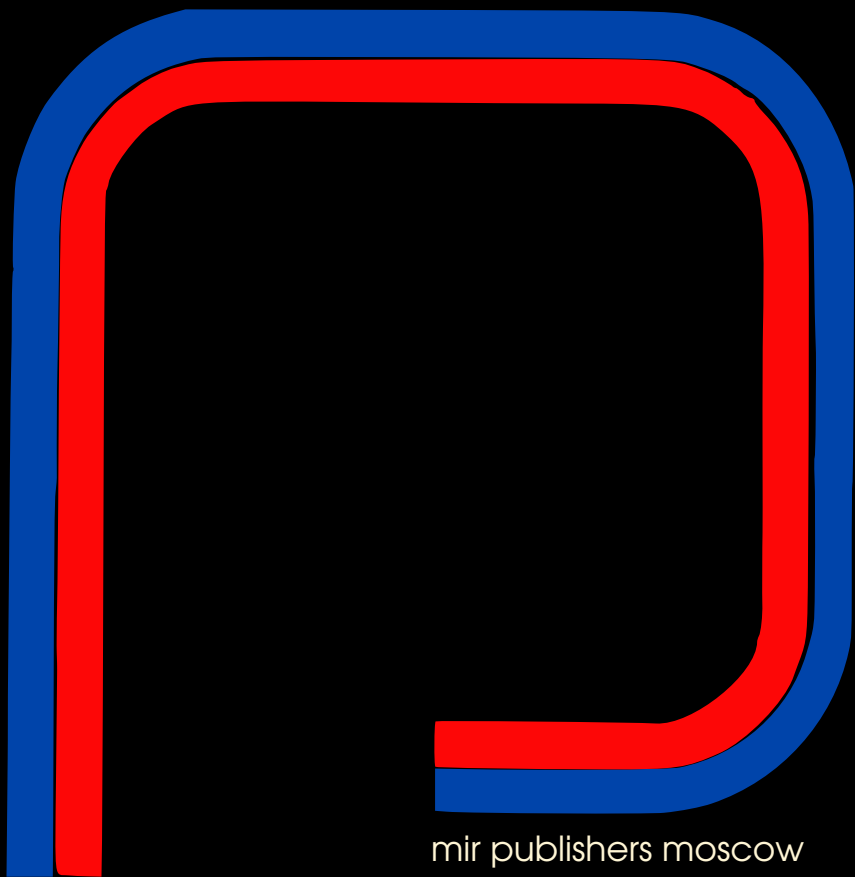# key problems of Physics and Astrophysics

V. L. GINZBURG

В. Л. ГИНЗБУРГ

О ФИЗИКЕ И АСТРОФИЗИКЕ

Какие проблемы
представляются сейчас
особенно важными
и интересными?

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KEY PROBLEMS

OF PHYSICS

AND ASTROPHYSICS

V. L. GINZBURG

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by Oleg Glebov*

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Preface

Physics has immensely grown and diversified in the recent decades which fact is being borne out by the emergence of such new sciences as astrophysics, biophysics, geophysics, chemical physics, physics of crystals, physics of metals, etc. This differentiation, however, has not deprived physics (perhaps, it would be more correct to say has not yet deprived) of a certain unity; here is meant the unity of the fundamentals, generality of many principles and methods as well as the bonds between various branches and fields of research. At the same time, differentiation and specialization are increasingly hindering visualization of the structure of physics as a whole, resulting, undoubtedly, in some disunity. This disunity seems to be, to a certain extent, inescapable, but the desire to compensate somehow for its consequences is quite justifiable, especially for young physicists, primarily students. It is a fact that even the best graduates of the physical (or related) faculties of our universities lack an overall view of the present situation in physics as a whole having specialized in a more or less narrow field of it. Of course, one cannot get a "bird's eye view" or at least versatility of knowledge overnight and the university training can hardly achieve these goals. But sometimes the lack of consistency or even lapses in knowledge are veritably astonishing. For instance, a person knows fine modern methods of quantum statistics or quantum field theory but has not the slightest idea of the prin-

ciple of superconductivity or the nature of ferroelectricity, he has not even heard of excitons or metallic hydrogen; he is unaware of the concepts of neutron stars, "black holes", gravitational waves, cosmic rays and gamma-radiation, neutrino astronomy and so on and so forth. This is, I believe, not because of human limitations or lack of time. It would, perhaps, take less time and effort for a student to get a basic physical "picture without formulas" of all the above and similar problems (or, at least, with the use of only the simplest formulas and quantitative concepts) than to prepare for a major exam. The difficulty lies elsewhere—a student does not know what to get acquainted with and how to do it. It is not enough for certain problems to appear in some of the numerous university courses or text-books. Moreover, the very problems that get the most attention at physical conferences or in periodicals are too novel to find their way into text-books or university curriculum.

It is hardly necessary to dwell on the subject and the conclusions seem to be self-evident. If we limit ourselves to discussing our good intentions or calling to improve and to update the university courses frequently our goal will never be reached. The most reasonable solution seems to deliver regular additional lectures for students according to a previously announced plan (8-10 lectures a year) not included into any of the established courses. Each lecture should be delivered by an expert in the respective field. These extracurriculum lectures should be each a review, simple but up-to-date, of a certain research field or problem. The Chair of Problems in Physics and Astrophysics at the Moscow Institute of Physics and Technology has scheduled a series of such lectures. But these lectures had

to be preceded by some sort of a general introduction, a "bird's eye view", an unavoidably fragmentary and cursory review of many problems, an attempt to give the presentation of current problems in physics as a whole. This task seems to be a difficult and, in a sense, not gratifying one, as its fulfilment can hardly be successful enough. Somehow, usually nobody delivers such lectures. However, since I considered such a lecture to be a prerequisite of the success for the above lecture series as a whole, I got down to work on it. This lecture was later presented several times to various audiences. The way it was received demonstrated unambiguously that such lectures are, to say the least, necessary and attractive (not only for students). This lecture eventually developed into a paper entitled "What Problems of Physics and Astrophysics Appear Now Especially Important and Interesting" that was published in the "Physics of Our Days" section of the journal *Uspekhi Fizicheskikh Nauk* [**103**, 87, 1971] and then translated into a number of languages and published as a book by "Znanie" Publishers (1971). The present small book is an extended and updated revision of that paper, it contains a few new sections, not to mention other alterations. Such alterations were caused, in particular, by accumulation of new data. It is hardly necessary to discuss the contents of the book in more detail here; one can get acquainted with it by looking through the Introduction and Contents.

There are reasons behind such a lengthy preface to so small a book. The thing is that its contents, character and style seem to be somewhat unconventional or, at least, not self-explaining. I have addressed my paper to budding physicists and astronomers; I have stressed that selection of "the most important and interesting

problems" is tentative and subjective in character; I have also marked that the evaluations under such circumstances are inevitably disputed but at the same time, I am on my part far from having any bias or pretensions to preach, to impose my opinions on the readers. Fortunately, as far as I have gathered, the paper has been accepted just this way by the majority of the readers, especially, by those to whom my message was addressed. But opposite opinions have been voiced, too. Some did not like the very idea of the paper. Others considered it to be intolerably biased, and especially against microphysics (I have even been granted a title of the "enemy of nuclear physics"). Still others charged me with a lack of modesty and with such-like sins that they inferred from my attempts to judge what is important and what is not, as well as from too frequent appearance of my name in the Bibliography which plays in the paper a purely auxiliary role. It would be inappropriate to answer all these allegations and reproaches here, all the more so that those, unfortunately, have not been published anywhere. But they are worth mentioning in order to caution the readers against possible danger to which they might be exposed and to stimulate thus a critical approach to the book. I myself have tried my best in this respect when preparing the present edition. But to pay attention to criticism does not mean to "fear the clamour of the Boeotians" and drop a cause which seems to be worthwhile.

As is clear from the above, I am interested in the opinions of as many readers as possible. I would be grateful for letters of criticism, suggestions and general remarks about the book. I am thankful to those whose advice has been used when this edition has been prepared.
1973

V. L. Ginzburg

Preface to the 1978 Edition

Mir Publishers proposed to prepare in a short time a new English edition of the book and I agreed to do my share of the work in a week's time since anyway I do not have now an opportunity to make a complete revision of the book. Therefore, only a few changes and improvements have been made in the text, a few references have been added and a short addendum has been specially written. Nevertheless I hope that the new edition will be useful.

February, 1978   *V. L. Ginzburg*

Contents

Introduction

Physics and astrophysics deal nowadays with an enormous number of various problems. The absolute majority of these problems are quite reasonable as the scientists attempt, if not to uncover the secrets of nature then at least to gain some new knowledge of it. No such problem may rightly be spoken about as devoid of interest or importance. Incidentally, it is hardly feasible to define consistently enough what is "not important" and/or "not interesting" in science. Meanwhile, there exists, in fact, a hierarchy of problems which is reflected in all the scientific (and sometimes not only scientific) activities. The "especially important physical problems" are frequently identified by the potential effect they will have on technology or economy. Often the reason is a special fascination or fundamental nature of the problem, but sometimes this is due to the effects of a vogue or some obscure or hazardous factors (naturally, we shall try to avoid discussing problems of the latter class).

It is not for the first time that a list of "the most important problems" is being compiled and commented upon. To this end conferences are frequently convened or special commissions are set up at which rather bulky reports are compiled. I do not mean to generalize but may state that I have never seen anyone reading these memoranda on the most important problems with great interest. It seems that specialists are not really

in need of them and they are not attractive for a wider reading public (of course, such documents may prove to be necessary for planning and financing the scientific developments).

And yet, budding physicists and astronomers (and not only they) cannot help asking a simple question: what is "hot" in physics and astrophysics? Or, in other words, what seems to be most important and interesting in physics and astrophysics at present? Assuming that a sufficiently large number of readers is interested in this question, this book attempts to give an answer. So this book is not a product of a commission's deliberations and not even the result of special investigations as writers are apt to say; it is, rather, a personal view of the author. This, at least, makes it possible to do away with the dryness and bare style of the more or less official documents.

Listed below are the problems that seem to me now to be especially interesting and important. At the same time, I do not attempt to define strictly the very concepts of importance and interest and to substantiate the selection criteria. Anyone has a right to his own views and he should not feel obliged to coordinate them with those of anybody else unless he declares his views to be authorized or superior. The author attempts nothing of the kind, to say nothing of the organizational suggestions; in order to stress this personal approach I do not even try to avoid using personal pronouns as is customary in scientific publications.

It would be interesting and, maybe, useful to compare the lists of the "most important problems of physics and astrophysics" compiled by various persons. Unfortunately, no such polls of scientists' opinions have ever been conducted as far as I know. Therefore, I can

only suggest that the majority of such lists would have much in common providing that the following difficult requirement is met, namely, that consensus of opinion is reached in defining a "physical problem" as distinct, say, from fields, tendencies or objects of physical studies. Without going into details of definitions I shall only note that by a problem I mean such a question the answer to which is substantially unclear in character and content. We should deal not with technological developments, measurement projects, etc., but with possibilities to create some new substance of unusual properties (for instance, a high-temperature superconductor), to establish the limits of applicability of a theory (for instance, the general relativity theory) or to throw light on something really unknown (say, the cause or mechanism of combined parity violation in the decay of K-mesons). This is just the reason why we practically ignore in the book quantum electronics (including the majority of laser applications), many problems in physics of semiconductors (including miniaturization of circuits and devices), nonlinear optics and holography and some other interesting trends in modern optics development, the problems of computer technology (including the problem of developing novel types of computers) and many other problems. The above issues are, undoubtedly, very important and give rise to a multitude of not only technological but also physical implications. However, they do not now involve any essential "physical problems" or, so to say, a basic "uncertainty" concerning the underlying physics. For example, there existed such an uncertainty prior to the development of the first laser, even though the principles used later for laser design were clear. Increasing the power or altering other parameters

of a laser or any other device may be a necessary, difficult and commendable task, but it is, of course, qualitatively different from developing a device or apparatus based on new principles. At the same time this example is rather typical for demonstrating the arbitrary nature of the boundary between the basic and technological problems in physics. For instance, increasing the laser power by many orders of magnitude (this problem is currently very important) cannot be classified as a pure technological or some kind of "nonbasic" task. The same may be said about the development of X-ray lasers and grasers, the laser analogues for X-rays and gamma-rays. X-ray lasers and grasers are not only far from being built, but there are yet no clear enough concepts for developing them; thus, they present a typical "important and interesting problem" in terms of our selection rules. The same is true for almost any field—a significant breakthrough almost always constitutes a problem. But not all such problems have ripened enough, not all the "prizes" seem to be fascinating enough to be worth striving for and there exists in fact a hierarchy of problems.

It has been suggested above that a "poll of scientific opinion", if attempted, would yield a large measure of agreement on selection of current "especially important and interesting problems". However, significant disagreements would be inevitable, too, particularly, as far as priorities in allocation of resources and concentration of efforts for solving various problems is concerned. This is clear, in particular, from papers [1-5]. The problem of resources and priorities is, however, related to a number of factors lying outside the scope of purely scientific problems. For instance, the design of mammoth accelerators is, undoubtedly, of great

scientific interest and what is argued primarily is whe-
ther the corresponding expenditures produce the results
that may justify the curtailing of researches in other
areas. We shall ignore this aspect and deal with the
scientific issues only. However, even with this "sim-
plification" and restriction, opinions may diverge
sharply. For instance, the following most important
problems of solid state physics are listed here: high-tem-
perature superconductivity, production of metallic
hydrogen and some other substances with unusual
properties, metallic exciton liquid in semiconductors,
surface effects and the theory of critical phenomena (in
particular, the theory of the phase transitions of the
second order). However, an article entitled "The Most
Basic Unsolved Problem in Solid State Physics" [5]
states that such a problem is the explanation of empi-
rical formula for the heat of formation of some crystals
from other substances. It was not without effort that
I found some interest in this problem but I completely
failed to understand why the problem is "the most
basic one" and moreover, I greatly doubt it. What
is the conclusion? There seems to be only one: no
absolute list of the most important problems can be put
forward, and there is no need for it. But it is necessary
and useful to deliberate what is important and what
is not, to argue about it, to be bold in bringing forward
suggestions and defending them (but not imposing
them). This is just the spirit this book contrives to
express.

Thus, the subjective and controversial character of
this book is quite apparent and the readers have been
warned (though, of course, such warnings are rarely
heeded). It is left only to note that the division of the
book into three parts (macrophysics, microphysics and

astrophysics) is quite arbitrary, too. Actually, we discuss superheavy nuclei under the heading of macrophysical problems though they may be said to constitute a microphysical problem. Also, the problems of the general theory of relativity are discussed in the astrophysical rather than in macrophysical part but the only reason for this is that the general relativity theory is used mainly in astronomy (to say nothing of the fact that the difference between astrophysics and, say, macrophysics is, essentially, of quite another character than the classification of physics into micro- and macrophysics). Finally, it should be noted that the book practically ignores biophysics, let alone other less important research areas related to physics and astrophysics. Meanwhile it is just the cooperation between physics and biology and the application of the physical methods and concepts that have proved especially fruitful and important for the development of biology and, potentially, medicine, agricultural sciences, etc. That would be too gross a mistake for physicists to avoid "biologically biased" problems on the grounds of their not being "physical" (this has been convincingly argued in [2]). Moreover, it is conceivable that the cooperation with biology and the attempts to solve some biological problems will stimulate the development of physics proper as physics was and still is a source of inspiration and new ideas for many mathematicians. Thus, though the book does not pay due attention to the bonds between physics and biology this is by no means due to my underestimating of their importance, the reasons are, first, my insufficient knowledge of biophysics and biology in general and, second, the necessarily limited scope of the book.

1

Macrophysics

§ 1. Controlled Thermonuclear Fusion

The solution of the problem of controlled thermonuclear fusion implies utilization of nuclear fusion reactions for power production. The following basic reactions are involved:

$$\left.\begin{array}{l} d + d \rightarrow {}^3He + n + 3.25 \text{ MeV,} \\ d + d \rightarrow t + p + 4.0 \text{ MeV,} \\ d + t \rightarrow {}^4He + n + 17.6 \text{ MeV} \end{array}\right\} \qquad (1)$$
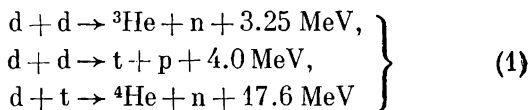
(here d and t are the nuclei of deuterium and tritium, p is the proton and n is the neutron).

A number of other reactions may be of some importance too, particularly the reaction ${}^6Li + n \rightarrow t + {}^4He$ allowing neutrons to be used and, at the same time, to regenerate tritium.

That nuclear fusion energy will be utilized in some way or other is hardly questionable: one has only to mention the "obvious" possibility of useful underground explosions. On the other hand, controlled thermonuclear fusion has been drawing great attention for over 20 years but the outlines of the future thermonuclear reactor are still far from being clear [6, 7].

The primary and at the same time, the most important requirement for a thermonuclear reactor, is the condition $n\tau > A$, where n is the concentration of elec-

trons or nuclei in plasma* and τ is the time of plasma
confinement, or lifetime, in the machine (for instance,
τ may be taken to be the time in which the plasma
energy decreases by fifty per cent or more). As for the
constant A, it characterizes the nuclear fuel and for
the pure deuterium $A \sim 10^{16}$ cm^{-3} s while for the most
advantageous mixture consisting of equal amounts of
deuterium and tritium, $A \sim 10^{14}$ cm^{-3} s. Thus, for
the reactor to function (the energy it produces should
be greater than that required to establish and main-
tain high temperature in plasma) the following inequa-
lity should be satisfied:

$$n\tau > 10^{14} \, \text{cm}^{-3} \, \text{s} \tag{2}$$

Physically, this condition is clear enough—the lon-
ger the process runs the less intense the reaction of
"burning" may be (its rate is proportional to n^2).

Magnetic confinement of plasma might appear to be
the simplest feature for the plasma reactor. Out of the
toroidal magnetic traps, *Tokamaks* seem at present
to be the most advanced (or, at any rate, most popular)
reactors of this type. In such systems, as in any other
magnetic confinement systems, the plasma concentra-
tion n cannot be too high, otherwise, the magnetic
"walls" will be destroyed by the high pressure of the
hot plasma. In the *Tokamak* now in operation the
concentration n may be as high as $3 \cdot 10^{13}$ cm^{-3} and
the hold-up time $\tau \sim 0.1$ s. Thus, to satisfy con-
dition (2) the product $n\tau$ should be increased at least

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* Of course, plasma is fully ionized at high temperatures
required for the reactor to operate ($T \geqslant 10^8$ K) so that under the
quasi-neutral conditions the concentrations of electrons and
nuclei of hydrogen isotopes (deuterium and tritium) are the
same.

thirty times. The plasma temperature should also be raised considerably (the deuterium-tritium mixture "burns" fast enough only at $T \gg 10^8$ K while the highest temperature of ions that can be attained in a *Tokamak* is only 10^7 K [7]). What is more, remember that the heat conduction to the walls in *Tokamaks* is still relatively high. Therefore according to [6b, 8] to set up a self-maintained reactor with equal amounts of deuterium and tritium (that is, to satisfy condition (2)) using the existing level of plasma thermal insulation and the magnetic field $H = 10^5$ Oe, the smaller radius of the toroidal chamber containing plasma should be $a = 1.4$ m. But in this case the radius of the coils creating the magnetic field is as large as 4.5 m. Thus, we have to set up an enormous field of the order of a few hundred cubic metres in volume. It stands to reason that the superconducting magnets should be used if this is at all possible (otherwise, apart from all other considerations, there seems to be no hope for obtaining a favourable energy balance).

Such extreme difficulties that may prove to be even greater in real systems, stimulate other approaches aimed at solving this problem. Numerous suggestions in this field have been reported [6-11]: the use of "open" magnetic traps, short time discharge ("rapid pinch") or high-frequency discharge in plasma; the heating of deuterium particles or plates with laser beams [9, 10] or powerful electron beams [11]; the acceleration of charged particles to heat deuterium and so on.

Recently, the possible use of lasers has drawn special attention ("laser thermonuclear fusion"). The laser thermonuclear reactor is, so to say, the opposite of the "slow" thermonuclear reactors of the *Tokamak* type and other types. Actually, the laser method (as well,

incidentally, as the electron beam heating) implies heating of solid particles with the initial nuclei concentration n of about $5 \cdot 10^{22}$ cm^{-3} (this is the nuclei concentration in solid hydrogen under the atmospheric pressure) on all sides. Therefore, to satisfy condition (2), it is sufficient for a particle to "function" for only $2 \cdot 10^{-9}$ s, while for a compressed material this period may even be shorter. But, firstly, the laser light pulse energy in this case should be as high as 10^4-10^5 J $= 10^{11}$-10^{12} ergs and, maybe, even higher. Meanwhile, the real lasers produce pulses with a duration of $\tau \sim 10^{-9}$ s having an energy E of no more than 10^3 J (the power $W = E/\tau \sim 10^{12}$ W). Secondly, the shape of the pulse must be chosen (and, above all, generated) so that the particle would not have time to be scattered but, on the contrary, would be pinched owing to heating of its surface and the "recoil" of the ejected (evaporated) material. And thirdly, the available powerful lasers producing short-duration pulses have rather low efficiencies—less than one per cent. To build a useful reactor (which is, naturally, the only one needed to solve the energy problem) the laser efficiency must be raised, apparently, up to not less than 10-20 per cent. If all these problems are solved, the outlines of the future thermonuclear reactor may be visualized as follows: small solid or liquid particles (of a radius of the order of 0.01-0.1 cm) of a deuterium-tritium mixture drop to or, in some other way, reach the laser focus in several chambers. When "burning", the particles emit neutrons with an energy of 14.1 MeV that may be used to heat the surrounding material and to regenerate tritium (the remaining 3.5 MeV of the energy released in the reaction d + t → ^{4}He + n are transferred to the ^{4}He nucleus; in principle, this energy may

be utilized, too). Complete "burning" of a particle approximately 1 mm in size releases energy of about $3 \cdot 10^8$ J, which corresponds to the explosion of about 50 kg of TNT.

Immense difficulties are yet to be overcome before both the thermonuclear reactor with the magnetic plasma confinement and the laser thermonuclear fusion or other explosion-type systems (pulsed heating of particles with the electron beam and other techniques) can be built; in particular, some characteristics of the existing set-ups (models) will have to be increased by a few orders of magnitude. Nevertheless, at present, in contrast to a comparatively recent past, the general feeling is that of optimism and it seems to be basically possible to develop some kind of a thermonuclear reactor. But what type or types of reactors will it be possible to build, when this will take place and what difficulties will have to be overcome—all these questions are by no means clear. Moreover, the difficulties involved are so significant that they cannot be considered purely technological. Hence, development of the controlled thermonuclear reactors should be counted among the most important physical problems. Likewise, there seems to be a need for competition between various approaches to the problem of thermonuclear fusion (we mean fair competition but not rivalry).

One more general consideration is clearly exemplified by the problem of controlled thermonuclear fusion: practically any large-scale physical problem does not stand apart from others but is closely related to the entire physics. Therefore great efforts made for solving a certain problem may bear fruit in a more general sense, they may stimulate numerous studies, give rise to new methods and approaches, etc. For instance,

plasma had attracted considerable scientific interest even before the early fifties when there emerged the problem of controlled thermonuclear fusion. But one can hardly overestimate the importance of the research in this field for the development of other aspects of plasma physics dealing with gas plasma, solid-state plasma and space plasma.

§ 2. High-Temperature Superconductivity

The phenomenon of superconductivity was discovered in 1911 and for many years it remained not only unexplainable (perhaps, the most puzzling phenomenon in macrophysics) but practically useless, too. The latter is due, primarily, to the fact that up till now superconductivity has been observed only at low temperatures. For instance, the first superconductor discovered—mercury—has the critical temperature T_c of 4.1 K. A high value $T_c \approx 21$ K is exhibited by a certain alloy of Nb, Al and Ge studied only recently. In 1973 it was found that Nb_3Ge has $T_c = 23.2$ K (there is a better-known compound Nb_3Sn found to be a superconductor in 1954 with $T_c = 18.1$ K). The utilization of superconductors becomes especially difficult in the vicinity of T_c (but, of course, below it, since by definition at $T > T_c$ a metal ceases to be superconducting). Suffice it to say that in this region the critical magnetic field H_c and the critical current J_c (i.e. the field and the current destroying superconductivity) are very small (when $T \to T_c$ the values of H_c and J_c tend to zero). Thus, the superconductors may be used only when cooled by helium (the boiling point $T_b = 4.2$ K) as liquid hydrogen (the boiling point $T_b = 20.3$ K) freezes already at 14 K (in general it is

both inconvenient and difficult to use solids as coolants).

As recently as 25-30 years the production of helium was low (it is not enough even now) and the liquefaction technique was inadequate. Only a small number of low-capacity helium liquefiers were operating in the world. The use of superconductors for the construction of superconducting magnets (which is the most important application of superconductors so far) was to a no less extent hindered by low values of H_c and J_c for the materials available at that time (for Hg the field $H_c \approx 400$ Oe even at $T \to 0$).

However, things changed radically already at the turn of the last decade. Liquid helium is readily available now. Where it is done properly laboratories and institutes even do not install liquefiers ordering instead by phone the required amounts of liquid helium from specialized firms or factories (helium is transported in large Dewar vessels). The "magneto-current barrier" has also been overcome, superconducting materials available now make it possible to construct magnets with a field H_c as high as hundreds kilooersteds (a magnetic field $H_c \approx 400$ kOe is required to destroy superconductivity of the above-mentioned alloy of niobium, aluminium and germanium with $T_c \approx 21$ K). It is true that the materials being in common use now have yet too low critical fields and currents for a 300-400-kOe magnet to be constructed. But this seems to be a purely technological problem. There seem in principle to be no factors hindering the construction of, say, a 300-kOe magnet at helium temperatures*. Quite

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* Superconductors with high H_c and J_c were, basically, the result of a large-scale research and technological development work. No decisive role was played here by theoretical studies

the reverse, a fundamental and vague problem in superconductivity is an extremely attractive possibility of creating high-temperature superconductors, that is, metals that become superconducting at liquid nitrogen temperatures ($T_b = 77.4$ K for nitrogen) or, even better, at room temperatures.

