over a few generations; it is therefore the time scale that prevents direct experiment. Even the comparatively rapid process of natural selection acting among individuals has been notoriously difficult to demonstrate in nature.

The third objection is, I think, by far the most interesting. It is simply that the hypothesis does not apply to ourselves. No built-in mechanisms appear to curb our own population growth, or adjust our numbers to our resources. If they did so, everything I have said would be evident to every educated child, and I should not be surveying it here. How is this paradox to be explained?

The answer, it seems clear, is that these mechanisms did exist in primitive man and have been lost, almost within historic times. Man in the paleolithic stage, living as a hunter and gatherer, remained in balance with his natural resources just as other animals do under natural conditions. Generation after generation, his numbers underwent little or no change. Population increase was prevented not by physiological control mechanisms of the kind found in many other mammals but only by behavioral ones, taking the form of traditional customs and taboos. All the stone age tribes that survived into modern times diminished their effective birth rate by at least one of three ritual practices—infanticide, abortion, and abstention from intercourse. In a few cases, fertility was apparently impaired by surgery during the initiation ceremonies. In many cases, marriage was long deferred. Mortality of those of more advanced age was often raised through cannibalism, tribal fighting, and human sacrifice.

Gradually, with the spread of the agricultural revolution, which tended to concentrate the population at high densities on fertile soils and led by degrees to the rise of the town, the craftsman, and the merchant, the old customs and taboos must have been forsaken. The means of population control would have been inherited originally from man's subhuman ancestors, and among stone age peoples their real function was probably not even dimly discerned except perhaps by a few individuals of exceptional brilliance and insight. The continually expanding horizons and skills of modern man rendered intrinsic limitation of numbers unnecessary, and for 5,000 or 10,000 years the advanced peoples of the Western world and Asia have increased without appearing to harm the world about them or endanger its productivity. But the underlying principles are the same as they have always been. It becomes obvious at last that we are getting very near the global carrying capacity of our habitat, and that we ought swiftly to impose some new, effective, homeostatic regime before we overwhelm it, and the ax of group selection falls.

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The Nature of Matter: Purposes of High Energy Physics

This group of articles consists of four chapters from a book published in January by Brookhaven National Laboratory. The introduction to the group was written by Luke C. L. Yuan, editor of the volume and a senior physicist at Brookhaven National Laboratory, Upton, New York. H. A. Bethe is professor of physics at Cornell University, Ithaca, New York. Victor F. Weisskopf is director general of CERN (European Organization for Nuclear Research), Geneva. Julian Schwinger is professor of physics at Harvard University, Cambridge, Massachusetts. G. C. Wick is a senior physicist at Brookhaven National Laboratory.

Introduction

This is a résumé of a recently published book in which 30 leading theoretical physicists present a remarkably unanimous plea for support for high energy physics and for the construction of much more powerful particle accelerators. This volume, entitled *Nature of Matter—Purposes of High Energy Physics* includes articles by H. A. Bethe, T. D. Lee, J. S. Schwinger, V. F. Weisskopf, C. N. Yang, and other prominent theorists, both American and foreign. It is intended to present to the general public as well as to the scientific community a collection of diversified views embracing many aspects of high energy physics (often referred to as particle or subnuclear physics) and aiming for a better understanding of the fundamental importance of the subject and its implications in all branches of science.

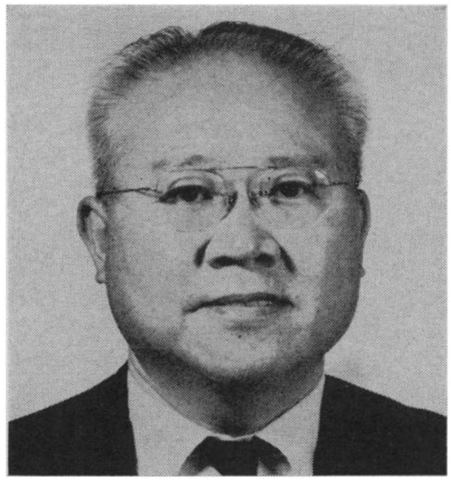
The main point of agreement among these scientists is that more extensive investigations into a considerably higher energy domain than presently available must necessarily be pursued in order to uncover the basic laws of nature. A higher energy accelerator, higher by a factor of the order of 10 to 30 than the 33-billion electron volt Alternating Gradient Synchrotron at Brookhaven, will be needed for the pursuit of these investigations.

