stars; see the next section).* But this is far from decisive evidence in favor of the existence of true singularities with $\rho \to \infty$. The point is that GRT is a classical theory. But there is no doubt that the true theory of the gravitational field should be a quantum theory. Usually, quantum effects in astrophysics are extremely small, as is the case for the majority of macroscopic problems, but it is precisely near the singularity that the quantum effects increase strongly. Let us imagine, for example, that there exists a fundamental length l_0 (see Sec. 9). Then there is practically no doubt that the classical GRT ceases to "work" for scales on the order of or smaller than l_0 and probably for densities† $\rho \gtrsim \rho_0 \sim \hbar/cl_0^4$. At $l_0 \sim 10^{-16}$–10^{-20} cm, the density is $\rho_0 \sim 10^{26}$–10^{40} g/cm^3. Conceivably in this case the densities $\rho \gtrsim \rho_0$ are not attainable and the singularity as well as all the divergences vanish. On the other hand, if there exists no fundamental length l_0 not connected with gravitation, then some gravitational length l_g is bound to appear on the scene (it is possible that this length will play the role of the fundamental length l_0). In fact, from the gravitational constant G [$\mathrm{g^{-1}cm^3sec^{-2}}$], the speed of light c, and the quantum constant $\hbar$ it is possible to make up a length

$$l_g \sim \sqrt{\frac{G\hbar}{c^3}} \approx 1.6\cdot10^{-33}\ \mathrm{cm}. \tag{4}$$

This length corresponds to a time $t_g \sim c/l_g \approx 0.5 \times 10^{-43}$ sec and to a density

$$\rho_g \sim \frac{c^5}{\hbar G^2} = \frac{\hbar}{cl_g^4} \approx 5\cdot10^{93}\ \mathrm{g\text{-}cm^{-3}} \tag{5}$$

Various considerations and estimates[24e,f] indicate that when quantum effects are taken into account, the density ρ cannot exceed in order of magnitude the value $\rho_g \sim 10^{94}$ g-cm^{-3}, and in any case the classical singular solutions of the GRT cannot be extrapolated to the region of larger densities. But the corresponding arguments are still not rigorous, since no consistent quantum theory of gravitation, let alone a quantum theory of cosmology, has as yet been created. The solution of this problem is apparently an exceedingly difficult matter, but nevertheless it is necessary and of deep fundamental importance.

The cosmological problem and the related problem of singularities in GRT occupies approximately the same position in astronomy, from the point of view of its character and type of problems, as microphysics does in physics. Furthermore, in this case the problems of the microworld apparently do not even border on macrophysics, but on astrophysics and cosmology. In all probability, new ideas are needed for understanding of all these problems; this is a field of searches, errors, attempts and new attempts to find the correct way.

*The foregoing does not pertain to systems with nonzero total electric or mesic charge (we have in mind the vector-meson fields; see [24g]).

†From the quantum constant $\hbar$ with dimension [g-cm^2 sec^{-1}], the speed of light c [cm-sec^{-1}] and the length l_0 [cm] it is possible to set up only the one indicated value of ρ_0 with dimension [g-cm^{-3}].

16. Quasars and Galactic Nuclei

Is it possible to expect deviations from the classical GRT solutions somewhere or sometime in the world apart from the earlier (in the sense of proximity to the classical singularity) phases of the evolution of the Universe? This question can also be expanded if instead of dealing with deviations from GRT we consider the more general possibility of deviation from already known physical laws.

In some sense this is apparently the age-old question that has disturbed many astronomers, namely, can everything in astronomy be reduced to "terrestrial" physics, to the physics that is valid in our laboratories? A similar question has been discussed for many years as applied to biology—does everything in biology reduce to physics, to molecular concepts, or not?*

It is impossible, of course, to answer such questions beforehand. The approach which is most natural (and which actually is the one most widely used) can be formulated as follows: let us apply the known physics without limitation; if really insurmountable difficulties are encountered on this path, then we are ready to analyze new concepts, and proceed to upset or generalize the physical theories. Probably nearly everyone will agree with this formulation, but this is far from meaning that there is a meeting of minds, for the question arises as to when a difficulty is insurmountable.

Physicists who work in astronomy are in this respect more conservative (in the good sense of this word, I am certain) than "pure" astronomers. An impression is gained that some astronomers literally have some inner need to throw off physical chains, to launch on a field of research not limited by any known physical laws. We present, for example, the following remark by Jeans:[25a] "Every failure in attempts to understand the spiral arms makes it more and more difficult to resist the suspicion that forces which are entirely unknown to us are in operation in the spiral nebulae, perhaps reflecting new and unexpected metric properties of space. The notion that suggests itself persistently is that the centers of the nebulae have the character of "singular points." At these points, matter flows into our world from some other and entirely extraneous space. By the same token, the inhabitant of our world sees the singular points as places where matter is continuously created."