I discussed in detail the current state of high-temperature superconductivity in [12]. Therefore, only a few considerations are presented here.

Superconductivity appears in metals in the vicinity of the Fermi surface as soon as the electrons are attracted to each other thus producing pairs undergoing something like Bose-Einstein condensation. The critical temperature of superconducting transition T_c is proportional to the binding energy of electrons in a pair and is determined, roughly speaking, by two factors: the force of attraction (bonding) which may be described by a factor g and the width $k\theta$ of that energy range near the Fermi surface where there still exists the attraction between electrons. We have

$$T_c \sim \theta \exp\left(-1/g\right) \qquad (3)$$

The majority of the known superconductors have $g \leqslant 1/3\text{-}1/4$ (formula (3) may be used directly for $g \ll 1$). The temperature θ in (3) depends on the mechanism giving rise to the attraction between the electrons. In the known cases this mechanism seems to be due to the interaction between electrons and lattice. Then $\theta \sim \theta_D$, where θ_D is the Debye tempera-

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especially in the case of high critical currents. On the contrary, some other advances were initiated by theoretical developments. Thus, the roads to success may differ essentially depending on the circumstances.

ture whose physical meaning is illustrated by the fact that $k\theta_D$ is the energy of the shortest-wave phonons in a solid ($k = 1.38 \cdot 10^{-16}$ erg $\cdot$ deg^{-1} is the Boltzmann constant). The wavelength of such phonons is $\lambda \approx a \approx \approx 3 \cdot 10^{-8}$ cm (a is the lattice constant) and $k\theta_D \sim$ $\sim \hbar\omega_D$ ($\omega_D \sim u/a \sim 10^{13}$-$10^{14}$, where $u \sim 10^5$-10^6 cm/s is the sound velocity). Thus, $\theta_D \sim 10^2$-10^3 K.

When $\theta_D = 100$ K and $g = 1/2$ we have, according to formula (3), $T_c \sim \theta_D e^{-2} = 13.5$ K and in general for the phonon mechanism $T_c \lesssim 30$-40 K. Thus, on the one hand, there seem to be possibilities left to increase T_c by conventional methods (creating new alloys and treating them), not mentioning the compounds of the metallic hydrogen type (see below). On the other hand, it is clear why it would be difficult or, rather, impossible to create really high-temperature semiconductors with $T_c \gtrsim 80$-300 K based on the phonon mechanism (here again we do not take into account metallic hydrogen).

Hopes for obtaining high-temperature superconductivity are based on using the exciton mechanisms of attraction between electrons. The point is that in solids apart from the lattice waves (phonons in quantum language), electron-type excitations called excitons may appear. In molecular crystals excitons are represented by an excited state of a molecule "jumping" from one molecule to another and, hence, travelling along the crystal. In semiconductors the simplest example of an exciton is an electron and a hole bonded by the Coulomb forces and thus forming a quasi-atom similar to the positronium atom. The energy of excitation (binding) of such excitons (meaning the excitons of the electron type; sometimes other types of excitations are called excitons) range typically from a few

hundredths of electron-volt to a few electron-volts. Similar to the exchange of phonons, the exchange of excitons may give rise to attraction between the conduction electrons. But in such a case we obtain in a formula of the type (3) the temperature $\theta \sim E_e/k \sim$ $\sim 10^3$-10^5 K (here E_e is the exciton energy; the energy $E_e \sim 1$ eV corresponds to a temperature $\theta \sim 10^4$ K). Therefore, if the exciton exchange could provide for a sufficiently strong attraction between electrons ($g \geqslant 1/4$-$1/5$) the critical temperature T_c would be high. There have been suggested a few approaches to making use of the exciton mechanism. In my opinion the most promising ones are those using laminated compounds and "sandwiches", thin metal layers with insulating plating.

We cannot discuss this side of the problem in more detail here (see [12] and the references cited there). It should only be noted that very few relevant studies, both theoretical and experimental, have yet been carried out. While the work on thermonuclear fusion has been going on in earnest for over 20 years, the studies of high-temperature superconductivity are only beginning to develop. Furthermore, there might be no need at all for any ultracomplicated syntheses of new compounds and there might be chances of success with comparatively modest (though up-to-date) means. Therefore I would not be too much surprised to read about the creation of a high-temperature superconductor in a current issue of a physical journal (though that would, probably, be the sensational news and we would hear about it over the radio or read in the newspapers). It is probable too that high-temperature superconductors are very hard, or essentially impossible, to produce. It is no wonder, there-

fore, that evaluations of the situation range from hope-ful [12] to quite pessimistic [14].

In the recent years there appeared various reports about discoveries of superconductors with $T_c \sim 60$-90 K. But all of them proved to be erroneous. I hope this will not happen to the report about the discovery of superdiamagnetism in CuCl at the temperatures up to 100-150 K [14a]. It is unclear, though, if this effect is superconductivity or a new similar phenomenon [14b]; in any case, the first priority now (February 1978) is to repeat the experiments (see also the Addendum at the end of the book).

§ 3. New Substances (Production of Metallic Hydrogen and Some Other Substances)

A great number of various substances exist on Earth under natural conditions or have been produced artificially: chemical compounds, alloys, solutions, polymers, etc. Generally speaking, creating new compounds is a matter of chemistry or technology but is not a physical problem. But this is not so in the case of truly unconventional (so to say, exotic) substances. Among them, we could mention high-temperature superconductors discussed above or hypothetical crystals (if they might be created!) with closely packed lattices which would possess extremely high mechanical and thermal properties [15]. Thus closely packed carbon ("superdiamond") would possess a hardness (compression modulus) greater by an order of magnitude than the hardness of diamond. Unfortunately, the author is not acquainted with the current state of the problem and even does not know if it could be

accepted as a physical problem at all. However, one such "new substance" is known whose study constitutes an important and interesting problem which attracts great attention during the last few years. This substance is metallic hydrogen.

Under normal conditions (say, under atmospheric pressure) hydrogen is known to be in a molecular state, it boils at $T_b = 20.3$ K and becomes solid at $T_m = 14$ K. The density of a solid hydrogen $\rho = 0.076$ g/cm³ and it is a dielectric. However, under strong enough compression, when the outer electron shells are crushed, any substance must convert into a metallic state. The density of metallic hydrogen can be estimated only roughly by taking the distance between the protons to be of the order of the Bohr radius $a_0 = \hbar^2/me^2 = 0.529 \cdot 10^{-8}$ cm. Hence $\rho \sim M a_0^{-3} \sim 10$ g·cm⁻³ ($M = 1.67 \cdot 10^{-24}$ g is the proton mass). A lesser density value is obtained by quantitative, though unreliable, calculations; for instance, according to [16] molecular hydrogen is in thermodynamic equilibrium with metallic hydrogen under the pressure $p = 2.60$ Mbar when the density of metallic hydrogen $\rho = 1.15$ g·cm⁻³ (in this case the density of molecular hydrogen is 0.76 g·cm⁻³); according to [17] in equilibrium, $p = 1$-2.5 Mbar; uncertainty here is due to the lack of reliable data on the equation of state for molecular phase. Metallic hydrogen may probably be superconducting with T_c reaching 100-300 K (for metallic hydrogen the Debye temperature $\theta_D \sim 3 \cdot 10^3$ K so that in accordance with formula (3) for $g < 1/2$ the temperature $T_c \leqslant 500$ K).

Creation of such, in some respects the simplest, metal as metallic hydrogen and determination of its critical temperature T_c are not only of obvious physical

interest but may be of urgent astrophysical significance (suffice it to say that large planets such as Jupiter and Saturn must contain to a considerable extent metallic hydrogen, see [18]). But it is still more important that metallic hydrogen may prove to be stable (though, of course, metastable) even in the absence of pressure. There are well-known examples of similar quite durable metastable modifications such as diamond that possesses a higher free energy than graphite at low temperatures and pressures. As for metallic hydrogen, there seem to be some indications that it will be stable in the absence of pressure [17, 19] but it is not clear if this phase can be maintained sufficiently long. Leaving apart the question of stability and lifetime of the metastable state, a theoretical study [17a] into a possible structure of metallic hydrogen has yielded interesting and unexpected results: under zero pressure metallic hydrogen should have a filamentous structure without ordering along the filaments, that is, it should have only two-dimensional periodicity (a triangular lattice is formed by filaments in a plane perpendicular to them). Under pressure metallic hydrogen may become liquid even before the equilibrium pressure is reached (the pressure under which metallic hydrogen coexists with molecular hydrogen). In this case solid molecular hydrogen will, obviously, turn under pressure into liquid metallic hydrogen. However, the liquid state may need pressures higher than the equilibrium one. However, the above results have not yet been verified and other views have been reported [17b].

We could hardly expect new advances in the studies of metallic hydrogen without resorting to experimental work, namely, the attempts to produce metallic

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hydrogen* (to do this, though, the parameters of mole-
cular hydrogen under high pressures must be deter-
mined as they are not yet known). The results of stu-
dies of various alloys of metallic hydrogen with hea-
vier elements may prove to be interesting. The problem
of metallic hydrogen (both light and heavy, that is,
deuterium) is rated now among the most urgent ones.
If the problem is solved "successfully" and metallic
hydrogen proves to be sufficiently stable (long-lived)
under low pressures and even superconducting into
the bargain, then production and investigation of me-
tallic hydrogen will become one of the primary tasks
of the macroscopic physics.

Development or utilization of materials having
extraordinary properties is a favourite subject for the
science-fiction authors. For them evidently, everything
is possible. But even respectable scientific publications
have been known to report the discoveries of quite
unusual substances not confirmed later. The reason is,
on the one hand, that in many cases substances are
produced in very small amounts for very short periods
of time (for instance, with an explosion) or, say, under
a very high pressure so that their properties are hard
to ascertain. On the other hand, the authors are, of
course, tempted to claim a great discovery. Such cases
are instructive, in particular, as a reminder that any
discovery should be comprehensively checked and re-
checked before being finally accepted.

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* The first positive results in this respect, even though they
have a tentative character, have been reported in the USSR,
USA and Japan.

§ 4. Metallic Exciton (Electron-Hole) Liquid in Semiconductors

When there are conduction electrons and holes in a semiconductor (for instance, generated by illumination), then at temperatures low enough they should combine giving rise to the above-mentioned excitons, hydrogen-like "atoms" similar to the positronium. In a first approximation the bond energy and the radius of such excitons in the ground state are

$$E_{0,e} \sim \frac{e^4 m_{eff}}{2\epsilon^2 \hbar^2} = \frac{E_0 m_{eff}}{m\epsilon^2} \text{ and } a_{0,e} \sim \frac{\hbar^2 \epsilon}{m_{eff} e^2} = \frac{a_0 \epsilon m}{m_{eff}} \quad (4)$$

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

Since in some cases $\epsilon \gtrsim 10$ and $m_{eff} \lesssim 0.1$ it is clear that the radius of excitons $a_{0,e} \gtrsim 10^{-6}$ cm and their energy $E_{0,e} \lesssim 10^{-2}$ eV ~ 100 K. These differences (in comparison to the hydrogen atom) are due to the fact that in this case the Coulomb interaction force is ϵ times weaker and the effective mass m_{eff} is small (as compared to the mass of a free electron m)*.

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* In the cases we are interested in, the exciton radius $a_{0,e} \gg$ $\gg a_0 \approx 5 \cdot 10^{-9}$ cm and this very fact allows us, generally, to use the Coulomb law to describe the interaction between an electron and a hole taking into account the effect of the surrounding medium (in this case the interaction energy between the charges $-e$ and $+e$ is attractive and has a magnitude of $e^2/\epsilon r$, where r is the distance between the charges).

We have already mentioned in discussing metallic hydrogen that a rough criterion of high density and metallization reduces to an equality of the electron shell size and the distance between the nuclei. For excitons in a semiconductor this means that their configuration is dense at a concentration $n_e \sim a_{0,\,e}^{-3} \sim 10^{18}$ cm^{-3}. Thus, a high density obtained for hydrogen under pressures of millions of atmospheres for excitons corresponds to a quite normal concentration of electrons and holes in a semiconductor $n \sim 10^{18}$ cm^{-3}. Just the sole possibility of simulating ultra-high pressures in a semiconductor lends sufficient importance to this problem. This conclusion is strengthened when we consider a possible behaviour of a dense system of excitons in a semiconductor [20]. Such a system should become liquid and form drops. Usually, they consist of an electron-hole metal, that is, are similar to a liquid metal. But it cannot be ruled out that in some cases, they have a "molecular" structure, in this case they are similar to a liquid hydrogen consisting of molecules H_2 (molecules in the molecular, and hence dielectric, exciton "liquid" are represented by biexcitons—double excitons). The electron-hole (exciton) liquid may, in principle, exhibit superconductivity or superfluidity. In short, exciton liquid in semiconductors should possess a number of fascinating properties and peculiarities, depending, of course, on the nature of the semiconductor "container" used. Extensive experimental studies of this problem are underway now [20].

Two more arguments could be brought forward to support the conclusion about the importance of this problem. Firstly, the excitons in a semiconductor will allow to simulate not only ultra-high densities (pressures) but the effect of ultra-high magnetic fields, too.

This will be discussed in § 6. Secondly, the next in line are the studies of excitons in unidimensional and two-dimensional systems: on solid surfaces (two-dimensional or quasi-two-dimensional system) and in various quasi-unidimensional formations (long polymers, intersection of crystal edges, "whickers"—thin crystalline filaments, and dislocations). Generally speaking, such systems can also produce electron-hole "atoms" [21] but the high density criterion here will be $n_e \sim a_{0,e}^{-2}$ (two-dimensional system) or $n_e \sim a_{0,e}^{-1}$ (unidimensional system). This implies that the "liquid" transition occurs in a two-dimensional system with the exciton concentration as low as 10^{12} cm^{-3} (at $a_{0,e} \sim 10^{-6}$ cm). Besides, there are other aspects of the problem of surface excitons which are of great interest, particularly, for high-temperature superconductivity [12] (see also § 8).

We have already stressed that the list of important and interesting problems discussed in this book is far from being complete and that the classification into the "important" and "unimportant" categories is a difficult and, above all, arbitrary procedure. We mentioned it once more because we discuss here only the exciton liquid out of all the problems of the physics of semiconductors. Physicists working with semiconductors would undoubtedly add a number of other problems: metal-dielectric phase transitions, disordered systems, etc. And still I would say that the problem of exciton liquid in semiconductors is distinguished by its novelty (in any case, it was so in 1973!) elegance and versatility.

§ 5. Second-Order Phase Transitions (Critical Phenomena)

The superconducting transition, the conversion of helium I into superfluid helium II, transformation of paramagnetic state into ferromagnetic one, many ferroelectric transitions and some other transformations of alloys—all these are the well-known examples of the second-order phase transitions. These transitions are distinguished by the absence of liberation (or absorption) of latent heat and discontinuity of volume or lattice parameters, i.e. the transformation may be considered, in a sense, continuous. At the same time discontinuities of the specific heat, compressibility and other characteristics are observed at the transition point and in the vicinity of it many of these characteristics behave anomalously. For instance, near the helium I $\rightleftarrows$ helium II transition and some other transitions the specific heat may be satisfactorily approximated by $C \sim \ln |T - T_c|$, where T_c is the transition temperature (the lambda-point temperature). In the case of ferromagnetic and ferroelectric transitions the magnetic permeability and the dielectric constant tend to infinity when $T \rightarrow T_c$ and are often approximated by the Curie law $\chi \sim |T - T_c|^{-1}$.

Some of the first-order transitions close to the so-called tricritical or critical Curie point are similar to the second-order phase transitions (see [22]). The crux of the matter is that variation of some parameters (for instance, pressure) may transform second-order transitions into first-order ones (the critical Curie point is just the point of contact of p-T curves for transitions of different types). The first-order transitions close to the critical Curie point are, naturally, akin

to the second-order transitions (the latent heat is non-zero but small, specific heat behaviour is anomalous and so on). The examples of such transitions are some ferroelectric transformations, the superfluid transition in a ^{4}He-^{3}He mixture and, apparently, the $\alpha \rightleftarrows \beta$ transition in quartz. Finally, there is a similarity between the second-order transitions and the liquid-vapour (gas) critical points and some others.

The problem here is to gain sufficiently full knowledge, both quantitative and qualitative, of various phenomena in the vicinity of the second-order phase transitions (and the transitions close to them; in this connection see also [23]). In particular, temperature dependences should be found for all the properties, that is, functions of the temperature difference $T - T_c$.

As second-order transitions are continuous, it is natural to treat them by expanding thermodynamic functions (for instance, a thermodynamic potential) in the powers of a certain parameter η which vanishes under the equilibrium at $T \geqslant T_c$. Then coefficients A, B, C, etc. in the respective expansion

$$\Phi = \Phi_0 + A\eta^2 + B\eta^4 + C\eta^6 + \ldots \qquad (5)$$

are, in their turn, expanded in powers of $T - T_c$ so that in the vicinity of a typical second-order transition $A = A'(T - T_c)$ and $B = B_0 = \text{const}$. Landau [22] developed the theory based on the consistent use of such an approach which could be traced back to Gibbs and Van der Waals.

The Landau theory yields the Curie law for susceptibilities $\chi \sim |T - T_c|^{-1}$, the spontaneous magnetization $\mathcal{M}$ and spontaneous polarization $\mathcal{P}$ vary at $T < T_c$ as $\mathcal{M} \sim \sqrt{T_c - T}$ and $\mathcal{P} \sim \sqrt{T_c - T}$ and so on. At the same time, the Landau theory is inca-

pable of explaining the anomalous temperature dependence of the specific heat and other characteristics at $T \to T_c$. Besides, careful measurements [24] have revealed that the Curie law and similar relations are not valid in the immediate vicinity of the transition point, where $\chi \sim |T - T_c|^{-\gamma}$ and $\mathcal{M} \sim (T_c - T)^{\beta}$; here $\gamma \neq 1$ and $\beta \neq 1/2$.

The Landau theory yields the same results as the model theories (like the well-known Weiss theory of ferromagnetism) which make use of the method of self-consistent (sometimes called molecular) field. This fact indicates what is basically clear by itself, namely, that the Landau theory is limited due to ignoring the fluctuations. Actually, the mean value, for example, of magnetization $\mathcal{M}$ is used. When $T \to T_c$, $\mathcal{M} \to 0$ while fluctuations of $\mathcal{M}$ not only do not vanish, but also grow sharply. Thus it is clear that the range for the Landau theory utilization is the one where fluctuations are relatively small and it is different for different transitions [25]. But in the vicinity of the transition point, that is, when the difference $|T - T_c|$ is rather small, the fluctuations should be taken into account which results in an anomalous specific heat behaviour, deviations from the Curie law $\chi \sim$ $\sim |T - T_c|^{-1}$, etc.

No quite consistent theory of the second-order phase transitions has yet been fully developed for three-dimensional systems though extreme efforts have been made to solve this problem*. However, they by no means have been wasted: though the solution of the

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* L. D. Landau told me once that his attempts to solve the problem of the second-order phase transitions had demanded greater effort than any other problem he had worked upon.

problem is yet to be completed, recent years have seen a number of important advances. These include, first of all, the similarity laws [24, 26] providing relationships between temperature dependences of various characteristics in the vicinity of T_c. By virtue of these laws and taking into account some experimental data it is possible, for instance, to predict that in some cases with $T \to T_c$ the magnetic susceptibility $\chi \sim |T - T_c|^{-\gamma}$, where $\gamma = 4/3$ (rather than $\gamma = 1$ according to the theory of Weiss or Landau). Moreover, the so-called critical indices (β, γ, etc.) for systems of various types can be fairly accurately calculated even without using experimental data.

Thus, a key problem of the solid-state physics is the development of a consistent theory of the second-order phase transitions and the related transitions accounting for typical features of various transformations as well as a generalized description of all the kinetic processes in the vicinity of T_c. Though some believe that the problem is basically solved, this cannot be said about various special cases of interest.

This can be illustrated by two more specific problems in this area whose selection may be called arbitrary being due to the author's interests. The first problem is the behaviour of helium II in the vicinity of the lambda-point. According to the Landau theory of superfluidity the density ρ_s of the superfluid helium component is considered to be a specified function of, say, the temperature T and the pressure p. But according to the general theory of the second-order phase transitions the density ρ_s cannot be specified, it must be determined from the minimum conditions for the thermodynamic potential. This approach [27] yields a number of interesting results: dependence of $T_c \equiv T_\lambda$

and the specific heat C on the thickness of the helium II film; nonuniformity of ρ_s close to a solid wall or a vortex axis in helium II and so on. These results seem to reflect the reality but, on the whole, the theory of superfluidity of helium II in the vicinity of the lambda-point and its experimental substantiation are far from being completed. The second problem is the light scattering in the vicinity of the second-order phase transitions and, particularly, near the $\alpha \rightleftarrows \beta$ transformation in quartz [28-31]. As the fluctuations grow when the temperature approaches T_c, scattering of X-rays, neutrons and light may be expected to increase in this range. A similar phenomenon (critical opalescence) has long been known to exist at the liquid-vapour critical point. A sharp increase in light scattering intensity is observed also in quartz [28] in the vicinity of the transition from α to β modification occurring at the temperature $T_c = 846$ K. The picture seemed to be basically clear but later it was proved to be more complicated [30] and indescribable in terms of the simple theory [29]. Apparently, the reason is that this theory "for the sake of simplicity" does not take into account the essential difference between the solid and the liquid, namely, that on straining solids develop also shearing stresses [31].

No doubt, light scattering in solids in the vicinity of phase transitions will be studied further on. Such studies may obviously be highly productive as is evidenced by the progress made in the extensive studies of light scattering in liquids and solids beyond the phase transitions range [32].

In conclusion it should be noted that the above examples are, so to say, everyday, or classic phase transitions. Recently, interest arose towards such

"exotic" transitions as, for instance, the phase transition of a liquid ^{3}He to a superfluid state [33], phase transitions in the exciton "matter" in semiconductors (see § 4), the transition to a superfluid in the molecular liquid hydrogen [34], the phase transitions in substances of ultrahigh density, in particular, in the neutron stars (see § 21), and so on.

We should also mention phase transitions in non-quantum liquids—in liquid crystals, in magnetic substances (ferromagnetic and antiferromagnetic transitions in a liquid phase), the ferroelectric transition in liquid (it seems to be possible), and, finally, phase transitions and various anomalies (such as, for instance, the alteration of the temperature dependence of the magnetic susceptibility occurring at a certain "point" T_a) in two-dimensional and one-dimensional systems.

We shall discuss in some detail here only the transitions in the liquid ^{3}He out of all the above transitions. It has been discussed for almost 20 years already that the atoms of ^{3}He may "adhere" to each other forming pairs with an integral spin and undergoing Bose-Einstein condensation (see § 2) transform to some superfluid state. Such a state is analogous to a superconducting state but, as ^{3}He atoms are neutral, the atom in this state must be superfluid rather than superconducting; however, superconductivity may also be called superfluidity, but in a system of charged particles.

It has been thought that ^{3}He atoms "adhere to each other" under the effect of rather weak Van der Waals interaction so that the temperature of the superfluid transition must be extremely low. Meanwhile, it was found in 1972 and 1973 [33] that not one but two phase transitions occur in the liquid ^{3}He under very low but

yet attainable temperatures of about $2.7 \cdot 10^{-3}$ and $2.0 \cdot 10^{-3}$ K (under the pressure of about 34 atm, though). Now it is known for sure that these are, indeed, the transitions to superfluid states which differ from one another, roughly speaking, in the total angular momentum of the pairs; the attraction giving rise to pairs seems to be due to the interaction between the atomic spins in ^{3}He (it is the so-called exchange interactions giving rise to ferromagnetism), rather than the Van der Waals interaction [33e].

Studies into the superfluidity of ^{3}He will, undoubtedly, make up a whole new chapter in the physics of low or, better to say ultralow, temperatures (incidentally, the abundance of ^{4}He is 10^7 times that of ^{3}He).

As for the problem of phase transitions as a whole, no doubt, it is one of the main subjects in macrophysics.

§ 6. Matter in Ultrahigh Magnetic Fields

The characteristic energy difference between the hydrogen atom levels is

$$E_a \sim \frac{e^4 m}{2\hbar^2} \sim 10 \text{ eV} \qquad (6)$$

The energy difference between the levels of a nonrelativistic free electron in a magnetic field is

$$E_H \sim \frac{e\hbar H}{mc} \sim 10^{-8} H \text{ eV} \qquad (7)$$

where the magnetic field is measured in Oersteds (or in Gausses as H may just as well mean the magnetic induction B). The estimate given by (7) is valid both for orbital levels and for spin levels (the electron mag-

netic moment $\mu = e\hbar/2mc$ and $2\mu H = e\hbar H/mc$ is just the energy difference between the levels with a spin parallel and antiparallel to the field).

Until recently, the magnetic fields observed were weak by their atomic standards, with $E_H \ll E_a$, that is,

$$H \ll \frac{e^3 m^2 c}{\hbar^3} = \left(\frac{e^2}{\hbar c}\right)^2 \frac{mc}{e\hbar}\, mc^2 \sim 3 \cdot 10^6\, \text{Oe} \qquad (8)$$

A similar criterion of field weakness for heavy atoms of the atomic number Z is $H \ll 3 \cdot 10^9 Z^3$ Oe, that is, even more enormous fields are needed to overcome it. Therefore, even not so long ago the problem of strong fields with

$$H \gtrsim 3 \cdot 10^9 Z^3\, \text{Oe} \qquad (9)$$

was considered to be rather abstract and did not receive any special attention. But now the situation has changed.

