

PHYSICS AND ASTROPHYSICS

A Selection of Key Problems

V. L. Ginzburg

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Translator

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Preface to the English Translation

Enormous numbers of books on physics and astronomy—textbooks, monographs, various collections of papers, and popular books—are published all over the world. But, to my astonishment, in recent years I have not seen a book similar to this present one. However, many readers would undoubtedly be interested in a general, albeit brief, review of many key problems of physics and astrophysics. Moreover, it would be desirable to have a choice of such books, since no single author can lay claim to his book being entirely objective and free of faults, to say nothing of a sufficiently detailed knowledge of the gigantic range of material to be covered.

At the same time, I must admit that the lack of other books of this type helps to reduce somewhat my anxiety about the publication of this translation. Unfortunately, there are still reasons for such anxiety and for feelings of dissatisfaction. Indeed, new problems arise continually and almost every week brings new data and new results. Therefore, the effective lifetime of each edition of this book is not long. Preparation of a revised and updated edition does not fully solve all the problems owing to the inertia of the previous editions, so to speak, which is difficult to overcome. In any case, if I wrote the book anew it would be different. I am particularly dissatisfied with the microphysical part of the book. It may be that in times to come the present period will be recognized as being no less significant than the years of the development of quantum mechanics. The gauge fields, the spontaneous breaking of symmetry, the quark model, the unified theories of different interactions—all these are now in the limelight and constitute an integral part of modern physics—but have not been adequately treated in this book. However, I hope that the book is, on the whole, modern and forward looking. Naturally, I have added new material while preparing it for translation. Most additions are placed at the ends of sections and are denoted by a bold asterisk. Besides being convenient, this method helps to identify and emphasize the most recent results.

In conclusion, I would like once more to stress my belief that books of this type are interesting and useful to many readers. So let my colleagues,

particularly those who disagree with me on many issues, write other, better books.

January 1981

Unfortunately, the publication of the translation has been delayed. I have therefore added a few notes reflecting some results which, in my opinion, are important and which have been obtained in the last two and a half years. I also note that, if I were to be writing this book now, I would also include amongst the key problems one concerning solitons, strange attractors and “chaotic” solutions which have been obtained in the analysis of a number of non-linear equations which describe many physical systems.

June 1983

V. L. GINZBURG

Preface to the Third Russian Edition

Five years have passed since the previous edition of this book. Only under exceptional circumstances do profound changes in science occur within such a short period. Such was the period between 1925 and 1930, for instance, when quantum mechanics was created, and largely developed. The last five years have not been exceptional for physics and astronomy. However, much has been done in this period, and of course this book should reflect the current state of the problems with which it deals. In general, irrespective of the success or usefulness of the present book, which the author has no right to judge, there is clearly a marked interest in literature of this type, as evidenced for example by translations of this book into English, French, German, Polish, Slovene and Bulgarian.

Since this is a new edition of the book, but with a similar title, I have been restricted, to a certain extent, by the previous editions. Therefore, I have only made additions and changes which concern the essence of the physical and astronomical problems discussed in the book.

In order to do this, I have reviewed a large number of new papers in a wide variety of fields, and in the process have realized how difficult it was to cover, even superficially, a considerable part of modern physics and astrophysics. In this connection, I should like to stress once more that I have never regarded this book as something outside the scope of popular presentation. To evaluate it as if it were a programme document or a philosophical treatise would mean losing the sense of proportion. Apparently I lost it myself in heatedly answering criticisms which I considered unjustified. I still believe that, with the above reservations, we can identify “the most interesting and important” problems, and we can and must discuss the relative significance of various research fields. I also believe that the author of such a book need not bear in mind the possible views of higher authorities or the special interests of some of his colleagues. On the other hand, this controversy is somewhat “outdated” now and, so as not to annoy my critics, I could smooth over some points and, for instance, write about “certain important and interesting problems” instead of “the especially important and interesting” ones. Thus, if I wrote the book anew it would look different. However, I have not made changes in this direction, and

have preserved all the general discussions and remarks, which are sometimes rather controversial. As I said, this is not the first edition; the author has nothing to lose, and the heated or even controversial character of the discussion can only make the reading more interesting.