These views of Jeans are presently referred to as if they were prophecies. But they were published in 1928, when not so much was known concerning the structure of the galaxies, and the theory of their evolution was hardly developed (and, furthermore, the problem of the origin of the spiral arms is now regarded as explained to a considerable degree).

At the present time we know much more about the galaxies; in particular, the fact has been established that a galaxy has a certain nucleus, which is sometimes active and plays a major role.[25b,c,d] But does this also imply the much more radical hypotheses of Jeans[25a] and Ambartsumyan[25b] concerning the role of the nuclei

*The evolution of the views concerning this question consists, in general, of an ever-increasing and frequently unlimited expansion of the "effective radius" of physics and biology. Bohr's change of mind on this subject is instructive (see [20] and the references contained therein).

as sources of matter, or that these nuclei are "a new form of the existence of matter possibly still unknown to modern physics"?[25c]

In the opinion of the majority of astrophysicists, this is not so, and one can by no means exclude the possibility of explaining all the phenomena observed in galaxies and nuclei, and also in quasars,[25e] without resorting to essentially new concepts (see, however, [25h]). Galactic nuclei and quasars may constitute, or contain in their central parts, supermassive plasma bodies ($M \sim 10^9 M_\odot$, $r \sim 10^{17}$ cm) with large internal motions of the rotational type and with large magnetic fields.[25f]

At the same time, the reference made above to the "majority" inevitably brings to mind Galileon, who emphasized that in problems of science the opinion of one person is sometimes more valuable than the opinions of thousands. I am therefore less prone to use the notorious "majority" as an argument favoring unlimited application of the physical laws known to us; I am merely stating the existing situation. The latter (if it is correctly reflected here) reduces to the fact that even the "opinion of the astronomical community," let alone the "opinion of the physical community," has by no means admitted that the evidence indicating a need for introducing essentially new physical concepts for understanding of processes in galactic nuclei and in quasars is fully convincing.

As to the possible presence of collapsed masses in galactic nuclei[25d] and in outer space in general, such an assumption does not go beyond the limits of GRT. On the other hand, the singularities that arise in relativistic collapse (where GRT, just as in the case of the cosmological singularity, is probably *not* applicable) do not appear at all as specific phenomena in outer space (see [22a,24b]).

Special mention should be made of the origin and occurrence of galaxies and quasars,[25g] which is closely connected both with cosmology and, of course, with the nature of galactic nuclei and quasars.

Thus, the nuclei of galaxies and quasars are regions where one suspects the existence of deviations from the known physical laws (GRT, quantum theory, law of baryon-number conservation, etc.). A verification of this possibility, let alone the development of a more complete theory of galactic nuclei and quasars, is a problem of outstanding significance.

17. Neutron Stars and Pulsars

The hypothesis of the existence of neutron stars was, insofar as could be established, advanced in [26a] in 1934 and then discussed widely for many years,[26b,c,24b] but only theoretically. Attempts to observe neutron stars at first seemed almost hopeless;* hopes were then raised of observing such stars so long as they were hot ($T \sim 10^6$–10^7 deg), by means of their x-radiation. Actually, however, neutron stars were discovered in 1967–1968 by their specific periodic radio emission: we have in mind the observation of pulsars, the identification of which with neutron stars is now generally accepted.[26d,e; 23]. The study of neutron stars and pulsars (it is still impossible to equate the two, all the more since not all neutron stars need produce observable pulsating radiation) involves a large number of problems. But the same can be said concerning stars of any class. Therefore neutron stars and pulsars appear in the present "list of most important problems" by virtue of special circumstances, of which there are several.

First, the greater part of the neutron star consists of matter having densities from 10^{11} to 10^{15} g/cm^3. The equations of state and all the properties of matter at such densities are not very well known, and its study is an important problem. Special notice should be taken here of the superfluidity of the neutron liquid and the superconductivity of the proton liquid in neutron stars[5b,26e] (at densities $\rho \sim 10^{13}$–10^{15} g/cm^2 the protons, and of course also the electrons, constitute several percent of the neutron content; since the neutrons, protons, and electrons form degenerate Fermi systems under such conditions, it is possible to regard such a mixture, with a certain degree of approximation, as consisting of independent neutron, proton, and electron Fermi liquids).