J. Robert Oppenheimer, director of the Institute for Advanced Study at Princeton, wrote the foreword of the book, providing a general account of the views presented. Oppenheimer states: "When the first particle accelerators were designed and built, more than three decades ago, they had a clear purpose. Apart from the quanta of light and gravitation, the only particles known to physicists were electrons and protons, and atomic theory explained their interactions. The accelerators were built to study nuclear reactions, to enable protons and other nuclei to approach closely to nuclear targets, despite the fact that both projectile and target were positively charged, and thus repel one another. This program led to the rapid development of nuclear physics. . . . Today, physicists have given serious thought and study to the very large enterprise of building an accelerator in the range of several hundred to one thousand billion volts to explore structure in the range below 10^{-15} cm, in times down to 10^{-25} sec. The general arguments still apply."

The largest accelerator in operation at present is the 33-Bev Alternating Gradient Synchrotron (AGS) completed at Brookhaven in 1960 at a cost of about $30 million. A machine more than twice as large is nearing completion near Moscow. Intensive studies made during the past four years at Bookhaven, the Lawrence Radiation Laboratory at Berkeley, the California Institute of Technology, and CERN (European Organization for Nuclear Research) have shown that it is feasible to design and construct an AGS in the energy region of 200 to 1000 Bev with a useful beam intensity. Proposals are now under consideration for new accelerators in the energy range mentioned above, both in the United States and at the CERN Laboratory.

Oppenheimer continues: "But the experience with the available great accelerators has been so rich, so puzzling, so obviously an unfinished story, that this small volume is concerned with reading from what we know and what we have learned, and above all from what we do not at all understand, what one may ask, what one may hope of the future. This volume contains contributions from some thirty theoretical physicists, all active in the field of high energy physics, to whom collectively the theoretical progress of the last years is largely to be attributed. Each author suggests some questions to which he would like the answer, or for which he surmises one, which require accelerators not now available. . . .

"Let me then turn to a brief description of what we know today, that may be compared to our knowledge of the electron and proton, and of atomic theory, when accelerators were first envisaged. Apart from the quanta of electromagnetism and gravity, we have come to know two sets of particles: the leptons, which as far as we know have no strong interactions at known energies; and all others, which do have such very strong interactions. The leptons themselves are a puzzle. They are the two neutrinos—we do not know why there are two—the mu meson and the familiar electron, all with their antiparticles. . . . We do not understand why the mu meson is some two hundred times heavier than the electron, or the apparent redundancy of the two neutrinos. We do not understand why



Luke C. L. Yuan

at low energies the weak interactions are as weak as they are. We do know that as energies promised by the new accelerators are reached, the description we give of these interactions at low energies will no longer be logically possible, and that either new particles whose existence has been conjectured and sought but not found, or new elements of structure, will be discovered, and that perhaps in that domain weak interactions will have turned strong. Puzzlement about these questions threads its way through most of the theoretical contributions. . . . These papers, for all their variety, clearly reveal one common belief. All authors recognize that we do not understand the nature of matter, the laws that govern it, the language in which it should be described. They are all aware of the innumerable times in the years just past when something unpredicted, unexpected, and rather ununderstood has come out of the great experimental centers. But there is more; they all have the gravest doubt that within the energy range now available there are or will be enough clues to make possible for us a theory of the nature of matter. . . . It is not only, I think, that the techniques of ultrahigh energy physics, experimental, observational, computational, mathematical, will prove of the greatest value throughout the sciences, and indeed often in technology. This I regard as certain to happen. It is not only because of the possibility of an unanticipated discovery of profound importance to technology and to human welfare. This I think not impossible, though there are few serious speculations as to what such a discovery might be. It is also this: the last centuries of science have been marked by an unabating struggle to describe and comprehend the nature of matter, its regularities, its laws, and the language that makes it intelligible. The successes in this struggle, from the Sixteenth Century until our own day, have inspired the whole scientific enterprise, and lighted the world of technology, and the whole of man's life. They have informed the education and the devotion of young people. They have played an ineluctable part in the growth, the health, the spirit, and the nature of science. We are now, despite tempting and brilliant topical successes, deep in the agony of this struggle. This volume attests the conviction of those who are in it that, without further penetration into the realm of the very small, the agony may this time not end in a triumph of human reason. That is what is at stake; that is why this book is written."

Other articles in the book describe the aims of high energy physics and give reasons from various viewpoints why further studies of it are of vital importance.

These articles are divided into three groups. The first group, under the heading "Purposes of high energy physics," contains those articles dealing with the more general and philosophical viewpoints. The second group, under the heading "Some problems of high energy physics," consists of articles which deal with more specific and technical aspects of the subject. The third group contains two articles of a somewhat different character, with more technical and detailed considerations.

Four articles from the first group of the book follow this introduction. I shall also present here a few quotations from the rest of the articles.

For example, G. Feinberg of Columbia writes: "Each human society excels at a small number of the many activities that people carry out. Our own society is pre-eminent at large-scale technological and scientific projects, such as the building of high energy accelerators. It is therefore an expression of the highest spirit of our culture to carry on with the task we have begun, the exploration of nature to all its limits. Indeed, it may well be judged that this spirit is our greatest contribution to the human outlook. High energy physics is clearly one of the subjects on the frontiers of such exploration. If we cut back on it for reasons of budgetary limitation, or political squabbling, I think we will have seriously damaged the best single element we have contributed to human culture. I would be grieved both as an American and as a scientist if we made that choice."