In conclusion, I wish to thank all my colleagues whose advice was useful in the preparation of this edition.

June 1979

V. L. GINZBURG

Preface to the Second Russian Edition

Physics has grown and diversified immensely in recent decades; this is demonstrated 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 (or perhaps it would be more correct to say has not yet deprived) physics of a certain unity. I mean here the unity of the fundamentals, the 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, leading undoubtedly to some disunity. This disunity seems to be, to a certain extent, inescapable, but the desire to compensate somehow for its consequences is quite justifiable.

This is particularly significant for young physicists, and, primarily, for students. It is a fact that even the best graduates of the physical (and related) departments 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 versatile knowledge, overnight, and a university training can hardly achieve these goals.

But sometimes the lack of consistency and even lapses of knowledge are truly astonishing. For instance, a person may know fine modern methods of quantum statistics or quantum field theory but have no understanding of the mechanism of superconductivity, or the nature of ferroelectricity; he may not even have heard of excitons or metallic hydrogen; may be unaware of the concepts of neutron stars, "black holes", gravitational waves, cosmic rays and gamma radiation, neutrino astronomy and so on. I believe that the reasons for this are not human limitations or the lack of time. It would perhaps take less time and effort for a student to get a basic physical "picture without formulae" of all the above and similar subjects (or, at least, with the use of only the simplest formulae and quantitative concepts) than to prepare for a major examination.

The difficulty lies elsewhere—the student does not know with what to get acquainted, and how to do it. It is not enough for certain subjects to be mentioned in some of the numerous university courses or text-books.

Moreover, the very problems that get most attention at physical conferences or in the journals are too novel to find their way into text-books or university curricula.

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 for the improvement or updating of university courses, frequently our goal will never be reached. The most reasonable solution seems to be to deliver regular additional lectures for students, according to a special schedule (8–10 lectures a year), that are not included in any of the established courses. Each lecture should be delivered by an expert in the respective field. These extracurricular lectures should each be 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 had 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 present the current problems in physics as a whole. This task seems to be a difficult and, in a sense, not a gratifying one, as its fulfilment can hardly be successful enough. Anyway, usually nobody gives such lectures. But since I considered such a lecture to be a prerequisite for the success of the above lecture series as a whole, I got down to work on it. The lecture was later delivered on a number of occasions to various audiences. The way it was received demonstrated unambiguously that such lectures are, to say the least, necessary and attractive—and not only for students. The lecture eventually developed into a paper entitled “What problems of physics and astrophysics seem now to be 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 version of that paper; it contains a few new sections and some other alterations. Such alterations were rendered necessary, in particular, by the 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 list of Contents and Introduction.

There are reasons for such a lengthy preface to so small a book. These are that its contents, character and style seem to be somewhat unconventional or at least not self-explanatory. I have addressed my book to budding

^{*} After this English edition had been prepared I published a paper with the same title but subtitled “Ten years after” (*Sov. Phys. Usp.*, **134**, 469, 1981). This paper presents my views on the development of physics and astrophysics in the last decade.

physicists and astronomers; I have stressed that the selection of the "most important and interesting problems" is tentative and subjective in character; I have also noted that any evaluations under such circumstances inevitably become controversial but, at the same time, that I am far from having any bias or pretensions to preach, or to impose my opinions on the readers. Fortunately, as far as I have gathered, the paper has been accepted in just this way by the majority of readers, especially by those to whom my message was addressed. But opposite opinions have also been voiced. Some did not like the very idea of the paper. Others considered it to be intolerably biased, especially against microphysics (I have even been granted the title of the "enemy of nuclear physics"). Still others charged me with a lack of modesty and suchlike sins that they inferred from my attempts to judge what is important and what is not, as well as from too frequent appearances of my name in the Bibliography which plays a purely auxiliary role in the paper. It would be inappropriate to answer all these allegations and reproaches here, all the more so since they, unfortunately, have not been published anywhere. But they are worth mentioning in order to caution readers against the possible dangers to which they might be exposed, and thus to stimulate a critical approach to their reading. 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 was prepared.