In 1967-1968 magnetic neutron stars called pulsars were discovered whose surface magnetic field may be as high as 10^{13} Oe (see § 21). This means that the matter on the pulsar surface and close to it is in a strong field even allowing for the fact that this matter may be mostly iron ($Z = 26$, $Z^3 \sim 20\,000$). In strong fields (see (9)) and especially in superstrong fields, when $H \gg 3 \cdot 10^9 Z^3$ Oe, atoms cannot be visualized in the forms conventionally used for atoms in weak fields or in the absence of fields. In ultrastrong fields the atomic electron shell is just stretched into a relatively thin needle parallel to the field. Under such conditions it becomes energetically favourable for two atoms (for instance, iron atoms) to form a molecule Fe_2 with a high bond energy. Combination of such molecules

gives rise, probably, to a polymeric-type structure also with a high bond energy (the behaviour of matter in high magnetic fields is discussed in detail in [35]). This is just the form the solid surface of neutron stars must have if their surface field is high enough (as mentioned above, this seems to be quite probable in a number of cases). It is very hard to vaporize or destroy this crust with an electric field, the fact that may have significant implications for the theory of pulsars [35, 36].

Studies of matter in superstrong magnetic fields are extremely difficult because pulsars are very far from us. Anyway, most physicists prefer to deal with the "terrestrial" conditions and feel little excitement about possibilities of astrophysics. Quite apart from these considerations, it is natural to ask if strong fields can be created and utilized under laboratory conditions. The outlook here does not seem to be particularly promising even in the fields created at the focus of a superpowerful laser (see §§ 7 and 15). Quite different prospects for studying the effect of superstrong fields are opened by simulating them in exciton systems of semiconductors. Actually we have seen (see Eq. (4)) that the bond energy of the hydrogen atom is greater than that of the hydrogen-like exciton by a factor of $m_{eff}/m\varepsilon^2$ that is, in practice, for instance, by a factor of 1000. The energy of electrons and holes due to their orbital motion in a magnetic field is estimated by formula (7) with m replaced by m_{eff}. This suggests that the magnetic field acting on exciton is strong [20] starting from the values as low as $H \geqslant 3 \cdot 10^9 \times m_{eff}^2/m^2\varepsilon^2 \sim 3 \cdot 10^5$ Oe (when $m_{eff} \sim 0.1 \, m$ and $\varepsilon \sim 10$) and sometimes even lower. Such fields are experimentally feasible, though usually in the pulse

mode. This will, probably, open the way for studies of the exciton "matter" in strong and even superstrong magnetic fields.

Our evaluation of the problem of matter in strong magnetic fields as an "especially important" one can once again give rise to objections and doubts. But I believe that this problem is distinguished by its refreshing novelty and unexpected implications concerning the neutron stars and exciton systems.

§ 7. X-Ray Lasers, Grasers and Superpowerful Lasers

Lasers play a great part in development of science and technology although fascination with lasers seems sometimes to be a matter of vogue. Somebody has even declared, albeit facetiously, that the age of atom has ended and the age of laser has begun. But, as already explained in Introduction, in this small book we cannot discuss the development of laser technology or applications and utilization of lasers, the problems of self-focusing and other problems related to nonlinear processes and phenomena (which are, undoubtedly, very interesting in many respects)*. But devices men-

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* By the way, it is hard to comprehend why the first laser was built in 1960 but not about 40 years earlier, soon after Einstein had developed in 1916 his clear-cut concept of stimulated emission which provided the basis for modern lasers [37]. The reason is apparently that for a long time it has seemed to be in principle possible only to amplify radiation by stimulated emission. But the gain is usally small and so the impetus to laser development was given by replacement of the amplification mode by the mode of generation due to multiple reflection from mirrors confining the active medium in the laser. The concept of genera-

tioned in the title of this section are the exceptions that can hardly be left out of any list of "especially important physical problems".

The modern lasers have the pulse power of up to 10^{12} W. The focusing is limited in fact to an area $\lambda^2 \sim$ $\sim 10^{-8}$ cm^2, where $\lambda \sim 10^{-4}$ cm is the wavelength typical of the powerful lasers in operation. Thus the energy flux would be as high as $P \sim 10^{20}$ W/cm^2. The best area of focusing obtained for a powerful laser so far is about 10^{-4}-10^{-5} cm^2, so that $P \sim 10^{16}$-10^{17} W/cm^2. In this case the electric (and magnetic) field intensity at the laser focus is $\mathscr{E} \sim 10^7$ esu/cm $\approx 3 \cdot 10^9$ V/cm $\left(\text{it follows from the expression for the energy flux of the electromagnetic field } P = \frac{c}{4\pi} \mathscr{E}^2\right)$. As mentioned in § 1, to build laser thermonuclear reactors the laser power must be increased by two or three orders of magnitude, let alone the efficiency which must be also increased by about two orders of magnitude. Even this task is complicated so that it cannot lie in the technical field; its fulfilment depends on the research results. And a purely physical problem is to increase the energy flux to about 10^{26}-10^{27} W/cm^2 for efficient production of electron-positron pairs in vacuum (see § 15).

Another fundamental physical problem is the development of the X-ray and gamma-ray analogues of

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tion was in itself an innovation that come more naturally from physicists working in radiophysics than from those in optics.

It should be noted however that the above considerations are somewhat lacking in fairness. It is too often that some important discovery or innovation seems to be long overdue (rather vivid examples are the Cerenkov effect and Mössbauer effect). And of course, no "belatedness" of a discovery or innovation can belittle the credit going to those who finally did it.

the laser which are often termed X-ray lasers (rasers) and grasers* or gasers, respectively.

Development of these devices depends on removing literally enormous obstacles. Firstly, the amplification factor falls fairly sharply with a decrease in the wavelength so that, generally speaking, an exceedingly high pumping power is needed to ensure the required inverted population of the excited levels (it will be recalled that stimulated emission is usually produced by an atom or the nucleus transitions from the excited levels to lower ones). Secondly, in the X-ray range, to say nothing of the true gamma range, it is very difficult to build a good resonator (reflectors) such that radiation would remain for a long time in the excited active medium (in lasers this is done by mirrors set up at the ends of the active medium). Of course, it is possible, in principle, to dispense with the reflector. but then

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* The term "laser" is the acronym for "Light Amplification by Stimulated Emission of Radiation". Therefore, the often-used terms "X-ray laser" and "gamma laser" seem to contain a contradiction in terms. The words "raser" and "gaser" are produced from "laser" by replacing the letter "l" (light) with the letters "r" (Roentgen) and "g" (gamma). One reason for this etymological discourse seems to be the lack, on my part, of more tangible subject matter, that is, any specific suggestions for constructing rasers or gasers.

In connection with the terminology, we should remember that there is no distinct boundary between X-rays and gamma-rays. Sometimes, the upper limit for the photon energy in the roentgen radiation is taken to be 100 keV (corresponding to the wavelength of about 0.1 Å). The gamma-rays then are the photons of higher energy. No less popular is the classification based on the origin of radiation, nuclear transitions are assumed to produce always gamma-photons. All the reported concepts of X-ray lasers and grasers that we know deal with energies of not more than 10 keV so that gamma-rays here are the photons generated by nuclear transitions.

either the amplification factor must be higher or the size of the system (the active medium) must be increased. Therefore, an X-ray lasers design may involve, for instance, the amplification of radiation in high-density relativistic electron beams [38] which may, in principle, be produced by heavy-current accelerators. A possible idea for a graser design is to use the Mössbauer transitions (very narrow lines are required) when the upper level is populated by capturing neutrons produced at a nuclear explosion (such should be the "neutron pumping" power) [39], but efforts are under way now to find another practical solution of the problem [39].

Such obstacles justify questioning the feasibility of building X-ray lasers and grasers which could make a contribution to physical research. The thing is that the success of laser utilization is due not only to the fundamental design principle used (there were no doubts about the possibility of stimulated emission of radiation in all spectral ranges) but also to utilization of all the optics techniques made possible by relatively modest requirements for pumping power and properties of the active medium.

But who knows... The history of physics presents so many examples when hopes for solving a problem seemed dim or even fantastic. And then new phenomena were discovered (for instance, fission of heavy nuclei) and hopeless became practicable and then even commonplace! Maybe, we can expect some breakthrough in the development of X-ray lasers and grasers, namely, the introduction of a new concept or discovery of a new phenomenon. However, there is no guarantee of a "happy ending". It may happen if somebody gives convincing evidence of the impossibility of developing an efficient and useful X-ray laser and/or graser.

§ 8. Studies of Very Large Molecules.
   ## Liquid Crystals.
   ## Some Surface Phenomena

The long title of this section includes three groups of problems that I would prefer to ignore but that must at least be mentioned*.

Gigantic molecules (nucleic acids and proteins) are prevalent in biology and physical methods have proved to be indispensable in studying them. But this is not enough, new efficient analytical procedures must be developed for studying the structure of large molecules, particularly when their amount is small and they are in a solution or in a mixture with other molecules [2]. This is, undoubtedly, a physical problem and it is both important and difficult.

Liquid crystals are those numerous substances which may be simultaneously in a liquid state and in an anisotropic state (substance in the liquid crystalline state flows but remains optically anisotropic). Liquid crystals have been studied for about 80 years but until recently they have been regarded as something exotic. This is easy to explain. As long as there was no sufficient understanding of simple substances, solids and liquids of relatively simple structure (chemical composition, etc.) there was little hope for explaining the

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* The same may be said about such an old, or better to say, ancient problem as the nature of the ball lightning. An exceptionally wide variety of suggestions has been put forward to account for this phenomenon (plasma formation, low-frequency or high-frequency discharge, antimatter, illusion or some physiological effect in the eye resulting from lightning, etc; see [40] and references cited there). This demonstrates that the mystery of ball lightning remains unsolved (see, however, ref. [40d]).

properties of considerably more complex substances. Moreover, studies of liquid crystals have not been stimulated by prospects of important scientific or technological applications. But now the situation has changed drastically. The "simpler" aspects of the physics of solids and, to a lesser degree, of liquids are now more or less clear and those who insist on studying the simplest objects and processes will look like a man in the well-known joke who looks for his lost keys under a street lamp for the simple reason that there is more light there. Moreover, liquid crystals not only have proved to be important for biology but they may be useful in some significant technological applications owing to high dependence of some of their properties on temperature and strength of the external electric or magnetic fields [41]. This has led to a sharp increase in the number of papers on liquid crystals which are published in the most widely-read and "influential" physical publications in addition to the journals of physical chemistry or a specialized journal *Molecular Crystals and Liquid Crystals*, whose publication is in itself a sign of growing interest.

Scientists for a long time have been interested in studying the surface effects and the properties of surfaces of solids and liquids. But only recent advances have made it possible to obtain really clean and perfect surfaces and to control and study them in detail [42]. In addition, the demands of miniaturization in technology and engineering lay special emphasis on studying the surface phenomena for their utilization. Thus, we may expect in the nearest future a sharp increase in high-level surface studies using the best modern methods. Physicists will intensify studies of various surface excitations, that is, excitations (phonons,

plasmons, excitons, spin waves, etc.) localized near the surface. Suggestions are made to look for surface analogues of such phenomena as ferromagnetism, ferroelectricity, superconductivity, superfluidity and liquid crystalline state. Actually, some phase transitions, in particular superconducting and superfluid transitions, in two-dimensional systems differ in character from those in three-dimensional systems.

The fact is that two-dimensional systems cannot produce superconducting, superfluid, and in some cases ferromagnetic, long-range order, i.e. no true ordering appears (for instance, the conditions that magnetic moments be parallel no matter how large the distance is are not fulfilled). But for finite, though sufficiently large surfaces, ordering is possible. Moreover, superconductivity and superfluidity, for example, may occur even in the absence of the long-range order (see [23]). Problems of ordering and various associated phenomena in two-dimensional and one-dimensional systems (polymer chains, crystal edges, etc.) should give rise to many studies of both the general theory and various particular cases or specific conditions.

The three problems discussed in this section are described more vaguely as compared with other problems mentioned above. Perhaps I failed to produce a sufficiently convincing argument and evidence for supporting and substantiating my opinion that studies of large molecules, liquid crystals and some surface phenomena are now of especial importance. It can only be added that all the above problems with the only possible exception of liquid crystals involve searching for the essentially new methods and phenomena and expanding the range of macroscopic physics.

§ 9. Superheavy Elements
(Far Transuranides)

The heaviest naturally found element uranium contains $Z = 92$ protons and $N = 146$ neutrons (we mean ^{238}U). Starting from 1940, scientists produce man-made transuranium elements by bombarding heavy nuclei (including uranium and transuranium nuclei) with neutrons and various nuclei. The first transuranium element 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, 105, 106 and 107 which do not have established names yet. The heaviest of the known transuranium elements live for a few seconds or even fractions of a second (the nuclei decay due to alpha- and beta-particles emission and spontaneous fission). A rough extrapolation indicates that the elements with $Z \gg 108\text{-}110$ must decay spontaneously with such a high rate that the production and analysis of such elements is hardly possible. However, the properties of transuranides do not change monotonously with increasing Z or, for instance, the parameter Z^2/A ($A = Z + N$ is the mass number) even though the transuranides contain 240-260 particles (nucleons) and resemble in this respect drops of a liquid. In other words, one-particle and shell effects may be noticeable and sometimes significant even for the heaviest elements. This suggests the possible existence of relatively long-lived isotopes of the elements with $Z > 105$. Specifically, it has been suggested that the element with $Z = 114$ has a closed-type shell (that is, 114 is a magic number) and the isotope $^{298}_{114}$

of this element containing $N = 184$ neutrons is even double magic [43, 44]. This does not imply that the $^{298}_{114}$ nucleus is the most stable as all the possible decay paths should be taken into account (spontaneous fission, alpha- and beta-decay). Some calculations suggest the greatest lifetime for the nucleus $^{294}_{110}$ whose half-life for decay $T_{1/2}$ is about 10^8 years.

It seems to be universally recognized that all such calculations are not accurate enough and have no quantitative significance whatsoever. However, an increased stability of nuclei in the vicinity of $Z \approx 114$ and $N \approx$ ≈ 184 seems to be possible and even a high stability for some isotopes or at least one of them cannot be ruled out. If so, such an isotope may be found on Earth, in meteorites or in cosmic rays. Apart from that, it may be hoped that the methods applied successfully for producing the known transuranic elements will be used to synthesize more or less stable isotopes (for instance, with $T_{1/2} \geqslant 1$ s).

All these approaches are being used now to look for the far transuranides [43, 44]. This search is of considerable interest for nuclear physics and, possibly, for astrophysics (and these attempts are no less fascinating than trying to find strange or extinct animal species). Therefore, one can hardly object to counting the problem of superheavy elements among the most important ones. It is quite another matter whether we can include a problem of nuclear physics among the problems of macroscopic physics. It is, of course, a controversial subject and it will be touched upon in the following section. Of more importance is the fact that only the problem of superheavy elements is discussed in the book while nuclear physics has other problems worth mentioning, for instance, isomerism of nuclei

due to differences in their shapes [45a]. On the whole, the atomic nucleus is a rather peculiar system owing to the fact, in particular, that even the heaviest nuclei consist of not so many particles (not more than 300). Therefore, a significant part is played in nuclei by surface effects and the distribution of levels exhibits various fluctuations (it is not monotonic). Finally, our knowledge of the nuclear forces is not sufficient and this is a basic difference between nuclear physics and atomic physics. Even taking into account the last remarks, this section does not seem to be capable of generating great interest among readers towards the problem discussed; apparently this may be done only by an expert in the field [45b].

2

Microphysics

§ 10. What Is Microphysics?

Our discussion of macrophysical problems did not require any introductory remarks. But before discussing microphysics we have to define the term. By microscopic size we mean the size of an atom (about 10^{-8} cm) and of course the atomic nucleus (about 10^{-13}-10^{-12} cm) so that atomic and nuclear phenomena should be classified as microphysics. But actually things are not as simple as that.

The notions "large" and "small" in physics (and not only in physics) can, of course, be used only with respect to a certain quantity (standard) which is taken to be neither large nor small. A natural standard of length (spatial distance) is a characteristic size of a human body, for instance, the metre. However, not only atoms and nuclei but, for instance, optical wavelengths and even some man-made objects are very small in comparison with this standard. And yet, one can hardly say that thin films or wires about 1 micron in diameter are microscopic objects. It should be added that, as compared to a metre, the size of the Earth and, of course, the distance between the Earth and the Sun being $1.5 \cdot 10^{13}$ cm are very large. Hence, there would be no less reason for the Solar System than for atoms and atomic nuclei to be distinguished from macroscopic objects of the order of metres if the distinction were made only by comparison with this standard.

Because of this and similar considerations micro-cosm is often defined as the domain of quantum laws while macrocosm is the domain of classical laws. This concept seems to be sufficiently profound though its conditional character is self-evident. Suffice it to say that the classical laws are sometimes readily applicable for describing collisions between nucleons while the behaviour of purely macroscopic systems is sometimes determined by quantum mechanisms (as illustrated by quantization of magnetic flux in superconducting cy-linders). Finally, it should be emphasized that, gene-rally speaking, development of science is accompanied by shifts of boundaries between various fields and sub-jects and by changes in concepts and notions.

All this suggests treating the boundary between mac-rophysics and microphysics as a historically variable concept. Indeed, it seems reasonable to assume that now atomic and nuclear physics belong essentially to macrophysics rather than to microphysics.

The reasons are as follows. First, atoms and nuclei are systems of particles which consist of a few most abundant particles (protons, neutrons and electrons). Second, atoms and nuclei are usually satisfactorily described by nonrelativistic approximation, that is, using highly developed nonrelativistic quantum mecha-nical methods. Both these factors bring atomic and nuclear physics close to macrophysics.

That the arbitrary boundary between microphysics and macrophysics shifts naturally is clearly illustra-ted by the following example. Before microscope was invented everything that could not be seen with a na-ked eye could be termed microscopic. Later applica-tion of the term microscopic was confined to things that could not be seen in a microscope, for instance,

individual atom. Now when the atomic-scale and, to a certain extent, nuclear-scale phenomena are sufficiently well-known and may be readily visualized* in mind, there are reasons to reserve the name of microscopic to those things that can be "seen" only poorly or not at all. Thus, there is almost no doubt that microphysics should include the field that was called and is still called the physics of elementary particles, though now it is more often called high-energy physics or, specifically, meson physics, neutrino physics, etc.

Hence, the subject matter of microphysics is, essentially, the "primary", "elementary" particles, the interactions between them and the laws governing them.

This definition of microphysics is conditional and, to a certain extent, even arbitrary as most definitions are. But it seems to be, at least, no less clear-cut and no less admissible than other definitions. Anyway, the term "microphysics" is used below in just the defined sense. This almost automatically makes microphysics, as in the past, the field of research where the very fundamentals are not clearly understood, let alone the secondary issues. As for the laws, microphysics (in the above sense) is at present mainly the domain of relativistic quantum theory. Finally, in terms of dimensions microphysics is now characterized by a length of the order of 10^{-11} cm or less (the Compton length of the electron $\hbar/mc = 3.85 \cdot 10^{-11}$ cm and for barions $\hbar/Mc \sim 10^{-14}$ cm)**.

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* However, it is already possible to observe individual atoms, so to say, directly in a field-emission ion microscope [46], or apparently, in a special electron microscope [47].

** The classification based on the type or nature of laws seems to be the most profound one. Therefore, the most consistent approach in physics seems to distinguish between the three

It should be noted that classifying nuclear physics as macrophysics, of course, cannot be done now without reservations.

The study of nucleus makes a significant contribution to our knowledge about interactions between nucleons and between nucleons and other particles; the relativistic effects in the nuclei are rather considerable and on the whole there are close and numerous ties between nuclear physics and physics of elementary particles [45b]. Thus, the author is probably somewhat ahead of time when he breaks the tradition by counting nuclear physics among the macroscopic problems. It is hardly probable, however, that the problem of classification has any real importance unless one believes that those who work in microphysics are "the salt of the earth" and that macrophysics is a second-rate science. Personally I do not subscribe, of course, to such peculiar (though not uncommon in some quarters) views; I believe that any man should be judged on his merits only. In ancient Russia noblemen were very keen on observing the rules of precedence at the tzar's court. But there is no tzar in science and a scramble for priorities seems to be out of place there (if somebody considers these remarks superfluous I refer them to Dyson's paper [2], for example).

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fields, the first governed by the classical laws, the second, by nonrelativistic quantum mechanics and the third, by relativistic quantum mechanics. These fields may be called macrophysics, microphysics, and, say, ultramicrophysics, respectively. But the most consistent approach is by no means always the most convenient and accepted one. Therefore, it seems best to use the old terms microphysics and macrophysics altering somewhat their definitions.

Solving the basic problems of microphysics is attended with difficulties similar to those encountered in development of the relativity theory and the quantum mechanics. Studies of this calibre, even if they yield comparatively modest results, put a great stress on a scientist requiring from him exceptional efforts and imagination; they give rise to a unique atmosphere charged with emotion; passions ride high... But we have deviated from our subject*; I have to state only that I cannot adequately describe the problems of microphysics and their peculiarities. However, this is not my aim; described below are only some microphysical problems and their peculiar features; selection of the problems is tentative and even more so than in the other fields considered. It might be just the feeling of dissatisfaction with the microphysical part of the present book that made me write this section and § 16 which, probably, was not the best thing to do. Fortunately, competent discussions of microphysical problems are not rare and we refer the readers to papers [1, 49-57] and those cited below.

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* This would be best done by a fiction writer and I cannot find any good example. However, I would like to cite Einstein's words concluding his lecture on the history of development of the general relativity theory dramatically illustrating the nature of fundamental studies [48]: "In the light of knowledge attained, the happy achievement seems almost a matter of course, and any intelligent student can grasp it without too much trouble. But the years of anxious searching in the dark, with their intense longing, their alternations of confidence and exhaustion and the final emergence into the light—only those who have experienced it can understand that."

§ 11. The Mass Spectrum
(The Third Spectroscopy)

Only three "elementary" particles were known before 1932, namely, electron, proton and photon. Later studies led to discoveries of neutron, positron, $\mu^\pm$-mesons (or muons), $\pi^\pm$- and π^0-mesons (or pions), mesons, hyperons, resonance particles (or resonances), electron and muon neutrino and antineutrino. Some of these particles are no less (though no more) elementary than proton or electron. Others (for instance, hyperons and resonances) appear rather like the excited states of lighter particles. The majority of particles are unstable, they transform into one another and are surrounded by "clouds" of virtual particles (for instance, nucleons are covered with π-meson "blankets"). Thus, the very notion of an elementary particle is very complex. There are too many reservations and definitions in this book, so we shall not go into the concepts of fundamental particles studied in microphysics. These particles are characterized by their mass, spin, electric charge, lifetime, and by some other properties and quantum numbers [49, 58].

The greatest achievement of physics, during the last 20-30 years is, probably, the development of the concept of matter structure, while in the past the matter was thought to consist of a few simplest "building blocks" (such as electron, proton and neutron); the present-day concept involves several groups of particles, namely, baryons, mesons and leptons. Nevertheless, all the above properties of individual particles are found, as before, either experimentally or, at best, predicted from certain semiempirical relationships and equations,

Thus, the primary task of microphysics which is far from being completed is to develop a theory which would make it possible, at least in principle, to predict masses and other properties of all particles. For simplicity, this problem is often called the problem of determination of the mass spectrum though, of course, not only the masses of particles have to be determined but other parameters as well*.

The current status of the problem of mass spectrum is similar on the whole to that of the atomic spectroscopy prior to the development of the Bohr's theory of atoms. At that time some spectral relationships were also known (primarily, the Balmer formula), but they could not be derived theoretically. Now the state of the third spectroscopy** is the same or somewhat better as regards the well-known quantitative relationships, but, of course, it is infinitely inferior compared to the state of atomic physics following the development of quantum mechanics.

The problem of mass spectrum is, in a way, a long-standing one because the difference in mass between the proton and the electron was attracting interest as far back as half a century ago. Later this problem was discussed in terms of a relativistic theory of particles with excited states which was being developed. Now

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* Moreover, the differences between baryons and leptons, for instance, are qualitative in character and more profound than those between baryons of different masses.

** We use here the terminology developed in [49] which seems both convenient and obvious. According to this terminology, the first spectroscopy deals with atomic and molecular levels, the second spectroscopy, with nuclear levels and the third spectroscopy, with levels of elementary particles (though in [49], the term "third spectroscopy" is applied primarily to the spectrum of baryons).

the data on such states and the experimental results accumulated in the third spectroscopy seem to provide at least an empirical foundation for solving this problem. But the theoretical results, in my opinion, are short of a great success, for the advances in systematics and classification of particles [49, 50], though important, are not so fundamental in character.