T. D. Lee, also of Columbia, presents "Comments on elementary particle physics," in which he says: "The purpose of science is to seek for that simple set of fundamental principles through which all known facts are understood and new results predicted. Since all matter is composed of the same fundamental units, the ultimate foundation of all natural sciences must be based on the laws governing the behavior of these elementary particles. High energy machines are not just expensive tools for particle physicists; they are, at present, the most effective tools to study these elementary particles in order to uncover the basic principles which underlie the multitudes of all natural phenomena."

In an article entitled "On the need for higher energies," A. Pais of the Rockefeller Institute states: "While during recent years there has been steady progress in the uncovering of new regularities in the particle patterns, it cannot be asserted that in the energy domain now available there are striking phenomena which show the way from particle chemistry to particle physics. It is evident therefore that to succeed in decoding the present growing complexity of phenomena we must go to higher energies. It would in fact be disastrous if we were to stop this pursuit, even though no one can say with any certainty what may be revealed in higher regimes of energy. Nor is this situation without precedent. At the time the Cosmotron at Brookhaven was planned, no one knew of the new particles which were going to be produced by this machine and which drastically changed our thinking about the structure of matter, as mentioned above. . . .

"A great society is ultimately known for the monuments it leaves for later generations. We cannot foretell what detailed results may come from a very high energy machine, which should in fact aim for energies 20 to 30 times as high as the present ones to bring a sufficient new range within reach in the reasonably near future. We can foretell, however, that such a machine, which is on the scale of a national effort, will without question be a source of inspiration for new science and a monument to our days."

According to L. Radicati of the Scuola Normale Superiore in Pisa, in an article entitled "Remarks on strong interactions": "If the study of matter had been confined to chemical phenomena, it would certainly have been hard to guess the elementary laws of quantum electrodynamics which are ultimately responsible for chemical binding. It was the study of atomic physics, in particular the study of the simplest atom, the hydrogen atom, and of the simplest particle, the electron, which made possible the discovery of quantum mechanics and later of quantum electrodynamics. In terms of the energy involved, the step from molecular spectra to the hydrogen spectrum corresponds to an increase by a factor of the order of 10^4."

In an article entitled "Why build accelerators?", S. Weinberg of the University of California says: "Scientists generally show a wise reluctance to debate the importance of their own specialities in public. However, the increasing cost of experiments in high energy nuclear physics has led to some questioning of the necessity of building further accelerators. It may therefore be appropriate at this time for physicists to share their thoughts on the importance of elementary particle physics to the whole of science.

"The first point is clearly one which the lay and scientific public must ultimately accept on the authority of the particle physicists. However, it may be useful to point out one instance of the value of large accelerators; this example is certainly not the most important that could be found, but it illustrates nicely how the progress of particle physics is shaped by the ordnance available in the arsenal of the experimentalist.

"A recent Brookhaven experiment . . . on the weak interactions of neutral K mesons seems to have provided evidence that nature does not in fact possess symmetry under what had been thought to be one of her most cherished transformations, the simultaneous interchange of particles and antiparticles (C) and of right and left (P) . . . . Who cares? Macroscopic phenomena will continue to be governed by an irreversible increase of entropy whether or not the weak interactions are time-reversal invariant. Are the symmetries of elementary particle physics just one more interesting area for scientific study, neither more nor less important than any other?

"I believe that such questions must be answered on the assumption that nature has absolute laws of great simplicity, from which all the sciences flow in an ordered hierarchy. Thus, working backward, the nervous system has evolved as it has because of certain facts of chemistry and classical physics, which themselves follow from the ordinary quantum mechanics of nuclei, electrons, and photons, which itself follows from—what? All scientists accept this ordering as a tool in their work—for instance, the biologist discovers laws governing life which are stated in purely biological terms, but (ever since the synthesis of urea) he is quick to reject any hypothesis which could not possibly have a basis in the physical sciences further up the hierarchy.

"It is generally recognized that the sciences furthest up this hierarchy at present are elementary particle physics and cosmology. The discovery that next moves up closer to the ultimate laws of nature will almost certainly be made in one (or hopefully both) of these two fields. For this reason particle physics and cosmology have an intrinsic interest not shared with any other science; we are interested in the structure of DNA because we are alive ourselves; we are interested in phase transitions because they are a challenge to calculate and have practical importance; but we are interested in the question of time-reversal invariance because it brings us as close as now possible to the absolute logical structure of the universe. It is a pity that new accelerators and telescopes happen to be expensive, but not to build them would mean that science must renounce the highest of its objectives, the discovery of the laws of nature."