Introduction

Physics and astrophysics are nowadays concerned with an enormous number and variety of problems. Attempts at solving these problems are usually worthwhile, as they allow physicists if not to uncover the secrets of nature, then at least to gain new knowledge. None of these problems may rightly be thought of as devoid of interest or importance. However, there does exist a hierarchy of problems which is reflected in all scientific (and sometimes not only scientific) activities. “Especially important” physical problems are often identified according to the potential effect they may have on either technology or the economy, a special fascination of the problem, or its fundamental character. Sometimes the choice is due to fashion, or to other obscure or hazardous factors. We shall, of course, avoid discussion of the latter type.

This is not the first time that a list of “most important problems” has been compiled and commented upon. Conferences are often convened or special commissions set up to do this. These may produce bulky reports. I do not intend to generalize, but must say that I have never seen anyone reading such reports on “most important problems” with any great interest. It seems that specialists do not really need them, and they do not attract a wider reading public. Of course, such documents may prove to be necessary for the planning and financing of the development of science.

And yet, physicists and astronomers—especially young ones—tend to ask 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 are interested in this question, I have attempted to answer it in this small book. The book is not a product of a commission’s deliberations and not even a result of special investigations. It is, rather, the author’s personal view. This, at least, makes it possible to avoid the dry and bare style of the more official documents.

The problems that seem to me now to be especially important and interesting are listed below. At the same time, I do not attempt to justify my selection criteria. Everyone has a right to their own views, and should not feel obliged to co-ordinate them with those of anyone else unless he or she declares his or her views to be authorized or superior to others. I attempt

nothing of the kind and, of course, make no organizational suggestions; in order to stress this personal approach I have not even tried to avoid using personal pronouns as is customary in scientific papers.

It would be interesting and perhaps instructive to compare the lists of the "most important problems of physics and astrophysics" compiled by a number of people. Unfortunately no such poll of scientists' opinions has ever been conducted, as far as I know. Therefore I can only suggest that the majority of such lists would have many elements in common, provided that the following difficult requirement is met: that a consensus is reached in defining the "physical problem" concept as distinct from, say, fields, trends, or objects of physical studies. By a problem I mean 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 the possibility of creating some new substance with unusual properties, say a high-temperature superconductor, establishing the limits of applicability of a theory—for instance, the general theory of relativity—or throwing light on something really unknown; for example, the mechanism for the breaking of combined parity in the decay of K-mesons. This is precisely the reason why, in this book, I practically ignore quantum electronics (including the majority of laser applications) as well as many problems in the physics of semiconductors (including miniaturization of circuits and devices), non-linear optics and holography and some other interesting trends in the development of modern optics, the problems of computer technology (including the development of novel types of computers) and many other problems.

These issues are, undoubtedly, very important and have many technological and physical implications. However, they do not involve any essential "physical problem" or any basic "uncertainty" concerning the underlying physics. There was, for example, such an uncertainty prior to the development of the first laser, even though the principles which were used later for laser design had been known. Increasing the power, or changing other parameters, of a laser or any other device may be a necessary, difficult and commendable task but is, of course, qualitatively different from developing a device or a machine on the basis of new principles. At the same time, this is a fairly typical example for illustrating the arbitrary nature of the boundary between the basic research problems and the technological problems in physics. For instance, increasing the laser power by many orders of magnitude (although a currently important problem) cannot be classified as a purely technological or some kind of "non-basic" task. The same may be said about the development of X-ray "lasers" and grasers, the laser analogues of X-rays and gamma rays. X-ray lasers and grasers have not yet been

developed and it is still unclear as to how this will be done, or whether such development is feasible: 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 are ripe enough for solving; not all prizes seem tempting and there exists in fact a hierarchy of problems.

At the same time, we cannot, of course, deal only with individual problems, however important, and ignore the wide variety of other tasks and problems which failed to make the grade of "important and interesting problems". Moreover, these problems can prove to be both very difficult and very interesting, at least, for those who work on them. I can illustrate this argument with problems from the theory of radiation from sources travelling through a medium (Cherenkov radiation, transition radiation, transition scattering, etc.). I am greatly attached to and fascinated by this field as I have been working in it throughout my research career⁽¹⁾. But one cannot help seeing that such problems in electrodynamics involve no real mysteries and in this respect they differ substantially from, say, the problem of high-temperature superconductivity or the problem of quarks and their confinement in the bound state. It is natural, therefore, that this book deals neither with transition radiation nor with a number of other problems in which I am, or have been, interested. Thus, though this selection of the "important and interesting" problems still is, in a sense, arbitrary and subjective, it is by no means based on the principle that the important and interesting problems are primarily those on which the author is working (I do not think that this remark is superfluous since one fairly often meets people who use precisely this selection principle).