Various approaches have been made to solve the problem of mass spectrum. One of them is the development of relativistic models of spinning tops, oscillators, etc. [59]. Another approach is the development of a unified field theory of fundamental particles [60] which may also be called the theory of primary matter as it is based on a primary spinor field with a spin of 1/2. The third approach is based on the concept of the so-called "bootstrap" stating that all the strongly interacting particles "consist" of each other and that they all have democratically equal rights and representation. The fourth approach tries to represent baryons and mesons as particles consisting of various combinations of three basic particles called quarks [49, 54]. The quarks have not yet been found in a free state and the theory of bound quarks is still in the cradle. Nevertheless, the hypothesis of quarks clearly has some relevance; the initial concept was that baryons and mesons were "made up" of particles (quarks) with charges of $-1/3e$ and $2/3e$ (e is the charge of a proton and a positron). For, quarks not only make a satisfactory description of some "static" properties of the baryon and meson families possible (quantum numbers and magnetic moments are in view) but also contribute to understanding of some "dynamic" properties (scattering of particles on each other and their transformations). Closely related to this hypothesis is the

concept of partons developed as a result of experimental studies of high-energy (up to 20 GeV) electron scattering on protons and neutrons [54]. These results suggest that nucleons not only have an internal structure (this has already been known for some time) but also contain quasi-point entities which were called partons. Or, more exactly, scattering of electrons by nucleons occurs in such a way as if nucleons have inside "point-like" (small enough) scattering centres. The picture here is, in a way, similar to that developed in nuclear physics over 60 years ago as a result of the experiments with alpha-particles scattered on atoms which were instrumental in proving the existence of the atomic nucleus. The partons may be quarks but there is no any proof of it. In addition, no evidence is available that quarks (or partons if they are not quarks) can exist in a free state, that is they are distinct entities.

On the contrary, it seems that strongly interacting particles, the hadrons (baryons and mesons), are complex dynamic systems which have some features in common with atoms and atomic nuclei but are essentially different from them just because they are indivisible into independent components. This concept seems to be fairly deep*. The development of atomic theory invariably resulted in introducing new primary "building blocks" of matter (molecules, atoms, atomic

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* I cannot miss the opportunity to cite a definition of a deep statement or observation belonging to N. Bohr and conveyed by word of mouth among physicists: "In order to define a deep statement it is first necessary to define a clear statement. A clear statement is one to which the contrary statement is either true or false. A deep statement is a statement to which the contrary is another deep statement" [117].

nuclei, electrons and nucleons); quarks would repre-
sent yet another stage in this process and then we would
have to look for the components of quarks. There is
a concept which seems to provide a deep and, at the
same time, quite natural in some respects solution to
this problem. Namely, the concept that the process of
matter division is not infinite and ceases at some point
in an unconventional way; baryons and mesons may at
the same time consist of some components (of a quark
type) and may not consist of them. This is just the
way to describe the circumstances when the compo-
nents cannot exist by themselves (in a free state) but
nevertheless behave in some respect analogously to
nucleus in the atom or nucleons in the nucleus.

We shall not dwell on the subject here though it has
many aspects worth discussing [61]. We shall mention
only one unusual and highly unconventional approach
to the problem of the "primary" matter, namely, the
concept of friedmons relating the macrocosm (and even
the Universe) to the microcosm and fundamental par-
ticles. As shown by the general relativity theory, the
closed Friedman's world which may be a model of the
Universe (see § 19) has zero total mass and charge (the
mass is zero as the "starting "mass of the particles com-
prising the world is compensated by gravitational mass
defect due to gravitational interaction between the
particles). If we consider a "semiclosed" world con-
sisting, approximately speaking, of an almost closed
world connected by a "neck" with an infinite space
devoid of matter, the mass and charge of this world
may be very small*. Thus, a whole world may look

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* A more detailed explanation would be out of place here.
A brief discussion here is meant only for the readers who are
acquainted with the subject, those who are not, see [61, 62].

like a small particle, a friedmon, for an outside far-away observer. Mass, charge and other parameters (spin) of friedmons can be defined in terms of the classical theory and the quantum theory of fried-mons has not yet been developed. We may, however, hope that in the framework of quantum theory fried-mons will possess the parameters of fundamental particles. Of course, this is just a fantastic chance so far, a hope, a dream. But this concept deserves careful consideration, if only for its boldness in showing a way for combining the opposites, the macrocosm and the microcosm. There are other, more specific, reasons for considering this concept, particularly in a wider context of the relationship of microphysics to the general relativity theory and its quantum generalization which has not been developed yet (see also § 19).

The particles discussed above are the strongly interacting ones (hadrons, that is, baryons and mesons). The mass spectrum of leptons (electrons, positrons, $\mu^{\pm}$-mesons and neutrino) presents a somewhat different though no less important and fascinating problem. For instance, the reason for the difference in mass between the electron and μ-meson has been for a long time and still is a complete mystery. Mention should be made of possible existence of some "exotic" particles which do not belong to families already known. Among such particles are, for instance, magnetic poles (monopoles) and tachions, hypothetical particles travelling at a velocity greater than the velocity of light in vacuum. The search for monopoles and tachions has already begun though all in vain. Most probably, such particles (especially tachions) cannot exist but it is characteristic of the present-day unsatisfactory state of the theory of fundamental particles that it can

provide us with almost no theoretical prohibitions. As an important example use may be made of the hypothesis of an intermediate boson, or W-boson, responsible for the weak interaction (until recently it was assumed that leptons interact with hadrons via "weak forces", so to say, directly; if the appropriate "intermediate bosons" do exist, the weak interaction consists of two stages: hadrons, baryons and mesons interact with intermediate bosons and the latter, with leptons). In the case of the existence of intermediate bosons it is evidently possible to unify to a certain extent the electromagnetic and weak interactions i.e. to reduce them to a "primary interaction" (as a result the constants of electromagnetic and weak interactions appear to be interrelated) [57c].

As mentioned above, none of the attempts to solve the problem of mass spectrum has met with an unqualified success. This state of affairs has not been changed for decades and nobody can predict when the things start moving on at last. But it has to happen some time and, despite all disappointments, this historic event is awaited with unceasing anxiety.

§ 12. Fundamental Length (Quantized Space, etc.)

The special and general relativity theory, nonrelativistic quantum mechanics and present theory of quantum fields use the concept of continuous, essentially classical, space and time (a point of space-time is described by four coordinates $x_i = x, y, z, ct$ which may vary continuously). But is this concept valid always? How can we be sure that on a "small scale" time and space do not become quite different, somehow frag-

mentized, discrete, quantized? This is by no means a novel question, the first to ask it was, apparently, Riemann back in 1854 [63] and it has repeatedly been discussed since that time. For instance, Einstein said in his well-known lecture "Geometry and Experience" in 1921 [64]: "It is true that this proposed physical interpretation of geometry breaks down when applied immediately to spaces of submolecular order of magnitude. But nevertheless, even in questions as to the constitution of elementary particles, it retains part of its significance. For even when it is a question of describing the electrical elementary particles constituting matter, the attempt may still be made to ascribe physical meaning to those field concepts which have been physically defined for the purpose of describing the geometrical behavior of bodies which are large as compared with the molecule. Success alone can decide as to the justification of such an attempt, which postulates physical reality for the fundamental principles of Riemann's geometry outside of the domain of their physical definitions. It might possibly turn out that this extrapolation has no better warrant than the extrapolation of the concept of temperature to parts of a body of molecular order of magnitude."

This lucidly formulated question about the limits of applicability of the Riemannian geometry (that is, in fact macroscopic, or classical, geometric concepts) has not yet been answered. As we move to the field of increasingly high energies and, hence to "closer" collisions between various particles (see § 13) the scale of unexplored space regions becomes smaller. Now we may possibly state that the usual space relationships down to the distance of the order of 10^{-15} cm are

valid or, more exactly, that their application does not lead to inconsistencies. It cannot be ruled out that the limit is nonexistent but it is much more likely that there exists a fundamental (elementary) length $l_0 \leqslant$ $\leqslant 10^{-16}$-10^{-17} cm which restricts the possibilities of classical spatial description. Moreover, it seems reasonable to assume that the fundamental length l_0 is, at least, not less than the gravitational length $l_g =$ $= \sqrt{Gh/c^3} \sim 10^{-33}$ cm (see § 19).

Various versions and modifications of the problem of fundamental length have been discussed for many years already (this length enters into the theory of "primary" matter [60], various versions of the theory of quantized space [65], the concept of evaporation of small black holes [65d] and so on). The problem of fundamental length is closely related to the problem of possible violations of causality in microcosm (it is also called the violation of microcausality, see [66]), as well as to some other subjects of microphysics and to the problem of singularities in the general relativity theory and cosmology (see § 19). If a fundamental length does exist it may, naturally, be assumed to be of importance and even of decisive importance in solving the problem of mass spectrum. It is probable that the fundamental length would be a "cut-off" factor which is essential to the current quantum theory: a theory using a fundamental length automatically excludes divergent results.

Experimentally, the search for the fundamental length involves studying the particle collisions at ever increasing energies and ultraprecise measurements of various properties at lower energies. Generally speaking, a disagreement between experimental results and predictions of a theory (of a quantum electrody-

namics type) would indicate a possible violation of the space-time concepts used and the necessity of introducing the fundamental length.

§ 13. Interaction of Particles at High and Superhigh Energies

Interactions of particles at high and superhigh energies are studied for various purposes: to probe the structure of particles and of the space itself for small-scale regions, to find new resonances (excited states of baryons and mesons) in ever increasing numbers, or to determine the energy-dependence of cross-sections for elastic and inelastic scattering. In collisions between nucleon and nucleon the closest distance is

$$l \approx (\hbar/m_\pi c)(Mc^2/E_c) \qquad (10)$$

where $\hbar/m_\pi c \sim 10^{-13}$ cm is the Compton length for π-meson, M is the mass of the nucleon ($Mc^2 \approx 1$ GeV) and E_c is the nucleon energy in a centre-of-mass system (in more detail this may be found for instance in [67, 68]). If one nucleon is at rest and the other has an energy $E = \sqrt{M^2c^4 + c^2p^2}$, then

$$E_c = \sqrt{1/2 \, (E + Mc^2) \, Mc^2} \qquad (11)$$

The highest energies obtained in accelerators are about 75 GeV (Serpukhov, USSR) and about 400 GeV (Batavia, USA), the accelerator energy in Batavia will, probably, be raised to a 500-GeV level. Even if $E \approx 500$ GeV, we obtain $E_c \approx 15$ GeV and $l \approx \approx 5 \cdot 10^{-16}$ cm. Particles with energies of up to 10^{20} eV are found in cosmic rays but in practice it will hardly be possible to study individual collisions of cosmic protons with energies exceeding 10^{16} eV for which

$E_c \leqslant 10^3$ GeV and $l \geqslant 10^{-16}$ cm (however, it is possible to get some data on particle interaction in cosmic rays for higher energies, especially for $E \leqslant 10^{17}$ eV). The method of colliding beams uses head-on collisions of particles. If each particle has an energy E' and the rest mass M the centre-of-mass system coincides with the laboratory system and $E_c = E'$. There have been produced in Europe colliding beams of protons with $E' = 25$ GeV. In terms of the same values of E_c this experiment is equivalent to using a proton beam with an energy

$$E = \frac{2\,(E')^2}{Mc^2} - Mc^2 \approx 1250 \text{ GeV} \tag{12}$$

hitting a stationary hydrogen target. In the not far future we may expect to obtain energies $E' = E_c$ of about 500 GeV making

$$E \approx \frac{2\,(E')^2}{Mc^2} \sim 5 \cdot 10^5 \text{ GeV} = 5 \cdot 10^{14} \text{ eV} \tag{13}$$

But even in this case the distance

$$l \approx \frac{\hbar}{m_\pi c} \frac{Mc^2}{E_c} \sim 10^{-16} \text{ cm} \tag{14}$$

so that distances less than 10^{-16} cm can hardly be studied even using the colliding-beam method.

It should be noted in passing that the enormous difficulties frequently encountered in studying high-energy particles, particles with small lifetimes, etc. give rise to the development of new methods for accelerating and detecting particles. Highly impressive advances have been made in the development of accelerators and detecting devices (bubble and spark chambers, various counters, etc.). In this connection,

it should be emphasized that one of the effects of microphysics on physics as a whole is the diversification and expansion of experimental hardware and software.

In collisions of particles without the strong interaction (muons, electrons, photons) the smallest distance at collisions is of the order of the wavelength in the centre-of-mass system, that is, $l = \dfrac{\hbar_c}{E_c} = \dfrac{\hbar}{m_i c} = \dfrac{m_i c^2}{E_c}$ (here $E_c \gg m_i c^2$, where m_i are the masses of colliding particles) so that the conditions for studying small distances are more favourable here than in collisions of nucleons. Moreover, high measurement accuracy and careful comparison with theoretical expectations provide a means for possible "probing" of distances being even smaller than the above rough estimates. However, it is perfectly clear that the difficulties involved in overcoming the barrier of $l \sim 10^{-16}$-10^{-17} cm are very great (at $E_c = 1000$ GeV, the length $l = \hbar c/E_c \approx \approx 1.5 \cdot 10^{-17}$ cm).

The main task of high-energy physics consists in comparing the predictions of the theory of scattering with experimental results for ever-increasing energies, as well as in studying new resonances for baryons and mesons and in determining the effective cross-sections for various scatterings.

High-energy phenomena involve not only the production and scattering of individual particles but, primarily, the multiple production of particles. Attempts are made to account for multiple production and its peculiarities using statistical and hydrodynamic methods [53]. This concerns primarily the collisions of strongly interacting particles, the hadrons (baryons and mesons); the interaction of matter with high-energy

muons and neutrinos produced in the Earth's atmosphere by cosmic rays (mainly, the neutrinos produced by the decay of muons and pions born by cosmic rays [56]) should be particularly emphasized.

In contrast to the problems of mass spectrum and fundamental length, the study of high-energy and superhigh-energy particle interactions may seem to be of secondary importance and lacking a well-defined and attractive physical goal. But this is not so and if such an impression has been produced it is only the author's fault. It should be mentioned that all the above problems of microphysics are, in fact, closely interrelated and highly interdependent. By discussing separately the problem of high-energy particle interactions we should like to stress once again that the high-energy physics is by no means confined to the problems of mass spectrum and fundamental length. Thus, a very profound and, in a sense, independent theoretical problem is the energy dependencies of interaction cross-sections for various particles (especially, for superhigh energies, or formally, for $E \to \infty$ and primarily for strongly interacting particles).

§ 14. Weak Interactions.
Violation of CP Invariance

In 1956 it was found that the spatial parity (P) is not conserved in the weak interactions. Physically this means that in beta-decay of radioactive nuclei whose magnetic moments are aligned by an external magnetic field (^{60}Co nuclei were used in the first experiments) different amounts of beta-particles are emitted both along the field and opposite to it—the fact that indicates violation of spatial parity (in more detail it is

discussed, for instance, in [52c]). However, in all the decays that have been observed up to 1964 the combined parity CP was conserved, that is, the interactions were invariant under simultaneous charge conjugation C (replacement of a particle by its antiparticle) and space inversion P. Violation of parity may be explained, for instance, as follows: a particle (say, a neutron) possesses an "intrinsic screw" due to which its beta-decay proceeds differently as to the direction, along or opposite to the "screw". Then CP conservation means that the particle and its antiparticle have opposite "screw" orientations.

The discoveries of violation of C and P conservation and confirmation of CP conservation were the utmost importance and increased still further the interest to the weak interactions, high enough though it was. And the weak interactions did not "disappoint their fans"—in 1964 there was made a discovery whose significance seems to be extremely high though far from being clear as yet. There was found the decay $K_0^2 \rightarrow \pi^+ + \pi^-$ ($K_2^0 \equiv K_L$ is a long-lived neutral K-meson decaying in this case into π^+- and π^--meson) which can occur only with the violation of the combined parity [52]. Thus, CP invariance can be violated in nature; it should be noted, though, that all the known processes with CP violation are by three orders of magnitude less probable than CP invariant processes. Violation of CP invariance seems to suggest a result of fundamental importance, namely, that direct and reversed times are not equivalent. The fact is that CPT invariance generally results from the field theory stemming from its very fundamentals. This means that the interactions (and, of course, all the phenomena involved) are invariant under the product of three opera-

tions (in any order): spatial parity (P), charge conjugation (C) (that is, changing particles into their antiparticles) and time reversal (T) (replacing t by $-t$). The fundamental significance of CPT invariance is quite clear from the fact that it guarantees identity of rest masses and lifetimes of particles and antiparticles in decays. There is no evidence of violations of these properties of particles and antiparticles.

If an interaction is invariant under CPT and not invariant under CP then it must be not invariant under T, that is, under time reversal. Both classical theory (mechanics, electrodynamics and general relativity theory) and quantum mechanics and quantum field theory are reversible in time (this formally means that equations are invariant under operation of changing time t into $-t$; therefore with the appropriate change in the initial conditions any process proceeds by the same pathway and via the same states as before but only in the reversed direction)*. Thus, in conditions of CPT invariance the discovery of violation of CP invariance opened the way for detecting violation of T invariance for fundamental interactions and processes, though it has not yet been proved rigorously. We may give this statement an interpretation similar to that accounting for violation of P and C invariance and involving the concept of "intrinsic screw" of particles. Thus, considering elementary particles to be very complex in structure (in a sense, this is just the case,

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* The irreversibility (nonequivalence) of the future and past (or noninvariance under changing t into $-t$) found in macroscopic physics, chemistry and nature is due to complexity of macroscopic objects (a great many particles are involved) which is responsible for the trend in the development from a more ordered state to a less ordered one (see, for example [52c]).

see above) we can imagine some oscillations or "saw-tooth" motion (asymmetric in time) taking place in a particle as if the particle had its own clock going in a definite direction.*

The violation of *CP* invariance in the weak inter-actions is to be analyzed further and it would be especially interesting to study it at high energies. It is possible that at high energies and small space-time distances corresponding to them (to a certain extent) *CP* violation and *T* violation may prove to be not "small" (for instance, their probability may be comparable with the probability of the weak interaction). An answer to this fundamental question may be obtained only by making use of neutrino beams from the most powerful accelerators (primarily, the accelerator in Batavia, USA, see [52d]).

However, the problem of high-energy weak interactions has many other important aspects apart from the violations of *CP* and *T* invariances. Suffice it to mention once again the problem of intermediate boson and the problem of energy dependence of the processes caused by the weak interaction [57]. Finally, it is in the weak interaction where the fundamental length could be of primary importance if it existed (in that case, apparently, the fundamental length would approximately be about 10^{-17} cm).**

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* As I have not met any discussions of such ideas in litera-ture I have to note that I was told about them by V. N. Gribov (though I could have misunderstood him and so V. N. Gribov is by no means responsible for the above discussion).

** The recent fairly successful attempts to develop a uni-fied theory of weak and electromagnetic interaction [57b] seem to invalidate the arguments for introducing the fundamental length $l_0 \sim 10^{-17}$ cm.

It is in the weak interaction processes where P and, probably, T invariances are violated. As mentioned above, though, these violations may be related to the particles themselves by introducing the concepts of "intrinsic screw" or "intrinsic clock". But on the other hand, the problems of parity P and time reversal invariance T involve at least our space-time concepts.

It might appear that violations of P and T invariances in the weak interactions yield a glimpse of more profound changes in the structure of space and time "on a small scale" or, specifically, at the characteristic length of the weak interaction $l_0 \sim 3 \cdot 10^{-17}$ cm. This is just what was meant as a possible relationship between the problem of fundamental length and the theory of weak interactions.

§ 15. Nonlinear Phenomena in Vacuum in Superstrong Electromagnetic Fields

The above problems of microphysics are formulated in highly general terms, each of them could be subdivided into a number of more concrete partial problems. But it is not our aim to increase the number of problems to discuss, to make the classification elegant and consistent, etc.; this would run contrary to our more modest goal, namely, to review briefly the general state of affairs in physics. Therefore, the following discussion of the processes in superstrong electromagnetic fields is given rather as an example of more special but highly important problems. Peculiar behaviour of matter in superstrong magnetic fields has already been discussed in § 6. In contrast to a magnetic field, a strong electric field generally destroys the atom. For, if the strength of an external

electric field $\mathscr{E}$ is of the same order of magnitude as the strength of the field of the nucleus (proton) at a distance of an atom radius $a_0 = \hbar^2/me^2 \approx 5 \cdot 10^{-9}$ cm, that is, if

$$\mathscr{E} \sim e/a_0^2 \sim e^5 m^2/\hbar^4 \sim 10^7 \text{ esu/cm} \approx 3 \cdot 10^9 \text{ V/cm} \quad (15)$$

then the hydrogen atom very rapidly disintegrates. In fact, such disintegration (atom ionization in an external electric field) occurs in weaker fields, too, but in the fields considerably less than 10^8-10^9 V/cm the lifetime of a hydrogen atom is long enough. The uranium atom will be completely "stripped" of electrons in a short time only in the fields of the order of magnitude of $Z^3 \cdot 10^9$ V/cm $\sim 10^{15}$ V/cm (the uranium nucleus charge eZ is $92e$ and the radius of the uranium K-shell $a_{0,z} \sim \hbar^2/me^2Z \sim 10^{-10}$ cm by virtue of which the field of the nucleus at this shell is about eZ^3/a_0^2). In still stronger external fields not only the electrons in the heaviest atoms do not "hold out" (they are stripped from the nuclei and accelerated by the field), but the vacuum itself does not "hold out". The fact is that the real (physical) vacuum is by no means the "emptiness"; vacuum is polarized by the field which may give rise to various particle-antiparticle pairs and the easiest to produce are the lightest electron-positron pairs. Intense production of such pairs occurs in the field $\mathscr{E}_0$ whose work at a distance of the Compton wavelength $\hbar/mc \sim 3 \cdot 10^{-11}$ cm is of the order of the pair rest energy $2mc^2 \sim 10^6$ eV $\sim$ $\sim 10^{-6}$ erg. Hence, $e\mathscr{E}_0\hbar/mc \sim mc^2$ or

$$\mathscr{E}_0 \sim \frac{m^2 c^3}{e\hbar} \sim 10^{14} \text{ esu/cm} \approx 3 \cdot 10^{16} \text{ V/cm} \quad (16)$$

Weaker fields can also produce pairs rapidly enough though not at a catastrophic rate. Therefore, fields with

this problem presents a certain interest and, of course, the problem of vacuum polarization and pair production near superheavy nuclei has more important implications [70]. Finally, of especial significance is the problem of pair production near singularities in cosmological solutions describing evolution of the Universe (see § 19).

§ 16. Microphysics Yesterday, Today and Tomorrow

All things are in flux and changing, and changes occur not only in the subject matter of the field we refer to as microphysics but also in its status in science as a whole and in physics in particular. One has only to look through physical, abstracting and popular-science publications to arrive to the conclusion that the share of microphysical problems in all these publications for the past 20-25 years is considerably less than in the preceding decades. Unfortunately, no quantitative data are available* but in my opinion the ratio between the numbers of publications on microphysics and macrophysics is now by at least an order of magnitude less than twenty five years ago. Other indices of

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* In this connection one cannot help deploring the fact that in this country too little attention is paid to statistical (or any other) analysis of the development trends in science, to roles played by various types of information, etc. It should also be noted that there is no reason to explain the above decrease in the share of microphysics by the fact that we have classified the main part of atomic and nuclear physics as macrophysics. Suffice it to recall that such branches of microphysics as high-energy physics, meson physics, neutrino physics, etc. were non-existent in the past. Yet our definition fully retains the vanguard position of microphysics (see § 10).

scientific activity (the number of graduate students specializing in the field, the number of conferences held, etc.) will, probably, yield the same result.

What are the reasons?

I believe that the primary reason is that in the recent past (to be more precise, about 30-40 years ago) microphysics occupied an exceptional place in science and now the things have changed.

Microphysics deals with the most fundamental, essential and therefore most attractive, problems of physics. There has been no change in microphysics in this respect. But up to the middle of this century the problems of microphysics exerted, in essence, a decisive influence on the development of the natural sciences as a whole. Indeed, at that time microphysics was mainly concerned with studying atoms and, later, also atomic nuclei. The development of many branches of physics, astronomy, chemistry, biology depended on the powerful impetus which was to be given by understanding of the structure of atoms and the mechanisms acting in them (to do that quantum mechanics had to be developed!). Similarly the studies of atomic nucleus have resulted in utilization of nuclear (atomic) energy providing a reason for calling the twentieth century an atomic age (it does not matter now that the significance of nuclear physics was not appreciated for some time).

The physicists working on the problems of microphysics were not concerned in the majority of cases with practical applications of the results of their investigations, the sources of their persisting enthusiasm were the interest to the problems in themselves, the urge to know "how the world runs" and the unquenchable desire to overcome difficulties and to come to the truth. However, the awareness of the significance of microphy-

sics for the development of natural sciences on the whole or even for humanity in general as a means for solving outstanding practical problems made quite an important contribution to concentrating efforts, extending the scope of work and providing support and attention of the society (in particular, of the scientific community).

Now the situation has entirely changed. The particles studied in microphysics either live for minute fractions of a second or, as it is with neutrino, freely pass through the Earth and can be traced only with greatest efforts.

However, it is evident that the scientific significance of a problem can neither be evaluated by the particle lifetimes nor by their penetrating capacity. The present-day problems of microphysics are no less mysterious and difficult than the problems of yesterday. In other words, microphysics is still (and under the definition used will always be) the most advanced and deep area of physics, its outpost, so to say*. But what has changed is the character and significance of the subject matter of microphysics. In the past microphysics was concerned with "everyday things", namely, atoms and atomic nuclei; now it studies outlandish

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* It should be noted that my opinion in this matter is not self-evident (see, for instance, [3]). Many problems of macrophysics, or, say, biology are quite sophisticated and independent; their solution is not made easier by the fact that the relevant fundamental laws (for instance, nonrelativistic quantum mechanics) are already known. Yet, the difference between macrophysics and microphysics seems to be significant enough for microphysics and, maybe, cosmology (see § 19) to be singled out. But, of course, it does not mean that the rest of the natural sciences are looked upon as something second-rank or inessential.

and rare animals (at least, by the Earth standards)*. But, as we have mentioned already, the literally domineering status of microphysics in science was, to no small extent, due to exceptionally high practical priority of the problems it dealt with.