C. N. Yang, of the Institute for Advanced Study, states: "While one may raise the question whether knowledge gained in such studies has invariably benefited mankind, one cannot possibly deny that high energy physics seeks knowledge that is of supreme importance in the eternal interaction between man and his surrounding, an interaction that is a major part of human history. . . . We are clearly at the beginning of a new era of sophisticated technology where man begins to manipulate smaller and smaller units down to atomic and subatomic sizes. Such technology is still in its infancy. Its further development will require increasingly more sensitive and more subtle *control*. High energy physics, in studying the most minute distances and the shortest time intervals, should be expected to serve as a source of new ideas and new stimulation that will be essential in such technological developments."

The question arises as to whether the accelerator art can be extended to the super-high-energy range and whether, if such machines were available, high energy physics would have available the techniques to exploit them. These questions are answered in the article "Experimental Feasibility at Super High Energies" by the author with the conclusion that accelerators up to and probably above 1000 Bev are indeed feasible and can be effectively used.

—LUKE C. L. YUAN

H. A. Bethe Discusses High Energy Physics

High energy physics is undoubtedly today the frontier of physics. The discoveries in this field of study contribute most of the advance of our fundamental understanding of nature.

When I began my career as a physicist the frontier of the subject was in atomic physics. In 1926–30, most problems of atomic physics were solved by the application of quantum mechanics. It was astonishing how quickly the subject developed, and how every problem yielded to theoretical treatment. Physicists were spoiled by this period of amazing success of a single theoretical approach. The same approach gave us in addition the theory of the chemical bond, and an understanding of the solid state. Solid state theory is still a very fruitful field, giving many important advances and new insights into the working of the nonrelativistic Schrödinger equation for complicated systems. However, one could hardly claim that it advances our *fundamental* understanding of nature.

The 1930's and '40s were characterized by the advance of nuclear physics. There the task was to find the force between nucleons, and the quantum states of nuclei under the influence of this force. Neither of these tasks is completed, and much interesting work remains to be done.

Particle physics, or high energy physics, is different from atomic and nuclear physics in being far removed from our daily experience. It is easy to justify work in atomic physics: The subject has direct intellectual appeal because it explains so much of the world in which we live. Moreover, its applications in chemistry and solid state physics are of great technical importance, as are some direct applications of atomic physics such as lasers. Because of these many practical applications, much of the progress in the field is now being made by industry. In nuclear physics, the practical application of nuclear power and atomic weapons is too well known to need discussion.

No such practical application has appeared, or is likely to appear, for particle physics. Indeed the processes observed in particle physics may not occur in nature outside the lab to any important extent. (They do of course occur when cosmic rays interact with matter, and may conceivably take place in those distant astronomical objects which emit energy far beyond the usual amount for a galaxy.)

There are at least three reasons for the fascination of particle physics. One is the conviction that this is indeed the most basic field of knowledge in the physical world. We want to know and to understand, and no other field will give us such deep understanding. The second reason is that particle physics will give us the basis for the theoretical treatment of another field, nuclear physics, which *is* related to the world as we know it. To find the nuclear force we must know the interaction of subnuclear particles with nucleons.

H. A. Bethe

The third reason is just the very difficulty of the theory. In contrast to atomic physics which yielded to one single theoretical approach, the Schrödinger equation, it has been necessary to try many different approaches to particle physics which supplement each other. One of the difficulties is the great strength of the forces which makes approximation methods inapplicable—and approximation methods were the key to success of most of atomic theory. Secondly, we do not have any differential or integral equation in closed form, a fact closely related with the possibility of creating any number of additional particles in the interaction of two high energy particles. A third difficulty is the essential involvement of relativity theory together with the presence of many particles. Interesting theoretical methods have however been developed for dealing with some of these problems, such as dispersion theory, Regge pole theory and many others. But because of the many difficulties of the theory, it is very hard to deduce the fundamental interactions from a given experimental result, such as the cross section of a certain process.

The difficulty of the theory puts greater demands on the experimenter. His experiments must allow a simple interpretation without involving complicated and therefore doubtful theoretical steps. A good example is the Brookhaven neutrino experiment which showed directly the existence of two different neutrinos, associated respectively with the electron and the μ meson. In the field of strong interactions, recent work has concentrated on finding resonances and their properties (charge, angular momentum, parity, etc.), rather than just to measure cross sections. This demand to get easily interpretable results is combined with the difficulty of experimentation—extremely large apparatus, small number of events, often many particles in one event, some of which are neutral and therefore invisible, etc.