It has been suggested above that a "poll of scientific opinion", if attempted, would show a large measure of agreement on the selection of current "especially important and interesting problems". However, significant disagreements would also be inevitable, especially as far as priorities in allocation of resources and concentration of efforts are concerned. This is clear, for instance, from the literature⁽²⁻⁵⁾.

The question of resources and priorities is, however, linked to a variety of factors lying outside the scope of purely scientific problems. For instance, the construction of mammoth accelerators is undoubtedly of great scientific interest, but what is argued is whether the expenditure involved produces results that may justify the necessary curtailment of research in other areas. We shall ignore this aspect of the discussion and concern ourselves only with scientific issues. However, even with this "simplification" and restriction, opinions may diverge sharply. For instance, the most important problems of solid state physics are listed here: high-temperature superconductivity,

the creation of metallic hydrogen and some other materials with unusual properties, metallic exciton liquid in semiconductors, surface effects and the theory of critical phenomena (in particular, the theory of second-order phase transitions). An article⁽⁵⁾ entitled "The most basic unsolved problem in solid state physics" states that this problem is to explain the empirical formula for the heat of formation of some crystals from other substances. With some effort, I found an interest in this problem but I failed completely to understand why the problem was thought to be the "most basic" one—and, what is more, I greatly doubt it. What is the conclusion? There seems to be only one possibility: no authoritative list of the most important problems can be suggested; and there is no need for one. But it is both necessary and useful to evaluate what is important, and what is not, to argue about it, and to be bold in putting forward suggestions and defending them (but not to impose one's own views). This is precisely the spirit in which this book is written.

Thus, the subjective and controversial character of this book is quite apparent and readers have been warned (although, of course, such warnings are rarely heeded). It is only left to note that the division of the book into three parts—Macrophysics, Microphysics, and Astrophysics—is quite arbitrary. For instance, we discuss superheavy nuclei under the heading of macrophysics, though they may be said to constitute a microphysical problem. Furthermore, the problems of the general theory of relativity are treated in the astrophysical part, rather than among macrophysical problems. The only reason for this is that this 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 difference between macrophysics and microphysics).

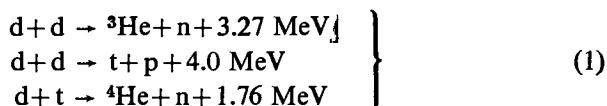
Finally, it should be noted that the book practically ignores biophysics, let alone other less important research areas related to physics and astrophysics. However, it is precisely the co-operation between physics and biology and the application of physical methods and concepts that have proved to be especially fruitful and significant in the development of biology, medicine, agricultural sciences, and so on. It would be a gross error for physicists to avoid "biologically biased" problems on the grounds of their not being "physical" (this has been convincingly argued elsewhere⁽²⁾). Moreover, it is conceivable that this co-operation with biology, and attempts to solve biological problems, will stimulate the development of physics proper, just as physics was, and still is, a source of inspiration and new ideas for many mathematicians. Thus, even though this book does not pay due attention to the links between physics and the biological sciences, this does not reflect an underestimation on my part of their importance, but rather my inadequate knowledge of biophysics and biology in general and, also, the necessarily limited scope of this book.

I.

Macrophysics

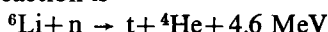
1. Controlled thermonuclear fusion

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

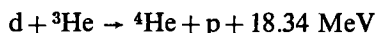


(here d and t are the nuclei of deuterium and tritium, p is the proton, n is the neutron).

Another important reaction is



since it gives rise to tritium, which does not occur naturally. Some other reactions may also prove to be useful; for example, the reaction



It can scarcely be questioned that nuclear fusion energy will be used in some way or other: one has only to mention the "obvious" possibility of useful underground explosions. On the other hand, controlled thermonuclear fusion has been attracting great attention for 30 years although a thermonuclear energy "yield" exceeding the thermal plasma energy has still not been obtained. However, installations are now being built, and which are to be tested during 1982–1985, as prototypes for the real thermonuclear reactor. According to some predictions, a commercial reactor will have been built by the end of this century or the beginning of the next.