Thus, in my opinion, the role played by microphysics both in physics and in the natural sciences in general has changed radically and I believe that this change will persist if not for ever, but at least, for a very long time (this assumption is the most controversial one).

Dispensing with scientific language I would say that in the first half of this century microphysics was the first lady of the natural sciences. Now and in future microphysics is and will "only" remain the most beautiful lady. But this is just the point: opinions about the most beautiful ladies may differ while by definition there can be the only one first lady (for instance, the President's wife). In my eyes microphysics was and still is the most beautiful lady in physics. But I differ from some of my colleagues in believing that adoration should not be accompanied by neglecting changes in age and character and by ignoring other objects worthy of admiration.

The above remarks seem quite commonplace... but only to those who agree with them. It is just because they are controversial that they are made here. I be-

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* There are, of course, some exceptions. For instance, muons live for microseconds and are of some interest for chemistry. Futhermore, the studies of proton and electron are, of course, going on but their level (at least, at present) is too deep for them to be directly relevant to our understanding of the problems of atomic and nuclear physics (I mean, for instance, the particles comprising protons—the partons, and so on).

came aware of that some 15 years ago when I said something along these lines on some other occasion [71]. Some critical remarks and objections, though, were, as usual, a result of misunderstanding or egocentrism. For instance, some understood the words about changing and, to a certain extent, decreasing, the role of microphysics if not as a call for stopping the construction of powerful accelerators, then at least as a justification of such measures. It goes without saying that I meant nothing of the kind*.

There is, however, one essential objection which is worth discussing. At the first stage of nuclear research the prospects of nuclear power production were far from being clear and their evaluation was sometimes quite wrong. Such examples are not rare. In general, it is hard and sometimes even impossible to make predictions about the development of science. Therefore, it seems possible or even rather probable, if one takes into account certain analogies, that microphysics will

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* I cannot help adding here that when attempts are made to relate closely (or even to tie up) the discussion of development and planning of science to narrow and special interests and problems of a certain branch of science under given local conditions, they arouse a feeling of protest. The problems of financing, construction, etc. depend on many factors among which purely scientific considerations may sometimes be of secondary significance as opposed, for example, to economic or technological ones. There are even less reasons for drawing practical conclusions concerning the organization of science taking into account only the scientific considerations without analyzing comprehensively the subject as a whole. The things would change considerably if the funds appropriated for the development of science were increased many times, for instance, by diverting the enormous sums of money wasted by humanity on various unproductive activities such as smoking or drinking. But something of the kind may now happen only in a science-fiction book.

assume once again its role of a generator of new large-scale problems on a par with nuclear energy. Then, naturally, the share of microphysics could increase dramatically.

To be sure, such a possibility cannot be completely ruled out. The very fact that there is a chance, albeit remote, of new important useful discoveries should in itself be a sufficient reason to encourage the development of microphysics in all possible ways apart from the interests of "pure" science.

At the same time, even if we recognize that a new reversal may occur as regards the practical importance of microphysics for the future it will by no means contradict the above remarks on its present-day position. Moreover, I fail to understand why it is heresy or bad manners to suggest (I do it without hesitation) that the most glorious times of microphysics seem to be over (in terms of its effect on the development of the society, technology, etc.). One has a right not to believe in the concept of "infinite matryoshka" (wooden, successively smaller hollow dolls, one inside the other)—open one doll, there is another one in it, open this one, there is yet another one and so *ad infinitum.*

Unfortunately, as regards the prospects of microphysics I have almost no hope to prove my case but on the other hand I shall hardly have to confess my errors for even the optimists tend to recognize that no radical change can be expected in the role of microphysics in science and technology within the lifetime of this generation. By the way, the prestige of microphysics is exceptionally high at present and only those who have become spoilt through getting used to high-priority attention may feel any dissatisfaction with it since they had to move away from the limelight. It is

only in this respect that the status of microphysics has been somewhat altered for it has been "shouldered aside" by astrophysics (including space research) and, particularly, biology. Surely, the dramatic advance of biology that we are now evidencing (more exactly, some areas of biology, molecular biology, biophysics, etc.) not only has a great scientific significance but also opens up fantastic prospects for solving such major human problems as elimination of diseases (in particular, cancer), considerable prolongation of natural human lifetime, artificial "test-tube life", mobilization of brain resources, etc. On the other hand, astrophysics, as a science, is deeply fascinating "by itself", irrespective of its prospects for useful applications which are, anyway, highly remote and uncertain. In this respect, the position of microphysics at present and in the immediate future is rather similar to that of astrophysics. Thus, to construct large accelerators is, clearly, no less essential than to construct large telescopes on Earth and on satellites.

Some of the observations made above are closely similar to those published by Dyson [2] (by the way, Dyson's paper and the first version of this book appeared almost simultaneously and are, of course, completely independent). Therefore, I would like to cite here three rules suggested by Dyson for getting out of the critical situations brought about by the development of physics (applicable on the scale of a laboratory or institute):

"Don't try to revive past glories.
Don't do things just because they are fashionable.
Don't be afraid of the scorn of theoreticians."

The first two rules are self-evident and moreover Dyson comments on them in [2]. But there might be

some misunderstanding about the third rule, especially if the personality of the author is not allowed for.

Physicists are customarily divided into two categories, theoreticians and experimenters. Ideally, an experimenter lingers about the apparatus he designed himself and makes measurements. Besides, he has to get money, materials and instruments for building experimental set-ups, to supervise the work of technicians and assistants (there are sometimes many of them), and to interpret the experimental data. This goes on and on, occasionally for years and the sole aim of all this work may be a refinement value of a parameter or a constant, for instance, proton's magnetic moment, mass of a resonance, etc. As for a theoretician, he, also ideally, sits at his desk in a clean, well-lighted room overlooking a garden or lake or, at the worst, lounges in an armchair at home and meditates on the "nature of things" or makes calculations making occasional breaks for fascinating discussions on various scientific and general topics. Both above notions are, of course, purely abstract but they contribute to understanding why there is often little love lost between an abstract experimenter and an abstract theoretician. In real life the things are, certainly, not as simple as that. As recently as the XIX century there was no clear-cut distinction between experimenters and theoreticians. Naturally, some of the physicists did more experiments while others performed more calculations according to their tastes and skills but most of them were apt to do both. It was only the increasing sophistication of experimental techniques, the dramatic rise in the number of physicists, the growing competition between them, the increase in scientific productivity and snowballing of information that gave

rise to a sharp "division of labour" in physics and, to a certain extent, carved separate niches for theoreticians and experimenters. The results of this differentiation are ambiguous. The statement that physicists have ceased to understand each other no longer sounds as a paradox or absurdity, too often it is sadly true. But what is the use of talking about physicists in general when even among the people called theoreticians one can meet all grades of specialization, beginning from mathematicians who got somehow interested in solving a physical problem to those down-to-earth physicists who for some reasons do not work with their hands or lost their experimental connection by chance. Naturally, the theoreticians belonging to the opposite poles of their profession, let alone theoreticians and "pure" experimenters, quite often fail to find a common language and hold each other in distrust.

Now, if one reads the third Dyson's rule without prior knowledge of the author's personality one might visualize an exasperated experimenter: the theoreticians got on his nerves by lecturing him on how to work and what to do and by hinting at his ignorance of true physics. But in fact Dyson is one of the most brilliant and well-known (and deservedly so) of the contemporary theoretical physicists. It is only knowledge of manners exhibited by a fraction of his fellow theoreticians that prompted his advice not to be afraid of their scorn. This is a manifestation of his fondness of "his trade" rather than a betrayal of it. The genuine theoretical physics is an integral part of physics as a whole; it cannot even exist without experimental physics, to say nothing of dominating it. A theoretical physicist is no prophet or priest, more often than not he is just a lucky chap free of those troubles which

incessantly pester an experimental physicist. This is why the scorn or ridicule of theoreticians can be but counterproductive (of course, the same may be said about disrespect and distrust towards theoreticians shown in some experimental quarters). To be sure, we are talking about exceptional cases but they justify exercising the right for self-defence the more so since I have heard already that "Dyson is a defector" and "Ginzburg is an enemy of nuclear physics". And all this was caused by the above remarks! I would not mention it publicly if the matter concerned only my hurt feelings nor would I attempt to answer the criticism. My reasons are quite different; my aims were to stimulate argument, perhaps by making the discussion more heated, to induce readers to think, to work out their own views and to express and defend them fearlessly but especially not to be indifferent. However, indifference is, perhaps, better than the intolerance and disrespect for unacceptable views of "outsiders" and egocentric protection of one's own views and interests by defaming one's critics. At the same time, one can hardly overestimate the contribution made to the development of science by enthusiastic, friendly discussions, debates and arguments, and, fortunately, they prevail in science.

3

Astrophysics

§ 17. Experimental Verification of the General Theory of Relativity

Einstein came out with the general theory of relativity (GRT) in its final form in 1915. By that time he had proposed his three famous ("critical") effects to be used for verification of the theory: gravitational displacement of spectral lines, light deflection in the gravitational field of the Sun and displacement of perihelion of Mercury. More than half a century has passed but the problem of the experimental verification of GRT is still as urgent as ever.

Why is that so?

All the Einstein's effects have been observed but the experimental accuracy is still low [68, 72]. Thus, the error of measurement of the gravitational displacement of spectral lines is about 1% and, moreover, the effect itself does not depend on the type of gravitational theory. The deflection of light rays in the field of the Sun has been measured to an accuracy of about 10% and within these limits the results agree with the GRT (the theoretical deflection is as high as 1.75″ if the light travels in the immediate vicinity of the solar disk). A higher accuracy has been obtained when measuring the deflection near the Sun of radio-waves travelling from quasars and the relativistic delay of radar signals in the vicinity of the Sun. The latter effect for the signals travelling near the Sun's surface is about $2 \cdot 10^{-4}$ s. According to some

recent data the radio wave deflection and the delay time agree with the GRT predictions within the obtained accuracy (the error of measurement 1-2%). The displacement of perihelion of Mercury has been measured to an accuracy of about 1% and the agreement between the measurements of this effect and the GRT predictions has been assumed some time ago to be the best substantiation of GRT (to say nothing of the accurate results on the identity of gravitating and inertial masses being proved with an accuracy of 10^{-12}). However, it has been suggested that this agreement only seems to be good as the quadrupole moment of the Sun has not been accounted for. This objection which at first seems to be highly contrived has been given some support by the observations of the Sun's flattening which has been questioned, though [72b]. There seem to be no doubts again now that the GRT agrees with the observed displacement of the perihelion of Mercury to 1%.

Thus, it can only be asserted now that even for the weak fields, i.e. for fields with a small parameter $|\varphi|/c^2$ (on the surface of the Sun $|\varphi|/c^2 = GM_\odot/r_\odot c^2 = 2.12 \cdot 10^{-6}$), GRT has been verified only to an accuracy of 1-2%. Under such circumstances there are reasons of at least, a ground for discussing the gravitational theories differing from GRT. The most popular of them until recently was the tensor-scalar theory in which the gravitational field is described, apart from the metrical tensor g_{ik}, as a scalar χ. Then the relativistic deflection of light should be $\alpha = (1 - s)\,\alpha_E$ and the displacements of perihelia of planets should be $\Psi = (1 - 4/3s)\,\Psi_E$, where α_E and Ψ_E are the respective values predicted by GRT (that is, according to the Einstein's theory which relates gravitation only

to the field g_{ik}) and s is the fraction of a body's weight due to the hypothetical scalar field χ. As is clear from the above, the observations yield now $s < 0.01$-0.02 and the tensor-scalar theory now seems to lack any appeal, at least its form we discussed here [72c].

Lagging behind in experimental verification of GRT is explained both by small values of the effects observable from the Earth and within the Solar System and by the relatively low accuracy of the astronomical methods used. Now the things have changed owing to the use of interplanetary probes (space rockets), radio methods, etc. Therefore, there seem to be good prospects for verifying GRT to an accuracy of about 0.1% and later even to a fraction of this value [72f]. It is hoped that the accuracy of less than 1% will be reached in the immediate future and it should be improved by one or two orders of magnitude (or even more) at least within this decade.

Provided that experimental measurements in the field of the Sun "okay" GRT (and I strongly hope so), the problem of verification of GRT will move on to the next stage. This will be the verification of GRT validity in strong fields or near and inside supermassive astronomical bodies, let alone cosmology. This will be discussed below (see §§ 19, 20 and 24).

Even if slight deviations from the GRT predictions were reliably shown to exist within the Solar System that would be a discovery of an exceptional importance. As the majority of physicists, I think that a probability of this is negligibly small. But what is the probability in such situations? Moreover, provided one still introduces the notion of probability of the discovery it would be consistent to use the notion of "mathematical expectation" of discovery which is the

product of the probability of discovery and its significance. In such a case a mathematical expectation for finding the deviations from GRT would be considerable even for negligibly small probabilities. However, such discussions amount to no more than a waste of words.

Clearly, no advances can be made in the problem of GRT verification without new observations and measurements. We are anxiously awaiting them.

§ 18. Gravitational Waves

Any relativistic theory of a gravitational field should provide for the existence of gravitational waves in vacuum analogous to electromagnetic waves. This analogy is even closer for GRT since in this gravitational field theory the waves are purely transverse. The concept of gravitational waves in vacuum appeared together with GRT and the well-known and widely used formula for the power of gravitational radiation by masses whose velocity is small as compared to that of light was derived by Einstein back in 1918 [76] (see equation (105.12) in [74], see also [75]).

Gravitational waves must be radiated by any masses with nonzero quadrupole moment of mass varying in time (for fast-moving bodies multipole moments of higher order also play a role). Binary stars or planetary systems are the simplest examples of astronomical objects of this type.

However, gravitational interaction is the weakest of all interactions known. As for the macroscopic manifestations of gravitation that we know so well from our everyday life, they are considerable only because of very large masses involved, for instance, the mass

of the Earth (for two protons their gravitational attraction is less than their electrostatic repulsion by a factor of $e^2/GM^2 \sim 10^{36}$ where $G = 6.67 \cdot 10^{-8}$ g^{-1} cm^{3}s^{-2} is the gravitational constant, $e = 4.8 \cdot 10^{-10}$ esu is the charge of the proton and $M = 1.67 \cdot 10^{-24}$ g is its mass). Therefore, one can hardly be surprised that the gravitational radiation power is usually comparatively small (for instance, for binary stars) and its detection is rather difficult. At any rate, no observations of gravitational waves have been made yet with any degree of confidence and the prospects of receiving gravitational waves from binary stars and pulsars seem to be very dim. Suffice it to mention that even if PSR 0532 pulsar in the Crab nebula emitted gravitational waves with an intensity* $L_g \sim 10^{38}$ erg s^{-1}, then the gravitational radiation flux at the Earth would be only $F_g \sim 3 \cdot 10^{-7}$ erg cm^{-2} s^{-1}. At the same time, the sensitivity of the available gravitational wave detectors corresponds to fluxes not less than 10^4-10^6 erg cm^{-2} s^{-1}, i.e. it is at least 11 orders of magnitude less than required (see [72a, 77a]). To use the available methods for detecting pulsar radiation with a flux of about $F_g \sim 3 \cdot 10^{-7}$ erg cm^{-2} s^{-1} a detector weighing a few tons should be cooled down to $10^{-2} \cdot 10^{-3}$ K (although if instead of a material of an aluminium type use is made of such a substance as sapphire with

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* This is the total luminosity of the Crab nebula in all the ranges of the electromagnetic radiation spectrum. I think that the gravitational radiation power cannot be so high, it is probably a few orders of magnitude lower. Possible exceptions may be X-ray pulsars and "collapsars" in close binary systems (see §§ 20 and 21). Under such circumstances the quadrupole moment of rapidly rotating compact star might be anomalously high.

a very low internal friction, then the mass of a detector may be reduced by a few orders of magnitude [78c]). This is possible but, of course, very difficult.

But nevertheless, one of the most sensational news of recent years was reported reception of cosmic gravitational radiation [77]. The author of the articles [77] reports that massive aluminium cylinders (1.5 tons each) spaced 1000 km apart start vibrating at a frequency of about 10^3 Hz under the effect of gravitational radiation coming from the regions near the Galactic centre. The power of this radiation, if it really originates near the Galactic centre (the distance of about 10^4 parsecs $\approx 3 \cdot 10^{22}$ cm), must be as high as 10^{50} (estimate of [77]) or 10^{52} erg s^{-1} and higher (estimates of [72a, 75]). The energy corresponding to the Sun's rest mass is $M_\odot c^2 \sim 10^{54}$ ergs and hence if there really occurs emission of radiation from the Galactic centre with the power of 10^{50}-10^{52} erg s^{-1}, then the mass of this region must decrease by $(10^3$-$3 \cdot 10^5)$ $M_\odot$ a year due to gravitational radiation alone. It is hard to believe that such a powerful gravitational radiation exists, although it is feasible energetically (the mass of our Galaxy M_G is about $(1$-$3) \cdot 10^{11}$ $M_\odot$, however, the mass in the regions of the Galactic centre is considerably less and even for the radiation power of 10^3 $M_\odot c^2$ year^{-1} it could not be emitted from the Galaxy during the period comparable to its age which is over 10^9 years). Nothing is known about a possible mechanism of such radiation. Attempts have been made to take similar measurements in a number of laboratories, but without success (the first such attempt has been described in ref. [78a]).

, It should be added that no correlation has been found between the events observed in [77] and other

events (radio pulses, cosmic-ray showers, X-radiation [78b], etc.) that might be caused by high-power processes occurring in the Galactic nucleus.

We know that in the history of science there was quite a number of "pseudodiscoveries" along with the real discoveries and sooner or later such mistakes were revealed. I believe that the observation of powerful gravitational radiation is one of such pseudodiscoveries.

However, the detection of cosmic gravitational radiation will be attracting serious interest for a long time to come. Reception of gravitational waves may (and will!) provide yet another channel for getting data on space. Therefore, a "gravitational wave astronomy" should be born sooner or later.

If the Universe contains no unexpectedly powerful sources of gravitational radiation studies will be made of the gravitational waves from binary stars and pulsars which are interesting enough, too. But under such circumstances one can hardly hope for a success in less than ten years or so.

§ 19. The Cosmological Problem. Singularities in General Relativity Theory and Cosmology

Cosmology studies space—time on a large scale and for long-time periods. Hence, cosmology is closely related to extragalactic astronomy and covers a wide range of studies. However, the key problem of cosmology is to find out the very character of the Universe evolution with time and to develop a cosmological model reflecting the reality (we assume here that the reader is acquainted with the basic concepts of contemporary cosmology and the landmarks of its develop-

ment; this is justified to a certain extent by availability of numerous publications in the field [74, 80-82]).

The homogeneous and isotropic cosmological models represent the Universe as an expanding system according to the observations (such models were first considered by Friedman in 1922 and 1924 and then by Lemaitre and many others*). It is of interest, that it was only in 1934 when Milne and McCrea showed this nonstationary state to be classical in character, i.e. with a certain approach it may be derived even from the Newtonian theory of gravity (the fact is that if in a system of bodies there act only gravitational forces corresponding to attraction, this system cannot remain at rest and in the absence of rotation it will either contract or expand according to the initial conditions). Irrespective of the nature of expansion, it clearly could not proceed eternally in the past. Indeed, in all homogeneous and isotropic models expansion either occurred at a certain moment after a contraction stage or was initiated at a certain moment $t = 0$ when the density of matter ρ was infinite (the singularity). Therewith, if the cosmological constant Λ is zero,

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* To be more precise, Einstein was the first to suggest a relativistic cosmological model which was therewith homogeneous and isotropic, in 1917 (see [83]). But this model was static. It corresponds to a single solution out of the two-parametric family of otherwise nonstationary solutions found by Friedman. Friedman did not assume the cosmological constant Λ introduced by Einstein to be zero. When $\Lambda = 0$ all the homogeneous and isotropic models are nonstationary. Note that local homogeneity and isotropy of space (and of the respective model) are possible in global models differing in topology. Probably, there may be many cosmological models, besides Friedman's one, which have not yet been studied but which agree with the available experimental data.

then all the solutions belong to the latter class and reveal a singularity (those solutions with $\Lambda \neq 0$ which do not reveal a singularity do not agree with the observational data [80c]).

Such singularities ($\rho \to \infty$) seem to be admissible in logical terms but many authors believe that singularities indicate some trouble, invalidity or limitation of a theory (I share this opinion). For some time hopes have been voiced that singularities in Friedman's models are due to their high symmetry and that such singularities will disappear in inhomogeneous and anisotropic cosmological models just as the image at the focus of a high-symmetry lens is blurred with its distortion. However, this has been shown to be wrong in the recent years [80e]: highly generalized anisotropic and inhomogeneous GRT solutions corresponding to cosmological models also have singularities (approximation to this singularity occurs, generally speaking, in a very peculiar oscillating fashion).

Thus, it seems impossible to avoid singularities in the problems of cosmological expansion (or of a collapse of supermassive stars, see the following section)*. But this is by no means a decisive evidence of the existence of real singularities with $\rho \to \infty$. The thing is that GRT is a classical theory. But there is no doubt that a true (complete and consistent) gravitational field theory must be a quantum theory. Usually, these quantum effects are extremely small in astrophysics as in most macroscopic problems but it is just in the vicinity of singularity that the quantum effects sharp-

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* This is not exactly so for the systems with nonzero total electric or mesonic charge (for the fields of vector mesons, see [62b]).

ly increase. Let us assume, for instance, an existence of the fundamental length l_0 (see § 12). Then it seems almost certain that the classical GRT ceases "functioning" at distances of the order of, or less than, l_0 and, probably, for densities* $\rho \geqslant \rho_0 \sim \hbar/cl_0^4$. If l_0 is about 10^{-16}-10^{-17} cm, the density ρ_0 is about 10^{30} g/cm³. It may be assumed that in this case densities $\rho \geqslant \rho_0$ are impossible and the singularity, as all divergences, disappears. If there is no fundamental length l_0 independent of gravitation, then, nevertheless, there will emerge a gravitational length l_g (possibly, it is just the fundamental length l_0). Indeed, the gravitational constant G with the dimension g^{-1} cm³ s^{-2}, the light velocity c and the quantum constant $\hbar$ may be used to derive a length

$$l_g \sim \sqrt{G\hbar/c^3} \approx 1.6 \cdot 10^{-33} \text{ cm} \tag{17}$$

This length corresponds to the time $t_g \sim l_g/c \approx$ $\approx 0.5 \cdot 10^{-43}$ s and the density

$$\rho_g \sim \frac{c^2}{\hbar G^2} \sim \frac{\hbar}{cl_g^4} \approx 5 \cdot 10^{93} \text{ g cm}^{-3} \tag{18}$$

It follows from various considerations and estimates [82] that, with an account taken of quantum effects, the density, as to the order of magnitude, cannot be more than about $\rho_g \sim 10^{94}$ g cm^{-3}. Moreover, in the vicinity of the singularity there should occur apparently an intense production of various particle pairs, apart from the growth of various fluctuations [82c and d]. This suggests that classical singular solutions of GRT

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* Only quantity ρ_0 (g cm^{-3}) alone can be constructed with the quantum constant $\hbar$ (g cm² s^{-1}), the light velocity c (cm s^{-1}) and the length l_0 (cm).

cannot be extrapolated to a density range above ρ_g and, in general, to the singularity itself. However, no consistent quantum gravitational theory has been developed yet, let alone quantum cosmology. Therefore, the limits of classical description cannot be established confidently enough. But this fact has no bearing on the need to develop quantum cosmology. The problem seems to be exceptionally difficult but it has fundamental importance and must be solved.

The cosmological problem and the related problem of the GRT singularities hold in astronomy (as regards its character and the type of tasks) a place similar to that of microphysics in physics. Moreover, the microscopic problems seem to interlock here with the problems of astrophysics and cosmology (let alone the friedmon hypothesis [61, 62]). Most probably, understanding of these problems depends on the development of new ideas, the quest for truth in this field will go through mistakes to new and new attempts to find a right way.

§ 20. Is there a Need for "New Physics" in Astronomy? Quasars and Galactic Nuclei

Can we expect deviations occurring ever or anywhere in space from the classical GRT solutions apart from the early (that is, close to the classical singularity) stages of evolution of the Universe? A similar question may be formulated in a wider context by considering, instead of deviations from GRT, a more general possibility of astronomical deviations from the known physical laws.

In a sense this seems to amount to the eternal question which worries many astronomers:—Does the astro-

nomy boil down to the "terrestrial" physics, the physics that is valid in our laboratories? A similar question has been troubling biologists for many years:— Do all biological phenomena reduce to physics and molecular concepts or not?* Of course, there is no an *a priori* answer to such questions. It appears that the most natural (in fact, most universally employed) approach is as follows: let us make use of the available physics without restrictions; if we come across really unsurmountable difficulties we shall be ready to analyze new concepts, to reconstruct or generalize physical theories. Practically everybody will agree, probably, to this approach but this will be far from being a consensus, for one has to agree to a definition of an unsurmountable difficulty first!

In this respect the physicists dealing with astronomy are usually much more conservative than the "pure" astronomers (I have in mind here the "good" or "healthy" conservatism). Some astronomers seem to have a sort of a complex for escaping from physical shackles and a yearning for speculations not restricted by any known physical laws. As an illustration, the following words of Jeans may be cited [84]: "Each failure to explain the spiral arms makes it more and more difficult to resist a suspicion that the spiral nebulae are the seat of types of forces entirely unknown to us, forces which may possibly express novel and unsuspected metric properties of space. The type of conjec-

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* The development of views on this problem amounts to an increasing, or frequently infinite, expansion of the "field of applicability" of physics in biology. Highly instructive in this respect is the development of Bohr's views (see [71] and references cited there).

ture which presents itself, somewhat insistently, is that the centres of the nebulae are of the nature of "singular points", at which matter is poured into our Universe from some other, and entirely extraneous, spatial dimension, so that to a denizen of our Universe, they appear as points at which matter is being continually created."