In spite of these difficulties, both theoretical and experimental, particle physics has already given results of great beauty. While at first sight there seems to be a confusing and overwhelming multitude of different particles, there appears to be a great amount of symmetry in their properties. Gell-Mann and others have shown that the particles can be grouped into families of 8 or 10 (perhaps in some cases more). The particles in each family have close resemblance, and the structure of the various families is either analogous or closely related.

This beautiful theory, known as SU3 symmetry, could be developed only after hundreds of painstaking experimental papers, and dozens of unsuccessful theoretical attempts at clarification. In addition to classifying the then-known particles, the theory predicted a new particle, the Ω^-, which was discovered at Brookhaven early in 1964. Both the theory and the experimental work have been written up in very good articles in the *Scientific American* [C. Chew, M. Gell-Mann, A. Rosenfeld, **210**, No. 2, 36 (1964); W. B. Fowler and N. P. Samios, **211**, No. 4, 36 (1964)]. The SU3 theory is probably not the last word in particle physics. It leaves many problems unexplained, in particular it gives a very incomplete account of particle masses and reaction cross sections.

Frequently the study of particle physics has given unexpected insights into the earlier branches of physics. For instance, the study of the decay of K mesons made it likely that in this process parity was not conserved. This led Lee and Yang to conjecture that the same might be true in other weak interactions, particularly in the beta decay of nuclei. Although beta decay had been studied for at least 25 years before, it was only after this suggestion that the violation of parity in this process was experimentally found. As a more recent example, in 1964 Fitch *et al.* discovered that in at least one decay mode of a K meson, even the reversibility of time may not be satisfied. Thus particle physics touches the most fundamental concepts of space and time.

It is no surprise that particle physics has attracted the most ambitious, and the best brains among the young physicists. It would be wrong to support this branch to the exclusion of others. There are challenging problems in nuclear physics, solid state physics, and other branches. I myself have devoted the last ten years of my research to low energy nuclear physics. But I believe that particle physics deserves the greatest support among all the branches of our science because it gives the most fundamental insights.

A Defence by Victor F. Weisskopf

Today the development of science has arrived at a critical stage. The cost of science in terms of money and manpower has reached a point where society is beginning to question its further uninhibited growth.

So far the cost of science has been negligibly small. All basic scientific activity ever undertaken from the times of Archimedes until today amounts, in terms of money expenditure, to less than ten days' output of the industrial world, an amount which is below the yearly increase of world production. This represents an impressive rate of return on a capital investment if one considers that almost all industrial production today is a consequence of basic scientific research. Still, it is true that the requirements of modern basic research are beginning to be substantial and a discussion becomes unavoidable of the importance of basic science and of the relative importance of its different branches.

Clearly, the main targets of attack are the most expensive branches which, in addition, have a certain flavour of "uselessness", that is high-energy physics and astronomy. Modern astronomy, however, has the advantage of being connected with "space"; it therefore profits from the present emphasis on everything that is related to space science. Clearly, this emphasis is not exclusively based on arguments of scientific merit. High-energy physics or —as it should better be named—subnuclear physics no longer enjoys such extraneous support, after having ridden on the coat-tails of nuclear energy for a number of years.

Looking at the development of science in the Twentieth Century one can distinguish two trends, which I will call "intensive" and "extensive" research, lacking a better terminology. In short: intensive research goes for the fundamental laws, extensive research goes for the explanation of phenomena in terms of known fundamental laws. As always, distinctions of this kind are not unambiguous, but they are clear in most cases. Solid state physics, plasma physics and perhaps also biology are extensive. High-energy physics and a good part of nuclear physics are intensive.

There is always much less intensive research going on than extensive. Once new fundamental laws are discovered, a large and everincreasing activity begins in order to apply the discoveries to hitherto unexplained phenomena.

Thus, there are two dimensions to basic research. The frontier of science extends all along a long line from the newest and most modern intensive research, over the extensive research which was recently spawned by the intensive research of yesterday, to the broad and well-developed web of extensive research activities based on intensive research of past decades.

One can easily distinguish four important steps of intensive research during this century: electrodynamics and relativity, quantum theory of the atom, nuclear physics and recently subnuclear physics. The extensive dimensions of electrodynamics, relativity and quantum theory reach very far today and are constantly expanding. Nuclear physics has already a large extensive part in the detailed studies of nuclear structure and in its astrophysical applications. Subnuclear physics is still mostly intensive in its character.