In order to make the thermonuclear energy yield higher than the energy consumed for plasma heating, the condition $n\tau > A$ must be satisfied, where n is the electron concentration* in the plasma at a temperature

*Of course, plasma is fully ionized at the high temperatures needed for the reactor operation ($T \gtrsim 10^8$ K) and the concentration of electrons is approximately equal to the concentration of deuterium and tritium ions. The equality is approximate since plasma always contains impurities—carbon, oxygen, etc. (see, for example, reviews⁹).

$T \sim 10^8$ K and τ is the characteristic time of plasma confinement (for instance, it may be the time during which the energy lost by plasma is of the order of its thermal energy). The constant A characterizes the nuclear fuel (and the concentration of the impurity atoms). For pure deuterium $A \sim 10^{16}$ cm⁻³ s and for a mixture of 50% deuterium and 50% tritium $A \sim 2 \times 10^{14}$ cm⁻³ s (A can be decreased by a factor of almost 10 by using the neutrons produced during thermonuclear reaction for fission of uranium). Thus, in order to make a reactor function (the energy it produces must be greater than the energy needed to establish and maintain high plasma temperatures) in the "pure" reactor, that is one which does not contain fissionable materials (uranium, etc.), we need to satisfy the following condition

$$n\tau > 10^{14} \text{ cm}^{-3} \text{ s} \quad (2)$$

The physical meaning of this condition (2), known as the Lawson criterion, is clear enough—the longer the time of reaction, the lower the fusion reaction rate—it is proportional to n^2 .

Magnetic confinement of plasma might appear to be the simplest approach to the plasma reactor design. Among reactors of this type the most well known and popular are the toroidal magnetic traps—tokamaks. In 1979 a record value of $n\tau = 3 \times 10^{13}$ cm⁻³ s was obtained in the MIT tokamak. This reactor was relatively small, but had a strong magnetic field of up to 90 kOe. The plasma temperature was about 10^7 K and the plasma concentration at the centre was up to 10^{15} cm⁻³. In the T-10 tokamak (built in the Kurchatov Atomic Energy Institute in Moscow) the energy lifetime is about 0.06 s and the ion temperature is about 1.2×10^7 K. Similar values have been obtained with the PLT tokamak, the largest in the USA. A plasma temperature of 6×10^7 K was obtained in this machine in 1978. Construction of the test reactor tokamak FTTR using a deuterium-tritium mixture was started in 1977 in the USA. Other machines to be launched soon are the European tokamak in Britain, the DT-60 tokamak in Japan, and the T-10M tokamak in the USSR. The plasma volume in such tokamaks is over 100 m³. They will, probably, make it possible to reach the value of $n\tau \sim 10^{14}$ cm⁻³ s before 1986. The cost of the FTTR machine is much greater than 200 million dollars. The next step is the development of a power reactor with the circulation of tritium, which should produce thermal energy. The development costs at this stage will be very high and discussions are now under way to review the feasibility of an international project for the development of the power reactor tokamak.

The magnetic field of the fusion reactor should be produced by superconducting coils; otherwise a favourable energy balance cannot be expected. The recently launched tokamak T-7 has superconducting coils, and the future tokamak T-15M will also have them. But many of the physical and technological problems of reactor operation have not yet been solved. They include the problem of the durability of the first wall of the reactor which is irradiated by a high-intensity neutron flux. The problem of plasma heating has also not been solved. The fact is that ohmic heating by itself is not sufficient to obtain the required plasma temperature. Work is under way to test the techniques of plasma heating by beams of neutrals (deuterium atoms with energy 20–100 keV) or microwaves. Moreover, we have an inadequate knowledge of the behaviour of the impurity atoms in tokamaks and of the causes of the high electronic heat conductivity.