Second, the question of the central region of the neutron stars, where there are appreciable numbers of mesons and hyperons in addition to the nucleons and electrons at densities $\rho \gtrsim 10^{15}$ (if such a density is reached, a question that depends on the mass of the star), remains open, and consequently very little is, on the whole, known concerning the equation of state.

If we disregard hypothetical states such as the regions near singularities (cosmology, collapse), then the density of matter in the central regions of neutron stars is the largest encountered in nature. This remark, it might seem, speaks for itself. We add that the gravitational fields in neutron stars are also the largest (again, with the exception of the fields dealt with, for the time being only in theory, in the analysis of the cosmological problem and the collapse). It is clear by the same token that the deviations from the GRT, if they do take place and furthermore do so at densities much lower than $\rho_g \sim 10^{94}$ g/cm^3 [see (5)], should become manifest first for neutron stars.*

Third, the electrodynamics of the pulsars and the mechanism of their emission still remain insufficiently clear. These problems contain so many complicated elements (see [26e]) that they cannot be excluded from the list of the "important and interesting."

Thus, neutron stars and pulsars are among the focal points of modern physics and astronomy; their investigation will probably remain at the center of attention for many more years.

*The radius of a neutron star is $r_0 \sim 10$–30 km, i.e., smaller by five orders of magnitude than the radius of the sun $r_\odot = 7 \times 10^5$ km. Therefore the light emission of a neutron photosphere having the same temperature as the sun, $T_\odot \sim 6000°$, would be smaller by ten orders of magnitude than that of the sun.

*In Sec. 15 it was indicated that if a fundamental length l_0 does exist, violation of certain laws can begin at densities $\rho_0 \sim \hbar/cl_0^4$. Since in atomic nuclei we have $\rho_{nuc} \sim 3 \times 10^{14}$, and no sharp anomalies of the "fundamental type" are observed, we arrive at the estimate $l_0 \lesssim (\rho_{nuc}c/\hbar)^{1/4} \sim 10^{-13}$ cm, which can hardly be doubted even on the basis of more convincing evidence (as indicated, it is presently assumed that $l_0 \lesssim 10^{-16}$–10^{-20} cm).

18. Origin of Cosmic Rays and of Cosmic Gamma- and X-radiation

It was established more than 50 years ago that strongly penetrating radiation—cosmic rays—arrives on earth from outer space. The nature (composition) of this radiation remained unclear for many years. It is presently known, however, that cosmic rays are charged particles, namely protons, nuclei, electrons, and positrons. To be sure, we also receive x-rays and γ-rays from outer space, and undoubtedly also neutrinos. It is presently customary to designate as cosmic rays only charged particles of cosmic origin (this convention is all the more justified because at high energies the role of the charged particles is dominant, for example, with respect to the size of the flux or the released energy). The problem of the origin of the cosmic rays has been under discussion for decades,[23,27] but remains sufficiently "important and interesting," since disputes on this matter still continue, and the great importance of the question itself is subject to no doubt.

The fundamental point in the problem of the origin of cosmic rays is presently the choice between three models: the metagalactic model, the galactic model with halo, and the disk galactic model. In the metagalactic model, the bulk of the cosmic radiation received on earth comes from the Metagalaxy, i.e., it flows into the Galaxy from the outside. In the galactic models, on the other hand, it is assumed that the cosmic rays are produced in the Galaxy itself, primarily in supernova explosions and near pulsars situated in supernova envelopes. I am convinced that only the galactic models are acceptable and the main question now is the choice between the model with the halo and the disk model. In the former, the cosmic rays fill a quasispherical halo with characteristic dimension $R \sim 5 \times 10^{22}$ cm; in the latter (disk) model they are concentrated in a disk (radius $R \sim 5 \times 10^{22}$ cm thickness $h \sim 3 \times 10^{21}$ cm). The difference between the models is most strongly manifest in the average lifetime of the cosmic rays in the galaxies, this being $T \sim 10^8$ years for the model with the halo and $T \sim (1-3) \times 10^6$ years for the disk model.

Besides the choice between the models, the problem of the origin of cosmic rays has, of course, many other aspects. We need mention only plasma phenomena in astrophysics, mechanisms of particle acceleration in supernova explosions and near pulsars, solar cosmic rays and their propagation in the solar system,[27d] the chemical composition of cosmic rays and the energy spectrum of its various components, including the electron-positron component. The region of ultrahigh energies, $E \gtrsim 10^{17}-10^{18}$ eV, should be especially singled out. The origin of cosmic rays with such energies (particles with energy reaching $3 \times 10^{19}-10^{20}$ eV have been observed) is at present completely unclear.