These concepts of Jeans are now quoted by some authors as a sort of a prophecy. But they date back to 1928 when rather little was known about the structure of galaxies and the theory of their evolution was practically nonexistent (moreover, the problem of spiral arm origin is now considered to be solved to a great extent).

Our present knowledge about galaxies is incomparably greater and, in particular, they have been found to possess a nucleus which plays an important role and is sometimes active [85, 86]. But can one make from the available data such far-going conclusions as those by Jeans [84] and Ambartzumyan [85] about the role of the nuclei as sources of matter (see also [85d]), or that these nuclei "are a new form of existence of matter possibly unknown to contemporary physics" [85c]?

Most astrophysicists do not agree with these conclusions, there still remains a hope (and quite a reasonable one) to explain all the phenomena observed in galaxies, their nuclei and quasars [86b, c] within the framework of the available physical theory. Galactic nuclei and quasars may well be supermassive plasma bodies ($M \sim 10^7$-$10^9 M_\odot$, $r \sim 10^{16}$-10^{17} cm) with a speedy internal rotary motion and magnetic fields, or contain such bodies in their central parts [86d] (there are, also, other possibilities—the nuclei or, more exactly,

their cores can be very dense clusters of stars or massive black holes) [86d].

The problem of the missing mass in clusters of galaxies may be dealt with in a similar way. Such clusters must be stable only if their total energy being equal to the sum of kinetic energy and potential energy of gravitational interaction is negative (the energy of gravitational interaction is negative as it is taken to tend to zero with increasing distance between masses). However, there are stable clusters clearly observed and their total energy is positive if only the known masses are taken into account (that is, mostly the masses of the stars comprising the galaxies) [87]. That would be changed if there could be found in the clusters some unknown masses making a sufficiently large contribution to gravitational interaction thereby stabilizing the clusters. The most probable suggestion for the missing mass is the intergalactic gas present in the clusters (now an increasing attention is also drawn to some low-luminosity stars [87f]). Another possible explanation involves collapsed masses (see § 21 and [87d]); refer also to the hypothesis of [87e] which suggests that the clusters may be stabilized by capturing the neutrinos if the rest mass of the latter is nonzero (though very small). However, it is very hard to observe an intergalactic gas or the low-luminosity stars let alone the collapsed masses in the clusters and no definite results have been obtained yet. Therefore, most physicists and astronomers believe that the missing masses will yet be found and the problem will be resolved to general satisfaction. But for the time being this has not been proved and there is a wide scope for contriving hypotheses, for instance, for suggesting that the known

physical laws are violated in the clusters (this concerns the laws of classical mechanics and the Newtonian law of gravitation, since the GRT effects in clusters are very small). If a new matter is created somewhere in the clusters of galaxies (for instance, in the nuclei of the galaxies comprising them), then the missing masses are not needed for explaining the observational results, for in this case the stability of clusters might be only seeming (new galaxies are produced continuously and other galaxies move away from the clusters). Thus, the problem remains unsolved, the difficulties involved cannot be ignored but one can hardly agree that a "lack" of mass in clusters necessitates altering the fundamental physical laws.*

In addition, the above references to the opinion of the majority of scientists cannot help calling to mind the words of Galileo who said that in science the opinion of a single person may mean more than that of a thousand. Therefore, I do not propose that the "majority rule" should be used in favour of the unlimited usage of the known physical laws, I just state the facts. And the facts are that even the astronomical community, let alone the physical community, has by no means agreed with the assertion that new fundamental physical concepts should be introduced for understanding the processes occurring in galactic nuclei, quasars and clusters of galaxies.

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* In the recent period the problem of "the missing mass" has somehow lost its relevance. When we take into account the low-luminosity masses at the outer regions of the galactic clusters or galaxies the stability of clusters can, apparently, be explained in the most natural way. The analysis of the problem of "the missing mass" is a good illustration of the triumph of the "healthy conservatism" in science.

There are other aspects to be discussed in connection with the possible discovery of new physical laws from astronomical observations. Here we shall only note the following considerations. Nobody questions the importance of introducing new physical concepts. This must be done, of course, in the field of microphysics and, most probably, for solving the cosmological problem and generally when coming to singularities (i.e. the singularities in the solutions of the classical GRT—the nonquantum gravitational field theory). But there are no reasons at all for suggesting to introduce or find new fundamental physical concepts and laws in those regions and for those objects where the conditions are within the ranges covered by conventional physics (density, temperature, etc.). On the other hand, one cannot rule out a possibility of finding some essentially new relationships under such conditions but in large-scale systems (galactic nuclei, quasars and galaxy clusters), for instance, owing to enormous masses and cosmic distances, contributions of processes of very low probability, etc. In other words, the problem should be considered under concrete circumstances. This proposal is, of course, self-evident. It aims, however, at emphasizing that relativity and incompleteness of our knowledge do not necessitate the introduction of new concepts and laws, as some people tend to do, even if there is no evidence of inapplicability of available physics (for more details see [88]).

Thus, it is possible (in my opinion, highly probable) that there is no need to turn to any "new physics" to account for the processes occurring in galactic nuclei, quasars and clusters of galaxies. However, there may be a need for it and in any case, it is just in galactic

nuclei, quasars and clusters of galaxies, where the search is going on for deviations from GRT, for violation of the baryon charge conservation, etc.* Studies of galactic nuclei, quasars and clusters of galaxies are of outstanding importance both for the above reason and because of the exceptional importance of these objects for astronomy as a whole (and cosmology, too). These studies aim at understanding not only the structure and dynamics of these objects but their origin as well. The question of origin of galaxies and their clusters, though, emerged long before the discovery of galactic nuclei and quasars. This question is, to a certain extent, independent as evidenced by the fact that many galaxies do not exhibit any nucleus. It is still unknown and open to controversy what are the causes of formation of galaxies and their clusters [87g, 89]. To find out and investigate these causes is one of the primary tasks in astronomy.

§ 21. Neutron Stars and Pulsars. Physics of "Black Holes"

As far as I know, the concept of neutron stars was reported for the first time in 1934 [90] and then was extensively discussed for many years but only in theoretical terms (see [91a] and references cited in [91b, c]). The attempts to observe neutron stars at first seemed almost hopeless but then it was suggested that such stars could be observed while they were

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* As mentioned above, it cannot be ruled out that the cores of the galactic nuclei and quasars are "black holes". If that is so then, of course, the problems of the galactic nuclei and quasars are, to a certain extent, related to the "new physics" discussed in connection with the "black holes".

hot by their X-ray radiation ($T \sim 10^6$-10^7 K)*. In fact, the neutron stars were discovered in 1967-1968 by their specific periodic radiation within the radio range: we are talking about the pulsars which are now commonly identified with the neutron stars [79, 92].

There are many problems associated with the studies of neutron stars and pulsars (still distinction must be made between them, the more so, as not all of the neutron stars necessarily produce the observed pulsed radiation). But the same is true for stars of any type. However, neutron stars and pulsars are discussed here owing to the following special circumstances.

Firstly, the bulk of a neutron star consists of a substance with densities varying from 10^{11} to 10^{15} g/cm^3. There is little knowledge available about the equation of state and all the properties of matter under such densities and their study is of considerable importance [93a]. Of special interest is the problem of superfluidity of neutron liquid and superconductivity of proton liquid in the neutron stars [92, 93]. (At densities about 10^{13}-10^{15} g/cm^3 the content of protons and, of course, electrons in the neutron substance is of the order of a few per cent; this mixture may be approximately treated as consisting of independent neutron, proton and electron liquids of the Fermi type as neutrons, protons and electrons comprise under such conditions degenerate Fermi systems.)

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* The neutron star radius is about 10-30 km, that is, by five orders of magnitude less than the Sun's radius ($7 \cdot 10^5$ km). Therefore, at the Sun's temperature (about 6000 K) the photosphere of a neutron star would radiate with a power by ten orders of magnitude less than that of the Sun.

Secondly, little is known about the central regions of the neutron stars where the density exceeds $5 \cdot 10^{14}$-10^{15} g/cm³ (the density depends on the mass of the star); under such densities noticeable amounts of mesons and hyperons should appear (that is, many kinds of strongly interacting particles, the hadrons), apart from nucleons and electrons, so that the equation of state becomes even more unclear [93a, c, d].

Leaving aside the hypothetical states suggested for the vicinities of singularities (cosmology, collapse), the central regions of the neutron stars exhibit the highest density of matter found in nature. I think the importance of the fact is self-evident*. It should be added that the gravitational fields encountered in the neutron stars are also the highest in nature (as above, with the exception of the fields considered theoretically in cosmology and collapse). This means that deviations from GRT, if they occur, should be found first of all in the neutron stars.

Thirdly, the electrodynamics of pulsars and the mechanism of their radiation are not yet sufficiently clear [92]. Of special interest is the structure of the neutron star crust [92], in particular, when the strong magnetic field is taken into account [35, 36], which may be

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* It was mentioned in § 19 that if the fundamental length l_0 exists the violations of the known laws can start at a density $\rho_0 \sim \hbar/cl_0^4$. Since the density in atomic nuclei ρ_N is of the order of 3·10¹⁴ g/cm³ and no sharp anomalies of the "fundamental type" are observed we have an estimate $l_0 \leqslant (\rho_N \ c/\hbar)^{1/4} \sim \sim 10^{-13}$ cm; this estimate is supported by even more convincing evidence (it has been mentioned above that the current estimate is $l_0 \leqslant 10^{-15}$-10^{-17} cm).

Nevertheless, the central regions of massive enough neutron stars, where $\rho > \rho_N$ are, evidently, of interest for microphysics, too.

as high as 10^{11}-10^{13} Oe in the pulsars. We should note also the discovery of X-ray pulsars Cen X-3 (Centaurus X-3), Her X-1 (Hercules X-1) and several other, besides the "conventional" pulsars which emit radio waves (the number of such pulsars is now over 100 and only one of them, PSR 0532, the pulsar in the Crab Nebula, has been reliably shown to emit radiation in the optical and X-ray ranges. For the X-ray pulsars emission in the optical and radio ranges (if at all noticeable) is clearly of secondary importance: the discovery and study of such pulsars depend on their X-ray emission [94].

Even more important is the observation that such pulsars, in contrast to the "conventional" ones, belong to close binary star pairs. Therefore, the nature of their radiation depends greatly or, rather primarily, on accretion, i.e. the flow of matter to the star. In this case, we mean the flow of matter from one component of the binary star (which is a more or less normal star) to its compact component, the neutron star*.

All the above problems concerning neutron stars and pulsars are full of uncertainties and complicated issues. There are obvious links between these problems and some of the key tasks of physics and astronomy. Therefore, the neutron stars and pulsars will, probably, be the object of high-priority studies for many years to come. However, whereas the discovery of neutron stars was just a dream up to 1967-1968, now the neut-

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* It cannot be ruled out theoretically that a white dwarf may be a compact star. In principle, not only the neutron stars but also white dwarfs (though only those with periods $P > 1$-3 s) may play the role of pulsars. There is no yet observational evidence of this happening.

ron stars are gradually turning into more or less familiar objects, though one has yet to get used to them.

The attention of the novelty hunters is now increasingly drawn by the search for even more exotic stars, namely, the "black holes". The problem, though, is not as novel as all that and the above phrase merely contrives to open up a new subject. In fact, the problem is forty-years old (we shall see later that its origin, in a sense, can be traced back to the XVIII century) and is linked to studies of stable configurations of cool, "dead" stars.

The stars are heated by the nuclear reactions occurring inside them and thus emit light. The pressure gradient developed in a hot star protects it from contracting under the effect of gravitational forces, i.e. maintains a state of quasi-equilibrium. But when the nuclear fuel burns out the star contracts and must transform to some final ("cool") state. If the star rotates only slowly (or does not practically rotate), cooling down occurs without explosions and the star mass M is less than $1.4M_\odot$ ($M_\odot = 2 \cdot 10^{33}$ g is the mass of the Sun), then the final state is a white dwarf configuration (the radius of 10^3-10^4 km, the mean density ρ of about 10^5-10^{10} g/cm^3). The star's equilibrium is maintained by the "zero" pressure of the electron gas*. But

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* Such stars are observed in a state when they still retain a part of their "fuel". As their surface area is small (as compared to normal stars) this makes the temperature of the surface (photosphere) of white dwarfs rather high and they appear "white", i.e. short-wave optical radiation prevails in their spectrum. But red "white dwarfs" have been observed, too, and the final state of any white dwarf (in the absence of accretion) is a "black dwarf", i.e. a completely cool and thus nonradiating dense star. The evolution of stars, especially its later stages, is discussed in detail in [91c].

the star "hates" dying: in the process of the nuclear fuel burning out it sometimes explodes (such explosions are seen as the appearance of novae and supernovae) ejecting a portion of its mass. Perhaps (this is not clear yet), a star completely disappears with such an explosion (i.e. all its mass is ejected), but other possibilities seem more probable. One such possibility is the preservation of a star with a mass M less than $1.4M_\odot$ which evolves further to a white dwarf state. Another possibility is the development of a neutron star produced by strong compression of the central region of the initial star during explosion. If the neutron star mass M is less than $1.4M_\odot$, we face a situation when a cool star possesses two stable equilibrium states. Which of the two states is realized (a white dwarf or a neutron star) depends on the "prehistory" of the star (if the evolution is slow the star, of course, turns into a white dwarf)*. Now, what happens to a more massive star $(M > 1.4M_\odot)$ if it could not shed off its envelope and thus get rid of a part of its mass?

As our knowledge of the equation of state is insufficient we do not know yet the maximum mass of neutron stars. This mass M_{max} has been reliably established to exist and to be not more than (2-3) $M_\odot$. Therefore, the neutron star is the final state of the stars whose mass M is greater than $1.4M_\odot$ but less than $M_{max} \approx (2\text{-}3)M_\odot$ **.

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* To be sure, one of the states is more favourable energetically for a mass $M < 1.4\,M_\odot$, too. But these stages are divided, generally speaking, by an enormous potential barrier. A star may "jump" over the white dwarf state and turn into a neutron star only with an explosion or as a result of the explosion.

** It cannot be ruled out that the neutron star maximum mass M_{max} is less than the white dwarf maximum mass which is

In the case of cool stars of a greater mass no substance can withstand the gravitational forces, and the star will contract infinitely, it will collapse and become a "black hole". A concise but a clear discussion of "black holes" is very difficult to make, especially if one cannot invoke general relativity theory (GRT). Moreover, such an attempt would deviate from the style of other sections of this book. Therefore, I shall only cite references (see [91c, 95a] and also [61, 62, 74, 80b, 95]) and make a few remarks.

An important part is played in the gravitational collapse by the gravitational radius

$$r_g = 2GM/c^2 \approx 3 \, (M/M_\odot) \text{ km} \qquad (19)$$

where M is the mass of the body, $G = 6.67 \cdot 10^{-8}$ is the gravitational constant and $c = 3 \cdot 10^{10}$ cm/s is the velocity of light; for the Sun (the mass $M_\odot = 2 \cdot 10^{33}$ g) the gravitational radius r_g is about 3 km, while the radius of the photosphere $r_\odot$ is about $7 \cdot 10^5$ km. For an "outside observer", i.e. when the star's radiation is received far from it, the gravitational radius plays the part of the minimum radius of the contracting star surface since light (or signals of any other nature) can leave the star only from distances r greater than r_g. If the star's radius in its reference system (the system linked to the star's material) is less than r_g, then the light cannot leave the star,

(1.2-1.4) $M_\odot$. Furthermore, the stars are assumed to rotate not too rapidly (the fate of rapidly rotating stars is substantially unknown; probably, upon contraction they become unstable and break up into several stars). It may readily be seen that all these reservations are insignificant in terms of our discussion (in more detail this is discussed in [91c, 95]).

it is "captured" by the star and falls together with the star's material to its centre. It should not be assumed that this effect can be derived only within the GRT framework. On the contrary, it was back in 1798 that Laplace noted (proceeding, of course, only from the Newtonian mechanics and the law of gravitation) that (as quoted by Eddington) "the largest luminous bodies in the Universe may, through this cause, be invisible to us" (quoted from [95a] as the original publication is difficult to find). Laplace's reasoning was correct and, moreover, he derived the correct expression for the gravitational radius!

Indeed, let us take the light to consist of particles of the mass m (according to modern concepts we may assume that $m = \hbar\omega/c^2$, where $\hbar\omega$ is the photon's energy). Such a particle may be removed from a distance r between it and the centre of a body of a mass M to infinity if $GmM/r = \dfrac{mv^2}{2}$, where v is the radial velocity of the particle. Assuming $v = c$ (the velocity of light) we derive a condition $r = 2GM/c^2$ which does not envolve the mass m. It is just the condition that demonstrates that light cannot be emitted from distances r less than $r_g = 2GM/c^2$. This calculation is not consistent enough, as, for instance for the bodies travelling at a velocity v comparable with the velocity of light c, the kinetic energy is $\dfrac{mc^2}{\sqrt{1 - v^2/c^2}} - mc^2$, rather than $\dfrac{mv^2}{2}$. Thus, the coincidence of the limiting Laplace radius with r_g is, to a certain extent, accidental. But it is by no means accidental that the Newtonian theory is capable of describing qualitatively and sometimes quantitatively the GRT effects; it

should be remembered that the classical mechanics and the gravitation theory are the extreme case of GRT*.

Now, let us turn back to "black holes". The name itself implies the fact that after some time τ the star dies out and becomes invisible for an outside observer. The time τ depends on the initial conditions, the sensitivity of equipment, etc., but its order of magnitude is estimated as follows:

$$\tau \sim r_g/c \approx 10^{-5} \, (\dot{M}/M_\odot) \text{ s}, \qquad (20)$$

i.e. the extinction occurs very rapidly, at least for the stars with a mass $M \sim M_\odot$ but not for the galactic nuclei and quasars with $M \sim 10^9 M_\odot$, though for them also the time τ is negligible by astronomical standards**. Nevertheless, a "black hole" cannot be said to disappear. First of all, it must be remembered that its gravitational field is fully preserved and for $r \gg r_g$

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* It was already mentioned in §'19 that the nonsteady state of the Universe is, essentially, classical in nature (moreover, the laws of evolution for the Friedman's models of the Universe may be derived from the Newtonian theory [80]). The first to predict the deflection of light in the Sun's, field was Zoldner in 1801; his quantitative result agrees with that of Einstein obtained in 1911 (for the calculation and reference see [96] as the original Zoldner's paper is difficult to get). But later (in 1915) Einstein found that the actual deflection of light was twice as large as that he calculated in 1911 and Zoldner in 1801. As mentioned in § 17, the observed values agree (accurate to 1%) with the Einstein's result of 1915 which follows from GRT (in 1911 GRT was not fully developed and Einstein's argument was based only on the principle of equivalence that was insufficient in that case).

** To avoid misunderstanding note that the time $\tau \sim r_g/c$ characterizes the last relativistic stage of the collapse when the star's radius r is about r_g (say, $r \leqslant 3r_g$). The process of contraction to a radius $r \sim r_g$ may occur slowly but all this period the star remains visible.

the star's gravitational potential is described by the usual expression $\varphi = -GM/r$. Thus, a "black hole" which is a component of a binary star pair has the same effect on the second star as a normal star. This is just where the "black holes" are to be looked for first of all; a search should be made for binary stars one of which is not radiating, while its mass M is greater than $3M_\odot$, so that it cannot be a neutron star or a dead (black) white dwarf. The task is by no means simple [91c, 97] but may probably have already led to a successful observation of a "black hole", namely the X-ray source Cyg X-1 (Cygnus X-1) [98]. In this case, though, the "black hole" cannot be said to be invisible. So much the better, of course! But this apparent contradiction in terms has to be explained. The fact is that the "black hole" is indeed invisible by itself (when the time $t \gg \tau$, see (20)), but this does not necessarily involve the matter falling to the "black hole" star (accreted by it). In the course of accretion the gas being accreted can be turbulized; as the gas approaches the region with $r \sim r_g$ growing magnetic fields arise in the gas and a fraction of the particles are accelerated. As a consequence, the substance being accreted will emit radiation (for instance, synchrotron radiation) and, hence, may be observed. The resulting radiation is distinguished by its variability with a quasiperiod $P \sim \tau \sim r_g/c \sim 3 \cdot 10^{-5}\text{-}10^{-4}$ s (for $M \sim (3\text{-}10)\,M_\odot$). Therefore, the "black holes" radiating as a result of accretion are referred to as "fluctuars" [98c]. Firstly, the X-ray source Cyg X-1 is a component in a rather close binary system (the period of 5-6 days); this provides for a massive accretion and, almost certainly, results in the X-ray radiation (the same may be said about the X-ray pulsars Cen X-3 and

Her X-1 where the compact component of the binary star is a neutron star, rather than a "black hole"; see above). Secondly, in contrast to the above X-ray pulsars, the radiation of Cyg X-1 has no definite period though it fluctuates sharply [98]. Unfortunately, no fluctuations with the characteristic period τ about 10^{-4} s have been recorded yet and only the oscillations with periods of the order of seconds or fractions of a second have been observed. On the other hand, the mass of the compact component has been evaluated to be about (10-20) $M_\odot$ and this result also confirms its identification with a "black hole" [98c, d]. However, the identification of Cyg X-1 with a "black hole" cannot be considered final as there are some doubts about an optical identification of the X-ray source Cyg X-1 and about some other aspects, as well [97, 98]. The most convincing proof of the existence of a "black hole" would be recording of rapid brightness fluctuations (in any spectral region) with a characteristic quasiperiod P of the order of 10^{-4} s.

In principle, not only a normal star (with a mass $M \sim M_\odot$) but also a more massive object such as a quasar or a galactic nucleus may develop into a "black hole". In particular, it has been suggested that the centres of our Galaxy and of some other galaxies contain nuclei which are dead quasars, that is, quasars transformed into "black holes" [86d, 99]. Any activity of such galactic nuclei must be due to accretion and in this respect they are similar to the above "fluctuar" model but on a much larger scale.

Vigorous hunt for "black holes" may be expected to develop in the nearest future followed by the analyses of radiation originating in their vicinities and of other their features. In such analyses account should be

taken of rotation of the "black holes" and the fact that if the rotation is sufficiently rapid the "black holes" give rise to configurations with essentially different characteristics (the so-called "naked singularities", see [95a]). Anyway, it is now that the investigation of "black holes" is becoming a real astrophysical problem attracting considerable attention though the first explicit treatment of them dates back to 1939 [95c]. I expect that in the immediate future the interest towards physical and astronomical processes and the effects associated with the "black holes" will keep growing.*

A new addition to the astronomer's vocabulary, apart from the "black hole" is a "white hole". Let us imagine that a star or a superstar (the mass $M \gg M_\odot$) does not contract and collapse but, on the contrary, expands or anticollapses. Naturally, for this to happen the matter of the star should be given an initial velocity directed away from the centre. However, astronomy has already encountered one "case" of the same, or more exactly a similar, nature: all the Universe observed is still expanding because ten billion years ago with a tremendous "initial velocity" it came out of an "initial" state which we do not know, or understand yet. Could it be possible for some of the expanding masses to have during that time radiuses less than the gravitational radius and only in our epoch to "cross the border" and become visible "white holes" observable as an explosion? In principle,

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* In the recent period a great interest was generated by the "black holes" with very small masses ($M \ll M_\odot$) in which the quantum effects may be significant. Such a "black hole" emits photons and other particles (see refs. [85f, g]).

this seemed to be possible [91c], particularly, in the context of the hypotheses [84, 85] about outflow or "production" of matter in the singularities. That is why it has been suggested that the "white holes" may be looked for among quasars and galactic nuclei. It has been suggested recently that observation of the "white holes" is unfeasible. Since we have to take into account both the accretion of matter at the "white holes" and the generation of particles by their gravitational fields in their vicinities the result is that we, apparently, cannot distinguish between "white" and "black holes". However, I should admit my poor knowledge of this problem so that I can only report that now there are no "troubles" with the "white holes". As for the search for "black holes" this remains one of the most urgent tasks of astronomy.

§ 22. Origin of Cosmic Rays and Cosmic Gamma- and X-Ray Radiation

It has already been known for over 60 years that a strongly penetrating radiation comes to the Earth from the space; this radiation is referred to as cosmic rays. The nature (composition) of this radiation for many years remained unknown. Now we know that cosmic rays consist of charged particles: protons, nuclei, electrons and positrons. To be more precise, the Earth is bombarded also by cosmic X-rays, gamma-rays and, undoubtedly, neutrinos. But the name of cosmic rays is now generally given to the charged particles of cosmic origin only (this restriction is justified also because charged particles dominate in a high-energy range as regards, for instance, the flux or energy release). The concentration of cosmic rays (for

instance, with the kinetic energy $E_{c.\,r.}$ of the order of
or more than 1 GeV) near the Earth and in a conside-
rable part of the Galaxy $N_{c.r.}$ is of the order of
10^{-10} cm^{-3} so that it is negligible in comparison with
the concentration of gas particles $n \sim 1$ cm^{-3} in the
galactic disk and even in the galactic halo ($10^{-3} \leqslant$
$\leqslant n \leqslant 10^{-2}$ cm^{-3}) or in the intergalactic medium
($10^{-7} \leqslant n \leqslant 10^{-5}$ cm^{-3}). But the energy density of
cosmic rays $w_{c.\,r.} \sim E_{c.\,r.} N_{c.\,r.} \sim 10^{-12}$ erg/cm^3 is
not less than the density of the internal (kinetic) gas
energy $\omega_g = \frac{3}{2}\, kTn \sim 10^{-14}\text{-}10^{-12}$ erg/cm^3 ($n \leqslant$
$\leqslant 1$ cm^{-3}, $T \leqslant 10^4$ K in the disk and $T \leqslant 10^6$ K
in the halo). The magnetic field energy density $w_H =$
$= H^2/8\pi$ in the disk (where $H \leqslant 5 \cdot 10^{-6}$ Oe) is also
not more than $w_{c.\,r.}$. Hence, relativistic particles
(the cosmic rays) are an important dynamic and ener-
getic factor even in our Galaxy (this concerns, of
course, the interstellar medium). The cosmic rays are
of even greater importance in the envelopes of super-
novae, in radio galaxies and quasars. These findings
due to developments in radioastronomy have made
a major contribution to advance in astrophysics over
the past decades [79, 100-103].