Significant advances have been made with open-ended magnetic traps using magnetic mirrors. The plasma parameters in these can be as high as $T \sim 10^8$ K and $n \sim 10^{14} \text{ cm}^{-3}$. The lifetime τ in such systems, however, is as low as 0.001 s, making for a low value of $n\tau \sim 10^{11} \text{ cm}^{-3} \text{ s}$. The reason for this is that in the magnetic traps even one collision of one ion with another removes an ion from the system. Perhaps plasma confinement in the traps will be improved by modification of the magnetic mirrors at the ends.

The above difficulties, which can prove even greater in real systems, justify attempts to devise other approaches to the problem. Therefore, apart from tokamaks and magnetic traps, other systems and techniques such as stellerators, high-frequency discharges in plasma, compression of shells thus creating magnetic fields of the order of a million oersteds, etc., are being tested and discussed.

In recent years, considerable attention has been focused on studies of the possibility of inertial confinement fusion. This method involves the use of micro-explosions accompanied by the liberation of energy up to 10^8 J. (For instance, a deuterium–tritium pellet, about a millimetre in size, produces energy of the order of 3×10^8 J in the case of complete fusion. This corresponds to the energy liberated by the explosion of about 50 kg of TNT.) The destructive effect of such an explosion is relatively small since the mass of exploded material is small, and hence the momentum is also small. Since the time of energy loss for the explosion is of the order of 10^{-8} – 10^{-9} s, the heating power should be about 10^{14} W (see below). In principle, such a high heating power can be obtained either with a laser beam, with an electron beam, or with a beam of ions. Accordingly, the fusion systems discussed are known as the laser, electron, and ion beam systems. Of course, the mechanisms of absorption of electrons, ions and laser radiation by the target (the fusion fuel) are different, but if we ignore

this difference we can readily see the similarity between the above methods. Indeed, whether we use laser radiation, electrons or ion beams to heat plasma, we have to irradiate as homogeneously as possible, solid spherical pellets of hydrogen (or, more exactly, deuterium or a deuterium-tritium mixture), at an initial concentration of the nuclei of $n \sim 5 \times 10^{22} \text{ cm}^{-3}$. This is the concentration of nuclei in solid hydrogen at atmospheric pressure. The nuclear fuel is sheathed with a number of shells known as pushers, rammers, rammer pushers and ablators. When the outer shell (the ablator) evaporates, it produces a pressure of up to 10^{12} atm , resulting in a compression of the nuclear fuel by a factor of 1000 or more. Of course, the structure of the shells and of the target pellet is chosen to provide the highest degree of compression of the fuel. The most important requirement is that the alpha particles produced in the fuel be retained in the target and maintain the reaction. It should be borne in mind here that the mean free path of the particles decreases proportionally with increasing fuel density, while the rate of decrease of the pellet radius is considerably lower (proportional to $n^{1/3}$).

The main problem for inertial confinement fusion systems lies in obtaining a large ratio, Q , between the liberated fusion energy and the energy of the light, electron or ion beam fed into the pellet. As estimated, Q may be as high as 60–70; then, in order to obtain a positive energy yield in the system, the laser efficiency should be as high as 10–20%. The efficiency of currently available lasers, which produce nanosecond pulses, is less than 1%.

Another important requirement is highly durable laser materials. It has been estimated that the laser glass must withstand 10^8 pulses before failing, but the lifetime of available materials is shorter by a factor of 10^4 . Of course we can attempt to continue the reaction in the pellet, and thus not at the expense of further laser heating, by means of self-maintenance (that is, by further heating with alpha particles). Q values of a few hundreds can, apparently, be obtained in this way, and the laser efficiency required can thus be lower. But this approach also has a number of difficulties related to the development of instabilities in the shells, the generation of fast electrons, and so on. Nevertheless, scientists hope to carry out a demonstration experiment soon. (This demonstration involves a fusion reaction with $Q = 1$, so that the fusion energy yield is equal to the energy consumed in heating the fuel.) The large-scale laser fusion installation Shiva (Livermore, USA) started operation in 1977. In the Shiva installation 20 laser beams feed the target about 10 kJ of energy. The first experiment with the Shiva installation was performed in 1978.

Laser fusion installations are being built and designed in the USSR at the Lebedev Physical Institute in Moscow (Delfin, UMI-35), and in other

countries. The fusion installations Angara-5 (Kurchatov Atomic Energy Institute, USSR) and EBFR (Sandia Laboratory, USA) will use electron beams. Work has been started on the design of fusion installations using ion beams. The expenditure for fusion projects in the USA was 500 million dollars in 1979.