The astrophysics of cosmic rays is an offspring of postwar astrophysics and is assuming an ever-increasing role in the latter. Of late, incidentally, one speaks more frequently not of cosmic-ray astrophysics but of high-energy astrophysics, which also includes questions of x-ray and gamma astronomy (to which the astronomy of high-energy neutrinos should be added).

The origin of cosmic x-rays and γ rays is not yet sufficiently clear.[27a, b, c, e, f] There are many known mechanisms of x and γ radiation, but which of them is decisive in the Metagalaxy, Galaxy, and in x-ray "stars" is unknown. Further, scattering of relativistic electrons by radiophotons, which constitutes the residual thermal radiation, is one of the sources of x-rays (γ rays are more readily produced by scattering of relativistic electrons by infrared and optical photons, since there are few electrons with very high energies in space). On the other hand, such scattering serves as one of the most effective causes of slowing down of electrons. Until recently, it seemed that the residual thermal radiation (it was discovered in 1965) could in all probability be regarded as thermal (black-body) radiation with temperature 2.7°K. But the region of the spectrum with wavelengths shorter than 2–3 mm still remain uninvestigated, and there are at present certain indications that this submillimeter part of the spectrum contains some additional powerful radiation. Whether this radiation exists and what its origin is are still-unanswered questions.

In general, certain aspects of x-ray and gamma astronomy have been mentioned here with sufficient justification.

19. Neutrino Astronomy

The hypothesis that the neutrino exists was advanced by Pauli in 1931. Only a quarter of a century later—not a short period in our stormy time—neutrinos were successfully registered near nuclear reactors. Naturally, the question arose: is it also possible to register neutrinos of extra-terrestrial origin?

Since the energy of a star comes from nuclear reactions, it is perfectly clear that neutrinos should be emitted by all stars. This pertains first, of course, to the sun (the distance from the earth to the sun is 1.5×10^{13} cm, and the distance to the nearest stars is of the order of 4×10^{18} cm; it is therefore clear, "other conditions being equal," that the flux of solar neutrinos should be 10^{11} times larger than the flux from the nearest stars). Attempts to detect solar neutrinos were begun a few years ago[28a] but have so far led to no affirmative results. Incidentally, even a refinement of the upper limit of the neutrino flux turned out to be quite valuable. In the nearest future we can expect registration of solar neutrinos by a very simple method (we have in mind the use of the isotope ^{37}Cl as the neutrino absorber), which will be followed, in all probability, by attempts to use other detectors as well (for example, ^{7}Li) with other reaction thresholds. The birth of neutrino astronomy is a major event, since the detection of neutrinos is the only known method of looking into the central regions of stars.

It is quite difficult to hope to be able to receive neutrinos from "ordinary" stars in the foreseeable future. The situation is different with supernova explosions and formation of neutron stars,* in which powerful neutrino fluxes can arise.[18f,24b,27b,28b] The same can be stated with respect to events that are still somewhat hypothetical, namely the collapse of supermassive stars (including galactic nuclei). Finally, it would be exceedingly

*It is possible that these are the same thing, but in principle, a supernova explosion can also lead to the formation of a white dwarf, a collapsed object, or to complete disappearance of the star.

important to register neutrinos produced during earlier stages of the evolution of the universe.[24b,28b,28c]. Unfortunately, the prospects in this respect are still not quite bright (the sensitivity of the known detectors must be increased by several orders of magnitude). However, it is precisely with respect to the prospects of improving measurement methods that, as taught us by the history of physics and astronomy, pessimism is least justified. Furthermore, existing estimates of the intensity of the "cosmological" neutrinos may turn out to be conservative. Bordering on all these trends in neutrino astronomy are the already mentioned (Sec. 10; see [18e, 27b] and [28b,28d]) investigations of high-energy neutrinos. Thus, neutrino astronomy "knocks on the door"; it is one of the most interesting new fields of scientific research, promising to yield valuable results, and perhaps also discoveries.

20. A Few Remarks on the Development of Astronomy

The last ten years alone have seen five discoveries of prime significance in astronomy (quasars, residual thermal radiation, x-ray "stars," cosmic masers operating on the lines of the molecules OH, H_2O, etc., and pulsars), not to mention many major accomplishments of somewhat smaller scale. In physics during the same time one can point to perhaps only two events of comparable importance—the discovery of the difference between the electronic and muonic neutrinos, and the observation of CP-invariance violation. If we also credit astronomy with some of the accomplishments in the field of cosmic research (the study of the moon and the planets), then the triumphant march of astronomy in our days becomes even more impressive.