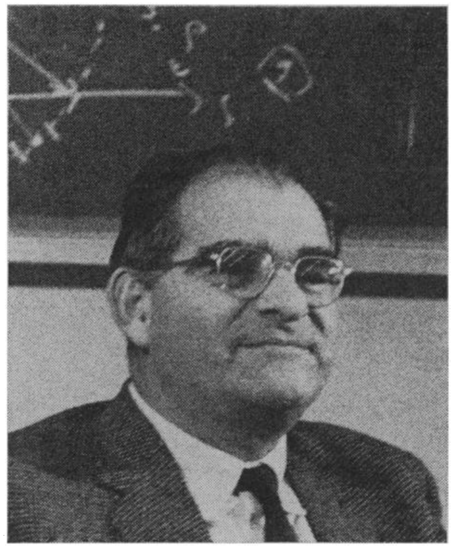
Each part of this scientific frontier is of importance. It would be most dangerous to neglect some parts relative to others. It is often argued that subnuclear physics should be given less support because this field leads to very little extensive research and because it attracts too large a proportion of clever scientists, and because the cost per scientist is much higher than in many other parts of the scientific frontier. These reasons, however, are inherent in the fact that subnuclear research is at the frontier of intensive research.

Obviously, the most advanced part of intensive research has yet very little bearing upon the understanding of other phenomena, and therefore its extensive component is small. After all, one is at the very beginning of understanding what is going on at the subnuclear frontier itself. Clearly, the same situation existed at earlier periods when other fundamental discoveries were at the frontier of science. Faraday did not know that electricity is the basis of the structure of matter; when the first steps were made towards an understanding of atomic spectra, nobody knew that this would lead to a complete understanding of chemical reactions. Thus the extensive effect of subnuclear physics is not yet visible, but even today it seems already probable that subnuclear phenomena are important for the understanding of the recently discovered galactic explosions.

The frontier of intensive research has always attracted a certain group of very clever scientists. To work in an uncharted field, to discover new laws of nature and completely new types of phenomena is a great lure for a scientist. One is placed at the spearhead of a great and successful tradition ranging from Galilei, Newton, Maxwell to Einstein, Bohr, Dirac and Heisenberg. It is improbable, however, that this field should in fact ever deprive other fields of science of skilled manpower. It is by its very nature a limited field. Competition is heavy, success is rare and depends more often than not on luck and opportunity. Many of the best scientific brains avoid this field because of the narrow choice of activities.

The high cost of subnuclear physics comes from the fact that it deals with new phenomena which were not previously observed. Subnuclear physics requires the study of matter under new conditions. As science progresses, these conditions become increasingly different from normal conditions on earth. Nuclear physics deals with intrastellar conditions and subnuclear physics submits matter to even more abnormal conditions. Obviously, it is increasingly expensive to create increasingly abnormal environments in a laboratory.

There is today a clear danger that the alleged narrowness and the high cost of subnuclear physics will, in fact, retard its development compared to other fields at the scientific frontier. Already the *Physical Review* shows a stronger increase in the number of



Victor F. Weisskopf

solid state physics papers compared to nuclear physics papers. This occurs just at a time when subnuclear physics begins to reveal the existence of a new world of phenomena within the nucleons. We see today the birth of a third spectroscopy compiling the excited quantum states not of atomic systems or of atomic nuclei, but of the nucleon itself. We find today the first indications of regularities in these level schemes, which will soon lead to an insight into the structure within the nucleon. This insight is bound to bring us nearer to the understanding of some of the most fundamental unsolved questions. Let us list three groups of such questions:

Today we understand the behaviour of matter on the basis of the interaction of atomic nuclei and electrons. But the basic question remains: why is it that the proton, the neutron and the electron are the elementary particles which make up matter under terrestrial conditions? Why are these particles, together with the light quantum and the neutrino, the most stable forms in a long series of particles including the hyperons, the numerous bosons and the heavy electrons? These questions concern the basis of everything scientific. As long as they are not answered, the structure of any form of matter remains essentially not understood. The great triumph of quantum theory was the explanation of the characteristic properties of the elements on the basis of the recognition that the field of a given electric charge admits only certain well defined quantum states of the electron. This idea is fundamental to all atomic physics, chemistry and molecular biology. However, it is valid only because of the existence of identical electrons and protons with fixed and well defined charges and masses. In fact, quantum theory does not really explain the existence of characteristic intrinsic properties of each element; it deduces it from another unexplained set of facts: the existence of a small number of elementary particles with their own characteristic intrinsic properties. Hence, the basic problem which underlies all physical sciences, that of the structure of matter, is still unsolved. It is precisely that problem which is attacked by subnuclear physics.

Another fundamental set of questions is connected with the problem of the different types of interaction between material particles. Physics has solved the problem of unifying a large number of interactions, such as electric and magnetic forces, chemical forces, cohesional forces, capillary forces, etc., all of which are reducible to the quantum effects of electric attraction between nuclei and electrons. But there is still no connection seen between nuclear, electromagnetic, gravitational and weak interactions. Hence, the task of a consistent understanding of nature has only begun and is in need of further development. It is again mainly subnuclear physics which attacks these problems; theoretical research in relativity theory and astronomical research into the structure of the universe will contribute to the solution.