Enormous difficulties still remain in the development of fusion reactors with magnetic plasma confinement, or inertial confinement reactors. Nevertheless, at present, in contrast to the fairly recent past, the general feeling is one of optimism, and it seems to be basically possible to develop some kind of fusion reactor. But what type or types of reactors it will be possible to build, when this will be done, and what difficulties remain to be overcome—the answers to all these questions are by no means clear. Moreover, the difficulties involved are so significant that they cannot be regarded as purely technological. Therefore, the development of fusion reactors should be classified as one of the most important physical problems. Also, there seems to be a clear need for competition between the various approaches to the problem of controlled thermonuclear fusion (by this I mean fair competition, not rivalry).

Incidentally, the following general principle is clearly exemplified by the problem of controlled thermonuclear fusion: practically no large-scale physical problem stands apart from all others, but will be closely related to a variety of different branches or fields of physics. Therefore, the especially strenuous efforts made in solving a given problem may bear fruit in a more general sense—they may stimulate new studies, give rise to novel methods and approaches, and so on. For instance, plasmas had attracted considerable scientific interest even before the early 1950s when the problem of controlled thermonuclear fusion emerged. But one can hardly overestimate the importance of the results of plasma physics obtained in this field for gas, solid state and space plasmas.

*Looking through the papers published during the last four Years, I could find no dramatic news of controlled thermonuclear fusion. Tokamaks are still favourite, but the interest in stellators has again increased (they differ from tokamaks in the additional coils that produce the azimuthal magnetic field). The work on open-ended magnetic traps continued in the hope of developing improved magnetic mirrors. It can hardly be predicted that the open-ended systems (which are the simplest and most convenient in some respects) will never compete with toroidal systems. Naturally, the work on the theoretical and practical aspects of inertial confinement systems is also continuing, with attention being focused on heating the fuel with laser or ion beams.

2. High-temperature superconductivity

The phenomenon of superconductivity was discovered in 1911, and for many years it remained not only unexplained (perhaps the most puzzling phenomenon in macrophysics) but also useless practically. This latter fact is largely because, up till now superconductivity has only been observed at low temperatures. For instance, the first superconductor discovered—mercury—has a critical temperature T_c of 4.15 K. One alloy of Nb, Al and Ge was found fairly recently to have one of the highest T_c values, about 21 K. In 1973 the compound Nb_3Ge was found to have $T_c = 23.2$ K (there is a better known superconducting compound, Nb_3Sn , with $T_c = 18.1$ K, which was discovered in 1954).

The use of superconductors becomes especially difficult around the critical temperature (of course, we mean below T_c since, by definition, a metal ceases to be superconducting at higher temperatures). Suffice it to say that in this temperature region the critical magnetic field, H_c , and the critical current, I_c (that is, the field and current that destroy superconductivity) are very small. When T tends to T_c the values H_c and I_c tend to zero. Thus superconductors can be used only when cooled by liquid helium (boiling point at atmospheric pressure $T_b = 4.2$ K) since liquid hydrogen (boiling point $T_b = 20.3$ K) freezes at 14 K and it is generally difficult and inconvenient to use solids for cooling.

As recently as 30 years ago the production of helium was low (it is not sufficient, even now) and liquefaction techniques were 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 so far) was limited to a no lesser extent by the low values of H_c and I_c of materials available at the time (for Hg the critical field is about 400 Oersted (Oe) even at temperatures tending to zero).

However, things changed radically at the turn of the 1960s. Liquid helium is now readily available. Where it is done properly, laboratories do not install liquefiers; instead, they order by phone the required amounts of liquid helium from specialized firms and helium is shipped in large Dewar vessels. The “magnetic and current barrier” has also been overcome; superconducting materials now available make it possible to build magnets with a critical field as high as hundreds of kilo-oersteds (the above-mentioned alloy of Nb, Al and Ge, which has a critical temperature of 21 K, has a critical magnetic field of about 400 kOe; the record observed value of H_c is about 600–700 kOe). It is true that materials currently used have critical magnetic fields and currents too low for a 300–400 kOe magnet to be