Different scientific trends, when speaking of the qualitative aspect of the matter, do not develop uniformly. Concretely, we can state that after the Second World War astronomy entered a period of especially brilliant development, a second astronomical revolution. The first such revolution is associated with the name of Galileo, who started to use telescopes. I (as well as many others) have had occasion to write on this subject many times,[23] but the astrophysical part of the present article must also be concluded with a few remarks on this account.

First, progress in astronomy is undoubtedly indebted to the development of physics and space technology, which have made it possible to employ fantastically sensitive apparatus and to raise it above the limits of the atmosphere. Second, the content of the second astronomical revolution can be seen in the process of the changeover from optical astronomy to all-wave astronomy.

Third, no matter how remarkable the latest astronomical discoveries may be, they still have not taken it outside the scope of the known physical concepts and laws; nor did they make it necessary to review anything in the fundamentals of physics.

What does the future hold, what is the trend in the development of astronomy? It is very risky to attempt to answer such questions. It seems to me, however, that it is better to err than to be cautiously silent. I therefore permit myself a few predictions of small import.

It can be assumed that in the present decade (or at the most within 15 years), the second astronomical revolution will be completed—astronomy will have become all-wave, and those discoveries which were, in a sense, "just below the surface," will have been made. This should be followed by a quieter period (I have in mind the study of remote objects; investigations of planets and problems of extraterrestrial civilizations do not concern us here). In other words, there will come a heroic period and changes will occur in astrophysics (perhaps only for a time), changes analogous in some respects to those now being observed in microphysics (see Sec. 12). Incidentally, one cannot fail to note that astronomers have rich reserves in the possible flowering of neutrino astronomy and the astronomy of gravitational waves.

Finally, the principal question (principal at least from the point of view of the physicists) is whether astronomy will lead to a change, so desired by many of its representatives, in the fundamental physical concepts. Examples of such changes would be the need for introducing a scalar field in relativistic theory of gravitation, the observation of changes in physical constants with time, or deviations from known physical laws at large densities inside or near tremendous masses (galactic nuclei, quasars, neutron stars), etc.

Searches for new fundamental ideas and representations in astronomy (including cosmology) deserve, of course, our most persistent attention, but by the very nature of the matter, nothing is given here to foresee. By the same token, the "principal question" raised above remains unanswered. I can only note that I myself would not be at all surprised (moreover, I am inclined to believe in such a possibility) if the "new" physics and astronomy were to be needed only near classical singularities, i.e., if it turned out to be essential only in cosmology and for understanding of the concluding phase of gravitational collapse.

IV. CONCLUSION

The arbitrariness and the weakness of any "list of especially important and interesting problems," it is hoped, has already been given emphasis enough. It is also obvious that different topics are not equivalent in their significance, and that any such "list" will change in time. Were we to find, for example, even one superconductor with a critical point at room temperature and were the factors leading to this attainment to be understood, then the problem of high-temperature superconductivity could easily be excluded from the "list." The same thing would occur if a question were answered in the negative, say, if it became clear that it is impossible to produce high-temperature superconductors or that no long-lived superheavy nuclei exist.

Finally, to avoid misunderstanding on the part of budding physicists, it should be noted that it is also essential to engage in problems not included in this "list." Even without speaking of the absence of any rigid partitions between many of the different physical and technical problems, between investigations and developments, it suffices to mention how a new "especially important problem" is born. In most cases its parents, as well as the sources or causes of the discoveries, are "routine" problems, just as a genius is born of ordinary parents. Hardly anyone would have said in the Thirties that it is important to study the luminescence of liquids under the influence of γ rays. But this is exactly how the Cerenkov effect was discovered. The same can be said of the Mossbauer effect and a number of recent astronomical discoveries (for example, the observation of pulsars), etc.

In other words, many remarkable discoveries and scientific accomplishments are unpredicted and unexpected.

Thus, while a certain concentration of attention on known especially important problems of the present day is natural and reasonable, this should by no means lead us to overlook other trends or lead to inharmonious development of physics and astrophysics as a whole.

In conclusion, I take the opportunity to thank all those who read the manuscript of this article for their remarks. Their names are not mentioned so as not to burden them even indirectly with responsibility for the content and shortcomings of the article.

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Translated by J. G. Adashko