Finally, the same three fields of research are about to tackle the problems of the history of the universe. The question of the origin of matter can already be discussed on scientific grounds. So far, rational ideas are developed only concerning the element formation from a gas of protons and electrons. But the problem of the origin of this gas begins to acquire some scientific aspects with the discovery of matter under extreme conditions of high energy at the centre of the galaxies. These phenomena are obviously connected with the interaction of particles at very high energy, as studied in subnuclear physics.

We are facing today a situation where all this promising research is threatened to be slowed down by constraining financial support to high-energy physics. And this constraint is based, partially at least, on a claim that the aim of this field is narrow and restricted. The three above-mentioned groups of unsolved questions should be sufficient to invalidate this claim. It is granted that further progress, say, in biology or in solid state physics is possible without any further research into the subnuclear field. But let there be no doubt that the style of the scientific community would change its character if the frontier of intensive research were hampered. It would subtly change towards over-emphasis on extensive research, and this would harm all fields of science. A spirit would be fostered, different from the one which created modern science, if basic questions that can be answered are left unanswered or are neglected by lack of attention. The questions remain, they cannot be overlooked.

This different spirit will do most harm in the education of young scientists. The study of science is based upon a burning interest for fundamental problems. The attitude of students would be perverted if they are not constantly aware of a lively quest for the solution of the basic problems of science. Even the scientist who will devote his life to purely extensive research must be aware of the existence and the spirit of intensive research. The reason is that, even in the most extensive research, at every step there is always an intensive component: at each unsolved problem one must go back to some fundamental idea, one must try to see more of the essence of the problem. This is an attitude which can be fostered and maintained only if intensive and extensive research has an equal standing in the scientific community. There is one broad front in science and each part of it must be pushed forward with full vigour.

We find strong support today for space technology, which may allow us to explore the unknown parts of the solar system. Exploration of the unknown was always a strong component of human endeavour in our modern civilization. But it must go together, as it always did, with an equally strong component: the explanation of the unknown in whatever form it faces us.

At the beginning of the Sixteenth Century, when the scientific era began, Magellan performed the first trip around the earth. But also in the same period Copernicus published his work on the motion of the planets.

Julian Schwinger on the Future of Fundamental Physics

The scientific level of any period is epitomized by the current attitude toward the fundamental properties of matter. The world view of the physicist sets the style of the technology and the culture of the society, and gives direction to future progress. Would mankind now stand on the threshold of the pathway to the stars without the astronomical and mechanical insights that marked the beginning of the scientific age? The quest for understanding has led outwardly to the galaxies and inwardly to the atom and then to the nucleus. Now it is the subnuclear world that is being actively explored. The goal here is not merely an organizing principle for subnuclear particles, a new periodic table of the elements, interesting and important as that may be. Rather we are groping toward a new concept of matter, one which will unify and transcend what are now understood only as separate and unrelated aspects of natural phenomena.

In past triumphs, physics has unified light with electromagnetism, mass with energy, and comprehended chemistry and the mechanical-thermal properties of bulk matter in the atomic laws of quantum mechanics. But the fundamental problems remain. What is the role of gravitation in coupling the remote stars to the atom? Can one understand the magnitude of the unit of electrical charge? These are traditional queries. Recent research has provoked a whole battery of additional questions. What is the relation between the newly revealed internal degrees of freedom and space-time? How can one connect the diverse interactions, of different strengths and characteristics, that are required to account for the birth and death of the subnucleonic particles? But perhaps the most important question concerns whether these particles must be accepted as basic and unanalyzable, to be described only in their own framework, or whether there exists a simpler and more fundamental substructure, a deeper level of description and understanding. These alternatives have been presented before in the history of physics. At the close of the nineteenth century it was strongly argued that the properties of bulk matter should not be accounted for by the characteristics of unobservable and hypothetical microscopic entities. Owing to the continued development of experimental techniques, this limited viewpoint had to be discarded and the atomic theory triumphed. A similar decision can only be given again if the tools will be at hand to continue the penetration into the totally new, totally unpredictable world of the microcosmos. And one should not overlook how fateful a decision to curtail the continued development of an essential element of the society can be. By the fifteenth century, the Chinese had developed a mastery of ocean voyaging far beyond anything existing in Europe. Then, in an abrupt change of intellectual climate, the insular party at court took control. The great ships were burnt and the crews disbanded. It was in those years that small Portuguese ships rounded the Cape of Good Hope.

High Energy Physics and the Rest of Physical Science: G. C. Wick

It is very difficult for me to add anything interesting to what others have already said so well about high energy physics, about its being the "frontier of physics" or that part of research in physics that explores the least known realm of Nature, the realm where one expects to make the most fundamental findings. Others have already pointed out, that to regard this kind of search as less important because of its alleged "remoteness from the rest of physical science" is in direct contradiction with the whole spirit of scientific inquiry; to adopt such an attitude would be, indeed, disastrous for the style of the whole scientific community.