The origin of the cosmic rays has been argued about
for several decades but it remains "an important and
interesting problem" as its significance is self-evident
and there is still a controversy about it. There are,
basically, three models suggested for explaining the
origin of the cosmic rays: metagalactic, halo galactic
and disk galactic models. The metagalactic model
assumes that the bulk of the cosmic radiation obser-
ved on the Earth comes from the Metagalaxy, i.e. comes
to our Galaxy from without. The galactic models, on

the other hand, assume that the cosmic rays originate
in the Galaxy itself (with a possible exception of par-
ticles with an energy $E_{c.r.} \gtrsim 10^{17}$ eV) primarily in
explosions of supernovae and also near the pulsars
found in the envelopes of supernovae or, perhaps, in
explosions of the galactic nucleus. In my opinion the
galactic models alone are valid and the main task now
is to choose between the halo model and the disk mo-
del. In the former model the cosmic rays fill a quasi-
spherical halo with the characteristic size $R \sim 5 \times 10^{22}$ cm; in the latter model the cosmic rays occupy
a disk (the radius $R \sim 5 \cdot 10^{22}$ cm, the thickness
$h \sim 3 \cdot 10^{21}$ cm). The difference between the models
affects primarily the mean lifetime of the cosmic rays
in the Galaxy: for the halo model this lifetime is about
10^8 years and for the disk model it is about 10^6-
10^7 years.*

However, the problem is not clear though there are
very serious doubts about the metagalactic models and
I, for one, have been trying for about twenty years to
disprove it. A good illustration of this is the article
[100c] which defends at some length the metagalactic
model. Basically, the question depends on the deter-
mination of the energy density of the cosmic rays
$w_{c.r.i.}$ in intergalactic space. The metagalactic mo-
del assumes that in a region surrounding the Galaxy
(or, maybe, in the whole Metagalaxy) $w_{c.r.Mg}$ is of
the order of $w_{c.r.} \sim 10^{-12}$ erg/cm^3 ($w_{c.r.}$ is the energy
density of the cosmic rays in the Galaxy, see above).
On the contrary, the models suggesting a galactic
origin for the bulk of cosmic rays coming to the Earth

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* Some convincing recent results support the halo model
[104c, d].

assume that $w_{c.r.\ Mg.} \ll 10^{-12}$ erg/cm³ (probably, even $w_{c.r.\ Mg.} \lesssim 10^{-15}$ erg/cm³). Unfortunately, no measurements of $w_{c.r.\ Mg.}$ are feasible yet so that various estimates and indirect evidence must be invoked and in the absence of direct proof the controversy goes on. But now a real possibility emerged for solving the problem, by direct observations, namely, by using gamma-astronomy which will be mentioned below.

The problem of the cosmic rays origin has, of course, other aspects apart from choosing an appropriate model. The following ones may be mentioned here: the plasma effects in astrophysics [104], the mechanisms of particle acceleration in the explosions of supernovas and near pulsars [100, 104, 105], the Solar cosmic rays and their propagation in the Solar System [106], the chemical composition of the cosmic rays and the energy spectra of their various components (including the electron-positron component). Of particular interest is the superhigh energy range ($E \geqslant \geqslant 10^{17}$ eV). The origin of the cosmic rays of these energies is quite puzzling (particles have been observed with energies as high as 10^{20} eV) and various hypotheses have been put forward to account for the origin of such particles and their behaviour [107].

Astrophysics of cosmic rays originated after the World War II is rapidly developing. Recently, however, this area of astrophysics is frequently referred to as high-energy astrophysics which includes the problems of X-ray- and gamma-astronomy and the high-energy neutrino astronomy.

The emergence of the X-ray astronomy (leaving aside the solar studies) dates back to 1962 when a powerful X-ray radiation source Sco X-1 (Scorpio X-1) was accidentally and unexpectedly discovered in the

course of rocket measurements [108a]. A number of other X-ray cosmic sources were discovered (the so-called X-ray stars); the most productive were the observations from the very first satellite specially designed for the X-ray astronomical measurements (this satellite was launched in late 1970 by the United States from Kenya and was named *Uhuru* meaning liberty in Swahili) [94a]. Over a hundred of X-ray stars have been discovered already including the Crab Nebula pulsar, the X-ray pulsars Cen X-3 and Her X-1 and a candidate for "fluctuar" Cyg X-1 (see § 21), other galactic sources associated with stars, the Crab Nebula itself and other envelopes of supernovae, as well as various extragalactic sources (galaxies and quasars). The diffuse X-ray background radiation has also been observed (i.e. the radiation for which no discrete sources have been identified, at any rate with the angular resolution used).

We know a number of effects giving rise to X-ray radiation: hot plasma bremsstrahlung, synchrotron radiation of the relativistic electrons, scattering of radiation in the radio-frequency, infrared and optical ranges by relativistic electrons transforming this radiation into X-ray radiation (this process is frequently referred to as the inverse Compton scattering). All these mechanisms are, clearly, involved in producing the radiation flux we observe but their contributions vary (for instance, for the Crab Nebula the primary part is played by the synchrotron radiation while in many other X-ray sources bremsstrahlung seems to prevail). Emission of powerful X-ray radiation, undoubtedly, depends greatly on accretion, especially in binary systems [108b]. The analyses should take into account the absorption of X-rays in

the interstellar gas, look for the characteristic X-ray radiation lines of atoms and so on. On the whole, the X-ray astronomy has just entered the race after having been gathering momentum and storing experiences for 8-10 years [94, 108]. Now it is already the third most important branch of astronomy, after optical astronomy and radioastronomy, using the classification according to the observational methods or operational spectral regions. As we have seen, over a short period the X-ray astronomy has yielded some high-grade results and I am sure that more are coming.

Things are different in the gamma-astronomy. Though the possibilities of gamma-astronomy were first discussed back in 1958 [109a] and repeatedly in later publications [109] (see also [100-103]) for a long time no reliable data have been accumulated in the field. The reasons are, so to say, technical. The gamma-ray flux value measured by the number of photons is very small though the energy flux is not so small because a single photon has a relatively high energy*. For instance, fluxes less than 10^{-5} photon/cm² s have to be measured to look for photons with an energy $E_\gamma >$ > 100 MeV (or even better, 10^{-7} photon/cm² s). Such measurements can be carried out only with the help of devices (counters, spark chambers, etc.) having large working areas and capable of operating (in space) long enough. Therefore, gamma-astronomy cannot make use of rockets though an early development of the X-ray astronomy was highly dependent on them. Taking measurements on high-altitude baloons

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* The name of gamma-rays is used here for electromagnetic radiation with the wavelengths less than 0.1 Å, i. e. for photons with energies $E_\gamma \geqslant 100$ keV $= 0.1$ MeV.

and satellites which have already been carried out, have met great difficulties that are not yet resolved completely. Therefore, I shall not discuss here some results which show the existence of gamma-radiation from certain regions of the Galaxy, from discrete sources and from the intergalactic regions [103, 104, 109d, e]. In our discussion two aspects have to be stressed.

Firstly, some of the gamma-astronomical observations are potentially extremely important and promising. Thus, a substantial part of gamma-radiation with energies $E_\gamma >$ 50-100 MeV should be generated by the proton-nuclear component of the cosmic rays in the interstellar and intergalactic media. The protons and nuclei of the cosmic rays collide with protons and nuclei of the interstellar and intergalactic gas giving rise to π^0-mesons among other particles. The latter immediately (the mean lifetime of a π^0-meson is $0.84 \cdot 10^{-16}$ s) decay into two gamma-photons, each with an energy of $\frac{1}{2} m_{\pi^0} c^2 = 67.5$ MeV (the π^0-mesons at rest are taken here). Gamma-rays are emitted also with the decay of Σ^0-hyperon ($\Sigma^0 \rightarrow \Lambda + \gamma$) and as a result of the decay of some mesons and hyperons giving rise to π^0-mesons ($K^\pm \rightarrow \pi^\pm + \pi^0$, $\Lambda \rightarrow n + \pi^0$, etc.). Such gamma-rays of "nuclear origin" have a characteristic spectrum (their energies are, mostly, in excess of 30-50 MeV) and therefore they can, in principle, be distinguished from the gamma-rays produced in other processes (for instance, by bremsstrahlung of the relativistic electrons). The flux of "nuclear" gamma-rays is proportional to the intensity of the generating cosmic rays and this relation allows us to estimate the intensity in the regions far from the Earth, in the galactic

centre, in the radio galaxies, etc. So far, we have obtained all the data on the main, proton-nuclear component of the cosmic rays far from the Earth either by extrapolating the circumterrestrial results on the cosmic rays or from the estimations involving additional assumptions (which are often quite reasonable, though) on the basis of radio-astronomical measurements*. If we manage to determine more or less directly the intensity (and the energy density) of the proton-nuclear component of the cosmic rays far from the Earth, the importance of this development can hardly be overestimated. For, this is just the way along which the hopes lie for finally resolving the protracted controversy about the galactic or metagalactic origin of cosmic rays [103]. The metagalactic model provides for similar energy densities of cosmic rays in our Galaxy and the surrounding regions and in the relatively small galaxies nearest to us, the Magellanic Clouds (about 10^{-12} erg/cm³). Hence, as we know the amount of gas in the Magellanic Clouds we can estimate that the gamma-radiation emitted from the Magellanic Clouds should reach the Earth with a flux of about $3 \cdot 10^{-7}$ photon/cm² s (at energies of $E_\gamma > 100$ MeV). The observations of lesser fluxes will refute the metagalactic model completely. The observation of fluxes close to or above the given value will leave the matter unresolved as the gamma-radiation could in this case be produced

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* Data on the relativistic electrons in the radio-emitting regions is deduced from the radio observations also invoking additional assumptions but at least in a more direct way (for more details see [100-103]).

by the cosmic rays originating in the Magellanic Clouds.*

Secondly, (this is the second aspect of the problem), the time is ripe for gamma-astronomy in a sense that the relevant technological capability has been developed at last. The available satellite-based equipment has the parameters providing for reliable measurements of the fluxes down to 10^{-7} photon/cm^2 s and, at any rate, 10^{-6} photon/cm^2 s which are even less than those expected to come from the galactic centre and a number of discrete sources (estimates for these sources were made by the results of the preliminary measurements [103, 109d]). Furthermore, on 15 November 1972 there was launched a satellite of the same type as the *Uhuru* satellite which was specially designed for gamma-astronomical measurements.

Since then other gamma-satellites have been launched. But even before the data from the gamma-satellites had been obtained a very important gamma-astronomical discovery was made: there were observed gamma-bursts of unknown origin [118].

Some years ago the USA launched four satellites of the Vela series which were designed for inspection in accordance with the treaty prohibiting nuclear explosions in the outer space and thus carried gamma-ray detectors. No nuclear explosions were detected but

* No measurements of the gamma-radiation from the Magellanic Clouds have been made yet. However, this has been done, to a certain extent, for the gamma-radiation coming from the direction to the Galactic anti-centre (that is, from the direction opposite to the direction to the Galactic centre). The observed gamma-flux has been found [109c] to be in disagreement (the flux is too small) with the assumption about the high density of cosmic rays outside the Galaxy (for more details see in refs. [104d,109e]).

from July 1969 to July 1972 there were recorded 16 bursts of gamma-radiation lasting from fractions of a second to tens of seconds [118a]. Of especial importance is that the bursts were recorded simultaneously by several Vela satellites at large distances from one another. Hence, it can be ruled out that a burst is an artifact caused by an instrumental fault in a satellite. Later, a check was made of the stored records from other satellites with an appropriate equipment which were in flight throughout the same period [118b, c]; this check revealed some of the bursts recorded by the Vela satellites (one can hardly expect one hundred per cent correlation between bursts recorded by all the satellites because the equipment does not function all the time, a satellite may be in the Earth's shadow and so on). One should not assume that satellites record bursts exactly simultaneously. This cannot happen as light (and gamma) photons have a finite velocity and the distance between the satellites may be large (for instance, the Vela satellites are at a distance of about 120 000 km from the Earth's centre so that the distance between the satellites may be as large as 240 000 km and the delay between bursts may be almost a second while the instruments measured bursts with an accuracy of a few hundredth of a second). Incidentally, the knowledge of burst delays and, of course, of the relative positions of the satellites makes it possible to show that the bursts do not originate in the Sun or in the Earth. The amount of data accumulated is yet small but so far no visible "unusual" objects (for instance, supernova explosion) have been found in those regions where the bursts come from. But these gamma-radiation bursts are fairly powerful (they have been observed in the 0.1-1.5 MeV energy range and occa-

sionally in the X-ray region) so that they should be related to some powerful cosmic explosion. Actually, for the burst that was studied best, the total energy received in 80 s (such was the duration τ of this burst) was $\Phi \sim 5 \cdot 10^{-4}$ erg/cm². If the source of radiation is in the Galaxy at a distance of, say, $R \sim 100$ ps $\sim \sim 3 \cdot 10^{20}$ cm, the total energy release in the source is $W \sim 4\pi R^2 \Phi \sim 10^{39}$ ergs and its power is $L \sim W/\tau \sim \sim 10^{37}$ ergs/s. If the source is in other nearby galaxies (for instance, for a supernova) and $R \sim 3$ Mps $\sim \sim 10^{25}$ cm, we have even $W \sim 10^{48}$ ergs and $L \sim \sim 10^{46}$ ergs/s. Finally, in the case of the farthest possible sources (such as collapsing galactic nuclei) we have $R \sim 10^{28}$ cm, $W \sim 10^{54}$ ergs $\sim M_\odot c^2$ and $L \sim \sim 10^{52}$ ergs/s. Note that the total power of the solar electromagnetic radiation (luminosity $L_\odot$) is $3.86 \times \times 10^{33}$ ergs/s; hence, the sources of the gamma-bursts are very powerful even according to cosmic standards. Nothing is known yet about the distance to such sources or about their nature in general. If they are flashing stars in the Galaxy they are, apparently, stars of some strange type ("gamma-stars"). We have mentioned already other suggested sources of gamma-bursts, namely, supernova explosions in other galaxies and collapse of galactic nuclei.*

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* Further studies of the gamma-bursts (see, for instance, ref. [118e]), though they have not revealed their origin, have indicated that these bursts are generated in our Galaxy (in this connection the remarks made below on the basis of ref. [118d] seem to be less interesting). Meanwhile, reports have appeared in 1975 about the discovery of the X-ray bursts which are detected significantly more frequently than the gamma-bursts (possibly, the latter are just "harder" X-ray bursts). The X-ray bursts, probably, are produced during accretion of gas at the neutron stars and, possibly, nuclear burning near surfaces of neutron stars within the Galaxy [118f].

The discovery of these and X-ray bursts is, probably, the most significant event in astronomy over the last years (after the discovery of pulsars). Discoveries of this calibre tend to have important consequences or open new vistas. As an illustration note that in the case of gamma-bursts the following suggestion has been made [118d]. If the source of gamma-bursts radiated in the same explosion radio waves with a power even less by a factor of several millions, then the available radio telescopes could receive the respective radio bursts. This is interesting in itself as the observation of such radio bursts would contribute to our knowledge of the sources of gamma-bursts and their nature. But if these sources are extragalactic there emerges another, more important possibility, namely, to determine the product of the distance to the source and the average concentration of the ionized gas along the way from the source to the Earth as it can be estimated from the delay of radio bursts of lower frequencies compared to those of higher frequency. This will, probably, allow us to estimate the mean concentration n_{ig} of intergalactic gas as its knowledge is critical for further development of extragalactic astronomy and cosmology (now it is only known that $n_{ig} \lesssim 10^{-5}$ cm^{-3} though it cannot be ruled out that $n_{ig} \sim 10^{-7}$ cm^{-3}). Finally, it may be suggested to look for gravitational radiation pulses and neutrino bursts related to the gamma-bursts.

If the reader was not fascinated with the above prospects of high-energy astrophysics this could be due only to my failure to depict convincingly the impressive achievements and significance of this new branch of astronomy.

§ 23. Neutrino Astronomy

Pauli predicted the existence of neutrino in 1931. It took 25 years (a longish time for our fast-moving world) before neutrino was observed near nuclear reactors. Then it was natural to ask if we could observe neutrinos of extraterrestrial origin?

As the energy of stars is released in nuclear reactions all the stars must, evidently, emit neutrinos. First of all, we should consider, of course, the Sun (the distance between the Sun and the Earth is $1.5 \cdot 10^{13}$ cm while the distance to the nearest stars is about $4 \cdot 10^{18}$ cm so that the solar neutrino flux must be 10^{11} times as large as the flux from the nearest stars, the other conditions being equal). Detection of solar neutrinos is being attempted for several years already by means of the nuclear reaction $^{37}Cl + \nu_e \rightarrow {}^{37}Ar + e^-$ (here ν_e is the electron neutrino and e^- is the electron) but the results are all negative so far [101, 110, 111]. Moreover, measurements yield an ever decreasing upper limit for the neutrino flux which is now as low as about 10^{-36} s^{-1} (^{37}Cl atom)$^{-1}$. The neutrino flux which results, on the average, in one neutrino capture per second by 10^{36} nuclei of ^{37}Cl is even referred to as a solar neutrino unit (1 SNU). The first predictions were that the solar neutrino flux should be considerably greater (about 10 times) but a variety of reasons were gradually put forward to account for the lesser value. However, if the solar neutrino flux proved to be even less than 1 SNU or, especially less than 0.5 SNU we would find ourselves in a serious trouble. We would have to admit that the state of the Sun is not steady (in particular, that the temperature in its central region varies and at present is lower than in the steady-state solar mo-

dels) or that neutrino is not stable (i.e. there is noticeable decay of neutrinos in 8 minutes it takes them to reach the Earth). Some even more radical suggestions have been made [111]. Such a situation would be the most interesting one in a way, but a quite possible outcome is that in the nearest future solar neutrinos will be detected using the above reaction with ^{37}Cl nuclei or other detectors with different reaction thresholds (for instance, ^{71}Ga and ^{7}Li).

The emergence of the neutrino astronomy is an event of great importance since detection of neutrino is the only known method of probing the central regions of stars. However, one can hardly expect the detection of neutrino from the "normal" stars (apart from the Sun) in the forseeable future. Things are different with supernova explosions and with the formation of neutron stars* which may give rise to intense neutrino fluxes [101, 110, 111]. The same is true about such, to a certain extent hypothetical, events as the collapse of supermassive stars (the galactic nuclei included). Finally, it would be an extremely significant achievement if we could detect the neutrinos produced at the early stages of evolution of the Universe [80b, 101, 111]. Unfortunately, the prospects here are hardly encouraging, as first the sensitivity of the available detectors must be increased by a few orders of magnitude. But as we know from the history of physics and astronomy, it is just the measurement technique prospects where the pessimistic predictions most often prove to

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* It is quite probable that these events are the same but, in principle, a supernova explosion may result in the formation of a white dwarf or collapsed object or in complete disappearance of the star.

be false. Moreover, the present estimates of the "cosmological" neutrino fluxes may be too low. Apart from the above areas of the neutrino astronomy we should also mention high energy neutrino studies discussed in § 13 (see [56, 110]).

Thus, the neutrino astronomy is an up-and-coming research area which promises to yield valuable results and, possibly, even discoveries.

§ 24. The Present Stage in Development of Astronomy

The sixties alone have seen five first-rate astronomical discoveries (quasars, relict thermal radiation, X-ray "stars", maser action in space with OH, H_2O and other molecules, and pulsars), let alone other important achievements of a lesser scale. Over the same period physics may boast of only two discoveries of comparable importance, namely, the difference between electron and muon neutrino and the violation of the CP-invariance (we may add the discovery of resonance "particles" though it occurred somewhat earlier). The recent advances of astronomy will look even more impressive if we add to them some space research achievements (studies of the Moon and the planets).

Different branches of science develop unevenly. As for astronomy, we may say that after the World War II it entered a period of spectacular growth which may be termed the "second astronomical revolution" (the first revolution was initiated by Galileo who was the first to use a telescope). I wrote about that on many occasions [79] (as well as many other authors) but I feel that the astrophysical part of this book should be concluded by some remarks of a general nature.

First, the astronomical advances are, undoubtedly, due to the development of physics and space technology which made it possible to design and use the equipment of fantastic sensitivity* and in some cases to launch it beyond the Earth's atmosphere. Second, the revolution may be said to consist in optical astronomy turning into the all-wave astronomy. Third, the latest astronomical advances, remarkable as they are, have not yet necessitated any revision of the fundamentals of physics as they may be explained within the framework of the existing physical concepts and laws.

Not everybody would agree to the second and third conclusions. Some would say that the most remarkable feature of the current stage in the development of astro-

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* This may be illustrated by an experience I had some years ago which impressed me in spite of the fact that by that time I had worked in radioastronomy for many years. This was at an exhibition organized by the radioastronomical observatory near Cambridge (Britain). There was a table at the exhibition with small sheets of plain paper lying on it and visitors were encouraged to take the sheets. A visitor took a sheet, turned in over and saw the following inscription: "Taking this sheet from the table you have spent more energy than all the energy received by all the radiotelescopes in the world throughout the whole history of radioastronomy".

The flux (or, more exactly, the spectral flux density) is usually measured in units of 10^{-23} erg cm^{-2}s^{-1} Hz$^{-1} = 10^{-26}$ W/m^{-2}Hz^{-1}. The flux of this density delivers an energy of $3 \cdot 10^4$ ergs $= 3 \cdot 10^{-3}$ J to an area of 1 km$^2 = 10^{10}$ cm^2 a year $= 3 \cdot 10^7$ s in a spectral band with a width of $\Lambda\nu = 10^{10}$ Hz. The existing equipment can detect sources whose radiation flux is one unit (or even weaker by 2 or 3 orders of magnitude). However, the sources under study usually have fluxes about ten times greater; the number of such sources does not exceed a few hundreds. These figures demonstrate the validity of the above example and present a vivid illustration of the amazing sensitivity of the radioastronomical equipment.

nomy is emergence of new concepts or revolution in ideas. However, the latest astronomical discoveries, great as they are, are hardly more significant or far-reaching than the discovery of the expansion of the Universe and estimation of its characteristic dimensions (the time $T \sim 10^{10}$ years, the distance $R \sim cT \sim \sim 10^{28}$ cm). All this was done basically in the twenties. Thus, the second astronomical revolution, if this term is used at all, may mean only that the optical astronomy has transformed into the all-wave astronomy. As for the claims that astronomy has recently given rise to some "new physics", it has been mentioned in § 20 that opinions vary in this respect and certain arguments against this point of view have been discussed (see, in particular, ref. [88]).

What will happen next? What are the further trends in astronomy development? Prediction is a risky business. However, it is better to make mistakes than to keep prudent silence. Hence, I shall venture some predictions which are far from being unorthodox, though.

It may be thought that in the current decade (or at most by 1985) the second astronomical revolution will, in some sense, be completed, the astronomy will master the whole electromagnetic spectrum and all the discoveries that are so to say, within the easy reach, will be made. The following period should be a less turbulent one (we are only talking now about the studies of far-away objects, leaving aside the investigation of planets and the fascinating problem of extraterrestrial civilizations [112]). Thus, the pioneering period will end and astrophysics will undergo changes (maybe, temporary) similar in certain respects to those that occur in microphysics (see § 16). However, it should be noted that astrophysics possesses rich potentialities depend-

ing on the development of the neutrino astronomy and the astronomy of gravitational waves.

Finally, let us consider the principal question (it is principal at least for physicists), whether the astronomy can lead to changes in some of the fundamental physical concepts which seem to be desired by some of the astronomers. Among such possible changes we can mention introduction of a scalar field into the relativistic gravitation theory, violation of conservation of the baryon and lepton numbers, variation of the physical constants with time* [113], deviation from the available physical laws at high densities inside or in the vicinity of enormous masses (galactic nuclei, quasars, neutrino stars) and so on.

The search for new fundamental ideas and concepts in the astronomy (including the cosmology) is, of course, of the utmost importance but essentially this problem defies prediction. Thus, the "principal question" stated above remains, in fact, unanswered. I can only note that, speaking for myself, I would not be surprised at all (moreover, I tend to believe in such development)

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* Of especial interest here is a possible time dependence of the gravitational constant G [119a, b]. The state of the Universe is not steady (it expands) and, on the other hand, its dynamics is determined by the gravitational interaction. Therefore, an assumption about time dependence of the gravitational interaction does not seem to be wholly groundless though it is by no means necessary (either for logical treatment or for explaining experimental and observational data). Anyway, only measurements can settle this question. Incidentally, the general relativity theory may be used as a basis for the cosmological studies only if $(1/G) \mid dG/dt \mid < 10^{-11}$ year^{-1}. The derivative dG/dt can be determined (or its upper limit can be estimated) by observing the orbits of the artificial and natural planets and their satellites as well as by using highly sensitive gravimeters [119c].

if a "new physics" were needed in astronomy (and sprung from astronomy) only in the vicinity of classical singularities, i.e. if it were essential only for cosmology and for explaining the final stage of the gravitational collapse.