I wish, therefore, to concentrate my attention on just one point. I think high energy physicists are much too tolerant if they accept the statement that high-energy physics is "remote" from the rest of physics, from chemistry, from biological science, and so on; in other words, the statement that discoveries in the field of elementary particles, intrinsically interesting as

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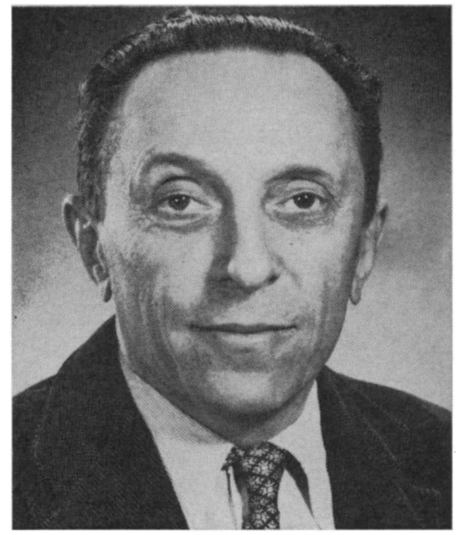
they are, will have little effect on the other branches of science.

I wish to question this statement. I want to point out, that when such statements are made, people usually hasten to concede that judgments of this sort can be destroyed by new findings. My contention is that the possibility of new findings is the crux of the whole matter. The point of all truly fundamental research is, and always must be, that one doesn't know what one is going to find, and also, what new ideas will come out in the process, hence one also doesn't know what the possible consequences will be. True enough, one does not enter into a line of research blindly, one has certain ideas about questions one wants to have answered, and it so happens that none of the questions high energy physicists are asking themselves now seem much related to other branches of physics; but as we all know, it usually turns out that the questions one had in mind were not quite the right ones, and as one proceeds further, new questions come up which are more pertinent and more interesting. Often these new questions are quite unexpected. One went in to find gold, and instead one finds oil or something which turns out to be just as valuable. What I am driving at, is that it is the essence of true pioneering research, that its results are wholly *unpredictable*. Therefore, it just does not make sense to ask: what will you do with your findings? One just goes ahead because the questions seem deep and interesting, and one wants to know the answers. The question—what will you do with them—is something one was used to hearing from engineers, industrialists, etc., i.e. from so-called "practical" people. We know, however, that it just is not a practical question at all, because it cannot be answered. We also thought people had learned by now that this is not because pure research has little practical consequence; in fact, just the opposite is true. But all pure research is just a big marvelous gamble, in fact the only gamble so far invented that really makes sense.

Now that old worn-out question is asked again, but not by "practical" people, but by fellow scientists from other branches of science, who want to know what is in it for them. My answer is: you should have a little patience. I believe that even now one can predict with confidence that discoveries in this field will have a major influence on other branches of science. The fact is that no major advance in physics, in the past, has failed to affect the other sciences profoundly. Who would have thought sixty years ago, that 100 kilovolt cathode rays could be of any interest to biology? Yet the electron microscope is now one of the major tools in that field. Who would have thought that 8 Mev alpha-particles could produce effects of more than marginal interest to the chemist, such as causing a crystal to become yellowish, or something of that sort? After all, the chemist's world is a "very low energy world," just a few dozen electron volts will destroy any molecule. What do you want to do for chemistry with millions of electron volts? Yet Rutherford's insistence on playing with alpha-particles has changed the whole face of chemistry. Now, of course, as we all know, Rutherford's experiments did not cost much money. But what has money got to do with it? An irrelevant question does not become meaningful just because money is involved. Quite the contrary, I feel that, just because a lot of money is involved, one should be very careful to ask intelligent, meaningful questions.



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I would like to contradict, as an example, an opinion which is often accepted much too easily, namely that high energy physics is not relevant to ordinary nuclear physics, which, as everybody agrees, is not only interesting in itself, but also important for a lot of other reasons. First let me note, that it is perfectly useless to try to draw a line and say: well, perhaps "meson factories" will be useful, but that is not *really* high energy physics. I think this is nonsense; it is bad enough to deny the essential unity of science, but to try to split an already specialized field into two, and to really believe that the two parts can go on independently without deeply affecting each other, seems to me the height of improbability. As a matter of fact, it seems to me very likely that if one ever arrives at a really deep understanding of the structure of the so-called elementary particles, our whole way of looking at the structure of plain ordinary nuclei and their lowest levels cannot fail to be affected very deeply. After all, nuclei do consist of protons and neutrons, which are elementary particles, and are kept together by interactions of the type that high energy physics is concerned with.

In conclusion, I would like to say: the inter-relation of High and Low energy nuclear physics is much deeper, much more varied and may be more full of surprises than one generally concedes. Let us explore it.