It may well happen otherwise and the astronomical discoveries may make a wider contribution to the fundamentals of physics. What is stressed here is that it is not necessarily so and no historic associations, general considerations or available knowledge can provide us with an answer to this question.

Concluding Remarks

This book deals with many subjects and problems. It is hardly possible (or necessary) to end it with any specific conclusions. I shall make only a few general remarks addressed to the unsophisticated or the so-called pedestrian readers.

The history of science abounds with wrong predictions. A good illustration of this is the speech of Rutherford, the discoverer of the atomic nuclei and nuclear fission, made at the annual meeting of the British Association for the Advancement of Science on September 11, 1933. Rutherford said in his speech (which was widely publicized in newspapers) that "all talk of using nuclear energy was moonshine". His opinion was shared by others and he was, in a sense, quite right because in 1933, indeed, there was no clear way for using nuclear energy. However, in barely five years the things changed radically as the uranium fission was discovered and in nine years (in 1942) the first nuclear pile was built.

This and similar examples may give rise to a profound distrust of any planning or prognostication in science. Specifically, doubts are raised about the very notion of discussing some "especially important" but unsolved problems. In this connection, I would like to stress the following. In many cases (or even as a rule) it is, indeed, impossible to plan or prognosticate the

course of fundamental scientific studies in the sense of specifying time limits. For instance, when will the high-temperature superconductivity be discovered? As follows from § 2, I would answer this question in the following manner: maybe the high-temperature superconductivity has been already discovered in some laboratory (but the discovery has not been reported yet); maybe it will be discovered tomorrow, and maybe such phenomenon cannot exist at all and, hence, will never be discovered. That is to say, no time limit can be set for making a discovery or solving a scientific problem and it is better not to talk about dates here.

But we can talk, of course, about the problem itself! For instance, when the nuclear mass defect was discovered it became clear that vast energy was stored in nuclei. This led to the emergence of the problem of nuclear (atomic) energy in the twenties. Naturally, this problem would find its way to any reasonably compiled list of the "most important physical problems" until the early forties when it was solved after 20 years of work. Generally speaking, the concept of the problem seems to be rather stable.

Thus, there is nothing to be said against planning and prognostication of fundamental research if they imply formulation of current problems, tentative evaluation of their potential importance, etc., rather than fixing of "deadlines" (of course, I do not mean fixing dates for putting into operation scientific apparatus, etc.).

However, any selection of "especially interesting and important problems" tends to be arbitrary. Different "important" problems are, clearly, not equivalent and hardly comparable and their selection varies with time. For instance, if there could be produced at least

one high-temperature superconductor with room criti-
cal temperature and with the full knowledge of the
factors involved, the problem of high-temperature
superconductivity would cease to be important in the
sense we use in this book. The problem would cease
to exist also if it is proved to be insoluble, for instance,
if it can be shown that superconductors with high
critical temperature or long-lived superheavy nuclei
do not exist.

To avoid misunderstanding, it should be stressed
that the problems not named here must be studied, too.
Letting alone the fact that there are no well-defined
boundaries between various physical and technological
research and development problems, one has only to
remember the way in which new "especially important"
problems tend to emerge. Mostly, they originate, as
discoveries do, from the "grassroots" problems as ge-
niuses are born to normal parents. Hardly anybody
could say in the thirties that studies of gamma-ray
luminescence of liquids were especially important and
yet these studies resulted in the discovery of the Ce-
renkov effect. The same may be said about the Möss-
bauer effect, some recent astronomical discoveries (for
instance, the discovery of pulsars), etc.

In other words, many remarkable discoveries and
advances in science were unforseen and unexpected.

Thus, though it is only natural and reasonable to
concentrate efforts on solving especially important
problems formulated by now, other areas of research
should not be ignored and physics and astrophysics
should develop harmoniously as a whole*.

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* Such development is, of course, very difficult to achieve. In
the USA special commissions (physical and astronomical) have
been established by the National Academy of Sciences to analyze

Finally, a few remarks about the "human factor".

The natural sciences study the nature, the numerous natural objects and processes and the laws governing them. Hence, the science, for instance physics, is by no means affected by the cognoscitive subjects, using the philosophers' parlance. But it is just these subjects, men and women, who work in science and there are more than one million of them in the USSR alone. Some types of research have industrial and economic links, involve considerable expenditures and so on. Therefore, the development of science is linked to politics, economics, technology, sociology and psychology and has numerous humanitarian implications. These interdependences are very complex, hard to analyze and poorly understood. Therefore, they receive relatively little attention, at least in the scientific publications. The literary style in science was greatly affected by the desire (which is largely justifiable and natural) to get rid of everything that is not essential to the matter at hand. A typical illustration of that, though not a very important one, is the elimination of the personal pronouns, particularly I, from the scientific literature. For instance, I just could not write I in a purely scientific paper and it was sometimes with an effort that I wrote so in this book which is, after all, a popular account of my personal views.

But the significance of the human factor is not diminished if we try to ignore it and concentrate on the science itself.

An invisible tape-recorder placed in a laboratory would reveal, I am sure, that not more than half the

this problem. The reports of these commissions [114, 115] contain interesting data (see also [116] on the development prospects of physics in the USA).

time scientists or students talk about science in itself.
They discuss also which scientific fields have more poten-
tialities, what is especilly important, interesting or
attractive (or even profitable or convenient), what
specialization to choose, etc. All these questions are
quite natural.

I came to the decision to write this book having in
mind that there was so much interesting in various
areas of physics and astrophysics and yet many bud-
ding physicists and students were not aware of this
and could hardly find out for themselves. So I decided
to do something constructive in this way, to describe
briefly some urgent problems of physics and astro-
physics. Then complications arose. It was unclear how
to select problems and what standards to use and, final-
ly, whom the book should be addressed to. These diffi-
cult questions have been discussed in Preface and In-
troduction to the book and now when I end this edi-
tion which is a product of several revisions I still
cannot furnish a clear answer to them. This is the
reason for numerous reservations in the book as, above
all, I tried to avoid misunderstanding or wrong con-
clusions.

The most erroneous conclusion would be to suspect
that the author tries to lecture and to impose his opi-
nions about what is "important and interesting" and
what is not. On the contrary, I believe that such a sen-
sitive question is bound to produce controversies and
differences in opinions. The consensus contributing
to the development of science can be reached only
in a constructive debate where the arguments and
counterarguments are freely discussed and compared
and the truth is, if not arrived at, then at least appro-
ximated. It should be added, though, that there are

all kinds of debates and some people tend to regard their scientific opponents as enemies who have to be insulted, humiliated and, if possible, destroyed (unfortunately, such attitudes are not unusual). What I call for is a debate on the development of science going on in the atmosphere of tolerance and goodwill. Particularly, I would like to urge my colleagues, physicists and astrophysicists, to write more often on the general problems of science development. Apart from other benefits, this would allow the reading public to get acquainted with different views and to make really well-founded conclusions.

Addendum

The Preface to this edition notes that only small changes have been made in the book. A few ambiguities have been eliminated and some remarks and references have been added. Meanwhile, about 5 years have elapsed since the manuscript of the book was written so that a natural question is what the author would do if he decided to rewrite the book completely or, at least, make all the necessary changes. The question, essentially, is what new developments have occurred in physics and astrophysics in terms of the selection criteria of the book. Some relevant notes are given below.

1. As a result of the energy crisis controlled thermonuclear fusion now draws even more attention than before. Incidentally, it was already a few years back that hopes had strengthened for developing a thermonuclear reactor and, possibly, even various types of such a reactor. This has given rise to a kind of a "thermonuclear boom" evidenced by increasing expenditure, construction of new machines and development of new designs, even for pilot industrial reactors. As far as I can see, however, in the last 5 years there were no fundamentally new events in the physics of thermonuclear fusion (see § 1).

The same can be said about metallic hydrogen (§ 3), phase transitions (§ 5), matter in ultrahigh magnetic fields (§ 6), development of rasers, grasers and super-

powerful lasers (§ 7), studies of very large molecules, liquid crystals and surface phenomena (§ 8) and production or search for superheavy elements (§ 9). Of course, some advances have been made in all the above fields. In my opinion the most impressive qualitatively new results have been obtained in the studies of the superfluid phases A, A_I and B in the liquid ^3He (see [33e]). As for the superheavy elements, of course, this field had its share of sensations when in the middle of 1976 discovery of very stable elements with $Z = 116$, 126, etc. was reported. But this study proved to be erroneous even though it had been carried out by skilled physicists and the report had been published in the highly respectable journal *Physical Review Letters.* I mention this fact here only to emphasize that only those who do not work can be sure of not making mistakes. Moreover, there are no justifications for postponing publication of the sensational results until they have been verified. The progress of science is better served by publication of wrong results (which thus can be speedily verified by other workers) than by withholding publication of important results until their confirmation. Of course, I do not propose to lower the quality requirements and to publish the "raw" data, I just think it unjustifiable to put forward excessive requirements and to critizize too severely those who have made mistakes (it should be borne in mind that an author who has published wrong results is strongly punished just by the revelation of his mistakes).

I have not mentioned above the high-temperature superconductivity (§ 2) and the metallic exciton liquid in semiconductors (§ 4). In the latter field the following advances have been made. The metallic exciton (electron-hole) liquid has been obtained and some of

its features have been studied [20]. However, this work concerns mostly germanium and some options mentioned in § 4 have not yet been investigated. Therefore it is, clearly, too early to remove the problem of the exciton liquid in semiconductors from our list of "especially important and interesting problems". Incidentally, the other problems of solid-state physics which, apparently, should be added to the list are the problems of quantum crystals (specifically, the crystals of ^4He and ^3He), some "magnetic glasses" and the metal-insulator transitions (including the superconductor-insulator transitions).

As for the high-temperature superconductivity, as recently as two months ago (mid-December 1977) I would have written that new developments are few and hopes tend to be low. Actually, I believe that there are no and never were any substantial grounds for pessimism in this field. A detailed analysis of the problem [12b] does not rule out a possibility of creating superconductors with the critical temperature T_c as high as 100-300 K. Of course, there is no guarantee of success since to do this especially favourable conditions and parameters have to be selected and the theory does not provide detailed guidelines for going this. Meanwhile, a high level of interest on the side of not only the lone enthusiasts but of the scientific "establishment", too, can be maintained only by successful results, in this case by development of materials with high critical temperatures. However, high critical temperatures have not been obtained neither for the insulator-metal-insulator "sandwiches", nor for quasi-one-dimensional, nor for lamellar compounds [12]. Moreover, people tend to forget that the studies of these compounds which are interesting and valuable

in many respects* themselves have been considerably promoted by the search for high-temperature superconductors.

The situation has sharply changed owing to the report [14a] about the finding of a very high diamagnetism (superdiamagnetism) in CuCl at the temperatures up to 100-150 K. Unfortunately, the effect is found only under the pressure of a few kilobars or more so that verification is difficult and a possibility of error cannot be readily eliminated. By the publication date of this edition, the fate of the study [14a] will, probably, be already known. If the superdiamagnetism, indeed, has been discovered in CuCl at rather high temperatures, this will open literally a new epoch in the solid-state physics. On the other hand, a discussion of an error, sad as it can be, would be highly instructive. We have carried a wide-ranging discussion of the report [14a] here in Moscow which has clearly shown the lack of any conclusive evidence against the possibility of finding high-temperature superconductivity; at this stage the only way to solve this problem is to carry new experiments and analyze a wide range of materials which sometimes have very complicated structures [12b]. Moreover, new ideas have appeared (more exactly, reappeared) concerning materials of a new type which is yet unknown [14b]. These materials possess superdiamagnetism**, similar to superconduc-

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* I shall mention here only the discovery of metallic conductivity (and superconductivity with $T_c \sim 0.2$-0.3 K) in polymeric sulphur nitride $(SN)_x$ which contains no metal atoms.
** The magnetic field cannot penetrate into the ideal super-

tors, but are at the same time insulators (more exactly, they have no conductivity or a low conductivity when connected to a normal metallic circuit). In principle, there are some other possibilities which I cannot describe here in detail, the more so as the formulation of the problem as a whole is far from being clear. Anyway, the high-temperature superconductivity (and the related issues) remains, in my opinion, to be the problem No. 1 of the solid-state physics.

2. I have attempted to reflect considerable advances made in astrophysics in the last 5 years in footnotes and changes in this edition. Doubts about the complete validity of the general relativity theory have lost, somehow, any foundation, at least, for the weak gravitational fields (see § 17; I shall take the liberty of noting that I never believed that this criticism of the GRT had any foundation). Some other "non-orthodox" hypotheses and suggestions have also melted into thin air, namely, the problems of the "missing mass" and "creation of matter" in galactic nuclei (§ 20), the report about the reception of powerful gravitational radiation (§ 18) and some other. Unfortunately, there were no essentially new developments in neutrino astronomy (§ 23) apart from the potentially promising use of the gallium detectors of the solar neutrinos (that is, the isotope ^{71}Ga which transforms into ^{71}Ge upon capture of a neutrino with the energy over

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conductor. Formally, this property can be described by saying that the magnetic susceptibility of superconductors is $\chi = -\dfrac{1}{4\pi}$. The term *superdiamagnetic* is applied to those materials for which $\chi \sim -\dfrac{1}{4\pi}$ or, say, $\chi \sim -\left(\dfrac{0.01}{4\pi} - \dfrac{0.1}{4\pi}\right)$ while for normal diamagnetics $\chi \sim -(10^{-4}\text{-}10^{-6})$.

0.2 MeV; such neutrinos comprise the bulk of the neutrino flux emitted by the Sun).

Definite advances have been made in solving the old enough problem of the origin of cosmic rays (see § 22); of particular importance here is the verification of the halo models [104d]. In the observational work the most successful results in this period have been obtained by the X-ray and gamma-astronomy [108, 109, 118]. Apart from many other results, the discovery of the X-ray bursts should be specially mentioned here (the first reports appeared in 1975 and the latest results available to me are reported in ref. [118f]). The studies of pulsars go on. As it became clear very soon after the discovery of pulsars in 1967-1968 the pulsars are, undoubtedly, magnetized neutron stars but the mechanism of radiation of pulsars and the structure of their magnetosphere are not sufficiently known yet [92]. Meanwhile, we still have not understood the nature of quasars whose discovery can be dated (though somewhat arbitrarily) to 1963. More exactly, it is not clear what are the cores of quasars and galactic nuclei (see § 20 and ref. [86d]). Other issues can be discussed here but I shall mention only the most important, though yet a purely theoretical, achievement. I mean the significant progress in the physics of the "black holes" and, specifically, the result that these "holes", owing to the quantum effects, continuously emit electromagnetic waves and, in principle, all the other particles, too [85f, g]. True, this effect is significant only for the "black holes" with small masses (a "black hole" with the mass $M \sim 10^{15}$ g needs for evaporation just the time of the order of 10^{10} years corresponding to the "age" of the Universe and therefore the effect of evaporation—emission of particles—can, appa-

rently, be observed only for the "holes" with the mass $M \lesssim 10^{16}$ g). But just such "holes" could be formed at the early stages of evolution of the Universe. Various questions can be formulated here. One question concerns the fate of the evaporating "black holes"— whether they will disappear altogether (thus violating the conservation of baryon charge) or whether their evaporation will be stopped at the quantum stage when their mass $M_g \sim \rho_g l_g^3 \sim \frac{\hbar}{c l_g} \sim 10^{-5}$ g? On the other hand, as discussed in §§ 12 and 19, if there existed the fundamental length $l_0 \gg l_g = \sqrt{G\hbar/c^3} \sim \sim 10^{-33}$ cm, the situation would be sharply different [65d]. For instance, it may be suggested that the role of M_g would be played by the mass equal to or even considerably higher than $M_0 \sim l_0 c^2/G$ which for $l_0 \sim 10^{-17}$ cm (this value is taken just as an illustration) amounts already to $M_0 \sim 10^{11}$ g $\sim 10^{16} \; M_g$.

The following points seem to be relevant, too. For massive enough "black holes" ($M \gg 10^{15}$ g) the quantum evaporation does not play any role but that does not mean that everything is OK. The fact is that the very concept of the "black holes" and the analysis of their behaviour as a whole proceed from applying the GRT to the strong gravitational fields (formally, this is the case when $|\varphi|/c^2 \sim 1$ or, more exactly, when the condition of the weakness of the field $|\varphi|/c^2 \ll 1$ is not satisfied). Of course, the profound harmonious logic of the GRT, the fact that it has been verified for the weak fields and analysis of some other gravitational field theories—all demonstrate convincingly the extremely profound character of the Einstein's theory of the gravitational field, that is, the

GRT. But on the other hand, we still have to make a very far extrapolation from the weak fields to the strong fields and, undoubtedly, the question of applicability of the GRT to the strong fields is a worthwhile one. Here we can discuss also the following suggestive argument following from the observational data. It would seem that if the GRT concepts for sufficiently massive "black holes" are valid these "holes" should be formed in fairly large numbers and found in the binary stellar systems, in the galactic nuclei, etc. However, no reliable proofs have been yet obtained for the existence of any "black holes". This problem has been discussed for some years already in connection with the X-ray source Cyg X-1 (see § 21 and ref. [98]) but it still remains open and no new observational suggestions have been made. The suggested existence of the "black holes" in the galactic nuclei and the quasars remains quite unclear, too [65d]. This cannot be an important evidence against the existence of "black holes"; maybe, the "black holes" are very rare (there are some grounds to suggest that their formation involves great difficulties) or maybe they are not so rare but no proper approach has been found for their observation (as it was the case with the neutron stars for more than thirty years). But the problems still remain unsolved and the studies of the "black holes" are among the high-priority tasks of physics and astronomy.

3. I have left the discussion of the microphysical part for the end since it is just this part of the book and the preceding paper (see Preface) that generated controversial comments from the very beginning. It is just psychologically curious why my numerous explanations and quite sincere protestations of love and res-

pect for microphysics* remained unnoticed. Everything was shadowed (I hope only for some people by my, deliberately debatable, remarks on the changing role and position of microphysics in contemporary natural sciences and in the life of the human society, generally. I shall not repeat here what has been said in this respect in § 16. I shall note only that the new brilliant advances in microphysics (which will be discussed below) have not had any effect on my opinion. Of course, this opinion could not be changed by these facts as it concerns not the contemporary microphysics itself but its relations with other branches of physics and other sciences which have not changed at all. Maybe, I should only repeat once more that the position of microphysics now is, in my opinion, equivalent to the position of astrophysics. And no better position there is! I should add that it is, of course, very good when sciences produce various benefits for industry, agriculture, communications, medicine, etc. However, the demands that the science should necessarily produce directly useful results seem to be unjust and unjustified. Firstly, in numerous cases the practical benefits of sciences cannot be directly evident and can be revealed only after many years afterwards. Secondly, for many people the research work is a personal necessity and the lifework just as other people need music, arts or poetry. So why

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* In this respect absolutely no alterations have been made in the text of this edition. I have always believed and, of course, still do, that "Microphysics deals with the most fundamental, essential and therefore most attractive problems of physics. There has been no change in microphysics in this respect" (p. 80). It is impossible to understand why this and other similar remarks cannot be taken as a proof of the lack of any negative feeling towards microphysics.

should scientists be subjected to more stringent demands than musicians? In general, I would like to stress that the suggestion about the decreasing importance of microphysics and astrophysics for the society (compared, say, with macrophysics or biology) should by no means be taken as a kind of a reproach.

Talking about the progress of microphysics in the last 5 years it should be noted that the advances are important, amounting, possibly, to a fundamental breakthrough. Firstly, this concerns the quarks— the concept suggested in 1964. At that time 3 quarks (and 3 antiquarks) were introduced. Now it is suggested that there are, at least, 12 quarks (and 12 antiquarks) 4 types of which are distinguished by their "flavour" and are, so to say, the fundamental ones. But each quark with a given "flavour" can have one of the three "colours". Hence, the total number of quarks and antiquarks amounts to 24 and in some theories to 48. Even protoquarks have made their appearance (on paper, of course). The 48 quarks and antiquarks (and, maybe, a larger number) together with other suggested particles (gluons) make up a not-so-small number compared with the number of the known strongly interacting particles—hadrons. But the most important issue is not the number of the "bricks" comprising matter but which of these bricks are the most fundamental and "primary" ones. In this respect the concept of quarks is a new step in the centuries-old quest of physics towards its basic goal—understanding of the nature of matter. The new and important development of the last years is the transformation of the exotic or speculative concept of quarks into a mature and highly probable theory. This transformation is, primarily, the result of the discovery of the

new particles containing the "charmed" quarks ("charm" is the fourth of the suggested quark "flavours") and, apparently, of even heavier quarks.

It would be difficult to describe the situation in short and it is better to omit detailed explanations. Undoubtedly, if the book were written anew its second part (microphysics) would pay a much greater attention to quarks (a very important property of quarks is that they cannot exist as single isolated particles), to new leptons (there have been, apparently, discovered heavy leptons—the particles belonging to the same family as electrons, μ-mesons and neutrinos) and to development of the unified theory of the weak and electromagnetic interactions and, possibly, the unified theory of all interactions (including the strong and gravitational interactions). Thus, the reader should bear in mind that the book has a limited scope concerning the latest advances in microphysics and its problems, even more so than in the fields of macrophysics and astrophysics. Anyway, the scope of one small book is necessarily limited and the reader should clearly understand it.

4. In conclusion I would like to discuss an issue concerning the rate of development of science. For a long time science has been developing exponentially, that is, the "scientific produce"—the numbers of journals, publications and scientists increased according to the exponential function

$$y_i(t) = y_i(0) \, e^{t/T_i}$$

where $y_i(0)$ is the amount of the "produce" at the moment $t = 0$ and T_i is the time interval during which the amount of the "produce" is increased $e = 2.718...$ times. The time T_i varies somewhat depending on the

science and some circumstances but for wide fields (physics, biology) the typical yearly increment of the above scientific "produce" is 5-7% (7% corresponds to $T_i \simeq 15$ years). I cannot go into the details here, particularly, discuss the imminent (or even already occurring) saturation in the development of science in the most developed countries (I made some remarks on this in the paper [88c]). We shall take into account one of the consequences of the exponential growth of the number of scientists and, namely, the fact that the ages of the overwhelming majority of scientists working now are comparatively low. I do not know the exact figures but, probably, the average age of physicists now is not more than 35-40 years. For a 35-year-old person everything that happened 30 or more years ago seems to be something prehistoric, and many scientific developments which occurred 15 or more years ago, that is, before the start of active professional work, seem to be antique, too (here some qualifying remarks are in order but I hope that my reasoning is clear enough without them). Such situation gives rise to a widely current in the scientific community overestimation of the development rates of science. Specifically, a young person feels that 10, 15 and, of course, 25 years are very long times not only in terms of a human lifetime but for science as well. The latter feeling is not always true, however. Suffice it to remember that the special relativity theory is over 70 years old, the general relativity theory is over 60 years old, the nonrelativistic quantum mechanics was developed over 50 years ago, superconductivity was discovered in 1911 and the cosmic rays were discovered in 1912. And after six decades both superconductivity and cosmic rays attract great attention

and various aspects of their studies are included in our list of the most interesting and important problems of modern physics and astrophysics (§§ 2 and 22). Moreover, the history of these two fields, of which I have a sufficient knowledge, shows that it took 25 or even 45 years to solve some of their problems (for instance, the microscopic mechanism of superconductivity was understood as late as 1957).

What are the conclusions? The only one is that one cannot expect breakthroughs in science every year or even every decade. During the 5 years we have discussed here physics and astrophysics have made, of course, significant advances but no scientific revolutions have happened. What will happen before January the 1st, 2001—the beginning of the XXI century? This day will come in only 23 years. For schoolchildren this is a long time but for persons working for, say, 40 years this time is not so long—just recall what has happened since 1955 (was the physics at the time significantly different from that of today?).

I do not have many chances to see the beginning of the next century and even fewer chances to be able to evaluate the state of science then. But I hope that the majority of the readers of the book will meet the XXI century in the prime of life and I would like them to think then about updating the list of the "most important and interesting problems". I would not be too surprised if a good half of the problems in our list reappear in the list compiled in 2001.

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* The book deals with so many issues that it would be unrealistic to try to provide more or less complete references. Therefore, reference is made, first of all, to review papers (mostly those published in *Uspekhi Fizicheskikh Nauk* (in Russian). This and other Soviet journals are translated in the USA (*Sov. Phys. — Uspekhi*, etc.). Besides, I refer to some original papers I had at hand when preparing the manuscript. This is the only reason for the comparatively high proportion of my own papers in the references.

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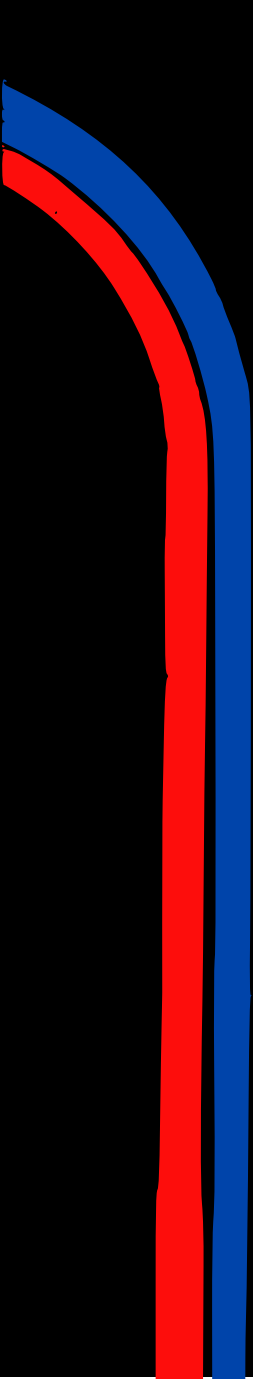
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